Chapter 13

Is the Addiction Concept Useful for Drug Policy?

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Introduction

The development of behavioral economics, with its prospect of integrating insights from economics and psychology, is surely one of the most exciting intellectual developments in the social and behavioral sciences in the past 20 years. And if any domain could benefit from this development, it would seem to be the domain of psychoactive drug use, where choices are so often pathological.

Thus, one can imagine my surprise and dismay when I was asked to prepare an essay on new policy insights that might follow from the leading behavioral economic theories of addiction,[1] and I discovered that there weren't any. Or, at least, hardly any. In this essay, I present evidence for that assertion, offer some speculative hypotheses about why it is true, and ask whether it is likely to remain true in the future.

Some Evidence

As evidence, I offer the behavioral record — the behavior of professional drug policy analysts in the form of two lengthy monographs on drug policy, both of which were published in 2001. Both monographs were prepared by interdisciplinary teams that included both psychologists and economists. I should emphasize that "the psychologist" in both cases was me.

The first is my recent book with Peter Reuter, *Drug War Heresies: Learning from Other Vices, Times, and Places* (MacCoun & Reuter 2001). The book is a comprehensive analysis of alternative legal policy regimes for controlling marijuana, cocaine, heroin, and other recreational drugs.[2] It is thoroughly interdisciplinary in scope — Peter is an economist, I am an experimental social psychologist by training, and our collaborators included the economist Tom Schelling and the historian Joe Spillane. The book includes chapters on economic theory, psychological theory, moral philosophy, history, cross-national analysis,

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and so on. But in a 479-page book, with 44 single spaced pages of bibliographic references, we made almost no use of the theoretical literature on addiction.

The other monograph is *Informing America's Policy on Illegal Drugs: What We Don't Know Keeps Hurting Us* (Manski *et al.* 2001), the final report of the National Research Council's *Committee on Data and Research for Policy on Illegal Drugs*. The monograph was produced by 16 members spanning a host of disciplines. This 407-page monograph devotes several pages to neuroscience and behavioral economic concepts of addiction (though not particular models), yet those concepts played almost no detectable role in the subsequent analyses of supply reduction policies, user sanctions, drug prevention, or drug treatment.

One might respond to these observations by suggesting that behavioral economics simply has a marketing problem — that theorists simply need to more aggressively disseminate and promote their theories. That is almost certainly correct. But I don't believe it is the source of my observations. In neither case did the authors simply overlook these theories in the preparation of the monographs. For example, during the nearly full decade Peter and I spent working on our project, I immersed myself in the neuroscience, economic, psychological, and philosophical literatures on addiction, assembling large collections of papers by the other presenters at this conference. It is riveting stuff, and I learned a great deal in the process. We simply found very little we could use in analyzing the question of the relative benefits and weaknesses of alternative drug-control regimes.

A related response might be that we as policy analysts simply failed to comprehend and appreciate the relevance of these models for drug policy. I am not well situated to assess this possibility; by definition, one cannot assess whether one suffers from miscomprehension or a failure of imagination. If others respond to this essay by demonstrating that I overlooked profound new implications of these theories for drug policy, I will happily concede and judge this essay to have failed in its arguments but succeeded in its consequences.

After reviewing these policy implications, I will consider a number of alternative explanations for why behavioral economic theories of addiction (henceforth, "BETA") have produced relatively few policy insights. I conclude that the limited policy implications stem from several features shared by BETA: the overlap in the causal factors that motivate "addictive" and "non-addictive" psychoactive drug use; the overlap between the policy implications of addiction theories and more conventional theories of drug control; and the notion that addiction is a unitary phenomenon with one correct theoretical explanation.

Some Caveats

Before I plunge headlong into my arguments, it is worth briefly clarifying what I am *not* arguing:

(a) I am decidedly not arguing against behavioral economics as a scientific enterprise.
(b) I am not arguing that there is nothing interesting or worthwhile about developing behavioral economic models of drug use or other potentially addictive behavior, although I will argue that the addiction construct is a distraction from the most useful aspects of the behavioral economic analysis of drug use.

(c) I am not disputing the existence of drug addiction, or the enormity of its consequences, though I do question whether "addiction" forms a discrete, coherent category. I am not simply echoing the positions of critics like Stanton Peele (e.g. 1996), Sally Satel (e.g. 2001), or Thomas Szasz (e.g. 1974), each of whom have criticized conventional uses of the addiction concept, though for differing reasons. My arguments in some ways overlap with theirs, but I approach the issue from a very different perspective, working backwards from policy analytic considerations rather than working forwards from a set of first principles about human conduct, liberty, or morality.

Why the Addiction Concept May Seem More Relevant Than It Is

What do Policy Analysts Want to Know?[3]

The left column of Table 1 lists the key levers that are conceptually (if not always politically) available to drug policy makers (see MacCoun *et al.* 1996):

Analysis of these policy levers follows two approaches, direct program evaluation (common for prevention and treatment, rare for enforcement) or theoretical analysis. The right column of Table 1 lists explanatory constructs relied on most heavily in recent theoretical analyses of American drug policy (e.g. Behrens *et al.* 2000; Caulkins *et al.* 2000; Kleiman

Table 1: Policy levers and related empirical uncertainties.

Policy Levers	**Key Empirical Uncertainties**
Drug prevention, education, and rhetoric from the bully pulpit	Cost-effectiveness and cost-benefit ratios of various interventions
Drug treatment	Prevalence and incidence of drug use, and statistical distribution of frequency and quantity of consumption
Criminal sanctions against users	Price elasticity of demand for drugs
Criminal sanctions against dealers	Time sensitivity and/or impulsivity of drug users
Interdiction and source country controls	Dose-response relationship between consumption and its acute and chronic effects
Taxes, advertising controls, and other regulatory mechanisms	Relative contribution of psychoactive effects vs. illegality in producing drug-related harms
Drug testing	Possible substitution, complementarity, and "gateway" relationships among drugs
Bans on employment, welfare, and other benefits	Unintended effects of use-reduction strategies on drug harms, and of harm-reduction strategies on drug use
	Distribution of effects of drug use across bearers — user, family, friends, neighbors, community, taxpayers

1992, 1998; MacCoun & Reuter 2001; Manski *et al.* 2001). It is clear that BETA make contact with these explanatory factors in myriad ways. But in the remainder of this section, I will attempt to illustrate how behavioral economic theories of addiction largely generate policy implications that are redundant with existing strategies. And the novel implications they do offer follow from general principles of self-control rather than a narrow and extreme end state called "addiction."

