Chapter 10

Rational Addiction and Injection of Heroin

Anne Line Bretteville-Jensen

Introduction

Theories of hyperbolic discounting, melioration, and relative addiction assume that individuals are non-rational in the sense that they do not consistently maximise utility over the life span. The theory of rational addiction (Becker & Murphy 1988), on the other hand, assumes that individuals have stable time preferences and exponentially discount the future. Becker & Murphy demonstrate how a rational consumer may start consuming an addictive good and end up in a situation in which the consumption level of the addictive good is very high and sub-optimal for that user. According to the theory, a high consumption level is a consequence of a series of previous choices, not the result of an “addiction illness,” lack of information, or irrational behaviour. The work of Becker & Murphy has been met with both enthusiasm and rigorous criticism.

Traditionally, economic consumer theory has been concerned with individual choices under constraints and a phenomenon like addiction has attained little attention. Based on standard assumptions of utility maximisation, stable preferences, etc., little effort has been spent on examining why people start consuming a certain good or why they eventually stop it. Further, consumption of addictive goods like cigarettes and heroin causing severe problems to the user and third parties, have been discussed more within other disciplines than among economists. As shown in Chaloupka et al.'s paper in this volume (Chapter 2), however, Becker & Murphy were not the first economists to show an interest in addiction, but they were the first to launch an addiction theory based on standard neo-classical assumptions. In this paper, one of these assumptions and three derived hypotheses will be examined empirically and discussed.

After a short introduction to Becker & Murphy’s theory and a description of the data sets, we examine how the assumption of stable time preferences and the hypotheses of price response and consumption stand after being “tested” against data collected among heroin addicts in Norway. Becker & Murphy’s theory of rational addiction postulates that drug addicts have a higher time preference rate than non-addicts, that addicts’ drug consumption is sensitive to changes in prices and income, and that past and future drug consumption
influences present consumption of the drug in question. In the concluding section we discuss our empirical results, the theory we applied, and possible policy consequences of our findings.

The Theory of Rational Addiction

Rational addiction sounds like a contradiction in terms, but Becker & Murphy claim that even this type of behaviour is amenable to analysis by standard theory. Addiction is defined as a strong complementarity between past and present consumption, i.e. an increase in past consumption causes an increase in consumption of the same good today. Rationality signifies that individuals take into account the interdependency between past, present and future consumption when utility over the life span is maximised in a consistent fashion. Becker & Murphy introduce a utility function specified with continuous time:

\[
U(0) = \int_0^\infty e^{-\delta t} u(C(t), Y(t), S(t)) \, dt
\]

(1)

where

\[
\dot{S}(t) = C(t) + L(t) - \delta S(t)
\]

(2)

\(C(t)\) is the addictive good, \(Y(t)\) is the vector of non-addictive goods, \(S(t)\) is the stock of “consumption capital” and \(\delta\) is the time preference rate. The rate of change in consumption capital is a function of consumption of \(C\) and biographical episodes \((L)\), along with consumption capital depreciated with a rate \(\delta\). Two key aspects of the addictive good set certain limits on the utility function and are defined by consumption capital:

- **Tolerance**, \(\partial u / \partial S < 0\), that is, the utility of any consumption level today will be lower if the individual had a high consumption level of the good in the past.
- **Reinforcement**, \(\partial C / \partial S > 0\), that is, higher past consumption stimulates the wish for high present consumption. Reinforcement means that the marginal utility of consumption today increases the more the individual has consumed in the past \((u_{CS} > 0)\). Under certain conditions, this could lead the individual to increase consumption of the good. This inherent dynamic in consumption is peculiar to addictive goods.

Consumption capital therefore has two opposing effects on utility and future consumption and, for a rational utility maximiser who consumes addictive goods over time, the reinforcement effect will be greater than the tolerance effect. Becker & Murphy (1988) show, through maximisation of a quadratic design of the utility function, that a sufficient and necessary condition for this is:

\[
(\sigma + 2\delta)u_{CS} > -u_{SS}
\]

(3)

The reinforcement implies “adjacent complementarity”; that is, the consumption of the addictive good over different periods is complementary. Equation (3) indicates that the higher the time preference rate, the higher the depreciation rate or the more the marginal utility of \(C\) rises with rising \(S\), the greater the potential for addiction. Strong adjacent
complementarity can cause unstable equilibrium points along the optimal consumption path. The existence of this type of unstable equilibrium point is key to the theory because it, inter alia, explains how things like peer pressure, divorce and the like can cause people to develop an addictive condition, as well as explaining how a person’s consumption of the addictive good can vary across time despite the absence of any changes in the person’s economic environment.

The individual sets his/her optimal consumption of goods on the basis of utility function, prices, income, initial stocks of consumption capital and time preference rate. The “full” price of $C$ corresponds to market price $P_C$ and the value of future costs (including damage to health, loss of social relationships, esteem, etc.). For constant values of the other factors, an optimal consumption path for a variety of combinations of $C(t)$ and $S(t)$ is given in Figure 1.

The consumption path illustrated in Figure 1 (a corresponding figure can be found in Becker & Murphy 1988) is based on a quadratic utility function. The steady state line gives all combinations at $C(t)$ and $S(t)$ where the consumption capital does not lead to any change in consumption ($C = \delta S$). If the optimal consumption level lies over the steady state line, the reinforcement mechanism will cause consumption to rise until equilibrium is reached. If the consumption path lies beneath the line, consumption will decline over time. Point A is an unstable point of equilibrium and B is stable. The optimal consumption path in Figure 1 corresponds to a high time preference rate.

Given that the assumptions underlying Figure 1 hold, the initial value of consumption capital will be the decisive factor in determining whether a person develops a high consumption level (addiction) of the good. The initial value will be $\geq 0$ since biographical events are also included in $S$ (cf. Eq. (2)). If the agent’s consumption level is low ($S \leq S_A$), s/he will be either to the left or at the unstable point of equilibrium. And there s/he may remain for a time. However, should a life crisis or similar life-shaking events occur, the agent may fall out of or be ejected from this state of unstable equilibrium, move towards the right along the consumption path and end up in a stable equilibrium state with a significantly higher consumption level (B). A consumption level equal to $C_B$ will be suboptimal for the person if the long-term negative effects of past consumption exceed the immediate gratification of a high present consumption rate. For a change to occur and consumption to be lowered, consumption capital needs to fall. A reduction in consumption...
capital is distressful in the short run and, with much emphasis on the immediate future, the person will not be willing to reduce utility on a provisional basis to increase it in the long run. When the individual arrives at equilibrium B, s/he will remain "imprisoned" there and only changes in the exogenous variables will be able to "liberate" the person.

Heroin Addicts in Norway

The data sets used to test Becker & Murphy's theory are based on interviews with heroin injectors. The interviews took place in the vicinity of a needle exchange service in the centre of Oslo. Three types of data were collected: (1) anonymous interviews, conducted on a regular basis from June 1993 to September 2000, with drug users who attended the needle exchange service; (2) re-interviews with a group of attendees one year after the initial interview (individual panel data); (3) interviews with "non-attenders"; a control group (non-addicts) with similar characteristics to a group of attendees and interviews with a group of former addicts.

The first data set is based on the regular interview sessions that took place first on a monthly basis, then quarterly, from June 1994. A total of 3,039 questionnaires were completed. There were 2–4 interviewers working 2–3 nights for each data-collection session and people were approached after they had used the needle exchange service. As many as possible were asked to participate, but asking everyone was impossible because people often came and left in groups. Except for a short period in 1997, when we were granted special authorisation by the Norwegian Data Inspectorate, the interviewees were anonymous. Thus, it was not possible to register participants in order to help recognise them from one interview session to the next. Some individuals will therefore have been interviewed more than once, but precautions were taken to prevent this from happening during the same interview session. The mean age for the whole sample was 31.6 years (29.6 for females and 32.5 for males). The youngest person was 16 years old and the oldest 59. Females constituted 32% of the sample.

The representativeness of the current sample is difficult to ensure and can be questioned as follows. First, are the drug users attending the needle exchange service representative of all drug injectors in the area? Second, are the individuals included in the sample representative of the attendees? These questions have been discussed elsewhere (Bretteville-Jensen & Bjørn 2003a) and, based on a comparison of variables like gender and age distribution etc., with what is known about this group from other studies, the sample may be assumed to be fairly representative of injectors in the Oslo area.

Out of the total of 3,039 completed questionnaires, 2,595 addicts reported that they mainly injected heroin. The remaining group of 444 consisted of people who mainly injected amphetamine, injected both drugs equally frequently, mainly injected other drugs (morphine, methadone, etc.), or did not respond to this particular question. Injecting is an extreme way of consumption. Even if other routes of administration, like sniffing and smoking, appear to be less dramatic, Norwegian heroin users seem mainly to prefer injection. We decided to include only heroin users in the present sample. Some questionnaires were excluded due to missing information on age, gender, ranking of income sources, or prices of the drug injected.
The second data set, containing individual panel data, was based on a group of drug users interviewed in the regular data collection who were asked if they would consent to being interviewed with the same questionnaire in about one year's time. Those who agreed were asked to identify themselves so that they could be contacted by the interviewer twelve months ahead. Out of a total of 286 persons interviewed from March to September 1997, 171 agreed to participate in the panel data study.

It is possible that, for various reasons, those who agreed to participate in the panel data study differed from those who declined to take part. As it happens, this does not seem to be the case and statistical testing showed that the two groups did not differ significantly. When comparing the 171 drug users in the panel data sample with the 115 who only took part in the first regular quarterly data collection, we found a markedly similar distribution among variables such as age, gender, education, age at first injection, number of hiatuses in drug career, income, amount of heroin per injection, and total amount of heroin consumed in the previous month.

Drug injectors constitute an unstable group with respect to where they live, how much and what types of drugs they consume, how they obtain money and so on. They often report poor housing conditions as many live on the streets, provisionally with a friend or in bed-sits in apartment blocks. They are often in prison, a treatment institution, or hospital. Thus, drug injectors are generally very hard to trace for re-interview. So, even though we had their address at the time of the initial interview, we spent a great deal of time later searching for those who had agreed to participate. As we regained contact with our sample, we began to re-interview those who were interviewed in March 1997 in March the following year, and by the end of 1998, 138 of the 171 drug users had been traced (retrieval rate of 81%) and re-interviewed. Among the group of 138, only 84 persons (61%) were still active drug users. Fourteen (10%) were in prison; 11 (8%) were in residential treatment institutions; 10 (7%) were dead; 10 (7%) did not want to give a second interview; 8 (6%) had stopped consuming illegal drugs; and one person (1%) was in hospital.

