

COMMENTARY

Health behavior change: a field just picking up speed. A comment on Ogden (2016)

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A central tension described in Ogden's (2016) editorial is that between the variability (the 'mess') that appears inherent to human behavior – in this case in response to health-related interventions – and the determinacy science seeks. For those studying and modeling human behavior, it is hard not to be dumfounded by its 'ill-fitting, multiple-influenced, volatile, situational and porous nature' (Rowson, 2015, p. 318). Yet, individual behavior is far too important an affair today not to be approached scientifically, thus seeking some level of predictability. Ogden's argument is that as this process is advancing, specifically through the effort of systematization illustrated by the work of Michie et al. (e.g., Michie, Atkins, & West, 2014), so is a profession, ultimately an entire field, at risk of perishing. My own view is that, as a scientific field, health behavior change is in its early stages and it is far too early for major paradigm shifts. Instead, I foresee that progress in this field will contribute to improve interventions for all, but where complexity and insufficient evidence will regularly force interventionists to rely on other sources of information to make decisions. Indeed, this is precisely where I would relocate the central tension of the field: to the fact that while health psychologists are in demand to build interventions that work *today*, currently these have to be based on rather limited data about what works, why, for whom, and under which circumstances.

For example, I question the value of looking *post hoc* at published interventions – most of which have been poorly designed and/or reported (Prestwich et al., 2014) – and coding them based on the Behavior Change Techniques Taxonomy to arrive at 'effective behavior change techniques (BCTs)' through meta-analysis (e.g., Michie, Abraham, Whittington, McAteer, & Gupta, 2009). Aiming to design evidence-based interventions, researchers have often referred to 'effective BCTs' (i.e., BCTs *known to change behavior*), apparently unaware of two key considerations. First, that the effectiveness of any BCT is dependent on a number of parameters, including not only the target behavior but also the population, setting, mode of delivery, and the presence of and interaction with other operating BCTs and related mechanisms of action (c.f., Kok et al., 2015). Second that, unfortunately, we still do not know which combinations of BCTs and related mechanisms work best in which circumstances. However, to get there I do not see a good substitute for a path that resembles this: explicitly deciding which theory or theories to use¹; formally identifying the theory-derived mechanisms of action and the techniques/methods to target them; effectively training interventionists to deliver them and/or adequately translating them to other modes of delivery (c.f., West & Michie, 2016); ensuring validity and fidelity at each step of the way; and finally evaluating for whom and under what circumstances, and/or modes of delivery, results are most convincing.

The complexity inherent to this process is undeniably great. And further complicated by the long timeframes associated with changes in the so-called *lifestyle behaviors*, and time is something that theoretical models often struggle to accommodate. Therefore, significant progress will require an equally great effort from the scientific community, including the capacity to produce, organize,

and rigorously analyze large amounts of information in a systematic way, for many years. The nature of research is also evolving, as evidenced by suggestions to complement full-fledged behavior change interventions and RCTs with other sources of information, for example, smaller controlled experiments or factorial designs using a reduced number of parameters and BCTs (Peters, de Bruin, & Crutzen, 2015), testing just-in-time adaptive interventions (Spruijt-Metz et al., 2015), or involving participants in the design of digitally delivered interventions from the get-go (Yardley, Morrison, Bradbury, & Muller, 2015). Overall, these and other initiatives (e.g., intervention mapping; Kok et al., 2015) are methods and tools whose ultimate goal is allowing researchers to collect and accumulate good scientific evidence. It is certainly possible that there is too much variance and too many sources of error in some of the most challenging behavior change scenarios for research to be definitive about it. And scientists should be exceedingly diligent in forewarning users about what we know and do not know of the applied utility of current theories, models, and techniques and encourage a process of *intelligent filtering and interpretation* of all recommendations for their use (Cribb, 2015). All this said, it is not clear to me what Prof. Ogden sees as the best alternative *scientific approach* to the development of better health behavior change interventions.

A separate question is whether theories should progressively be integrated into more parsimonious models. While for improving statistical *prediction* of behavior, modeling short, theory-eclectic sets of 'best of' predictors may do the trick, we clearly want more than prediction. We want behavior to be coherently and causally *explained*. And we want models that ultimately help *influence* behavior. From a practical standpoint, reducing the apparent construct redundancy and the burden of having dozens of psychological theories to choose from is also appealing. However, I fear that in this path we may be losing both critical detail and internal coherence, which ultimately may limit the ability to design better interventions. I will briefly expand on these two points.

Many of the theories being 'integrated' are much richer than what is explicit in the simplified models. Human behavior is an intricate nut to crack and my view is that we cannot afford dispensing with the richness and insight contained in many frameworks, often acquired through decades of theoretical and empirical testing. For illustration, I will use the case of self-determination theory (SDT), a theory that is mentioned in Ogden's editorial and sometimes used in integrative models. It is a broad theory of human motivation covering elements of individual differences, goals and aspirations, interpersonal dynamics, psychological needs, and psychological well-being (Deci & Ryan, 1985). However, autonomous motivation is often the only construct carried over to integrated models (e.g., Hagger & Chatzisarantis, 2014). Consequently, many of the subtleties present in the source material are likely lost in translation, which could reflect negatively on interventions based on these models. An example is the fact that autonomous motivation is already a composite concept including qualitatively different types of motives (three, according to SDT) with partially distinct intervention and psychological correlates.

