Journal of Behavioral Economics for Policy, Vol. 3, Special Issue, 5-7, 2019

Editors' introduction — What works: When & why are nudges sticky, scaleable and transferable?

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Abstract

Why isn't there a single discipline with its own experimental paradigms, and an overarching theoretical framework that consolidates findings from studies on behavioral interventions (e.g. nudges)? This reflects the frustrations of some, particularly practitioners, who simply want to know which methods will reliably lead to positive behavioral change at a population level. At the same time, they acknowledge that it is important to know why some methods work better than others in particular contexts, i.e. it's not just about the effect size of the intervention. Other concerns that are voiced include the fact that scientific studies conducted to test behavioral interventions often compare one or two interventions against a baseline, within a single context, with hypothetical decisions made, within a sample that is not always representative. Also, interventions are tested within a variable time frame (though rarely longitudinally) (Bauer & Reisch, 2019). Results from these studies ultimately represent the upper bound for the reliability, sustainability, and generalizability of the behavioral interventions that are being tested (Lin, Osman & Ashcroft, 2017).

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This is the arrival point of our special issue, for which a portion of the title refers to basic science concerns, and the rest of the title raises issues that are most obviously of applied scientific concern. What we mean here is that basic science is work driven by an understanding of how things work, vs. the need to find ways to apply science to solve a practical problem. In this introduction to our special issue we take the opportunity to get behind both types of concerns, by provoking the reader to think about the fact that research on behavioral interventions presents scientists with dilemmas about what function academic research on behavioral interventions currently *does* serve, and what it *can* or *should* serve.

A recent field study (Brandon, List, Metcalfe, Price & Rundhammer, 2019) examined the effectiveness of two behavioral interventions designed to reduce household energy consumption. The findings suggest that the two social comparison style interventions (allowing people to judge their decisions and actions relative to those of a comparator) were less effective individually than they were in combination. Their testing of two interventions separately and in combination against a control allowed the authors to speak to an issue of significant concern to policy makers i.e. competition between different types of interventions (e.g., soft behavioral interventions versus hard regulatory instruments – taxes, mandates, bans)[1]. Brandon et al. were able to say that both behavioral interventions not only complemented each other but produced an additive effect so that the contribution of both interventions together exceeded the contribution of each individually. The research serves as an excellent example of a field study that tests multiple interventions using a randomised control trial (RCT) method, on a substantially sized sample of actual households (n = 42,100). It is a shining example of exactly what the policy maker would want if they were faced with the question of: What behavioral interventions should we use to reduce household energy consumption at peak load usage? Moreover, it is the kind of study that can also help to answer other related applied questions, such as: What works in this context? Is it scalable?

While Brandon et al's (2019) study is classified as a basic social and economic science paper, it is essentially an applied study. Arguably, applied studies are often associated with insufficient focus on the underlying mechanisms that deliver successful policy and are not easily generalisable beyond a specific, narrow policy question. Brandon et al. take a social policy issue, and investigate the application of social policy interventions. Their focus is on gauging effectiveness, without explaining from an economic or psychological perspective why those interventions were chosen over other possible behavioral interventions. They do not fully explore why those interventions were effective, and why, in combination, they had a marginal additive effect in changing behavior. Regard-

[1] The authors refer this as "crowding-out" – a misappropriation of the term from economics and psychological theories of motivation – in which "crowding-out" has entirely different and highly precise meanings.

less, there are studies of this kind that are invaluable evidence for applied research, and are of obvious value to practitioners too, but they do not directly address basic science concerns, such as understanding why some interventions work, and why and when they don't.

In broadening the contribution of these types of studies, a different research tactic is needed and one option is to embrace a basic science approach. The focus of a basic science study isn't designed to specifically answer: Is the intervention scalable? Is the intervention transferable? A basic science pursuit should be in developing mechanistic models of behavioral interventions that can allow researchers to test different combinations of interventions in the same and different contexts armed with some hypotheses about which combinations of interventions are likely to work – both relative to each other, and relative to a control. Current theories and frameworks of behavioral change are of limited use, because they can only make vague recommendations. They provide no description of behavior that could be sufficiently characterised by a formal model precisely capturing properties that are considered critical, and from which model testing and prediction can be conducted (Grüne-Yanoff & Hertwig, 2016). A separate, but important point to consider is whether the current depiction of behavior that theories and frameworks adopt is at all accurate (Gigerenzer, 2015; Lin et al, 2017). Does it neglect other important psychological facets that need to be considered, such as personal agency (Gigerenzer, 2015; Lin et al, 2017; Osman, 2014)? Personal agency in this context refers to people's strong drive to preserve their sense of control by feeling that they are the principal agents in determining critical lifestyle decisions (Osman, 2014). The concern with the nudge programme and similar behavioral change approaches is that interventions are designed in such a way as to circumvent rather than support this critical component of our cognitive and social day-to-day functioning. The problems with vagueness and imprecision may be the result of a lack of incentive to be precise, because the field so far is preoccupied with finding out what works and what doesn't – only now realising it simply cannot provide a good answer to "Why does X work, and why does Y not work?" because it does not have a good theory, model or framework to do so.