Comparisons between those who were active heroin injectors both in 1997 and 1998 and those who injected heroin only in 1997 showed few differences between groups regarding the variables we are able to control for. Of the initial sample of 171 drug injectors, 156 reported that they mainly consumed heroin, and 78 out of the 84 re-interviewees reported the same. The final analyses of this second data set are restricted to the group of 78 heroin injectors.

The third data set covers three groups of people: G1 is a group of 110 heroin injectors who answered an additional set of questions when they responded to the regular questionnaire; G2 is a case control group of 110 non-injecting people with characteristics similar to G1 with respect to gender, age, and education. In accordance with discounting theory, we assumed that it was age and education in particular that we should control for. The non-abusers were approached in the street, shopping centres etc., and were randomly selected from passers-by. In addition to questions on the discount rate, respondents were asked about their consumption of intoxicants over the last year. G3 consists of 50 former drug abusers. This was the group that was hardest to find. In the main we employed the "snowball method": on the basis of our acquaintanceship with a few former abusers, we were able to make contact with others. A person was defined as a former abuser if they stated that they had previously been a long-term abuser of either heroin or amphetamine. By including the latter group it
was assumed that we might get an indication of the stability in preferences within a group of people who had all been seriously addicted. In contrast to the face-to-face interviews for the groups above, the interviews for this group of former users were mainly conducted by telephone. The questionnaire used here was the same as for the group of non-users.

**The Questionnaire**

Interviewees we contacted at the needle exchange service were asked to give details about their levels and sources of monthly income, levels of drug consumption and the prices they had paid for the different types and quantities of drugs. Initially, concerns regarding the response rate in these outdoor interviews put constraints on the feasible length of the questionnaire and only a few socio-economic variables (gender and age) were included. After some time, however, we decided to add questions like age at first injection, education, number and total length of drug-free periods, housing conditions, experience with non-fatal overdoses, etc.

**Income:** Respondents were asked how much money they got from six possible sources: work, state benefits, stealing, sale of drugs, prostitution and "other" sources. This involved two stages: individuals were first asked to rank the six possible sources of income in proportion of total income; then they were asked to estimate the amounts acquired from each source. Besides social benefits, drug dealing among men and prostitution among women were the most frequently reported income sources.

Information on income was far from easy to obtain. Most interviewees responded to the ranking exercise, but some failed to give an estimate of their monthly incomes from the different sources. To avoid possible bias due to non-response, we imputed the missing values. For example, an individual who reported drug dealing as the second most important income source could be assumed to have obtained the average amount reported by others who ranked dealing second. This allowed us to assign an income value to 44 heroin users who had only responded to the ranking exercise. In addition, it increased monthly income for some who had not completed the questions for all income sources they had ranked. The interpolation raised slightly the sample's average income (from 38,000 to 44,000 Norwegian kroner or 4,270 to 4,950 US$).

**Consumption:** For heroin users, a combination of three variables was used to estimate monthly consumption: quantity of drugs for last injection; number of injections during previous day; number of injecting-days in the previous month. The wide variation in the amount-per-injection was only recognised after some time into the interview period, but a question to pin down the exact amount was then included in the questionnaire. Heroin dealers reported consuming more of the injecting drug than their non-dealing counterparts. Females consumed more heroin than males.

Most respondents also reported extensive use of other intoxicants. The number of drug-taking/drinking days for the month leading up to the interview was available for alcohol and cannabis. We constructed a dummy variable for each of these which we set to unity in cases of 20 or more days of use, since we assumed that this consumption frequency would be needed in order to classify a potential substitute or complementary good to heroin.
For pills, we only have information on consumption or non-consumption in the previous month and the corresponding dummy variable was set to unity if the respondent reported consumption.

**Prices:** Price data were obtained by asking people what they would have to pay for different types and quantities of drugs. Dealers were asked how much they had paid for the last quantity of drugs they had bought (at least partially) for dealing purposes. The number of units into which a gram of heroin was "cut" declined over the period and we have taken account of this in calculating equivalent unit prices for those who buy in grams. The price of heroin decreased throughout the study period. For heroin users not reporting any amphetamine price, we constructed a variable where dealers and non-dealers were given the average price reported by amphetamine dealers and non-dealers in the corresponding interview session.

The market purity of drugs could be an important determinant of the price-responsiveness of consumption, and trends in market prices should ideally be presented in quality-adjusted terms to reflect the possible effects of purity changes on behaviour. Police seizures indicate large variations in heroin purity at the wholesale level, though it is somewhat more stable at the retail level. However, the purity is often unknown to the buyer at the time of purchase, so quality-adjusted prices may not be very useful after all. In any case, it was not possible to collect any purity data here.

**Testing the Addiction Theory of Becker & Murphy**

Implications of the theory have resulted in several testable hypotheses such as the following: (1) consumption of the addictive good will have a bimodal distribution in the population; (2) addicted persons will cease consumption suddenly (cold turkey); (3) addicts will be sensitive to changes in economic factors; (4) adjacent complementary implies that current consumption will be positively correlated with past and future consumption; (5) long-term price elasticity will be greater than short-term price elasticity; (6) the effect of expected price changes will be greater than the effect of unexpected changes; (7) the effect of permanent price changes will be greater than the effect of temporary changes; and (8) the time preference rate (TPR) of addicts will, on average, be higher than for non-addicts (high or gives, ceteris paribus, higher likelihood of addiction). We will examine (3), (4), and (8) in addition to exploring the assumption of stable time preferences.

**Hypotheses Regarding Individuals Time Preference Rates**¹

The four theories of addiction discussed in this volume are all to some extent concerned with the time aspect of addiction and, thus, with people's discounting. Time preferences reflect how individuals view utility at different times and a discount function gives the present value of utility obtained at some later point in time. People's discounting is linked to addiction in that addiction is a consequence of a series of choices made over time and their impact will often reverberate far into the future. (Severe health problems from smoking can take decades
to materialise.) The rational choice theory assumes that individuals have stable preferences, i.e. it is assumed that their time preference rate is constant and that they discount the future exponentially. A departure from this assumption allows for the possibility of non-rational agents.

Becker & Murphy claim that people with high preferences in the present are more likely to end up with sub-optimal consumption levels corresponding to $C_B$ in Figure 1. The full cost of current consumption includes possible negative consequences occurring at later points in time, but with heavy discounting these negative consequences are given less weight. The time-preference rate contributes to determining where the path of the optimal consumption curve will be situated for the individual person and the shape of the graph (degree of curvature). Becker & Murphy make three statements about consumers' time preferences: (1) time preferences are stable; (2) there is great interpersonal variation in the size of the TPR; and (3) people with a high TPR are more likely to become addicted, that is, a high TPR is assumed to be a contributory cause of addiction. The "pure" TPR ($\sigma$) is here conventionally defined as the marginal rate of substitution between present and future utility and the corresponding discount factor is $1/(1+\sigma)$. The higher the value of the TPR (and the lower the discount factor) the higher the preference for the present.

Empirical estimations of how people balance utility between different periods are, however, difficult to make and no single method has yet been agreed upon. Analyses of both revealed and stated time preferences are commonly based on people's consumption (Deaton 1992) and may at best be taken as indicators of true TPR. The method of stated preferences in response to hypothetical questions has been employed here.

To examine the time preference rate, respondents were asked to imagine that they had bought lottery tickets and had won a specified amount of money. They were asked to give an estimate of how much they would be willing to sell the winning ticket for now if the prize money was not going to be paid out until one year ahead. In other words, we aimed to pin down the amounts that would give the respondents equal utility if they obtained the money now, compared to the utility the lottery prize would give them at some later point in time. The calculation of the discount rates was carried out as follows:

$$r = \frac{1 - (x/y)}{x/y}$$  \hspace{1cm} (4)

where $r$ represents the annual discount rate; $x$ is the amount the respondents state they would sell the winning ticket for if the winnings were to be paid out in one year's time; and $y$ is the amount won. Discount rates were estimated for all respondents and the mean values for each of the three groups are presented in Table 1.

Becker & Murphy's theory considers "pure" time preferences and the estimates obtained here are assumed to be indicators of this, but the empirical rates will additionally be affected by other factors such as individuals' income and wealth, life expectancy, and the extent of uncertainty and risk involved:

$$r_j = f_j(\sigma_j, \pi_j)$$ \hspace{1cm} (5)

where $r_j$ is the estimated discount rate for person $j$; $\sigma_j$ is the "pure" TPR representing individual $j$'s balancing of utility in different periods; and $\pi_j$ is a rate vector of all the other factors.
Table 1: Average of estimated annual rate and corresponding discount factor for the groups of active users, non-users, and former users of hard narcotic substances.

<table>
<thead>
<tr>
<th></th>
<th>Annual Disc. Rate ( (r) )</th>
<th>Annual Disc. Factor [1/(1+r)]</th>
<th>Size of Sample ( (n) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Active</td>
<td>0.90 (1.77)</td>
<td>0.53</td>
<td>110</td>
</tr>
<tr>
<td>Non-users</td>
<td>0.05 (0.13)</td>
<td>0.95</td>
<td>110</td>
</tr>
<tr>
<td>Ex-users</td>
<td>0.15 (0.24)</td>
<td>0.87</td>
<td>50</td>
</tr>
</tbody>
</table>

Standard deviation in parentheses.

Variations in estimated discount rates will be caused by individual differences in either \( \sigma_j \) or \( \pi_j \). If, however, \( \pi_j \) has a low inter-group variation, any difference found in the mean value for the three groups could be assumed to stem from differences in the “true” TPR. The effect of differences in income, wealth and credit restrictions on the estimates of the discount rate is difficult to evaluate. One could argue, however, that the reported differences in Table 1 may partly be influenced by differences in \( \pi_j \), but that these differences cannot fully explain the observed variations in discount rates.

