Open Peer Commentaries

on Kristian Martiny's "Varela's Radical Proposal: How to Embody and Open Up Cognitive Science"

Loud Crisis, Quiet Crisis: Varela's Proposal Resonates in Contemporary Psychological Science

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> Upshot • Varela's proposal that science should be open to the phenomena of experience is radical primarily because of the strangely constrained practices of psychological science. Methodological and professional crises within contemporary psychological science resonate with the issues raised by Varela and others, and addressing them effectively will mean following Varela's, and Martiny's, advice.

« 1 » Kristian Martiny opens his target article with mention of the significant shift toward openness within contemporary science. Within psychological science, this shift toward openness is closely associated with an on-going crisis of trust and confidence. Open methods, open review, open analysis, are all part of an attempt to address "perverse incentives" (Bouter 2015), and problematic practice in psychological research (Nosek, Spies & Motyl 2012), whereby professional reputation and awards of promotion often depend more on the sheer number or novelty of results than their accuracy or reliability. Psychologists have increasingly recognised flaws in standard practices, and the fragility of conclusions in the face of under-powered, improperly controlled, or badly analysed experiments (Nosek & Bar-Anan 2012).

« 2 » The past few years have seen significant effort made in raising awareness of these failings and to identify effective means of addressing them. The diagnoses for these problems have focused almost exclusively on issues of methodology and professionalism. Questionable research practices (QRPs), under-powered experiments, publication bias, and other systemic problems are to be mitigated through improved education in statistics, better checking of data, much greater transparency in procedures, materials, and data (Nosek, Spies & Motyl 2012). In addition, the changing of incentive structures to focus on methodological soundness over novelty of findings or statistical significance, through such innovations as pre-registration of studies (Chambers 2013; Nosek & Lakens 2014), should help to improve the state of our research culture and professionalism, by providing better goals to aim for.

« 3 » Almost all of the conversation concerning this crisis in the science is directed at such practical and pragmatic issues. The methodological concerns, and attendant professionalism-related problems, are a loud crisis. But there is also a quiet crisis in psychological science, one brushed against by Martiny's discussion of the need for a more radical change to our approach: *we are just not good at theory*.[1]

1| Cf. "Theory, and why it's time psychology got one" by Andrew Wilson, retrieved 13 October 2017 from http://psychsciencenotes.blogspot.com/2011/11/theory-and-why-its-time-psychology-got.html

« 4 » Psychologists distinguish very clearly between the reliability and validity of measurements (Haslam & McGarty 2003 and other textbooks on psychological research methods). Both of these concepts come in several different flavours, but the broad description of each is as follows: *reliability* concerns whether the measure is consistent (and therefore measuring the same thing each time it is used); *validity* concerns whether you are measuring what you think you are measuring.

« 5 » Of these two concepts, it is easier to think about reliability, and easier to address it. Most of the changes to practice proposed as mitigations of QRPs or problematic incentives address the reliability of our practices and measurements. This is the right place to start. Until you are consistently measuring or observing something, you cannot hope to properly address the question of what that something is. We need reliability in our methods.

« 6 » Validity, on the other hand, is determined by how good our theories are. In order to be confident that we are measuring what we think we are measuring, however reliably, we must have a clear and coherent concept of what it is that is being measured. We should also be able to explain why the measurements in question are appropriate to that concept.

« 7 » The concerns about the clear limitations of our current psychological knowledge focus almost entirely on factors affecting the reliability of the methods used to develop that knowledge. This is a loud crisis, one that has seen arguments, proposed solutions, counter-arguments, and controversies flow through journals, blogs, and twitter.

It has spawned enormous research efforts (Open Science Collaboration 2012, 2015), and even professional societies.[2]

« 8 » A currently much quieter crisis is that the problems with these methods are not new, and the existing body of knowledge developed using those flawed methods, which determines how we frame questions, design procedures, and interpret results, cannot be drawn upon without great care. The current revision of approaches to methods in our science needs a complementary revision of our approach to theories.

« 9 » This is not a novel observation. Like the statistical and methodological concerns now commanding the discipline's attention, questions about theory have been raised numerous times in the history of psychological science (e.g., Fiedler 1991; Gigerenzer 1998, 2010; Meehl 1978; Watkins 1984).