To illustrate, imagine the following. There are two types of behavioural interventions being tested in a large-scale field study: one is a social comparison-based intervention; the other is a text-based simple provision of information prompt. Both are applied in the context of home energy usage and tested against the background conditions of a carbon tax and subsidy for green energy usage. A second field study adopts the same behavioural interventions, again applied in a wide-scale field study, but in an educational context, this time with the aim of encouraging high achieving low income students to accept offers at "selective" academic institutions. In this second field study the behavioral interventions are also tested against the backdrop of a subsidy for low income families alongside a high income tax on families earning a combined annual salary of, say, $120K USD. In both field studies, the effect sizes are greater when the interventions are combined than when they are implemented individually. While it is difficult to implement a randomised control type design, in both field studies positive behavioral change was also found when compared against a baseline; this involved comparing the effects of the interventions against outcomes (e.g. home energy usage, student uptake of offers) in the previous years when the interventions were not implemented. Imagine, for this illustration, that there also exists a behavioral change formal mathematical model called Fudge. The researchers running the studies claim that the Fudge model predicted the findings of both field studies.

The Fudge model cannot say anything about the psychological processes that were tapped through the interventions, or about why it is that the two interventions in particular that were used were effective. The Fudge model is elegant in its simplicity. Based on extracting from a large database of all known interventions tested in all known contexts, it can accurately predict that any combination of two or more behavioral interventions implemented at the same time increase the effect size of behavioural change by (say) $\sim 2\%$, with returns diminishing to 0 over time in repeated decision-making contexts. The Fudge model would be able to provide basic answers to each of the questions presented in the title of this special issue, both basic and applied. But is this adequate? And who is it adequate for? It may be enough for a policy maker, and is perhaps exactly what policy makers are hoping researchers can provide them, but it should not be an adequate answer to a basic science audience. Where both audiences ought to profit, is in understanding the underlying psychological mechanisms. For a model to provide an adequately precise answer, it won't be able to also answer questions of when and why behavorial interventions are sticky, scalable, and transferable. But it may be able to tell someone why, in a specific context, they should work, and when and why they are likely not to work.

Using these issues to provoke a debate, in this special issue we have invited a selection of authors to contribute their insights about behavioural science in the context of policy-making – specifically to explore the generalisability, scaleability and transferability of empirical insights around behavioural interventions. We have deliberately sought insights from a diverse range of thinkers – from practitioners working in commercial as well as public spheres, as well as academic researchers engaging more deeply with policy-makers than has been traditional. Our contributors' brief was also to confine their insights to a relatively short 1,000 or so words – so that the collection of insights stands as a whole – as a vehicle for comparing and contrasting the diverse visions that characterise the modern community of behavioural science scholars.

We start the special issue by exploring the theme of the importance of not just showing but also understanding why and how nudges work – with a contribution "Nudging: To know 'what works' you need to know why it works" from Pelle Guldborg Hansen of Roskilde University and iNudgeyou. Then Eliza Kozman and Michael Sanders from the Behavioural Insights Team present an outline of their analysis of education policy interventions using randomised controlled trials in "Examining the potential for nudges to tackle 'undermatch' in higher education: existing evidence and implications for scaling". They show how educational nudges have worked whilst also emphasising the important role to be played by academic researchers in helping policy-makers to engage with research literatures to ensure that nudges transfer strongly into real-world policy settings.

Bringing-in perspectives from the commercial sphere, Colin Strong and Tamara Ansons (Ipsos) offer new perspectives in their contribution "Moving from Nudge to Holistic Behaviour Change", specifically arguing that more attention needs to be given to nuanced practitioner perspectives. Steven Johnson, in the contribution "What Works: When & Why are Nudges Sticky, Scaleable and Transferable?", calls for 'epistemic humility' in our assessment of the power of nudges, arguing that "the only responsible answer to 'what works?' questions in relation nudges is that 'we don't know… yet'" and not because there are problems with the discipline but because it is natural and valid for an emerging discipline to be engaging with unanswered questions. In the same vein, we conclude with a contribution from Pete Lunn (Economic and Social Research Institute, Ireland) "Nudger Beware: Diagnosis Precedes Remedy". He gives a timely warning about overweighting the findings from a limited range and number of empirical studies of behavioural interventions, and advocates a more systematic approach in which remedies are matched to a thorough and precise diagnosis of a policy problem.

With sincere thanks to our authors for offering their insightful and provocative insights for fuller contemplation by JBEP's readers. We hope that our collection of insights and observations will catalyse some new thinking around how and why we are interested in empirical studies and what we can do to ensure that a rich evidence base contributes as much as possible to robust and powerful behavioural interventions in the future.

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