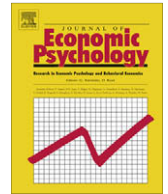




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Book Review

Addiction: A Disorder of Choice, Gene M. Heyman. Harvard University Press, Cambridge, MA (2009). IX+200 pp., \$40.00 (hc), ISBN: 978-0-674-03298-9 (hc)

*The Toronto Star*¹ builds its review of Gene Heyman's book around quotations from other reviews. Given the prominence of these remarks on the Internet, aiming to discuss the book in its context of controversy requires that I follow suit. I therefore quote the *Star* quoting others:

"His argument crashes and burns," says Tony George, the head of addiction psychiatry at the University of Toronto. "I don't think there's too many self-respecting scholars in the addiction field who would agree with him. I'm shocked that Harvard University Press would publish that."

"These guys – I don't know, academia, they just kind of take what they want, and they don't care about the truth, or what the studies show," says Norman Miller, a professor of medicine at Michigan State University.

"What aspect of disease," says Norman Hoffman, a psychology professor at Western Carolina University, "does he not understand?"

Economists who read Heyman's book should be both amazed and dismayed by these remarks. Nearly 100% of economists who have any professional views on addiction can now take it that, according to the authorities above, they lack self-respect, do not care about the truth, and do not understand what a disease is. For Heyman's discussion is exemplary of the core attitude shared by economists, who are in consensus about little else, on the general relationship between incentives and proximate causes of behavior.

Here is how Heyman frames that relationship. Some behavior is voluntary and some is not. By this distinction we need not wade into metaphysical issues around so-called 'free will', which help keep philosophers from doing useful things with their time. The distinction can be based simply on the fact that the frequency of some types of behavior is sensitive to changes in costs and benefits while the frequency of some other types of behavior is not. To cite one of Heyman's examples, when scorn began to be directed at male executives who made a habit of winking at female staff, the production of executive winks declined. By contrast, no amount of criticism directed at people who blink in sandstorms, no matter how much we wish they would keep their eyes on the camel, will produce any effect. One must distribute goggles instead. Noting this distinction does not rest on any underlying belief that the causes of blinks are neurological and mechanical while the causal relationship between incentives and winks is a magical one that has no physical basis. Thinking that winks are voluntary behavior is entirely compatible with knowing that people whose facial muscles are paralyzed cannot wink.

I confidently expect that there is no economist in the world who disagrees with this story about winks and blinks. Every incentive that influences a behavior must do so through particular causal channels. In a typical case, many such channels are available. I can reliably cause you to give me a dollar by offering you my car in exchange, or pointing a gun at you and challenging you to choose between your dollar or your life, or, if we're sort of friends, telling you that I do not have the right change for the soda machine today. In each case, the change I produce in your incentives will require that your brain be in one of a restricted range of states. In each case, it must be awake. In the third case it probably needs to have been trained to process English. And so on.

Heyman recognizes that people would not get addicted to drugs if drugs did not have characteristic effects on their brains – in particular, on neurotransmitter circuits that alter attention, cognition, emotional state and motor preparation, with different drugs affecting different such circuits in idiosyncratic ways. He also recognizes that some brains are disposed by genetic and developmental factors to be more powerfully influenced by these neurochemical effects than others. Thus the single most useful everyday datum you could have if you wanted to predict the probability that someone has battled with or will battle with addiction is that they have an identical twin who has so battled.

¹ 16 May 2009.

Why, in defending such obviously sensible opinions, is Heyman viewed by eminent professors of medicine as ignorant or cavalier about the truth? They have two closely related complaints. First, he avows that the majority of addicts not only can quit given adequate incentives to do so, but in fact *do* encounter such incentives before they reach the age of 40 and *do* quit. Second, in light of the fact that most addiction is voluntary in the sense that incentives influence it, he prefers that addiction not be called a disease.

To support the first belief, Heyman reports the relevant epidemiological statistics. The relationships revealed by these statistics are remarkably clear. A main reason his book needs no equations is that no fancy modeling or econometric testing is necessary to see that the majority of addicts try and fail to quit 3–7 times, then before reaching middle age succeed in either reducing consumption to the point where it is compatible with normal life (practice ‘chipping’), or stop altogether. One *does* need some regression analysis to be convinced that changes in costs and benefits are significant predictors of the exact timing of the inflection points in this typical pattern, so perhaps Heyman should have risked his publisher’s wrath by producing a few models and tables displaying coefficients. But he shows the reader where she can look these up. As someone who has carefully examined and worked with the data on which Heyman relies – which are, by the way, *all* of the relevant data – I can testify that if most series of observations economists analyzed were this unequivocal in their trends, then econometrics would never have been invented and we’d have a lot more spare time.