BETA's Implications for Demand Reduction

Prevention Some authors have argued that BETA have implications for drug prevention. For example, Herrnstein & Prelec (1992: 357) argue that their model "suggests that society should at least provide people with more information, on the grounds that they are less likely to go down the path if they know where it is headed." Heyman (1996: 573) argues that ". . . the ideas presented here indicate that treatment should attempt to bring drug consumption under the control of overall rather than local value functions. . . . Thus, methods that increase the salience of distant behavioral consequences should move individuals towards more rational use of drugs. This point suggests that persuasion is a potentially powerful weapon in altering people's behavior."

These recommendations fall short on two grounds. First, they restate the obvious; public information campaigns on the risks of long-term drug use have been a staple of American drug policy for over 30 years. Second, they ignore the evidence that such information campaigns have been remarkably ineffective at discouraging drug use (and risky sex) and are generally recognized as insufficient by prevention researchers. (See Manski *et al.* 2001: Chapter 7 for a detailed review and meta-analysis.) In fairness, past anti-drug information campaigns might have been more effective if they had been more credible and less moralistic. In contrast, the prevalence of cigarette smoking fell by half in a generation following the release of a series of highly factual, morally neutral Surgeon General reports. But even there, it is discouraging that tobacco initiation rates among youth have remained remarkably stable.

Treatment A more likely mechanism by which BETA might contribute to drug policy would be via new and better methods of drug treatment. Behavioral economics research has already made significant contributions to the design of drug treatments. For example, the NRC report (Manski *et al.* 2001: 248) highlighted the behavioral economic work of Stephen Higgins and his colleagues as among the most promising developments in cocaine treatment research. This approach applies community reinforcement techniques and a "token economy" system of vouchers for retail goods to help cocaine users remain abstinent (see Bickel *et al.* 1995; Higgins *et al.* 1995). These studies are invaluable. It is highly plausible, but not very helpful, to be told that drug problems might be reduced by eliminating joblessness and poverty. It is nearly incredible, and extremely helpful, to learn that heavy cocaine users will provide three clean urine samples for a $10 gift certificate.

But while this treatment method is decidedly "behavioral economic," it does not depend in any direct way on a behavioral economic account of *addiction*. The same logic would follow from a behavioral economic analysis of self-control difficulties — or indeed

from a more traditional applied behavioral analysis (the contemporary term for behavior modification).

For the sake of argument, imagine that insights into effective drug treatment eventually emerge from behavioral economic analyses that require a notion of addiction per se, rather than a broader analysis of self-control. A radical improvement in drug treatment effectiveness would dramatically alter the drug policy landscape, although I argue later that it would not eliminate our drug problems. But if the improvements were only incremental in magnitude, they would be unlikely to have a noticeable impact at the policy level. It is difficult to detect any major impact of past treatment research on policy decisions (see Reuter 2001). And there is sufficient uncertainty about the true efficacy and effectiveness of treatment that any improvement may fall well within existing error bounds (see Horowitz *et al.* 2002; Manski *et al.* 2001) — and short of the more extravagant claims. Finally, the drug policy budget is an imaginary construction — the funds aren't fungible in the sense that dollars could simply be shifted from enforcement to treatment (Murphy 1994), although there could be a reallocation of funds within the treatment portion of the budget.

Pl. check the reference "Horowitz *et al.* (2002)," which is missing in the reference list.

BETA's Implications for Supply Reduction

Behavioral economic theorists have also drawn various implications of their theories for supply reduction policy.

Availability Several BETA theorists have suggested the importance of minimizing opportunities to obtain drugs: e.g. ". . . differences in prevalence rates will depend importantly on exposure to drugs . . . it seems likely that increasing the availability of addictive drugs would substantially increase the frequency of addiction" (Heyman 1996: 573). This is surely correct but, like the advice on prevention, redundant. Over half of our annual national drug control expenditures go to supply reduction efforts, roughly a third for interdiction and source-country controls. It is difficult to imagine a more aggressive supply reduction effort than the one we've experienced, and yet student surveys show that drugs remain readily available at schools, and cocaine and heroin prices have fallen to about a third of their 1981 levels after controlling for inflation (see MacCoun & Reuter 2001: Chapter 2).

Prices Changes in prices have little import for addicts' drug-use rates under a traditional "enslavement" view of addiction. (It might, however, influence the number of crimes some addicts commit to finance their habits.) Under that model, addicts were considered to be extremely insensitive to prices. Until Becker formulated his rational addiction theory (e.g. Becker *et al.* 1992), drug experts largely ignored users' "price elasticity of demand" (the percent change in drug use for a 1% change in price). But recent studies (reviewed in Caulkins & Reuter 1996) suggest considerable price sensitivity, with elasticities for cocaine ranging from -0.7 to -2.0. In other words, addicts reduce their consumption when prices rise. The emphasis on drug prices is surely one of the most important contributions of the economic approach to drug policy.

Unfortunately, in a prohibition regime, there isn't much we can do with this knowledge. Prohibition itself keeps prices artificially high, but beyond that, our supply reduction efforts

are spectacularly ineffective at influencing prices at the margin. A legal regime would provide considerably more leverage, through taxation, price controls, and other regulatory possibilities (MacCoun *et al.* 1996). Thus, BETA probably has greater potential policy impact in the tobacco and alcohol domains than in the domain of illicit drugs.

Smart deterrence and coerced abstinence Kleiman (2000, 2001b) has offered a persuasive behavioral economic analysis of ways we might enforce prohibition more effectively. He argues that hyperbolic discounting implies the need to shift our emphasis from severe but uncertain and delayed sanctions to a regime in which sanctions are modest but swift and probable. His "coerced abstinence" model of aggressive drug-testing of probationers offers a radically different way of deploying law enforcement resources for drug control. But nothing in Kleiman's analysis requires the notion of "addiction." Coerced abstinence makes sense if heavy users make impulsive choices; it would make little sense — indeed, it would be inhumane — if they were incapable of choice.

BETA's Normative (Welfare) Implications

There is a third category of potential policy implications that are normative rather than empirical.

Is the state justified in prohibiting drugs? Do BETA tell us whether government intrusion into private choices is justified? A tradition dating back to John Stuart Mill considers such intrusion justified if an act harms others (see MacCoun & Reuter 2001: Chapter 4). There is overwhelming evidence associating drug use with such externalities, but we still know very little about the relative contribution of three causal mechanisms to this association: psychopharmacological effects of drug use; overlap in the dispositional propensities to use drugs and commit crimes (see below); and criminogenic consequences of prohibition and its enforcement (MacCoun *et al.* in press). Unfortunately, BETA have remained largely silent about this question by focusing on drug consumption but not its consequences.

Is drug addiction an involuntary state? A second normative question is whether drug addiction is involuntary, such that addicts aren't capable of making rational choices. In theory, penal sanctions are unjust if actors are incapable of controlling their actions. In theory, paternalistic government behaviors are justified if actors aren't capable of protecting their own welfare.

