Bernheim, D. R., & Rangel, A. (2004) Addiction and cue-triggered decision processes. *American Economic Review,* 1558-1590. December.

ABSTRACT

We propose a model of addiction based on three premises: (i) use among addicts is frequently a mistake; (ii) experience sensitizes an individual to environmental cues that trigger mistaken usage; (iii) addicts understand and manage their susceptibilities. We argue that these premises find support in evidence from psychology, neuroscience, and clinical practice. The model is tractable and generates a plausible mapping between behavior and the characteristics of the user, substance, and environment. It accounts for a number of important patterns associated with addiction, gives rise to a clear welfare standard, and has novel implications for policy.

Will you take 'neuro' with that?

Neuroeconomics is an interesting idea that has an epistemological worm at its core. Classical microeconomics is a science of outcomes and its mathematical tool has always been optimality theory. As *Wikipedia* puts it: "The demand for various commodities by individuals is generally thought of as the outcome of a utility-maximizing process"—"*a* process" not any specific process. Until very recently, economics had no interest at all in the details of the process that allows people to maximize their utility. But the "neuro" in Neuroeconomics must refer to the physiology of some very specific optimizing process. Unfortunately, there is no guarantee at all that the process corresponds to "explicit optimization" in which courses of action are well-defined, their utilities explicitly computed and the highest one reliably chosen.

But a few Nobel prizes change everything. First John Nash (*A Beautiful Mind*) Reinhard Selten, and John Harsanyi got the Economic Nobel for their contributions to Game Theory, and then Daniel Kahneman (with Vernon Smith) got his for his and Amos Tversky's work showing that people are not always rational or, if rational from moment to moment, may not be rational in the long run. These folk showed that the process that sometimes produces optimal behavior, but often doesn't, isn't "explicit optimization" at all. So now, economics is casting about for a better theoretical base, and its eye has fallen upon the Golden Girl of the new millennium: brain science. But how likely is it that concepts from optimality theory, like "risk" and "utility" will find an easy representation in the brain if, in fact, the brain doesn't work as a literal optimizer?

A similar, but less philosophically suspect, transformation overtook behavioral ecology when it became enamored of optimality theory in the late 1970s. The biologists had the advantage that no one doubted the mechanism for optimization: it was natural selection. If anything was being optimized, it was Darwinian *fitness*, although in practice proxies like food *profitability* (energy gained divided by time taken to extract it) had to be used instead. But when biologists and psychologists began to demonstrate clear instances of failure to optimize; and when some of these could not be explained by cognitive or other constraints; then it became clear that researchers should be looking not for optimality, but for cause-effect processes (behavioral ecologists used the phrase "rules of thumb") as explanations for foraging behavior and operant conditioning (see, for example, Krebs & Davies, 1978, or Staddon, 1980). Now, apparently, it is time for economics to make the same transition from optimality theory to some kind of mechanism. The question raised by the target article is whether the new focus on neuroscience is a step in the right direction, or a dead-end.

This is a clever paper by authors who know how to play the economics game. But overall, I think it fails: their reach much exceeds their grasp. It is not clear that casting everything in an economic language/model that looks to me rather ad hoc in fact adds much to one's intuitions. Much of what they say about addiction and its control seems sensible, but could have been arrived at in many other ways, both scientific and intuitive.

Addiction is an interesting concept. Addiction to nicotine, for example, seems to be as regulated as eating. If dose size (i.e., nicotine per puff) is increased, subjects smoke less, maintaining blood-nicotine level more or less constant—as they do for blood glucose in feeding. The

big difference seems to be *tolerance*. The effectiveness of most addictive drugs decreases with successive doses. Feeding lacks this effect: a calorie is a calorie is a calorie, more or less, as far as the regulatory system is concerned.

The dynamic regulatory process for feeding seems at base to be rather simple: a delayed satiation effect that turns it on and off in bang-bang fashion (e.g., Staddon & Zanutto, 1997) . It might be interesting to add the tolerance factor and see to what extent it can account for the dynamics of addiction.

The references to neuroscience in the target article are rather empty, I'm afraid. Unless you believe that behavior is *not* totally caused by the brain, the fact that different behaviors are associated with activity in different areas is just what one would expect. Such a finding becomes interesting only if the brain areas have properties that shed light on the behavior in a way that is not already available from purely behavioral studies. Unfortunately we know so little about how the brain actually works, as a real-time machine, that this is rarely possible. For example, behavior shows that animals are sensitive to changes in reward size. Schultz has found neurons that seem to be sensitive to the discrepancy between expected and obtained reward. That's nice; but we know there has to be something in the brain that is sensitive to this difference; what's interesting is where these neurons are, the details of their dynamics and exactly how they are connected. Unfortunately, the connections are invariably multiple and complex and the dynamics usually very hard to understand. So this finding signals the beginning of an investigation, not its conclusion.

The authors talk a lot about "hedonic sensation," "wanting" and "liking" and other subjective measures. Some of this is from neuroscience — Kent Berridge, for example, has used the latter two terms. This terminology connects with many readers, but gives a causal role to these subjective states that is not warranted — certainly as far as the monkey research is concerned. Berridge's work is interesting in its own right without involving these unmeasureable ideas. B&R do not stint on largely undefined terms: "cognitive override" (wha' that?), "hot" and "cold" states (how are these identified/measured?), "higher cognitive control," "welfare" (now there's a tough one!), etc. They also make a few assumptions that seem odd to me. For example, on p. 1572, last para. and p. 1581, they talk about "preferences" and seem to assume they must be static — which they obviously are not. Preferences change all the time; that's why some kind of dynamic analysis is necessary.

A core issue seems to be the role of stimuli in triggering addictive behavior. The authors treat this as if it were in some way surprising or required a special explanation. But it is absolutely elementary in the experimental analysis of behavior that the "strength" of a response is the sum of a number of factors which include the controlling stimuli (as well as things like motivation and time). This is really Behavior An. 001!

So, in sum I am not impressed by this approach. This article too long, too obscure (rather than just obscure enough!), and too full of undefined and undigested concepts to be a serious contribution to our understanding and control of addiction.

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