A second method was used to elicit participants’ time preferences. In order to take account of the special, often acutely felt, financial circumstances in which drug abusers may find themselves, respondents were asked to choose between two ways of having the winnings paid out. Both methods of payment provided a daily sum large enough to cover the purchase of a single dose of heroin or amphetamine (200 Norwegian kroner). The choice was between two ways of payment, in which the first would provide 400 kroner daily for the first half-year and 200 kroner daily for the second half-year, while the other method gave 200 kroner the first half-year and 600 kroner the second half-year. For the addicts, the decision could be seen as a choice between two “free” daily doses of narcotics the first half year and one “free” dose the next vs. one dose the first half year followed by three daily doses for the rest of the year. By choosing the first method of payment, in which the larger sum came in the first half-year, they would end up with a lower total sum when the payment year was over (110,000 kroner as against 145,000 kroner). Impatience would therefore mean a loss in the total amount paid out. For each group the percentages choosing the two possible payment plans were calculated and the results can be seen in Table 2.

Table 2: Percentages opting for the two possible methods of payment of winnings.

<table>
<thead>
<tr>
<th></th>
<th>Proportion Choosing A ( (%) )</th>
<th>Size of Sample ( (n) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Active</td>
<td>32% [23.3, 40.7]</td>
<td>110</td>
</tr>
<tr>
<td>Non-users</td>
<td>8% [2.9, 13.1]</td>
<td>110</td>
</tr>
<tr>
<td>Former users</td>
<td>8% [0.5, 15.5]</td>
<td>50</td>
</tr>
</tbody>
</table>

Confidence intervals in brackets.
The average of estimated annual discount rates in Table 1 differ significantly among the three groups. Comparisons between the groups show that former users vs. non-users and active users vs. former users both differ significantly at a $p < 0.001$ level.

Non-users in Table 1 quote an annual rate roughly corresponding to interest rates for large capital deposits in the market. The corresponding rate for former abusers lies somewhat higher, but in the vicinity of the level for non-users.

The difference between the estimated discount rates for active users and non-users can be said to conform with Becker & Murphy’s theory. Table 1 shows that the active users state that they have a significantly higher discount rate. The high discount rate and correspondingly low discount factor ($0.53$) is consistent with what Becker et al. (1994) report in a study based on a data set of annual per capita cigarette sales for 50 states plus the District of Columbia for the period 1955–1985. By applying the rational addiction framework the authors obtain estimates of the discount factor ranging from $0.31$ to $0.64$ for different empirical models. However, Becker and his colleagues find their point estimates to be implausibly low and put forward the unrealistic assumption of perfect foresight as an explanation for their finding.

The great difference between the rates of active and former users, on the other hand, does not seem to accord with Becker & Murphy’s (1988) assumption of stable preferences. If high discounting is a contributory cause of drug abuse, one would expect former users also to have a high value on their discount rate. According to the theory, consumption capital will be reduced when the consumption of addictive goods ceases, but the actual discounting ought to remain unchanged.

The percentages choosing to have their money paid out under method A (a larger daily sum for the first half-year, but a lower total amount) and method B (a larger daily sum for the second half-year and a larger total sum) are shown in Table 2.

Here too, a multiple comparison indicated that the groups differed significantly, and we find that active users are more impatient than the groups with whom the abusers are compared. The difference between active vs. non-users and ex-users is significant at a $p < 0.001$ level. As many as $32\%$ of active users say they would have chosen method A, while the corresponding figure for non-users and former users in both cases is $8\%$.

Both Tables 1 and 2 provide estimates that tell the same story, namely that active users’ discounting differs significantly from the discounting that is quoted by the groups of non-users and former users.

Does the assumption of stable preferences set forth by Becker & Murphy actually hold? Two possible explanations may be given for the differences between active and former users reported in the tables above.

1. The estimated discount rate may be lower in the group of former users if there is a selection mechanism at work. Given that active users as a group have a relatively high level of discounting, there will probably be large individual variations within the group. If giving up the habit is not a matter of chance, it seems probable that individuals with a relatively low discounting level will have a better chance of controlling their addiction.

2. Alternatively, active users may have a relatively high discount rate because addiction to intoxicants by itself has changed their discounting. Addiction could influence people's
Rational Addiction and Injection of Heroin

balancing of future and present income through changes in their “pure” time preference (\(r\)), or through changes in other factors (\(m\)) influencing inter-temporal balancing discussed in relation to Equation (5). High discounting may be a consequence of addiction if physical dependence influences inter-temporal balancing. Craving and abstinence pains may, perhaps, make people less able and willing to think in terms of future scenarios and stick to chosen strategies. The present may be given more weight. Abuse of substances such as LSD may, in addition, change their perception of reality and ability to think rationally and thus change their “pure” time preferences. Furthermore, the lives led by abusers of hard narcotic substances involve a heightened morbidity risk, something which, according to discounting theory, will make actors less willing to postpone consumption to later periods. Abusers are also uncertain as to whether they will be in a position to experience tomorrow’s consumption on account of the risk of overdosing, imprisonment, etc. With a lower life expectancy they will have fewer years over which to discount, something that in itself will reduce the empirical discount factor.

If drug abuse itself influences discounting, this may contribute to explaining why the rates were so different for active and former users, since this influence, wholly or partly, will cease when the abuse ends.

Former users also report a somewhat higher discount rate than non-users (Table 1). This may simply reflect that those with a lower preference level for the future were those who became drug abusers in the first place. However, the difference in the rate value may also turn out to be a consequence of addiction if the influence on time preferences mentioned above does not totally disappear after the cessation of drug use. A small, but lasting, increase in the risk of diseases and early death may, for instance, have occurred during the period of abuse. In addition, economic prospects may be less promising for former users than for the group of people who have never been addicted to narcotic substances.

Given that the estimated discount rates in this study can be taken as rough indicators of the time preference rate, the results largely confirm variations in TPRs among people with different experiences of addiction. Active users had a significantly higher discount rate than non-users. This is in line with the hypothesis of high TPR as a contributory cause. Ex-users’ significantly lower discount rate could be caused by a selection mechanism, but it could also be that the assumption of stable time preferences does not hold.

Addicts are Sensitive to Changes in Economic Factors

Michael Grossman, who has been involved in much of the empirical testing of Becker & Murphy’s theory, writes (Grossman 1993: 93): “Indeed, in my view the main contribution of Becker & Murphy’s (1988) model of rational addiction is to suggest that it is a mistake to assume that addictive goods are not sensitive to price. Even if one does not accept all the aspects of their model, one can examine this proposition in the context of the standard theory of consumer behaviour.” This will now be done here.

First, a switching regression model will be applied on the cross-sectional data set from heroin injectors. The main aim is to examine whether drug users respond to changes in
economic factors in accordance with standard economic theory, i.e. whether they increase their consumption when prices decrease or incomes increase. A problem with applying the cross-sectional data set, however, is that it is impossible to take the special features of addiction into account as a one-period model cannot take cognisance of possible effects on subsequent periods. We have therefore also used the panel data set to estimate the effect on heroin consumption of changes in prices and income. By following the same individuals over a period of time, the panel data allow us to incorporate aspects of addiction when estimating price and income elasticities. The latter data set will also be employed below when we attempt to examine the possible effect on current consumption of past and future consumption of heroin. In the present section, however, only contemporary effects are in focus.

In modelling heroin consumption, one needs to recognise the fact that consumption patterns of dealers differ from those of non-dealers. Due to dealers' dual role as supplier and consumer, any price changes affecting heroin will have two conflicting effects on their own consumption. Further, user-dealers have more ready access to the drugs they sell, have better knowledge of purity and may be able to buy on credit. Consequently, dealing status itself may have a separate influence on people's heroin consumption. Thus, even if dealing income is treated no differently than income from other sources, the "dealer impact" may be of some importance. As dealing is a frequently reported income source by drug users (46% in the sample report some income from dealing the month prior to the interview), models of heroin consumption should differentiate between individuals according to their dealing status.

An appropriate method needs to allow for the fact that heroin consumers choose whether or not to become dealers and the most efficient method is to apply a switching regression model (Greene 1995: 668). The procedure takes account of a situation in which the whole consumption function may differ between dealers and non-dealers. (A fuller description of the method is given in Bretteville-Jensen & Bjørn 2003a.) The independent variables applied are prices of heroin and amphetamine, income, gender, age, education, consumption of other drugs (alcohol, cannabis, pills).

As mentioned, the panel data approach provides an opportunity to incorporate aspects of addiction in the estimations. Physical and psychological "stocks of habits" accumulated by previous heroin consumption are potentially important factors when attempting to explain observed heroin consumption. This habit effect may be considered an additional effect to the standard observable economic factors, sociodemographic variables, etc. In a dynamic model of individual behaviour, the addiction to heroin can be represented by a time-dependent variable incorporating the "stock of habits" determined by each individual's past heroin consumption (cf. Grossman & Chaloupka 1998). Unobserved habit effects can alternatively be considered as individual "properties," represented within a static model as (components in) time invariant, latent variables (individual specific). The latter approach may be the most convenient when individual data in the form of short panels from a sample of individuals are available. It should be noticed, however, that estimates of the variation in the latent variable will also represent variations in genetic dispositions, attitudes towards health risks, and other valid explanatory variables not specified in the model.

Heroin consumption is here explained by three kinds of variable. The first is a vector of variables that vary across individuals and time, e.g. income and price. The second is a vector
of variables that vary only across individuals, e.g. gender. Third, the additive latent variable specific to the individual contains, inter alia, the psychological stock of habits attached to the drug and affects all observations of individual i's consumption of the drug. Two static panel data models are considered, i.e. one random-effect and one fixed-effect model. (More details about the procedure are given in Bretteville-Jensen & Børn 2003b.)

As the hypothesis of current interest is the possible influence of economic factors, only the estimation results for heroin price and income are reported here. The price and income elasticities in Table 3 indicate how much the consumption of heroin would change in response to a 1% increase in prices or income, respectively.

As can be seen from Table 3, all model variants attain negative and significant price elasticities and all, except one, attain positive and significant income elasticities. In line with the hypothesis, the estimates indicate that consumption would increase substantially if the price of the drug decreased. In two of the three models, dealers seem to be less price responsive than their non-dealing counterparts. The income elasticities vary to a larger extent in the panel data models than in the switching regression model between the two groups of heroin injectors.