Becker's "rational addiction" model is provocative precisely because it suggests that addicts freely choose their situation with full recognition of its eventual consequences. Analyses by O'Donoghue & Rabin (e.g. 1999) and Gruber & Koszegi (2001) persuasively challenge this extreme characterization. But it isn't entirely clear what's at stake in this debate for the normative choice among policy regimes, since BETA also model addiction as a choice process — albeit a constrained and distorted choice process. From a moral philosophical perspective, the models offer not black or white but shades of gray.

Politically, it might not matter; it is not as if the public is teetering on a moral knife edge where evidence might tip us one way or the other. Americans of a conservative stripe

insist on strict norms of individual responsibility; American liberals endorse paternalism for far more trivial consumer choices than heroin consumption (Skitka & Tetlock 1993). Yet Americans of both stripes largely reject the notion that drug dependence is completely involuntary; if the addict doesn't choose today's injection, she certainly chose her first injection (see Mannetti & Pierro 1991; Weiner *et al.* 1988). Hamilton (1980) argues that Americans judge others not by scientific causation but by the question, "could the actor have done otherwise?" It is not clear that Becker and his BETA competitors actually differ in their answer to that question.

BETA's Policy Implications: Summary

To date, most of the proposed policy implications of BETA are either redundant with current policies, or have less policy import than meets the eye. Significantly, almost all the policy implications discussed here were also suggested by Bickel & DeGrandpre (1996: 46–47) in their analysis of behavioral economic principles of reinforcement. This point is noteworthy because that analysis made only a passing reference to the notion of "dependence" and no reference to the word "addiction." This suggests, to me at least, that most of the important implications of behavioral economic analysis don't actually require the concept of addiction.

Possible Explanations for the Limited Usefulness of the Addiction Concept

I am confident in my thesis that BETA have offered few new policy insights, at least so far — a rather depressing conclusion. I am less confident that I know why. Here I offer six speculative explanations, one of which seems unpersuasive and five more plausible.

Theory Where No Theory is Needed?

One possibility is that this is just an example of the classic division between "basic and applied research." On this account, it is foolish to ask for policy relevance from basic science. This proposition might be correct at the extremes, but it is certainly not defensible as a general proposition. There is usually good reason to accept Kurt Lewin's (1951) well-known dictum that "there's nothing so useful as a good theory." And it is clear that at least some major theorists in this area do in fact desire to inform drug policy.[4]

In a classic essay, Milton Friedman (1953) defended an "as if" meta-theory of economics, drawing an analogy to a billiards expert who behaves "as if" solving a complex set of differential equations without actually doing so. One possibility is that a formal model of addiction might yield useful predictions of this sort, even though it is not a valid model of the actual addiction process. It would serve as a valid "black box" model of the functional relationship between causal antecedents and consequences, while remaining mute as to the underlying mediational processes.

Or one might ignore causal antecedents altogether. James Q. Wilson (1983) argued that "... one can intelligently make policies designed to reduce crime without first understanding

the causes of crime . . .” It is hard to know how seriously to take this quote, since only two years later Wilson published (Wilson & Herrnstein 1985) a lengthy tome on the causes of crime. At any rate, BETA researchers clearly aspire to develop valid models of causal process as well as input-output association, as they surely should.

The Wrong Level of Analysis?

Another possibility is that BETA are framed at the wrong level of analysis to be relevant for policy analysis. Interestingly, George Ainslie (1992) has referred to his BETA as “picoeconomics,” as distinguished from microeconomics and macroeconomics. It is often the case that collective social phenomena are more than the sum of individual actions. Indeed, the public health movement has made important conceptual advances by adopting “population thinking” as an alternative to an individual-based clinical perspective. But I would not try to defend the position that good policy analyses can do without a model of the individual actor, and that is surely not what Ainslie has in mind either.

Still, it is conceivably the case that the notion of “addictiveness” might be useful for individuals in governing their own conduct (individual policy), without being useful for the governing of aggregate conduct (public policy). A personal theory about addiction might itself be an important self-control device (Ainslie 2001; Bateson 1971). Ainslie suggests that:

> “. . . people cultivate the belief that street drugs are always irresistible once tried, rather than just making an overt rule against trying them. This cultivation is apt to take the following form: An authority teaches that irresistibility is a fact; you encounter evidence to the contrary, for instance in statistics on ex-users who used only casually; you discount or somehow don’t incorporate the contrary evidence, not because it seems to be of poor quality, but out of a feeling that it’s seditious” (Ainslie 2001: 109).

Later, he notes that when Ockham *et al.* “pointed out that the ‘facts’ on which people based moral norms weren’t found in nature, they encountered violent objections on the grounds that these discoveries would undermine morality” (Ainslie 2001: 112).

Overlap With Other Theories

Another reason why BETA might fail to produce novel insights is that they overlap in broad ways with more popular conceptualizations of drug use, even when they differ radically in their details.

One source of overlap is lay common sense or folk psychology. One can describe heavy drug users as “giving in to temptation,” that they are “self-indulgent,” “impulsive,” “short-sighted,” and “selfish,” without any knowledge of the subtleties of BETA.

But there is also considerable overlap with contemporary criminological theory. In their highly influential “general theory of crime,” Gottfredson and Hirschi argue that:

> Crime and drug use are connected because they share features that satisfy the tendencies of criminality. Both provide immediate, easy, and certain short-term pleasure. . . . Evidence to support our contention is found in the correlation between the use of cheap drugs, such as alcohol and tobacco, and crime . . . [and] by the connection between crime and drugs that do not affect mood or behavior sufficient to cause crime (such as tobacco) (Gottfredson & Hirschi 1990: 41).

Whether Gottfredson & Hirschi's central construct of "low self-control" is isomorphic with the BETA notion of hyperbolic discounting is still unclear. Vuchinich & Simpson (1998) found only weak and inconsistent correlations between personality measures of impulsivity and hyperbolic discounting behavior among light and heavy drinkers. The personality measures were better discriminators of light vs. heavy drinking than were discounting scores, at least in that experimental setting and sample.

A Problematic Construct?

Analytic use of the addiction concept may be hindered by its lack of adequate construct validity, in the psychometric sense of a unitary concept that can be adequately delineated and distinguished from other concepts. One can dispute the usefulness of the addiction construct without disputing the ontological reality of addiction or making snide reference to a metaphysical "ghost in the machine." The question is whether the construct would be more useful if it were disaggregated into distinct features.

