RESEARCH HIGHLIGHT

Let's agree to agree: a comment on Hogarth (2020), with a plea for not-so-competing theories of addiction

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Perhaps you have heard of Reviewer 2, the anonymous peer with an axe to grind against a manuscript. Reviewer 2 is internationally loathed on such websites as "Reviewer 2 must be stopped," "I don't mean to be Reviewer 2, but…," and "We all know who Reviewer 2 is."

For the paper in this issue by Hogarth [1], Reviewer 2 was me. Things ended well; the paper was informative and insightful from the start, and it has only gotten more so. But I did grind my axe along the way, and the editors have kindly afforded me this space to display it. This is not a rebuttal to a submission you never saw; it applies to plenty of published work.

Hogarth's paper [1] evaluates evidence for three theories of addiction: addiction as an overlearned motor habit (divorced from its consequences), addiction as a punishment-insensitive compulsion (which, in my reading, may be more affectively laden than a habit, but is still divorced from its consequences), and addiction as an excess in goal-directed choice (driven by the desired aspects of its consequences). As formulated by Hogarth, the "goal-directed choice" theory includes the idea that, for some people, the attractiveness of intoxication as a goal is exacerbated by expectation of relief from negative affect.

Like Hogarth, I see far greater support for a theory of goal-directed choice than for theories of compulsion or habit. Most addiction researchers give short shrift to the use of "drugs as instruments," especially in users who are addicted. Hogarth's paper offers a sound corrective, marshaling evidence from lab animals and humans to show that a theory of goal-directed choice seems to account for more addictive behavior than the other two theories.

The axe I ground along the way: in writings on addiction, theory is often treated in a "winner take all" fashion. In a "winner take all" world, evidence supporting a habit theory must surely oppose a theory of goal-directed choice, and vice versa, except in grudging asides.

To see this done by proponents of habit theory, witness the flabbergasting paper title "Carrots and sticks fail to change behavior in cocaine addiction" [2]. That is patently false: carrots and sticks, in the form of systematic contingencies, are the most effective known treatment for cocaine addiction [3]. Findings of behavior change in that context are as firmly established as nearly anything in current behavioral science.

On the other side, some carrot-and-stick theorizers are equally ungiving: "[M]odels that portray addiction as a disorder of compulsion and habit cannot be reconciled with observations that…drug use is an operant behaviour that remains sensitive to its consequences, as evidenced by the effectiveness of contingency management for the treatment of addiction….Therefore, drug use is not 'compulsive' at the time it is carried out" [4]. True

enough, except for patients who do not seem to respond to any practically achievable schedule of contingencies [5]. Or: true enough, except when people lapse absentmindedly (an occurrence that may account for only about 3–6% of lapses [6], but 3–6% does not round down to 0% for someone who wants to avoid a lapse). Or: true enough, except in instances that look like sign tracking, when people with addiction histories handle drug paraphernalia quasi-fetishistically, with no follow-through to intoxication (see online Supplementary).

But the broad brush persists. Even some otherwise incisive behavioral analyses reject the possibility of contingency-responsive and compulsive subtypes of addiction (citing data that are unlikely to reveal subtypes) [7].

This is not parsimony, this first-past-the gate selection of a theory. Parsimony rests on making the fewest possible assumptions. To suggest that one theory accounts for all the clinically and scientific important aspects of addiction, while any evidence for a different theory is artifactual or otherwise dismissible, is assumption on a worryingly grand scale.

Yet I often see assertions that "addiction is driven by X (and not Y)" spoken with the same universalizing confidence with which we might say "protons have greater mass than electrons (and not less)." That kind of truth is rarely possible in generalizations about behavior. At best, we behavioral scientists might alight on a "truth" such as "men can lift heavier weights than women." That sex difference turns out to be a useful touchstone for reliability. Its Cohen d is 1.2 [8], conventionally considered large. Visualize it as two curves side by side, with 55% overlap. A randomly selected man has an 80% probability of being stronger than a randomly selected woman. Now consider almost any other finding about behavior: it is probably much smaller [8]. For an effect with a d of 0.5 (the respectable medium-size effect of contingency management for addiction), visualize two curves overlapping by a whopping 80%, with a "probability of superiority" of only 64%.

If you amass such findings into a theory of addiction, and then assert that it accounts for everything important about addiction (whether in etiology, phenomenology, course, or intervention), then all it takes to falsify your assertion is a countervailing anecdote.

For that reason, the "winner take all" approach is increasingly rejected in psychiatry, where the quest for "oligocausal theories" [9] is ceding to recognition of "fuzzy cross-level explanations" [10]. This trend is complemented by progress in biostatistics, where horse races between null and alternative hypotheses are ceding to calls for precision in determining the size of each observed effect, an endeavor that requires judgment about the effect's importance or ignorability [11].

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This is not mere armchair philosophy. When ideas about addiction that are true *less often* than other ideas are given the scent of the *debunked*, what happens? If I were a reviewer for a funding agency, I would stop giving good scores to projects that recognize those ideas. If I were an NHS official (in the UK) or a health insurer (in the US), I would stop paying for treatments that recognize those problems. When we discard entire theories, we punch holes into what could be an integrated set of approaches to a heterogeneous clinical entity. This pitfall is easy to avoid this pitfall, both conceptually [12] and operationally [13].

In a preprint called "The generalizability crisis," Yarkoni [14] argues that conclusions in the behavioral sciences rarely generalize beyond the methods and measures used—so a correctly stated conclusion will often sound comically specific ("priming undergraduate Plymouth students with 40 cleanliness-related words increases 21-point moral disgust ratings for six specific moral dilemmas"). I hope we never have to dial it down that far. I intend to continue discussing my research results in the broadest terms for which I can make a credible case. But if you catch me saying that my case obviates your case, stop me. I recently named a paper "Some of the people, some of the time: field evidence for associations and dissociations between stress and drug use"—and I was not being noncommittal. I was committing to a view of addiction in which many theories can be true. The questions are *when*, *for whom*, and *to what extent*.

FUNDING AND DISCLOSURE

ADDITIONAL INFORMATION

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