The effect of including a latent, time invariant variable representing addiction on the estimated elasticities seems to be small when comparing the results for the switching regression model and the panel data models. Further, when applying the random effects model, the estimated indicator of addiction (not reported in Table 3) was smaller than one a priori would expect. This might partly be explained by possible measurement errors, the low number of observations, the sample consisting of only heroin users (not including non-users), and, perhaps most importantly, the one-year interval between the two observations. A one-year gap between the interviews may be large in relation to the usual "habit cycles" for this damaging drug. We might therefore expect that the estimated strength of the habit formation would have been larger if the drug addicts had been observed at, say, a monthly or quarterly interval.

Although the estimated coefficients vary across the models, the results in Table 3 clearly show that heavy drug users are also responsive to changes in economic factors. Both the results from the SRM and the two static panel data models paint the same picture. As expected, the price responsiveness of dealers and non-dealers differs in every model, although not all the results point in the same direction. A more thorough examination of a subset of the data also reveals that there are gender differences in price responses (Bretteville-Jensen 1999b). Females have a higher price elasticity than their male counterparts. The larger response may indicate that females are more likely to judge other commodities as close substitutes for heroin or that reducing consumption through treatment seems more attractive to them. As the female injectors report consuming more heroin on average than male injectors, the latter result is surprising.

The results in Table 3 are basically in line with previous studies which show great variations in estimated values. Silverman & Spruill (1977) obtained, in an indirect manner, an estimated long-term price elasticity of heroin \[-0.25\], and van Ours (1995) presented an estimate of \[-1.0\] for the long-term price elasticity of opium demand in the Dutch East Indies. Saffer & Chaloupka (1999) estimated a participation price elasticity for heroin from a national household survey in the USA, and by assuming that elasticity of demand is roughly twice that value, they obtained an estimate of \([-1.60, -1.80]\).
Table 3: Results from a switching regression model (SRM) and two static panel data models.

<table>
<thead>
<tr>
<th>Variables</th>
<th>Switching Regression Model ($n = 1311$)</th>
<th>Panel Data Model ($n = 78$)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Dealers</td>
<td>Non-dealers</td>
</tr>
<tr>
<td>Heroin price</td>
<td>-0.49 (0.13)</td>
<td>-0.97 (0.12)</td>
</tr>
<tr>
<td>Income</td>
<td>0.51 (0.06)</td>
<td>0.58 (0.05)</td>
</tr>
</tbody>
</table>

Dependent variable is heroin consumption. Variables are in log form. (More results can be found in Bretteville-Jensen & Bjørn (2003a, b).)
Current Consumption is Influenced by Previous and Future Consumption

The last hypothesis derived from Becker & Murphy's theory to be examined here suggests that past and future consumption of the addictive good is positively correlated to current consumption. In this section we follow Grossman & Chaloupka (1998), whose point of departure is the empirical testing of the rational addiction model found in Becker et al. (1994). The inclusion of both past and future consumption among the regressors distinguishes the derived demand equation from other approaches. A positive and significant estimate of the past consumption coefficient is interpreted as the drug in question being addictive, and a positive and significant estimate for the leaded consumption coefficient is taken as an indication of addicts acting in a rational or foresighted manner.

Grossman & Chaloupka (1998) assume that consumers maximize a lifetime utility function given by:

\[ V = \sum_{t=1}^{\infty} \beta^{t-1} U(Y_t, C_t, C_{t-1}, \varepsilon_t) \]  

in which \( \beta \) is the time discount factor, \( \varepsilon_t \) reflects the effects of measured and unmeasured life cycle variables on utility, and \( Y_t \) and \( C_t \) are as previously defined. Assuming a quadratic utility function and a rate of time preferences corresponding to the market rate of interest, the following demand function is derived:

\[ C_t = \theta C_{t-1} + \beta \theta C_{t+1} + \theta_1 P_t + \theta_2 \varepsilon_t \]  

Ideally, individual panel data for at least three periods should be available properly to test the hypothesis. Unfortunately, such data do not exist in the case of heroin injectors. There are, however, two other sets of data available: first, the genuine panel data set where the same individuals have been interviewed twice; second, we take advantage of the fact that the interviews in the regular data collection procedure are collected over a seven-year period with 28 quarterly interview sessions. For every session a mean value for each of the relevant variables is estimated in constructing aggregate time series.

With only two observations per respondent available in the panel data set, it is not possible to apply Equation (7) as it stands. Instead, we split the analysis by first estimating only the effect of previous consumption on current consumption (disregard \( C_{t+1} \) in (7)) and assume on that basis that individuals ignore the future (a myopic model). We then estimated only the effect of future on current consumption without including possible effects of previous consumption (disregard \( C_{t-1} \) in (7)). We estimate the two reduced versions of Equation (7) by a two-stage least squares (2SLS) method. This is because past and future consumption may be correlated with the error term and an OLS approach would thus lead to biased estimates of the parameters of interest. As in the previous section, we run separate estimations for dealers and non-dealers.

By calculating the average of each of the relevant variables for dealers and non-dealers in each quarterly interview session, we obtained the other data set to be employed here — the time series. We can now estimate the effect on current consumption of both previous and future consumption simultaneously. For the same reasons as described above, we again employ a two-stage method as an OLS approach could lead to biased estimates. We instrument
$C_{t-1}$ and $C_{t+1}$ by using the predicted values obtained in a first step by regressing these two variables on: (i) income and drug prices; (ii) income and drug prices in addition to the full set of explanatory variables (age, gender, and dummies for heavy use of alcohol, cannabis, and tablets) in the corresponding quarters as instruments. The effective number of quarters in the estimation is 26. Separate models were applied for the two groups of heroin users.

For both estimation procedures, we follow Grossman & Chaloupka (1998) and assume that predicted future variables equal actual variables. The results from the estimations based on the re-interviewed sample are reported in Table 4.

The "myopic" model with lagged heroin consumption indicates that heroin is addictive, as the estimate of the lagged coefficient is positive both for dealers and non-dealers. However, it is not statistically significant at the 5% level for either of the two groups. Also, the lead estimates are positive and insignificant. The parameter values, especially for dealers, are relatively low. In spite of the low number of observations, most of the price and income elasticities are significant at the 5% level and had the expected signs.

Splitting the analysis by estimating the effect of previous and future consumption separately does not constitute a satisfactory test of the rational addiction theory as the theory predicts that both variables influence current consumption simultaneously, i.e. it might create an "omitted variable problem". Becker et al. (1994) and Grossman & Chaloupka (1998) both report results from myopic versions of their models corresponding to the first two columns of Table 4. They find that the parameter values of the myopic versions differ to some extent from those obtained from the full model, as the price elasticities are smaller for the myopic versions. The difference was not statistically significant, however. Still, the finding suggests that the omitted variable bias has caused the current estimates to devalue their real value and the low number of observations may explain why these coefficients, both for dealers and non-dealers, are insignificant.

Table 4: 2SLS for dealers and non-dealers with separate inclusions of a lagged and a leade variable for heroin consumption.

<table>
<thead>
<tr>
<th>Variables</th>
<th>LAG</th>
<th>LEAD</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Dealers $(n = 26)$</td>
<td>Non-dealers $(n = 48)$</td>
</tr>
<tr>
<td>Heroin price</td>
<td>-1.09 (0.42)</td>
<td>-1.68 (0.73)</td>
</tr>
<tr>
<td>Income</td>
<td>0.38 (0.22)</td>
<td>0.57 (0.19)</td>
</tr>
<tr>
<td>Lagged heroin consumption</td>
<td>0.11 (0.07)</td>
<td>0.34 (0.20)</td>
</tr>
<tr>
<td>Leaded heroin consumption</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.28</td>
<td>0.49</td>
</tr>
</tbody>
</table>

Dependent variable is current heroin consumption. Variables are in log form. Standard deviation in parentheses. (More results can be found in Bretteville-Jensen & Børn (2003b).)
Table 5: Estimations based on the constructed time series (t = 26 observations).

<table>
<thead>
<tr>
<th>Variables</th>
<th>Dealers</th>
<th>Non-dealers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model (i)</td>
<td>Model (ii)</td>
</tr>
<tr>
<td>Heroin price</td>
<td>-0.00546 (0.00165)</td>
<td>-0.00599 (0.00275)</td>
</tr>
<tr>
<td>Income</td>
<td>0.000014 (0.000004)</td>
<td>0.000015 (0.000007)</td>
</tr>
<tr>
<td>Lagged consumption</td>
<td>0.03222 (0.03327)</td>
<td>-0.01089 (0.03637)</td>
</tr>
<tr>
<td>Leaded consumption</td>
<td>-0.02814 (0.00926)</td>
<td>-0.01720 (0.01155)</td>
</tr>
<tr>
<td>Box-Pierce stat.</td>
<td>3.3433</td>
<td>0.8944</td>
</tr>
<tr>
<td>Box-Ljung stat.</td>
<td>4.3827</td>
<td>1.1718</td>
</tr>
<tr>
<td>Price elasticity</td>
<td>-0.61</td>
<td>-0.67</td>
</tr>
<tr>
<td>Income elasticity</td>
<td>0.42</td>
<td>0.45</td>
</tr>
</tbody>
</table>

Dependent variable is current heroin consumption. Standard deviation in parentheses. Model (i) is based on the full set of regressors, model (ii) is based on just income and prices as regressors in the first step of estimating instrument variables.
Positive and significant estimates for future consumption have been used as an argument for rational versions outperforming myopic versions of addiction models. In line with this, the positive, although insignificant, estimates reported in Table 4 give some support to the rational addiction theory. Whether a positive correlation between current and estimated future consumption is the result of a utility maximization as described by the theory may, however, be discussed.

The main results from the time series models are presented in Table 5. The estimated coefficients confirm that heroin consumption is negatively related to own price and positively correlated to income. The estimates in the first four rows of Table 5 are not directly comparable to estimates in Tables 3 and 4 as we have applied a different functional form here. Price and income elasticities are calculated on the basis of variable means, however, and are reported in the last two rows of Table 5.

The results do not unambiguously support the hypothesis of a positive influence on current consumption of previous and future consumption. Two of the four coefficients for previous consumption are, on the contrary, negative, although insignificant. Also future consumption comes out with two positive and two negative coefficients (both for dealers) and one of them is significant at the 5% level. Keeler et al. (1993) have tested the Becker–Murphy theory with aggregate time series for cigarettes and Olekalns & Bradleys (1996) have applied aggregate data for coffee consumption. These studies, as well as others that have applied individual and aggregated panel data for addictive goods, report support for the rational addiction theory, i.e. they report positive and significant coefficients for the lagged and the ledged consumption variable.