The DSM-IV permits a diagnosis of substance dependence when any three of the following are observed in a 12-month period: tolerance, withdrawal, using more than intended, desire to quit and/or difficulty quitting; considerable time spent obtaining, using, or recovering from the drug; interference with other activities; and/or persistent use despite problems caused by use. The DSM-IV definition of dependence has fairly high inter-rater reliability (Heyman 2001), and the inter-item correlations are reasonably high (Feingold & Rounsaville 1995). But a construct can have high reliability without having high construct or predictive validity (e.g. astrological signs).

The DSM items may hold together empirically, but it is not clear that they do so conceptually in a way that makes the best analytic use of the data. At least as currently used (with the "any three" criterion), these items don't form a meaningful Guttman scale, as they would if the components had a logical, cumulative order (e.g. None, A only, A + B, A + B + C, etc.). One can interpret the debate between Ole-Jørgen Skog & Nick Heather at this conference as a debate about what a defensible Guttman scale of addiction might look like (see Chapter 5, this volume).

The DSM-IV dependence checklist items don't look anything like interchangeable, substitutable indicators of a latent construct, in the psychometric "domain sampling" sense. They aren't like items on a personality scale or intelligence test that can be thought of as tapping identical construct variance plus idiosyncratic item error. Instead, each component on the checklist is conceptually distinct. Moreover, the critera confound the condition of addiction with its antecedents, its consequences, and its context, thereby begging the very

questions that theory (and policy analysis) need to answer. Finally, epidemiological studies (e.g. Anthony *et al.* 1994) demonstrate considerable heterogeneity in the qualifying criteria displayed across individuals receiving the same "dependence" diagnosis, and even greater heterogeneity across addictive substances (alcohol vs. tobacco vs. opiates vs. cocaine vs. cannabis).

An Overdetermined Phenomenon?

Discussions about the relative merits of addiction theories often seem to accept two implicit assumptions: that addiction is a single, unitary phenomenon, and that it is caused by a single process.

Addiction theorists too often rely on "sufficiency" arguments in favor of their theories (MacCoun 1996). Some stylized facts about addiction are reviewed, and it is then demonstrated that the theory in question can produce such patterns. Even if correct, such arguments show that the theory is sufficient to produce "addictive" behavior; they do not establish that the hypothesized mechanisms actually produce the actual addictive behavior we observe in the world. In essence, behavioral economics theorists have tended to start with the model (rational choice theory) rather than actual behavior; the goal has been to teach the model new tricks — how to act addictively — in the fewest steps possible.

But there are good reasons to believe that real-world addiction is *overdetermined*, with a complex set of interrelated distal and proximal causal antecedents. A very partial list would include factors discussed in detail in this volume: classical conditioning of cues; operant conditioning (especially schedules of reinforcement); tolerance, withdrawal, opponent processes, and other neurochemical adaptations; impulsivity due to hyperbolic temporal discounting.

And many researchers would list additional mechanisms falling outside the theoretical framework of either neuroscience or BETA, such as: biased cognitive expectancies (e.g. Stacy *et al.* 1990; Tversky & Kahneman 1974), including "optimism bias" (the tendency to believe that generic population risks don't apply to oneself, e.g. Weinstein & Klein 1995); sensation seeking (Zuckerman 1994); "social scripts" (automatized behavioral schemata, see Wegner & Bargh 1998); maladaptive self-regulatory strategies for dealing with conflicting goals (Baumeister *et al.* 1994; Baumeister 1997; Carver & Scheier 1998; Tice *et al.* 2001; Wegner *et al.* 1989); attentional control (e.g. Steele & Josephs 1990); self-handicapping and other self-presentational strategies (e.g. Higgins & Harris 1988; Isleib *et al.* 1988).

With such a lengthy list, it seems strange that many experts still consider addiction to be "paradoxical." For example, Elster & Skog argue that: "On a theoretical level, addiction raises the paradox of *voluntary self-destructive behavior*. The challenge is to explain why people engage in behaviors that they know will harm them" (Elster & Skog 1999: 1). This notion of a paradox follows naturally from a rational choice perspective, or from a less sophisticated "folk psychological" theory in which actors are conceived as making coherent, conscious choices on the basis of a stable set of beliefs and desires. But it is less clear why addictive behaviors should be viewed as "paradoxical" from the perspective of contemporary scientific psychology or neuroscience. There is ample evidence that self-defeating behaviors are commonplace among otherwise well-functioning, non-clinical populations (Baumeister

et al. 1994). Baumeister (1997) notes that none of these mechanisms require any explicit self-destructive motives. They are overdetermined by a variety of fairly normal processes, especially cold, warm, or hot cognitive biases of information processing and/or perverse side-effects of self-regulatory strategies for pursuing conflicting goals.

Self-regulatory models in cognitive, social, personality, and developmental psychology do imply a purposive actor, but they are not built on rational or quasi-rational choice principles. This makes them less rigorous deductively, but the models do make clear, testable predictions that can and have been tested using experimental methods (see Carver & Scheier 1998; Muraven & Baumeister 2000; Tice *et al.* 2001; Wegner & Bargh 1998).

Undue Emphasis on the Extremes

The distribution of drug consumption across users The proposition that "addiction" is overdetermined has testable implications. "Single mechanism" theories may propose qualitative discontinuities — thresholds beyond which a user passes from "non-addiction" to "addiction." But such analyses are ceteris paribus. Presumably, the multiple mechanisms of "addictiveness" are highly correlated, but they are not isomorphic, so a discontinuity in one mechanism might well be obscured by the operation of other mechanisms. A series of superimposed step functions might collectively form a smoothly continuous function. If so, one would not expect to observe stark discontinuities between "addicted" and other heavy users. In principle, this should be testable using psychometric techniques for empirically distinguishing discrete typologies from continuous, dimensional traits (Meehl 1995).

Some indirect evidence on this point comes from the National Household Survey on Drug Abuse for 2000 (NHSDA 2000).[5] Figure 1 shows the number of days of drug use per year among Americans aged 12 and older who used in the past year, separately for marijuana, cocaine, and alcohol. (Unfortunately, cigarette data are not available for this variable.) For marijuana and alcohol, but not cocaine, the distributions are bimodal. The largest mode is at "1 to 11 days per year" (very casual use), but the second mode is at "100 to 299 days per year," *not* "300 or more" as one might expect given the ease with which we use the label "addict."

Figure 2 focuses more narrowly on past-month users, thereby screening out most of the very casual users. The data for cigarettes match the profile of "an addictive drug," with the modal user using 20 or more days out of the month. But for marijuana, cocaine, and alcohol, even among past-month users, few use 20 or more days a month.