« 10 » The theories we have are also a product of our practices, but not simply those of statistics, or experimental control. Rather, they are our practices of research question identification, results interpretation, and explanation (Danziger 1994; Kuhn 1970). Theories are driven by the phenomena we choose to investigate, the means by which we choose to engage with them, and the techniques that frame our interpretation of them. Addressing this issue, improving our theories, will require a kind of openness, and honesty with ourselves, akin to that advocated by Francisco Varela, and others, but one that is radical only within the strange confines of psychological science.

« 11 » Decades ago, researchers were already noticing that psychology had closed its doors to the world around it, paying close attention only to behaviours that go on within the sterile environments of its laboratories, with little heed paid to "the psychologist-free environment" (Barker 1968: 4). James Gibson (1967) lambasted psychologists for being too closed-minded about the phenomena of interest. Regardless of whether cognitivist or behaviourist, psychologists had decided that explanations involved the characterisation of the relationships between stimuli and responses, and not the manner in which an organism is embedded in and coordinates its activities with the ecosystem of energy, objects, and other patterns of activity around them (Gibson 1960, 1966, 1986). As a result, rather than developing theories of behaviour, we developed micro-accounts of encapsulated laboratory-created phenomena.

« 12 » Gerd Gigerenzer (1998, 2010) has noted for years the striking lack of interest in building theories, or integrating them, within professional psychological research. This runs counter to practice in such disciplines as chemistry, biology, and physics. A failure to set theories beside one another, to run experiments that directly compare their power to explain behaviour, leaves psychological studies isolated from one another, with little to nothing by way of over-arching structure to explanations. This lack of structure within our collection of theories, in turn, makes it next to impossible to draw clear lines of implicature between research conducted and the base phenomena of everyday behaviour and mental life.

« 13 » Martiny's re-emphasis of Varela's, and others,' exhortations to be open to life as it is experienced would therefore not be considered radical at all in other sciences – any more than relating biological science back to the animals living in their habitats would be considered a radical proposal to biologists. This is, after all, the very stuff of psychology! That it can be considered radical, and is met with such distress, or distrust, by psychologists and other cognitive scientists, is an indication of how carefully we have conspired to limit our investigations to behaviour that can be elicited within the blandly empty and white-painted rooms of university psychological laboratories.

« 14 » Following Gibson's work, Alan Costall (2017) has recently made the radical claim that we must do away with psychology as a discipline altogether, a claim driven by precisely the kind of openness, and sensitivity to life, behaviour, and the psyche, that Varela suggests. He argues that under open-minded scrutiny, the kind of encapsulated psychological subject assumed by modern Western science (and society), simply does not show up, and interpretations (or scientific disciplines) that depend upon its existence become problematic.

« 15 » What, then, do we do? Well, in the first instance we note that Varela was not alone in arguing for such an openness to the world, to phenomena, and phenomenology, typically foreclosed upon by psychologists. Enactive theorists have allies amongst Gibsonians (see, e.g., Chemero 2009; though any such alliance will not be without its wrinkles; Fultot, Nie & Carello 2016).

« 16 » Other psychologists have also recently argued for the production of knowledge of the cognitive system, not in the abstract, but in its messy and variegated details, precisely mirroring the recommendation of Varela (1999) in his lecture series *Ethical Know-How*. The work of psychologists studying developmental (e.g., Fischer et al. 1993) and individual differences (Mischel & Shoda 2010), along with those who have identified fundamental mathematical problems with the kinds of inferences upon which a great deal of the extant research literature depends (Molenaar 2008), has been used to argue for a "science of the individual" (Molenaar 2004; Rose, Rouhani & Fischer 2013; Rose 2016). A recognition of, and open-eyed grappling with, psychological phenomena as textured, dynamic, and context-dependent.

« 17 » There exists, at present, a readiness to change the way psychological science is done. Achieving this requires not only being open to the phenomena of our science (as any good science should), but also to the interdisciplinary coalition of other scientists with whom we share common, if often unacknowledged, ground.

2| Cf. Society for the Improvement of Psychological Science, http://improvingpsych.org

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