Our results, especially the negative estimates for past consumption, are surprising. If there is an effect of previous consumption on current, one would expect it to be positive. The above-mentioned works based on time series data have been criticised for not taking sufficient account of the trend in the data, hence causing possible spurious results (Gruber & Köszegi 2001; Skog 1999). We have aimed at avoiding this pitfall. In the last estimation, however, we have applied the same functional form as the other studies. Thus, if we are to believe Becker & Murphy’s theory, we cannot explain the findings of a negative relationship between past and present consumption other than by claiming that aggregated data are not well suited to explaining individual behaviour.

Discussion

In Becker & Murphy (1988) it was emphasised that even addictive behaviour could be analysed within a frame of classical economic theory based on stable (time) preferences. The nature of the data and methods applied here, however, do not give sufficient evidence for either a rejection or a confirmation of the assumption of stability in preferences, but the analysis indicates that there may be reasons to question it. The significant differences in estimated time preference rates between active and former addicts are difficult to interpret within rational addiction theory. One possible explanation is that addictive behaviour itself may change an individual’s time preference. A high time-preference rate may be one among several causes of addiction, but great impatience and short-sightedness may also arise as a result of addiction. Endogenous time preferences in the case of addiction will contribute to undermining Becker & Murphy’s theory.
That greater impatience can arise as a result of addiction is mentioned in a more recent paper by Becker & Mulligan (1997). Their point of departure is that endowed time preferences are excessively high and that rational people will, to varying degrees, have incentives to invest in "future-oriented capital" in order to reduce the size of their TPR. As Skog (2001) argues, however, their theory is about enhancing future utility and not about increasing patience, which is a very different phenomenon. Allowing for an endogenous determination of the TPR is nevertheless a very different starting point than the one that was taken in the Becker & Murphy (1988).

An approach in which the assumption of stable preferences is less binding may, however, be more fruitful in discussing the relationship between addiction and discounting. The fact that, in terms of Becker & Murphy's theory, individuals seem always to act in accordance with their own, long-term interests is contrary to the understanding of many professionals in the addiction field (see, e.g. Heather 1998). The assumption of stable time preferences leaves no room for individual vacillation, impulsive acts, relapses, etc. The other addiction theories discussed in this volume are better capable of incorporating such features of the addiction phenomenon.

Previous conventional wisdom said that the demand for addictive goods is unlikely to be responsive to price (cf. Koch & Grupp 1971, 1973; Rottenberg 1968; Stigler & Becker 1977). There are, however, studies that have estimated individuals' responses to changes in economic factors. Not many of them have done so by also attempting to incorporate the special features of addictions. Some, however, have followed the Houthakker & Taylor (1966) approach and included past consumption in the demand function through a "stock of habits" representing the depreciated sum of all past consumption. Others have tested whether there are asymmetries in price responses of addictive goods (Pekurinen 1989; Young 1983). Becker & Murphy explicitly underline the importance of addiction as an independent influence on the consumption of addictive goods within a time-consistent framework and derive several testable hypothesis of the relationship between consumption and price. They even claim that addictive goods would be more price responsive than non-addictive goods in the long run.

The results reported in above confirm that addicts too are sensitive to price and income changes. The estimated values vary across the models and data sets applied. Even the estimations based on the low number of observations in the panel data set, however, arrive at significant price and income elasticities. Estimates of drug price and income elasticities indicate how drug users respond to changes in economic factors and, hence, should be of interest to policy-makers and others who deal with the drug problem. The legalization debate is an example where estimates of the consumers' price response may have an important role to play. De-criminalizing and/or legalizing consumption and sales of drugs that today are illegal will cause the full prices of the goods to fall and, for dealers, income also. Consumers' responses will be of importance when evaluating the consequences on individuals and society from such a policy change.

What does it mean to say that the demand for heroin is sensitive to price? Generally, the larger the elasticity, the larger the scope for substitutes or alternative means to meet the need. Other drugs are obvious candidates as heroin substitutes. Even though drugs like heroin and amphetamine have fundamentally different physiological impacts, heroin users may turn to amphetamine if the price of heroin increases sharply. As many as 42% of the interviewees who reported mainly injecting heroin in the month leading up to the interview
stated that they also had injected amphetamine during the previous four weeks. Reducing the amount consumed or stopping consuming drugs altogether, either by professional help or by oneself, is, of course, another strategy that may be chosen if prices rise. Additionally, the need can be met by other means, like better socialisation, as suggested in the relative addiction theory (Rachlin 1997). In any case, the relatively high price elasticity reported here indicates that there are alternatives to heroin injectors' continued drug use in adverse circumstances.

The current analyses have suggested that heroin injectors have both a relatively high price elasticity and a high time preference rate. The combination of the two is in accordance with Becker & Murphy's theory which claims that younger, less educated, and lower income persons will be more responsive to price changes than others. The finding of addicts' sensitivity to price changes is also in accordance with the relative addiction theory (Rachlin 1997) in which individuals are assumed to substitute consumption of addictive goods with other activities like socialisation. The theories of hyperbolic discounting (Ainslie 1992) and melioration (Herrnstein & Prelec 1992) are more concerned with individuals' ways into and out of addiction and are not explicit on active drug users' probable price responses.

The interdependency of future and current consumption is a special feature of the rational addiction theory. Neither of the two data sets applied here for testing the interdependency hypothesis are ideal for the job. The genuine panel data have only two observations for every individual and the total number of individuals is relatively low. Aggregated time series data, on the other hand, cannot be expected to adequately fit a model of individual behaviour. The coefficients for the leaded variable in Tables 4 and 5 are insignificant and in two models also have the wrong sign. As mentioned earlier, however, Becker and colleagues have previously employed aggregated data when testing the theory and their results generally seem to support the hypothesis (Becker et al. 1994).

Are the results in Tables 4 and 5 only a consequence of poor data quality or are there other possible explanations? To what extent should we expect future consumption to positively affect current consumption of heroin? Some objections could be made: (1) In an illegal market like the one for heroin, price fluctuations are perhaps more unpredictable than in a market for legal goods, and factors like available resources for buying the drugs, the availability of heroin in the market, the likelihood of apprehension, etc., may contribute to making future consumption an especially uncertain variable. Furthermore, employing actual values instead of predicted future values in the estimations is problematic econometrically and there are reasons to question the interpretation of the model's coefficients (see Wangen 2002 for a discussion of these latter points); (2) Even if future variables could be predicted relatively precisely, is it likely that optimal future consumption imposes a significant influence on current consumption? According to the theory, an expected price rise in some time periods ahead lowers the corresponding optimal consumption level and will also cause current consumption to fall. To what extent will heroin addicts take expected future price changes into account and how will future dispositions be balanced against the acutely felt need for present consumption? It will surely be optimal for addicts to reduce their current consumption in line with the theory, but will it actually happen? The theory assumes that people decide on an optimal path for the future and follow it without problems. No struggling, no impulsive actions, no relapses, no weakness of will, etc. Thus, even if data from
Rational Addiction and Injection of Heroin

this study, in addition to other empirical studies, support some parts of the theory, there are still parts that are highly questionable. There may be reasons to examine certain implications of the theory another time.

To sum up, by applying data collected among addicts attending the needle exchange service in Oslo we have tested some hypotheses derived from Becker & Murphy’s theory. We found that heroin addicts act in accordance with the theory regarding the responsiveness to changes in economic factors. The finding of a larger time preference rate among active addicts is also in line with the theory. We did not, however, obtain support for the assumption of stable time preferences. Nor could the data give significant support to the hypothesis of future consumption being a positive influence on current consumption. More research, preferably analyses of individual panel data, is needed, however, before firm conclusions can be drawn about the theory.

Note

1. This section of the paper is partly based on Bretteville-Jensen (1999a).

References

Comments on Bretteville-Jensen

Michael A. Morrisey

I will preface my remarks by noting that while my wife claims I am addicted to economics, I am not an addiction economist. The distinction is important. To understand the functioning of markets it is important to understand the institutional “details” of how the market functions. Some might say one needs to know the culture and mores of the society. Economists would tend to say we need to know the incentives and constraints. All this is to say that, if what I say sounds silly or naive, it probably is!

Anne Line Bretteville-Jensen’s paper (hereafter B-J’s paper) is very ambitious in undertaking three sets of complicated analyses. I particularly like the unusually thoughtful discussion that accompanies the analysis. It presents alternative explanations and suggests reasons for unexpected results. I was less pleased with the absence of full regression results that underpin each of the component of the study. Their absence makes it difficult to appreciate the work, to fully understand the empirical specifications, and to glean additional insight.

The paper undertakes three separate studies using related data sets. It examines differential time preferences of addicts, former addicts and non-users; it examines the price sensitivity of addicts to differences in prices and incomes; and it examines the interdependence of future and current consumption of heroin. All are efforts to test the rational addiction model of Becker & Murphy (1988) and all essentially rely on in-person interviews with heroin addicts conducted in Oslo, Norway between June 1993 and September 2000.

The third study, focusing on interdependent future and current consumption, is the least satisfying. The theory argues that past and future consumption of an addictive good is positively correlated to current consumption. Unfortunately, as B-J notes, the data are not up to the task. In her true panel, she only has two periods of observation. The absence of the third period biases the estimation and the results often lack statistical significance, perhaps because of the scarcity of observations. Her efforts to overcome this in a time series of aggregated responses is problematic in that the results vary across regressions and often have the wrong signs. More fundamentally, she is forced to make aggregate data speak for individual behavior.

On the other hand, her second study, examining price and income sensitivity, is strong. I think she is right in separately analyzing drug dealers and non-dealers and in treating dealer status as endogenous. Dealers and non-dealers may have very different preferences for heroin and may operate in very different markets. B-J’s results indicate that non-dealers are more price sensitive than dealers. This is consistent with a view that non-dealers have better substitutes for mind-altering substances. This would seem to square with a view that they are not as dependent upon a single type of drug.
However, the empirical result may be driven by variables for which the study is unable to control. Perhaps most importantly, the price and quantity data are not adjusted for drug quality. One would expect that dealers have a better sense of quality. If so, one would also expect that they would have higher quality adjusted price elasticities than would those with less knowledge. In as much as price may proxy for quality in the empirical specification, the differences in elasticity may reflect the non-dealers' lack of quality discrimination.