Unfortunately the NHSDA sampling and self-report procedures are thought to underrepresent heavy cocaine use. Figure 3 shows use frequencies in a sample arguably less susceptible to such biases — a snowball sample of recent cocaine users in Amsterdam (Cohen & Sas 1995). Despite a very different sampling strategy and a far more tolerant culture, self-reports of use during the last three months, and during the users' first year of use, look quite similar to the pattern in the NHSDA data. Even for the "period of heaviest use," only 20% reported daily use. Compulsive use, in which lives are dominated by drug consumption, is an extremely important part of the policy picture, but it is clearly not the whole picture by any means.

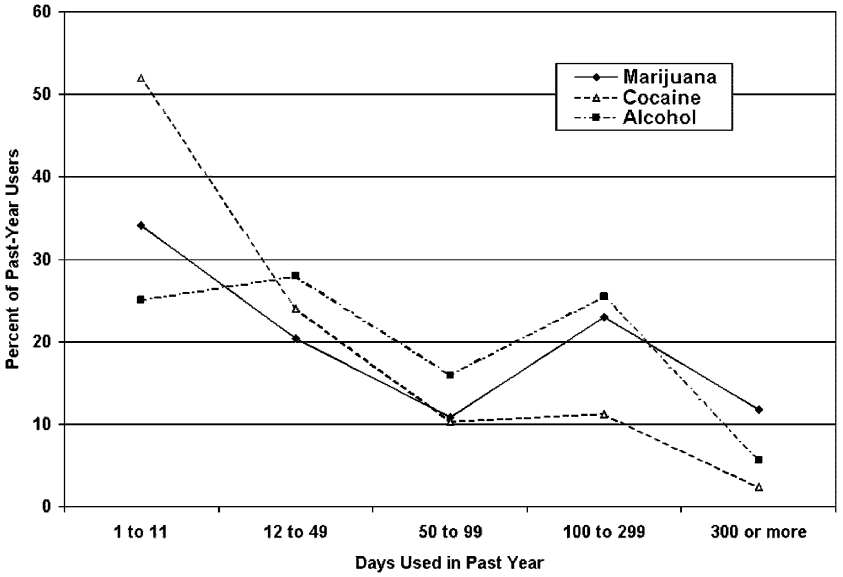

Figure 1: Frequency of use among past-year users.

Loss of information due to the choice of study populations By treating addiction as a category rather than a continuum, BETA researchers frequently rely on clinical populations that fail to represent the full range of patterns of consumption of a given drug. According to Heyman (2001: 91), "most addicts recover, but this is only apparent if the addicts are

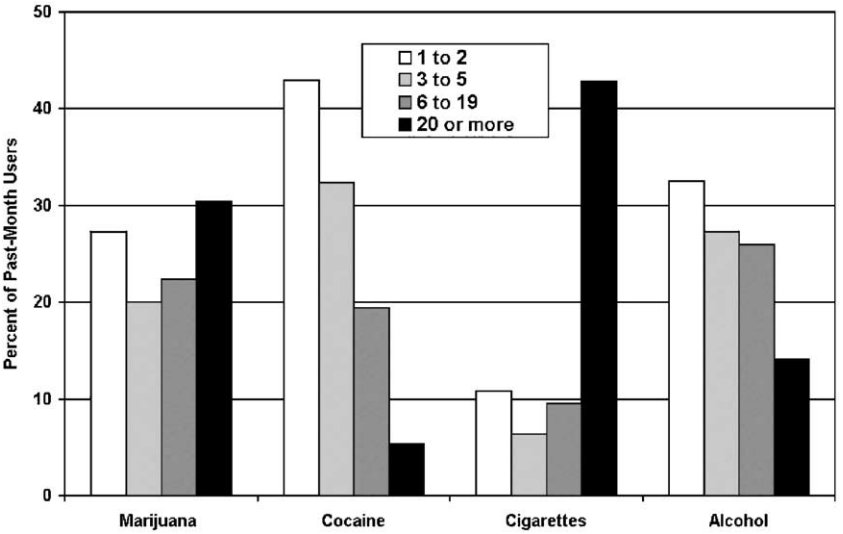

Figure 2: Days of use in past month.

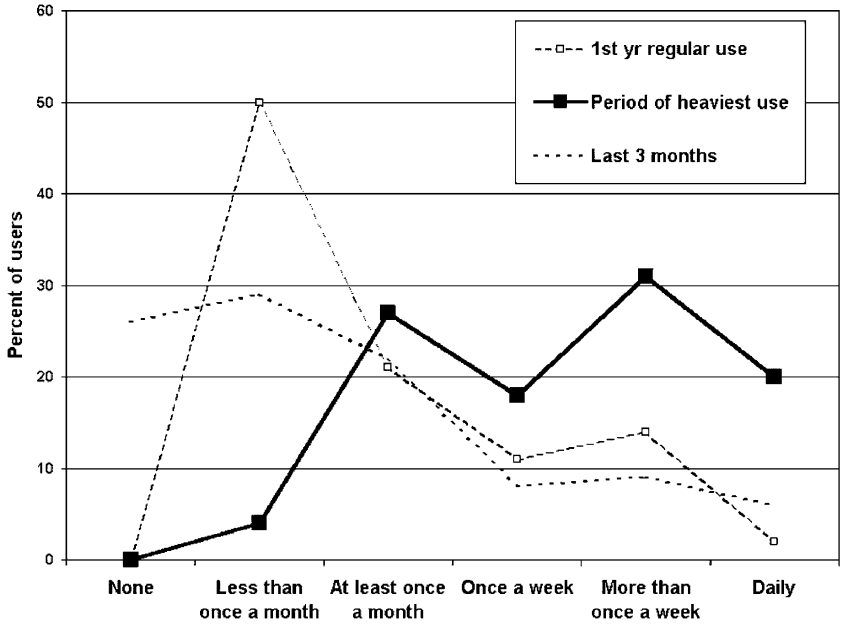

Figure 3: Frequency of cocaine use among Amsterdam users, 1991.

selected independently of their treatment history." Two BETA studies present evidence suggesting that hyperbolic discounting might vary gradually rather than discontinuously between clinical "addict" populations and other users. Vuchinich & Simpson (1998) found that while heavy drinkers and/or problem drinkers showed stronger temporal discounting than light social drinkers, the 75th percentile for discounting among light drinkers fell near the median for heavy drinkers. Bickel *et al.* (1999) found a multimodal distribution of delay discounting parameter values among current cigarette smokers, overlapping considerably at the low end with values for ex-smokers and never-smokers.

Would we eliminate the drug problem if we eliminated addiction? One can reasonably defend a focus on clinically-defined addicts as follows: not all drug use is harmful; a society that values individual liberty ought to concentrate its attention on those users who are harming themselves and/or others. I have much sympathy for this viewpoint. But matters are not so simple; the risks of drug use vary continuously across users with no apparent "step function."