My inclusion of ‘relevant’ in the previous sentence is important. To yield these clear relationships one *must* use randomized and very large *community* samples. The samples must be large because addiction has low prevalence. They must not involve oversampling (let alone exclusively sampling) addiction clinics, because most people who seek professional treatment for addiction are precisely those with the most intractable conditions. These are an extremely small proportion of all the addicts. They are also far more likely than others to suffer from so-called Axis-I comorbidities (depression, schizophrenia, anxiety, etc.) or psychiatrically recognized atypical personality traits (e.g., high impulsivity scores). Economists are at last called upon to get out the box of fancy tools to help to confirm causal hypotheses here. Many people suffering from (for example) depression appear to resort to mood-altering substances such as alcohol as self-prescribed medication for it.² Since alcohol at worst kills you slowly and depression can kill you very quickly (by way of non-magical causal channels only, I might add), these people have much stronger than usual incentives to keep drinking. This pattern helps to explain the widespread belief that addiction is chronic and non-voluntary in Heyman’s and the economist’s sense. The vast majority of studies of addicts use clinical samples, and until 25 years ago *all* studies did. Any economist will see at a glance what this was bound to lead to. Imagine that you set out to see if poor people were likely to be morally irresponsible, and the only places you could think of in which to find convenient samples of poor people were prisons.

Heyman’s semantic preferences with respect to the word ‘disease’ are, I suspect, a major source of the rhetorical fury his book has caused. Here he might have been more nuanced. ‘Disease’, being a term of everyday language, has no precise meaning. Thus it’s quixotic to set out to *refute* those who ascribe it to addicts. However, there’s an argument for forcing the semantic point, because it’s directly connected to the basis for the policy significance of this whole contretemps. Consider this quote from Heyman:

Dr. Enoch Gordis ... a previous head of NIAAA, writes: “the disease concept ... has helped remove the stigma from a chronic disorder [alcoholism] that is no more inherently immoral than diabetes or heart disease.” Taking this argument to its logical conclusion, a group of leading addiction researchers argued (in the pages of the *Journal of the American Medical Association*) that insurance plans should provide the same coverage for heroin addiction, crack addiction and alcoholism as they do for traditional diseases such as cancer, arthritis and high blood pressure (p. 90).

One need not be inclined to regard addiction as ‘immoral’ (I do not) or think addiction to anything should be criminalized (I do not) in order to see, on economic grounds, the potentially gigantic moral hazard problem at stake. Cancer and arthritis are not fun.

Economists with any interest in behavioral phenomena should all read Heyman’s book, so as to be able to defend it against ridiculous attacks of the sort quoted at the outset of this review. They will like it despite its lack of formal models. It has plenty of clear charts and graphs, so the quantitative side of the argument is not missing. It is written in a plain, pleasing style. It includes a useful contribution to the debate over modeling addiction in economics that goes back to [Becker and Murphy \(1988\)](#), since it takes that venerable model as its analytic starting point, but then reminds us that Becker and Murphy rely on a curiously *un-economic* conception of utility as additive instead of marginal. (This is not how Heyman puts it, but that’s what his valid and important point amounts to.) The possibility of switching between these two views of utility is the basis of the addict’s famous ambivalence, according to Heyman. I think this is correct.

The book is not perfect. Again, a few regressions should have been reproduced. On pp. 142–144 there is an argument against the hypothesis that dopamine flooding in ventral striatum is the core mechanism in all addiction. This argument is a non-sequitur, and I believe that its conclusion is false. It also is not the least bit necessary for Heyman’s main case. (Analogously: it makes no difference to the claim that all financial behavior is voluntary that all financial transactions crucially involve monetary instruments.) Another objection I have is that 12-step programs are said to “work” as therapy, with insufficient regard being paid to the fact that they are worse than useless for the majority of those addicts (a minority, of course) who are very well educated. But these are minor complaints about an excellent book.

I wonder what aspect of incentive Norman Hoffman does not understand?

² Again, Heyman explains this without showing the models. Again, they are easy to look up.

Reference

Becker, G., Murphy, K. (1988). A theory of rational addiction. *Journal of Political Economy*, 96, 675–700.

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