My concern with internal coherence relates to whether the theories being integrated have a sufficient meta-theoretical common base. To use the same example, SDT has its roots in humanistic psychology and is an 'organismic' theory that assumes innate tendencies and needs (Ryan & Deci, 2000). By contrast, other social cognitive theories commonly applied to health behavior, such as those rooted in learning theory, make no such assumptions (e.g., Bandura, 2008). While for box-and-arrows models these considerations may be secondary, especially if supported by strong correlational or structural equation data, differences in the meta-theoretical bases from which these constructs derived may collide in practice. As one very simple example, might an 'effective' technique to influence perceived social norms (from the Theory of Planned Behavior) be felt as pressuring and ultimately diminish participants' perceived autonomy (from SDT)? A related issue is the substitution of constructs from one theory by those from another (given their 'conceptual proximity') when in fact important differences between the constructs may exist (e.g., Rodgers, Markland, Selzler, Murray, & Wilson, 2014) and influence judgments about the selection of mediators in an intervention. Being familiar with the depth and detail of the research within the scope of many single theories,² it is

hard for me to come to terms with a scenario where broad, comprehensive theories are simplified for the sake of parsimony, while retaining the same potential for insight and progress.

In sum, I am not indifferent to some of the concerns expressed in Ogden's commentary and I also feel that efforts to systematize and integrate information must be balanced with preserving depth, detail, and diversity in the path to more completely understand, and more successfully influence, human behavior. In fact, when the authors of the behavior change wheel (BCW) write that their guide is only a starting point for intervention design and 'not a substitute for a detailed understanding of the behavior in question' (Michie et al., 2014, p. 14), I believe they are stating a similar position.

Finally, will the effort of systematizing intervention techniques result in improved practice by health professionals? As a warning sign, we have the case of Motivational Interviewing, where there is preliminary indication that 'manualized' interventions are less effective than those that are not (Lundahl, Kunz, Brownell, Tollefson, & Burke, 2010). Some may argue that it would perhaps be preferable to teach health professionals about the core theories and let them make their (hopefully, theory-congruent) choices on a patient-by-patient basis. Although it is not entirely explicit, this is perhaps the solution Prof. Ogden would prefer, namely for health psychologists. Unfortunately, despite the fact that it contains a very interesting empirical question, I do not see how this would lead to building the evidence base we need to have an impact on population health. And so perhaps a middle road must be sought. A road where formal, explicit, and rigorous scientific principles and methods are used to ascertain which theories, mechanisms, and techniques work best, for as many combinations of behaviors, populations, modes of delivery, and settings as possible. And where we also accept that results will remain variable, but stay hopeful that variability will progressively diminish. Indeed, on the issue of whether we have been stalled in recent decades dealing with behavioral problems around health, recent reviews present a not so negative scenario (Irvin & Kaplan, 2016; Lin et al., 2014) especially considering that Behavioral Medicine and Health Psychology are relatively new fields, and are dealing with a widely diverse set of behaviors (Nudelman & Shiloh, 2015) under fast-changing conditions (e.g., recent increases in use of mobile technologies).

Perhaps what separates a good from a bad health professional is the extent of their knowledge of and experience with the best theories, the critical mechanisms of actions, and the best techniques to be used in each behavior change scenario (c.f., Dixon & Johnston, 2010). But perhaps what will always separate an excellent practitioner from a good one will be the additional flexibility, intelligence, and the overall ability to use all that information – in relation with other human beings and in concrete situations – to navigate the untidy waters of human behavior and how it changes. Unfortunately, 'all that information' may be too little at the present time for many behavior change situations. And in these conditions, the BCW is certainly among the very best tools to guide the design of *real world* interventions, especially by those less familiar with the area. Of course, this should not detract behavior change *researchers* from using intervention work to test whatever mechanisms and whatever theories they see as most fruitful for a given behavior change problem, as long as they remain true to scientific principles and free from undue personal biases. After all, we do not have the evidence until we collect it! Nor should it detract health psychologists or other professionals from using their best clinical judgment in their practices, as long as they remain informed of, and prepared to apply the best scientific evidence at any given point. It may be that using more or less standardized behavior change interventions and techniques, especially by non-psychologists, will not result in *excellent* outcomes in many cases. But from where we stand today, I think there is considerable margin of progress for creating *good* interventions – those that have a positive and sustained impact on population health and well-being, and that can be successfully employed by a range of professionals involved in health care.

Notes

1. Note: This can be informed by suggestive empirical findings and/or by theoretical (or other) considerations, especially those related to the specificities of the target behavior. Alternatively, Michie et al. suggest we start

with a “behavioral diagnosis” and identify key mechanisms of action from the COM-B system or the theoretical domains framework (Michie et al., 2014).

2. Coincidentally – and generalized poor use of theory notwithstanding – there are indications that single-theory interventions may be more effective than multiple-theory ones (Gourlan et al., 2016; Prestwich et al., 2014).

Disclosure statement

The author is currently involved with Prof. Susan Michie and colleagues as an advisory board member on the project “Theories and Techniques of Behavior Change” (<https://www.ucl.ac.uk/health-psychology/research/theories-techniques>).

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