For the sake of argument, let's say we actually cured addiction — i.e. any user who crossed a certain behavioral threshold could be recalibrated — restored to a state of non-addiction, perhaps even one permitting "controlled use" of the drug in the future. Clearly, this would significantly shift American policy away from a primary emphasis on law enforcement to a greater emphasis on treatment; indeed, it may be the only way such a shift could occur given the great political advantages of being "tough on drugs." And the "drug problem" would be reduced dramatically. But not completely. How much of a problem would remain?

The answer depends on some empirical questions that have received some attention in alcohol epidemiology (Edwards *et al.* 1994), but have been largely neglected in the illicit drug domain (MacCoun 1998). What does the consumption distribution look like? What are the dose-response functions that link consumption to various health and safety harms?

We know that the distribution of users by consumption levels is positively skewed. Presumably, addicts are mostly located in the long right tail. We can reduce the harm of drug consumption by either targeting the heaviest users (the right tail) or, as some alcohol experts recommend, by trying to target the great majority of users near the middle of the distribution (see Rose 1992; Skog 1993). Presumably, the greater the share of total consumption due to heavy users, the greater the efficacy of targeting them. So if addiction were cured, would the right tail be eliminated, or just "thinned out?"

A cure for addiction might reduce, but will surely not eliminate, the acute harms of intoxicated driving, parenting, work behavior, and so on. A few facts about alcohol are sobering. It is estimated that teenager drinkers — few of whom are likely to be "addicted" (at least not yet) — account for 11.4% of all alcohol consumed in the U.S. (CASA 2002). In the 2000 Drug Abuse Warning Network study, 12–17 year olds account for 17.7% of all emergency room mentions of alcohol (SAMHSA 2001). Among drivers in fatal accidents, the age 21–24 group consistently has the highest proportion with blood alcohol levels exceeding 0.10 (NHTSA 2000).

Indeed, the literature on "compensatory behavioral responses" to risk reduction (reviewed by MacCoun 1998) suggests that a cure for addiction might actually encourage much intoxication that would not otherwise take place. Existing users might have less reason to fear a binge; non-users would have less reason to fear initiation. Whether these increases would be large enough to offset the sizeable reductions due to the elimination of addiction is not clear.

Much depends on the parameters of the relevant dose-response curves linking drug use to its various consequences. Such curves are usually *S*-shaped. When they are very steep, even moderate consumption levels are risky. Presumably, some "acute" risks are primarily sensitive to dosage per incident (e.g. driving accidents, overdoses, unsafe sex, and what Goldstein [1985] calls "psycho-pharmacological violence"), whereas other risks are triggered more by chronic use over time (e.g. deteriorating health, bad parenting, and Goldstein's [1985] "economic-compulsive violence").

Interestingly, the recent Swiss heroin maintenance trials suggest that these dose-response functions can vary dramatically with legal context (Reuter & MacCoun 2002). Registered addicts who were eligible to receive heroin from government clinics massively increased their daily doses, yet they significantly increased their legitimate work participation and significantly reduced their income-generating criminal behaviors.

Elsewhere my colleagues and I have decried the American tendency to almost single-mindedly equate drug policy with "prevalence reduction" — a reduction in the number of Americans who use a given drug. Arguably, a more sensible overarching goal is "total harm reduction" – reducing the total social harm caused by a given drug. But *total harm* = *average harm per use* × *number of users* × *average amount used*, and the emphasis on prevalence reduction (something we're not very good at) leads to the neglect of two other strategies – quantity reduction and harm reduction (MacCoun 1998; MacCoun & Reuter 2001).

It is surely better to categorize users into "addicts" vs. non-addicted users, rather than mindlessly (and moralistically) lumping heavy users together with extreme casual light users (see Caulkins 1997). But we should be wary of reifying an extreme corner of a continuous, multidimensional space constituted by the dimensions of frequency of use, quantity consumed per use, and harmfulness of conduct while intoxicated. Doing so begs the questions I noted above — the need to know the shape of the consumption distribution and the relevant dose-response functions linking use to harms.

With Friends Like These . . .

It is regrettable that this paper has such a critical tone. My purpose in raising these arguments is not to discourage behavioral economic work on drug use — far from it. But a candid assessment suggests that, at least so far, BETA's insights into drug policy fall into two categories. They are either largely redundant with the conventional wisdom as expressed by existing policy strategies (viz., drug prevention and supply reduction), or they are quite innovative but seem not to require any conception of "addiction" as a distinct state or category of experience (viz., treatment and self-control strategies). The first category is no fault of the theorists, but the second suggests that the addiction concept just isn't that useful. In my view, the value of the behavioral economic comes from its analysis of self-control (a broad category), not from its analysis of addiction (a very narrow one) — in short, from BEAT (the behavioral economic analysis of temptations) rather than BETA (behavioral economic theories of addiction).

Are there policy implications I (and the BETA community) have overlooked? Probably. I can see at least four areas for future development.

(a) Structuring of the very "local" (in time and space) economy to help facilitate better self-control (an idea floated in various ways by several authors of this volume; see Wertenbroch 1998; Loewenstein & Kalyanaraman 1999 for marketing examples).
(b) The development of more psychologically realistic law enforcement tactics for achieving deterrence (Kleiman 2000, 2001b; MacCoun & Reuter 2001: Chaper 5).
(c) The incorporation of behavioral economic principles into analyses of the dynamics of drug epidemics and the strategic timing of interventions (Behrens *et al*. 2000).
(d) A behavioral economic analysis of the triage problem in the design of heroin and other opiate maintenance schemes — deciding who should be eligible, and when (Reuter & MacCoun 2002).

If I can name four, then hopefully readers can come up with many more. I see no reason why an assessment of the policy payoffs of a behavioral economic analysis won't be considerably more upbeat a decade from now.

Notes

1. I take the members of this set to include the recent work of such theorists as Becker *et al.*; Prelec *et al.*; Ainslie; Rachlin; Elster; O'Donoghue & Rabin. Much of this work has been summarized in

various chapters in the recent volumes, *Addictions: Entries and Exits* (edited by Elster 1999), *Getting Hooked: Rationality and Addiction* (edited by Elster & Skog 1999), *Breakdown of Will* (Ainslie 2001), and *The Science of Self-Control* (Rachlin 2000). Note that Ole-Jorgen Skog at this conference (see Chapter 5) questions whether Becker's model is in fact a model of "addiction."

2. The book is the major product of a grant from the Alfred Sloan Foundation to the RAND Corporation's Drug Policy Research Center.

3. This section draws heavily on arguments developed in much greater detail in MacCoun & Reuter (2001) and Manski *et al.* (2001). But many of these arguments were independently developed and presented by Mark Kleiman at a conference on "The Uses and Misuses of Science in Public Discourse," Boston University, April 1, 2000 (see Kleiman 2001a).