The nature of the dealer's purchasing "network" may also be imbedded in these price elasticities. If a dealer serves as an independent broker in a wholesale drug market, one would expect that he/she has access to a wide range of supply. As such, one would expect the dealer to have better substitutes in supply and, therefore, greater elasticity of demand. On the other hand, if the dealer is essentially a "franchisee" who is tied to a single source of supply, then the "franchiser" may not allow purchases to fully reflect differences in the wholesale price. Future work should focus on some mechanism to quality adjust the price and quantity measures. It may also be fruitful to better understand the workings of the supply network.

B-J uses sources of income in this analysis to identify dealers and non-dealers. This is obviously a successful approach. However, these data suggest additional research that could focus on the various sources of income and their effects on use. From a policy perspective it would be very useful to know the effects of government subsidies, for example, on heroin consumption. Knowledge of the links between prostitution and stealing as sources of income and heroin consumption would be useful to allow consideration of the indirect effects of changes in policing efforts on these behaviors and their indirect effects on drug use.

The first B-J study offers some clear insight into the question of whether addicts have different discount rates than former addicts and non-users. She finds that they do. She uses a fascinating set of questions dealing with the hypothetical willingness to trade-off lottery winnings over time to obtain estimates of time preference. This is innovative although there is an empirical literature in economics on hypothetical and actual personal discounting (see Warner & Pleeter 2001 for a discussion). B-J's results indicate that addicts have a very high discount rate, in the neighborhood of 90%. Former addicts have a much lower rate, 15%, and non-users had five percent discount rates. She interprets this as suggestive that the rational addiction model is incorrect. The model hypothesizes that addicts will have higher discount rates. The lower rates for former addicts, in her view, are inconsistent with this theory.

This may well be true, but there are other explanations that should also be considered. First, there is substantial variance in the reported discount rate among addicts. The standard deviation reported in Table 1 is nearly twice that of the mean. It is conceivable that the definition of an "addict" is too broad. It may include people who are not actually addicts but who nonetheless completed an interview at the needle exchange center. If the theory is correct, these erroneously coded folks will have the lower reported discount rates within the cohort of "addicts" and actually may have much more in common with "former addicts" than with true addicts.

A second alternative explanation has to do with the nature of the personal capital market opportunities the respondent has available. B-J has argued that the process of addiction may lead one to have higher and higher discount rates as one spins deeper and deeper into addiction. In this view lower discount rates of former addicts may reflect their re-emergence
from addiction. Suppose instead that addicts do have stable and high discount rates. They would not necessarily report these high internal rates of time preference to the interviewer. They may have many sources of capital from which to obtain funds over the course of a year. They could borrow from friends, from a bank, from a credit card, from a pawn shop, or from a loan shark. If they could borrow from a credit card at 18%, one would expect that they would list something like 18% as the relevant trade-off between obtaining the lottery proceeds today and next year. The point is that the time preference rate for a spiraling addict may remain constant, but the social decline in the addict’s ability to interact with friends and with markets may foreclose low cost opportunities to borrow. From this perspective, the relatively low discount rate reported by former addicts may reflect a process of re-attachment to the labor force. Indeed, if the results of the second component of the B-J study are to be believed, and addicts do respond rationally to differences in prices and incomes, then they would also be expected to respond rationally to differences in the personal capital market.

All this said, a better way to test the theory would be to take advantage of the interview/re-interview nature of the ongoing research effort. In future waves of the interview in which the respondent has agreed to be identified, the investigator can again ask the lottery question. This would allow a much cleaner test of the constant, high discount rate hypothesis.

Overall, Bretteville-Jensen has provided a well done and thought-provoking set of studies. Like all good studies it raises as many new questions as it answers. The exciting feature of this investigation is that the unique and ongoing data collection effort provides the opportunity to investigate many of the new questions.

References


Reply to Morrisey

Anne Line Bretteville-Jensen

I greatly appreciate Michael A. Morrisey's comments on my paper. He demonstrates that it is more than sufficient to be addicted to economics — it is not required to be an addiction economist — to understand the core of areas under discussion. His comments highlight a need for clarification of certain points, he suggests alternative explanations for some of the findings and he also points to weaknesses in the analyses. Further, Morrisey says he misses the full regression results that underpin each of the components. The reason for omitting the full sets of results is, of course, due to space; the paper would simply have been too long for the purposes of the conference. The complete tables of results are naturally available to anyone upon request. Morrisey has some remarks on each of the three studies, and I will start with those related to the hypothesis of addicts’ time preference rate.

Morrisey suggests that other explanations underlying the substantial difference in estimated time preference rates between active, former and non-users of hard drugs should be considered. He notes the presence of wide variance in the reported discount rates among active and former addicts and suggests that the definition of “addict” may be too broad in that it might include people who are not actually addicts but who nevertheless completed the interview at the needle exchange service (NES). If that were the case, these erroneously coded people would have lower discount rates than “real” addicts and may have more in common with former addicts.

In the study, people are defined as active drug users if they stated at the interview when frequenting the NES that they had injected heroin and/or amphetamine in the four weeks leading up to that point in time. Out of the 110 individuals coded as addicts, 91% reported injecting on a daily or almost daily basis. People defined as former addicts reported on average their most recent injection as being about six years ago (mean number of months was 73 with a standard deviation of 73.0). Therefore, the high proportion of drug users injecting on a daily/almost daily basis and the relatively long period since former addicts’ most recent injection indicate that the problem of coding would probably not have influenced the results to any large extent. Further, if people had been coded erroneously, a corrected estimate of the time preference rate (TPR) for former addicts would increase, but so would the new estimate for average TPR for active drug users. Consequently, it is not certain that the gap between the two groups would have narrowed anyway.

Morrisey proposes a second explanation for the findings and, admittedly, I cannot exclude the possibility that at least parts of the differences in estimated discount rates between former and active users might be explained by better opportunities to borrow money for the former group. The estimated discount rate, $r_j$, will, however, be influenced by both $\sigma_j$ (the “pure”
TPR representing individual j’s balancing of utility in different periods) and \(\pi_j\) (a rate vector of all the other factors influencing \(r_j\), cf. Equation (5) in the paper). If the high value of estimated discount rate among addicts is just a result of an increase in \(\pi_j\), due to worsened conditions in the credit market, and the “pure” time preference rate has remained unchanged, then the estimate of former addicts’ discount rate is to be taken as the relevant estimate for all addicts. In that case, the revealed difference in time preference rates between addicts and non-addicts is not very large any more, especially since there are reasons to believe that addicts’ \(\pi_j\)-vector has a larger influence on \(r_j\) than non-addicts’. If the time preference rate is stable, the data will give less support to Becker & Murphy’s hypothesis of a high time preference rate being a contributory cause of addiction than indicated in the paper.

When commenting on the second analysis, Morrisey reminds us of the possibility that the price elasticity estimates could be driven by variables that I am unable to control for. He is especially concerned that the price and quantity data are not adjusted for drug quality. Although it definitely would have been advantageous to have access to such information, I do not believe it represents a problem of great importance for the estimation of non-dealers’ price elasticities. This is because non-dealers, at the time of purchase, do not possess such information either.

The data reveals that the market price paid by non-dealers for the smallest unit of heroin was very stable across individuals within the same interview session, although it varied substantially over the study period from 1993 to 2000. It is possible, therefore, that if we had had the chance to test heroin purity in real purchases, quality-adjusted prices would have biased the estimates if the quality had varied substantially without being reflected in the market’s retail prices. What seems generally to be the case in the Oslo heroin market, however, is that fierce competition between dealers at street level is “forcing” suppliers to offer a certain standard of quality. Even if cheating (selling heroin of very low quality) occurs, chemical analyses of small-unit seizures, performed at the National Bureau of Crime Investigation back in the 1980s, indicate that dealers at this level adjusted the quantity of heroin per unit according to purity of the drug. The National Bureau found that the content of heroin per unit sold at the street level remained surprisingly stable. Unfortunately, due to budget constraints such analyses are not carried out any more. A relatively stable price and purity level of units sold to non-dealers suggest that quality-adjusted price and quantity data would not necessarily contribute to improve price elasticity estimates for non-dealers.

On the other hand, Morrisey’s argument may be more relevant for the estimates of dealers’ price response, as it might be more likely that prices at the wholesale level are quality adjusted. In contrast to purchases at street level, wholesale purchases are normally based on mechanical weighing in the presence of the buyer. With quality-adjusted prices, one would expect dealers’ price elasticities to be smaller than for those who deal in “quantity-adjusted” prices. If units at the retail level are quantity adjusted and at the wholesale level quality adjusted, this may partly explain the observer differences in price elasticities among dealers and non-dealers. There are, however, theoretical reasons to expect additional differences between dealers’ and non-dealers’ estimates as a price change will have two conflicting effects on dealers’ own consumption, while non-dealers will only experience the direct price effect.

Morrisey’s last point regarding the interpretation of the reported price elasticities I do not quite understand. He says that the dealer’s purchasing “network” may be embedded in
the estimates as the elasticities will be influenced according to whether the dealer serves as an independent broker or as a "franchisee." Morrisey argues that a broker would have better supply substitutes and therefore greater elasticity of demand. I do not see his point here as the prices both dealers and non-dealers are asked to report are the prices they have to pay for the drugs they consume. Whether a dealer operates as a broker or a franchisee should not make any difference to how s/he responds to a given price increase with respect to own consumption of the good. A related but still different topic, however, is the question of whether income really is an exogenous variable. If addicts respond to price changes by adjusting not only their consumption but also their income, the estimated price elasticities will be biased upwards provided that the income elasticity is positive. Future work should focus on the relationship between income and price responses.

I agree with Morrisey when he says that the third study, focusing on interdependent future and current consumption, is the least satisfying as the data at hand are not ideal for the job. The absence of a third period biases the estimations based on the two-wave panel data set. As mentioned in the paper, however, similar analyses including only lagged variables are also reported by Becker et al. (1994) and Grossman & Chaloupka (1998), in addition to their results from the full models. In both studies the estimates for the lagged variables had a smaller value in the myopic version compared to the full model, indicating that the bias caused by the absence of a third period tends to underestimate the value in the former type of models. If that is the case also in the present estimations, the estimates reported in Table 4 understate the true value to some extent.