4. "To design treatments and policies that will make people quit their addictions or never become addicted in the first place, it is useful to have an understanding of the causes of addiction and relapse" (Elster & Skog 1999: 1). "If economists want to contribute to the police debate over how to deal with addictions, we need to develop a systematic approach to analyzing self-control problems and other errors rather than assume them away. We hope our analysis will prove useful in this regard." (O'Donoghue & Rabin 2001: 37 of preprint version). Becker and his colleagues (1992: 362) consider "highly tentative inferences concerning the effects of legalization . . ." and Herrnstein & Prelec (1992) devote three pages to a section on "policy implications" of their theory of addiction.

5. http://www.samhsa.gov/oas/nhsda/2kdetailedtabs/Vol_1_Part_4/sect6v1.htm#6.2b

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Comments on MacCoun

Charles R. Schuster

The issue addressed by Rob MacCoun's paper is whether addiction theory is relevant to drug policy. Specifically he is concerned in the context of this conference with the relevance of behavioral economic theories of drug addiction to the development of drug policy. His conclusion is that neither behavioral economic theories of drug addiction nor any other theories of addiction are relevant to the major issues that drug policy experts address. His evidence for this conclusion is that neither in his book (MacCoun & Reuter 2001), which I consider a major contribution to the field, nor in the NRC Monograph by Manski *et al*. (2001) is there any substantial discussion of "theories of addiction." If I understand Professor MacCoun's position correctly this irrelevancy stems from two sources: (1) drug policy experts are concerned with all levels of non-medical drug consumption, not just the use of drugs by those who are addicted; and (2) the fact that behavioral economic theories of addiction are simplistic and make unwarranted assumptions about the nature of addiction.

Let me address these contentions separately. Clearly our society must be concerned with the consumption of powerful mind-altering, performance-impairing drugs whether these drugs are taken once or repeatedly. From my viewpoint as a psychopharmacologist the harms created by drugs are often greatest in inexperienced drug users. I have seen many first-time users of psychoactive drugs enter emergency rooms because of panic attacks brought on by the novelty of the psychedelic experience. Automobile accidents associated with ingestion of alcohol or other performance-impairing psychoactive drugs can occur after the first drug experience. Obviously, the devastation to the individual, their family and community increases as the magnitude of drug use increases, i.e., as the individual makes the transition from drug experimentation to regular drug use and finally compulsive drug use (addiction). Clearly, opportunities for calamitous accidents, and interference with normal work and social relationships increase with the number of incidents of intoxicating drug exposures. However, the numbers making this transition from drug experimentation to addiction is comparatively low for most illegal drugs. Thus, the total harm produced by illegal drugs may be greatest for the myriad of drug experimenters rather than the relatively few who might be labeled as addicted. Therefore, drug policies must be concerned with the prevention of drug-induced social and public health harm to society at all levels of consumption. I agree with Professor MacCoun that we cannot confine our concern only to those that meet some arbitrary criterion that allows them to be labeled addicts. Thus, theories of addiction per se are not relevant as they only apply to a minority of the problems that can be attributed to illicit drug use.

MacCoun lists a number of means which drug policy experts recommend to limit non-medical psychoactive drug use: drug prevention, education, and rhetoric from the bully pulpit; drug treatment; criminal sanctions against users; criminal sanctions against dealers; interdiction and source country controls; taxes, advertising controls, and other regulatory mechanisms; drug testing; and bans on employment, welfare, and other benefits. These interventions are not limited in their application to those who are addicted to drugs. Rather they are aimed at decreasing all non-medical drug use and its associated social and public health consequences.

Similarly MacCoun lists a number of explanatory constructs used in the analysis of drug policy. These include: the prevalence and incidence of drug use; the statistical distribution of quantities consumed and frequency of consumption; the price elasticity of demand for drugs; the time sensitivity and/or impulsivity of drug users; the deterrent effect of drug-law sanctions (certainty, severity, and celerity); the dose-response relationship between consumption of a drug and its various acute and chronic effects; the relative contribution of psychoactive effects of a drug vs. its illegality in producing drug-related harms (criminality, morbidity, mortality, impaired functioning, etc.); possible substitution, complementarity, and "gateway" relationships between the use of tobacco, alcohol, and marijuana, and subsequent use of harder drugs; the unintended effects of use-reduction strategies on drug-related harms, and the unintended effects of harm-reduction strategies on drug prevalence and consumption; the distribution of the harms (and perhaps benefits) of drug use across bearers – the user, family, friends, neighbors, the community, taxpayers (MacCoun, Reuter, and Schelling 1996). Here I can see more potential relevance of theories of addiction, such as behavioral economic theories, to some of these constructs. For example, how does price elasticity change when one progresses from regular drug use to "addiction?" How do individual differences in delay discounting functions predict those who may be at greater risk for progressing from regular drug use to addiction? It is clear to me, however, that the important contributions of behavioral economics in this context are in providing important analytic tools rather than a behavioral economic theory of addiction. I believe that Professor MacCoun and I would agree that although behavioral economic "theories" of addiction may not be relevant to drug policy, the methods of analysis, i.e. the tools provided by behavioral economics, are extremely important for the analysis of drug policy issues.

Let me illustrate this importance. A recent study conducted at Wayne State University looked at the impact of "cost" incurred by patients in methadone maintenance treatment programs for opiate addiction (Borisova 2000). Because of state and federal regulations designed to prevent diversion of methadone, patients early in their treatment must come to programs on a daily basis to receive their dose of methadone. After several months, only those who are showing progress in controlling their use of illicit drugs are allowed to take methadone home for periods up to seven days. Analysis of the "cost" of out-of-pocket expenses, time in transit and waiting in the clinic for medication dispensing were shown to be excellent predictors of retention rates. The higher the cost of daily program attendance, the lower the retention rates of patients. This clearly has policy implications for the siting of methadone clinics and other regulatory issues.

I am very excited about the application of the analytical tools of behavioral economics to our description and understanding of drug consumption, drug abuse and drug dependence. I agree with MacCoun, however, that the theories of addiction generated by behavioral

economics have not been helpful to me as a drug policy analyst, laboratory researcher or treatment provider. I also agree with Professor MacCoun that the concept implicit in economic theories of addiction that addiction is a unitary phenomenon with one theoretical explanation is overly simplistic. A functional analysis of the pathways leading from drug experimentation to regular use to compulsive drug use reveals that there is no single pathway to addiction. Individuals begin drug use for a number of social and psychological reasons. Whether drug use is continued depends both on individual reactions to the drug, co-existing psychopathology, the social consequence of drug use, and alternatives to drug use that are available, not to mention the public policies in effect within the individual's culture. Psychoactive drugs may attenuate the negative symptoms of various forms of psychopathology and continued drug taking may be at least initially a form of self-medication. Drug experiences may not at first be positively reinforcing and may even be aversive. However, if powerful social reinforcers are contingent upon being part of the drug using sub-culture, this may override the aversive effects of drugs. Tolerance to the aversive effects may develop and the positive reinforcing effects of the drug emerge. Individuals who lack the opportunity or ability to find other positively reinforcing pro-social activities find that the drug-using life style fills a void. Professor David Deitch of University of Southern California, who is himself a recovering heroin user, has asked: "who do you know that wakes up every morning, thinks of their life goal and achieves it every day? A heroin addict" (personal communication). I cite all of these things simply to state the obvious. There is no single pathway to addiction.

Professor MacCoun has alluded to the contingency management interventions for the treatment of substance abuse developed by Steve Higgins and colleagues at the University of Vermont. These interventions have proven to be extremely effective in decreasing illicit drug use. They clearly can be viewed as behavioral economic interventions involving providing robust positive reinforcers for drug abstinence. I would argue, however, that these procedures stem from classic principles of behavioral analysis and not behavioral economic theories of addiction.

Although I agree with Professor MacCoun that theories of addiction in general have had little if any impact on drug abuse policies, I would argue that, to the extent that such theories influence the manner in which the problem of drug abuse is conceptualized, they may be of great importance. My views in this area are based upon my nine years with the federal government during which I was the Director of the NIDA. At the risk of over-simplification I would state that, in regard to federal drug abuse policy makers, there are those who wish to conceptualize drug abuse and dependence as a problem stemming from ethical and moral weakness and those who would conceptualize it as a public health problem. As has been stressed previously (Moore & Gerstein 1981), the most fundamental and important determinant of policy is the manner in which a problem is conceptualized. Policies generally flow from relatively simplified "conceptions" that determine the "governing ideas" from which specific instances of policy are derived. If drug abuse/dependence is conceptualized as the expression of a problem of weak moral constraints leading to unfettered hedonism, then the governing ideas and derivative policies flow almost inexorably. Governing ideas are usually short, easily remembered "slogans." "Drug Free America" or "Zero Tolerance for Drugs" are two examples of governing ideas stemming from a moralistic conception of the drug abuse problem. They lead to an emphasis on punitive approaches to deter drug use and

policies such as a ban on needle exchange programs and other harm minimization interventions. One cannot have "Zero Tolerance for Drugs" and make it safer for people to use them. On the other hand, if theories of addiction can amass sufficient, compelling arguments that drug dependence is most usefully conceptualized as a chronic relapsing disorder, similar in its characteristics to other diseases such as morbid obesity or arthritis, the policies adopted to control this problem will be quite different. Clearly, if drug dependence is a chronic relapsing disorder it should be conceptualized as a public health problem. The governing idea flowing from the conception of drug abuse as a public health problem is to treat it as we do any other public health problem. Derivative policies from this conceptualization would be to conduct surveillance activities to determine incidence, prevalence and harmful consequences of drug abuse. Resources would be devoted to the development and implementation of cost-effective prevention, treatment and other harm minimization interventions. I want to emphasize the utilitarian approach that I am suggesting here. Whether drug abuse is conceived of as moral weakness or as a disease has less to do with the *rightness* of the definition of the problem than whether the conceptualization leads to policies and programs which are cost-effective in decreasing the problem of illicit drug use and its tragic consequences. By cost-effective in this context, I include not only the usual cost considerations, but as well, the costs of an intervention to the society's citizens in terms of their loss of civil liberties and freedom from government intrusion.

The complication for the area of drug abuse policy is that, in fact, drug abuse is both a problem of ethics and morality and a public health problem. Thus, both conceptions of the problem are justifiable, but I would argue they are useful at different stages in the natural history of the development of the problem. Moral training can of course deter individuals from ever experimenting with drugs. For instance, adolescents who spend more time in religious activities are less likely to use illicit substances (Johanson *et al.* 1996). There are, however, significant limitations to the role of religious and ethical training as a deterrent to drug experimentation. First, only a minority of youth in the United States has a significant involvement with formalized religion. Further, the high prevalence of psychopathology, especially antisocial personality, found in drug abusers sets limits on the population who are amenable to drug abuse prevention through ethical and moral constraints (Regier *et al.* 1990). Finally, where there is a breakdown in the structure and functioning of the family and community, children may not be given the ethical and moral training that would deter drug use. If such ethical and moral constraints are ineffective or absent, for whatever reason, and the individual escalates from drug experimentation to dysfunctional use and addiction, the problem changes. Then the problem is most usefully conceptualized as a public health issue and the individual as afflicted with the disease of drug addiction. At this stage of drug addiction, moral constraints alone are as likely to be effective as they would be in the treatment of arthritis. Indeed, one goal of treatment for drug addiction could be conceived of as engendering a state in which ethical and moral constraints against illicit drug use can be effective in maintaining abstinence.

In the United States there appears to be a large schism between those who conceive of drug addiction as an ethical and moral problem and those who see it as a public health problem. Unfortunately, I believe that an overly simplistic interpretation of behavioral economic theories of drug addiction may give support to the conception of drug addiction as a problem of morality. "Choice" is a central tenet of behavioral economic theories of

drug addiction that can easily be misconstrued to imply that addicts "choose" that life-style. It is not easy to sell policy makers on the notion that there are determinants of choice — not unfettered free will. I think it is imperative that behavioral economic theories of addiction make it very clear that there are biological (genetic) and behavioral constraints on choice which must be considered if we are to develop effective prevention and treatment strategies. To maintain the concept of "choice" without unwittingly giving support to moralistic solutions to the problem of addiction is a serious challenge for behavioral economic theories of addiction.

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Reply to Schuster

Robert MacCoun

In reaction to Professor Schuster's insightful comments, I offer only two brief clarifications.

First, I agree that behavioral addiction theories probably oversimplify the addiction phenomenon, but I would argue that the main explanation for their lack of policy relevance is their considerable overlap with popular intuitions about low self-control and impulsiveness, which have already shaped our drug policies.

Second, my essay did question the existence of a bright line between addicts and other heavy users. And I did argue that I believe recreational users, because of their large numbers, can contribute substantially to aggregate drug harms — though I think the relative contributions remain an open empirical question. But I would not want readers to believe I see all drug use as equally troubling. Elsewhere, I have decried the American preoccupation with prevalence (drug users vs. non-drug users), which I would replace with a focus on the harmfulness, quantity, and frequency of drug use (MacCoun 1998; MacCoun & Reuter 2001). Bright lines are rhetorically convenient, and may even help individuals control their behavior, but they are not encouraging constructive thinking about strategic drug policy.

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