Further, I share Morrisey’s scepticism towards letting “aggregate data speak for individual behaviour.” Many of the empirical tests of Becker & Murphy’s theory found in the literature, however, including the authors’ own test (Becker et al. 1994), are based on aggregated data. The last analysis is therefore meant as a comparative exercise and the functional form applied there (the economic variables were not log-transformed in this case) corresponds better to the previous analyses.

Finally, I want to thank Michael Morrisey for his constructive criticism. He also pointed to new research questions and I will probably look into some of them in the near future.

References


Chapter 11

Social Interaction and Drug Use: Rachlin vs. Schelling

Hans O. Melberg

Introduction

Howard Rachlin has written that “addicts are addicts because they are lonely” (Rachlin 2000: 145, emphasis in original). This claim is part of his “relative addiction theory” and the purpose of the first section of this paper is to present and evaluate some aspects of that theory. To that end I use findings emanating from a survey of about 500 drug users in treatment, as well as large surveys of the general population. The use of this evidence in turn raises the question of what, if anything, can be learned about the causes of addiction by asking drug users. For instance, Davies (1997a, b) claims that such surveys shed very little light on the causes of addiction and the second part of the paper discusses this view. In the third section I focus on social interaction, not as a variable in an explanation (as Rachlin does), but as a mechanism in the sense used by Elster (1998) and Schelling (1998). I exemplify this by exploring a small model of two mechanisms: observational learning and the dynamics of social stigma.

Rachlin’s Theory of Relative Addiction

The theory of relative addiction presents addiction as the outcome of a process whereby a person is driven by a constant (inelastic) need for something — for instance, anxiety reduction or mood elevation — that can be satisfied either by social interaction or by drug use (separable substitutes). The choice is assumed to be governed by the relative return of the two activities. The relative return, however, is not constant. More and more drugs are required to produce the same effect and, in this sense, it becomes more and more expensive the more the individual has used drugs previously (price habituation). Social interaction, on the other hand, is assumed to be analogous to playing classical music or doing sports, in the sense that the more one does it, the easier and/or more pleasurable it becomes. So it could be said that social interaction, unlike drugs, becomes cheaper and cheaper the more one
Figure 1: Relative Addiction. The curves indicate the instantaneous utility of using drugs or engaging in social interaction. Individuals to the left of the intersection will tend towards A (no consumption of addictive commodity), but a shock that increases the price of interaction will shift the return curve (for interaction) down and an individual who previously was to the left of the intersection, might find him/herself on the right, in which case s/he will use more and more drugs (tending towards C).

engages in it (price sensitization). These differing effects on instantaneous utility can be illustrated by letting the utility of drugs decrease the more the individual uses drugs (within a given time window) and the net return to social activity increase the more social activity he or she has engaged in (see Figure 1).

Runaway addiction can follow when a change in the direction of more drugs increases the price of drugs less than it increases the price of social activity, i.e. the curve describing the return to social activity is steeper than the curve for drug use (relative price sensitization). For instance, the loss of a friend makes social activity more costly (shifts the curve down) and makes the use of drugs cheaper relative to social activity. Faced with this change in prices the person may opt for more drugs. Assuming relative price sensitization, this change makes it even more costly to engage in social interaction, so the person will use even more drugs. In short, without enough social interaction to satisfy the undefined need, the person will compensate by using drugs and, given the dynamic consequences of using drugs (making interaction even more difficult), this may create a situation in which the best perceived alternative is to use even more drugs.

The theory of relative addiction is related to several well-known theories of addiction. It is compatible with the view of Herrnstein & Prelec (1992) that agents tend to choose the alternative that has the highest local or instantaneous utility as opposed to the overall best alternative, but it is also well suited to illustrate the phenomena of adjacent complementarity that is important to Becker & Murphy’s (1988) theory of how people end up as excessive
consumers of some goods. Building on Stigler & Becker (1977), it also provides an account of how properties of a substance may lead to excessive consumption but, unlike Stigler and Becker, the properties are justified in more detail by Rachlin and the resulting theory is compatible with evidence that the demand for drugs is elastic. Rachlin also argues that the theory can explain the shuttling between abstinence and relapse that is characteristic of many addicts. As they consume more and more drugs they are increasingly driven by a need to avoid pain (the behaviour is increasingly negatively reinforced). If people dislike being driven by negative reinforcement, they may eventually reach a point where the feeling of negative reinforcement has such a high cost ("I am a slave") that they decide to quit. A similar story but in reverse explains why such a person may start to use drugs again (Rachlin 2000). In sum, the theory of relative addiction presents an account of why and how people start, continue and stop (and start again) to use alcohol, tobacco and illegal drugs. The question is, how we should evaluate this theory?

An explanatory theory can be evaluated both by the realism of its crucial assumptions and the truth of its implications. Instead of trying to explore all kinds of evidence relevant to all assumptions and implications, I shall concentrate on a limited range of aspects of the theory based on limited evidence. This will allow me to go into greater detail about the relationship between loneliness and addiction, but it also implies that a wider view may contradict the picture that emerges from my narrow pool of evidence.

Evaluating the Assumptions

The assumption that you need larger and larger doses of drugs to achieve the same effect rests on the phenomenon of tolerance or the increasing negative side-effects of using drugs. One might, of course, ask whether the process is as nicely behaved (linear) as Figure 1 might indicate, and Rachlin himself has noted some minor exceptions, but these complications do not seem essential to the argument and in any case they are not the focus of this paper.

The same applies to the assumption that social interaction grows rusty unless it is practiced. A person with few or no friends may have a hard time engaging in social interaction, but he may also have a very high reward from the few interactions that he does have. Moreover, we do not know whether the skill grows rusty in a nice linear way. However, once again, as a first approximation, it would seem acceptable to assume that engaging in social interaction is easier and/or more pleasurable for people who have engaged in much social interaction in the past.

It is far less obvious that social interaction and drug use are separable substitutes. To argue his case, Rachlin (1997) relies on a summary of the relationship between smoking and social interaction as summarized by Fisher (1996). The evidence shows, for instance, that people with little social support smoke more than those with much social support, that smoking increases after losing social support and that attempts at cessation work much better if accompanied by social support. Rachlin also cites evidence to the same effect for alcohol and heroin. Finally he argues that the price elasticity of alcohol and other drugs is indirect evidence for substitutability.

There are several problems with the evidence cited above. First of all, substitutability between two goods is measured by their cross-price elasticity, as opposed to the own price
elasticity of one good. Second, evidence of correlation — even of the type that treatment is more successful for those who have social support — must be treated with caution as long as it is not accompanied by a discussion of possible third variables that could exert an impact. Finally, even if we grant the assumption of (strong) substitutability, there is still the question of separateness. Why should we assume that only social interaction or drugs are able to satisfy the undefined need?

It is important to note that Rachlin’s argument not only relies on the common-sense notion that social support can be helpful in order to beat the habit of using drugs, or that losing social support — e.g. after a divorce — sometimes can lead to more drug use. The claim is much stronger than that because, he argues, loneliness is the cause, and additionally that loneliness causes a particular form of drug use in which social interaction and drugs are separable substitutes for some undefined deeper need. Indeed, it is the strong claim which makes the theory interesting as a general theory of addiction. To argue that that social support is often helpful when trying to quit drugs — or to avoid starting drug use in the first place — is neither surprising nor amounts to a general theory.

**Strong Implication: All and Only Lonely People Use Drugs?**

The theory of relative addiction implies that there should be some kind of relationship between use of drugs and loneliness. If we really assume that social interaction and drug use are the only activities that can satisfy an undefined and constant need, then individuals who have much of one must have little of the other. Hence, when an estimated 1.9% of the Norwegian population claim to be “very lonely” in a large survey from 1998 (Lunde 2001), we would expect this group (and only this group) to have a high consumption of various drugs. In short, an implication of the strong version of the theory is that all addicts should be very lonely, but no one else.

It is not difficult to throw doubt on the strong version of the theory. For instance, in a large survey of addicts in treatment, 25% denied that they had felt lonely “at all” during the past week (see Table 1). Hence, it seems that many people use drugs even if they are not lonely. Moreover, many lonely people do not use drugs of any kind. In a survey of health among all the inhabitants of a county in Norway ($n = 65,400$), 17% of those who were most lonely did not smoke or drink alcohol at all (data from same source as in Table 2). Not only are there significant numbers of abstainers among those who are lonely, there are relatively

| Table 1: Feelings of loneliness among drug users. (How much have you been bothered by feelings of loneliness during the past week?) |
|---|---|---|---|---|---|---|
| Not at All | A Little | Moderately | Quite a Lot | Very Much | No Answer |
| (%) | 25.3 | 28 | 18.0 | 17.4 | 10 | 1.2 |

*Source: Survey of 482 drug users in treatment organized by the Cost-Benefit Project under the National Institute for Alcohol and Drug Research in Norway. Data collection organized by Edle Ravndal and Grethe Lauritzen.*
Table 2: Feelings of loneliness, smoking and alcohol consumption by gender and age.

<table>
<thead>
<tr>
<th>Age</th>
<th>Male</th>
<th></th>
<th>Female</th>
<th></th>
<th>Overall</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>20–49</td>
<td>50–69</td>
<td>70+</td>
<td>Total</td>
<td>20–49</td>
</tr>
<tr>
<td>I have been very lonely during the past two weeks</td>
<td>3.2</td>
<td>4.2</td>
<td>7.8</td>
<td>4.1</td>
<td>3.3</td>
</tr>
<tr>
<td>Do not drink alcohol (teetotallers) (%)</td>
<td>4.3</td>
<td>9.4</td>
<td>22.4</td>
<td>8.5</td>
<td>6.8</td>
</tr>
<tr>
<td>Smoke cigarettes daily (%)</td>
<td>30.4</td>
<td>29.6</td>
<td>17.7</td>
<td>28.1</td>
<td>37.0</td>
</tr>
<tr>
<td>Drink alcohol several times a month (%)</td>
<td>82.2</td>
<td>66.2</td>
<td>36.3</td>
<td>70.5</td>
<td>66.2</td>
</tr>
<tr>
<td>Average number of times per month drinking alcohol</td>
<td>3.7</td>
<td>4.3</td>
<td>4.2</td>
<td>3.9</td>
<td>2.9</td>
</tr>
</tbody>
</table>

Source: The Nord-Trøndelag Health Study (HUNT), A comprehensive survey of health in a Norwegian municipality 1995–1997, N = 65717, 70% of those invited responded.
more abstainers among those who are lonely than in the general population. There were 9.8% abstainers from both alcohol and tobacco in the general population compared to the afore-mentioned 17% abstainers among those who were the loneliest.

Supporters of relative addiction theory can make at least two possible responses to the criticism above. First, they can question the interpretation of the evidence. Second, they could weaken the assumptions slightly in order to make the theory fit better with the evidence.

One interesting way to question the interpretation of the evidence would be to argue that since the theory assumes that drug use is a substitute for social interaction, then the feeling of loneliness is muted by drug use. In other words, the person would be lonely if he were not using drugs but when he is using drugs the feeling goes away. The situation is analogous to the relationship between amphetamine and hunger. Amphetamine is said to reduce the feeling of hunger. For this reason some people have used the drug partly in order to avoid feeling hungry (e.g. when trying to lose weight). In this case, hunger is clearly part of the reason why they take the drug, but when asked whether they are hungry, they would say no precisely because the drug has muted their sense of hunger. In the same way one might argue that all drug users really are lonely, but that it does not show in the survey since the feeling is eliminated by the drug.

Unfortunately, such an interpretation is not compatible with the fact that many drug users also report being lonely. If drug use really mutes the feeling of loneliness, then we would not expect 27% to report that they feel quite or very lonely. Moreover, the theory of relative addiction assumes that drug use and loneliness are connected through an undefined deeper need. Hence, the analogy with amphetamine is misleading since, in that case, drug use and hunger are directly related. Finally, claiming that reportedly non-lonely drug users really are lonely (they just don’t know it themselves) is not compatible with other indicators of loneliness. For instance, when asked about number of close friends, 78% of the drug users said they had one or more close friends. Although this is lower than the national average (84%), it seems difficult to argue that the existence of not-lonely drug users can be explained away by arguing that they “really are” lonely individuals with no friends.

Is it possible to reconcile the theory of relative addiction with the existence of addicts who have friends (on average they claimed to have between two and three close friends in our survey) and do not feel very lonely? Rachlin argues that the observed social activity of addicts is only “pseudo social support.” It “is not a source of social support in the sense that a family or community is. The opium addict does not go to the opium den for the social support (if any) to be found there” (Rachlin 2000: 156). In this case the cause of drug use is lack of social support, not loneliness, and we need some kind of operational definition of social support — other than feelings of loneliness and number of friends — to test the theory. I have no such definition or data that could be used to distinguish between good and bad friends. Rachlin, too, ends up by appealing to common sense in the quotation above, but he relies on the most plausible case (heroin). The intuition is weaker when it comes to other addictive substances meant to be covered by the theory, such as smoking which is often viewed as a social activity (but see Ennett & Baumann [1993] for contradictory evidence). As long as the theory is meant to cover addictions in general, it cannot restrict itself to evidence that fits the theory by using heroin (and not smoking) to claim the absence of complementarity between social activity and use of addictive substances, while using tobacco and alcohol consumption.
and not illicit drugs to make the theory fit the age and gender structure of substance use. Also, since the theory operates at the level where all drugs (alcohol, smoking, illicit drugs) are considered to be the same, it cannot explain why some people smoke while others use alcohol.

**Implications of a Weaker Version of the Theory**

The arguments above may seem too strong. After all, it does seem to be the case that drug users tend to be significantly lonelier than the general population. About 10% of our respondents reported that they had felt “very lonely” during the past week, while — as mentioned — 1.9% of the general population felt the same over the past two weeks. This suggests that there might be something to the claim that loneliness is related to drug use, but we need to weaken the theory to avoid the strong implication that all addicts (and no others) are lonely. Instead the implication should be that addicts *tend to be* lonelier than the general population. This seems like a much more reasonable statement. To arrive at this result we simply substitute the assumption that using drugs and social interaction are always separable substitutes with the assumption that they tend to be separable substitutes.

Indeed, one might argue that this interpretation is by far the most reasonable and that the arguments made against the strong version were really attacking a straw man. As Rachlin writes, a “central assumption of relative addiction theory is that addictive activities and social activities are walled off... — they are at least *moderately* substitutable for each other but not for any third activity” (Rachlin 2000: 151, emphasis added). Although the implications of the term “moderately” are not explored by Rachlin, one might argue that it is perfectly legitimate to present a simplified theory in which we explore the consequences of strong assumptions, believing that the tendency of the conclusion will hold even if the assumptions are weakened.

I have two arguments against the weaker version of the theory. First of all, there is empirical evidence that is incompatible even with the weaker version. Second, I do not think the move from the strong to the weak version is as innocent as it might seem.

Although drug users in general tend to report being lonelier than the general population, a closer look at the numbers indicates anomalies even for the weaker theory. For instance, the age and gender pattern of loneliness do not match the facts known about the population using illegal drugs. While this population is dominated by young males, loneliness is more predominant among older people and females. The same applies to other drugs; the tendency is that the group that is most lonely drinks and smokes less than the general population. At the extreme, the loneliest group — older females above 70 — both drink and smoke far less than the rest of the population.

To explain anomalies in the pattern of use, the inexact nature of the relationship (not all addicts are lonely) and why some smoke while others use alcohol, one might introduce new variables. For instance, females might have larger barriers against taking drugs as a result of socialization, there might be genetic factors related to willingness to engage in risky behaviour and there are obvious selection effects that explain why older people smoke less than young people. In this way we could maintain the argument that
loneliness is the cause of drug use in the face of evidence that the relationship between the two is at best imperfect. The price, however, is a further weakening of the theory by the introduction of a ceteris paribus condition and allowing other variables into the relationship.

The introduction of third variables leads to my second argument against the weakened theory of relative addiction. As soon as we relax the strong assumption of separable substitutes and allow for other variables to enter the relationship, the question immediately becomes why we should focus on loneliness as a cause of addiction as opposed to the other variables. To answer that we need an empirical investigation into the strength of the many different variables that could be related to drug use — childhood experiences, genetic differences, discounting, risk perceptions, income opportunities, and so on. In this picture loneliness becomes one among many possible factors and to claim that loneliness is much more important than the others might be correct, but in the absence of comparative empirical investigation it is nevertheless an unsubstantiated one.

The empirical evidence needed to justify the focus on loneliness cannot be satisfied with showing a correlation between loneliness and drug use. We need to demonstrate a causal connection between the two. To illustrate the problem, consider a person who starts to use drugs after his only close friend suddenly dies. Clearly this sounds like a good example of the theory of relative addiction. The causal story — the explanation for the increased drug use — would be that the price of social interaction has increased since the person no longer has a close friend. When one activity (social interaction) increases in price, the person will start to use more drugs in order to satisfy a deeper need for "feeling good" or something else that is satisfied only by either drug use or social interaction. Imagine that we have a group of people like this person and that we have been lucky enough to have data on feelings of loneliness and drug use over a long time period — both before and after the death of the friend. Using these data we find that the death of a friend leads to a great increase in a sense of loneliness and a subsequent increase in drug use, as predicted by the relative addiction theory. Although the correlation in this case is compatible with the theory of relative addiction, we might still question the conclusion that loneliness is the causally important variable. The death of a friend may cause many emotional reactions at the same time — sadness, loneliness, anger, guilt, fear, bitterness. Even if it is true that I am lonely after the death of my friend, starting to use drugs might be a reaction to my guilt or my sadness rather than my loneliness. Hence, what we need is not only evidence that loneliness is strongly correlated with drug use, but that it is causally important in the way specified by the theory of relative addiction.

Is it possible to find this kind of evidence? As a starting point one might try to use the answers provided by drug addicts themselves as to why they started to use drugs. We have already seen that many drug users — but far from all — report that they are lonely. The question is now the extent to which they think this variable played a causal role in the development of their addiction. If the theory of relative addiction is correct, then loneliness should play a very special role and it is not enough that loneliness is an indirect part of the causal chain of events (e.g. death of friend caused loneliness which caused bitterness which caused drug use). The theory of relative addiction explicitly states that drug use is the result of loneliness and it is only evidence of this kind of relationship that would support the theory.
Using Qualitative Evidence to Test the Empirical Relevance of Loneliness

There are, of course, many problems with using and analysing qualitative evidence like the answers we receive from drug addicts when asking, “Why did you start to use drugs?” In an ideal world, we would have a representative sample of drug users who both knew the true reasons behind their drug use and were willing to tell us without self-serving bias. In the real world these conditions are rarely met. The sample on which the analysis below is based is made up of almost 500 drug users who entered treatment (both inpatient and outpatient) in 17 different treatment programmes during 1998. There was no intentional selection of individuals since we tried to interview everyone who was admitted. The 17 treatment facilities, however, were not randomly selected since financial and practical considerations forced us to limit ourselves to institutions in the southern part of Norway. Whether the results can be generalized to all drug users (not only those seeking treatment) all over the world (and not only the southern part of Norway) is obviously an open question.

Even if the sample were representative, arguments based on qualitative evidence are often viewed with suspicion because the biases of the researcher can easily colour the selection and classification of evidence. Stereotypically the researcher presents an argument which is illustrated with a couple of quotations from some of the interviews. This has the obvious disadvantage that the researcher can pick the responses that fit his or her own preconceived notions about the causes of addiction. To reduce the potential for this kind of bias, we could try to code the data into more general and countable categories and present information about the total number of responses in the different categories. However, there remains a lingering suspicion that the coding will nevertheless be influenced by the views of the researcher. In order to reduce these two problems, I have chosen to rely on quantitative analysis of the qualitative information using automatic (not manual) coding of the data. More specifically, the analysis is based on the frequency of different words in the responses of the individuals. It will become apparent, however, that the reduction in bias from this procedure also has a cost in terms of possible misclassifications and loss of context.

Table 3 shows the result of the frequency analysis of some terms used in answer to the question, “Why did you start to use drugs?” By far the most frequent words in the responses are “curiosity” (mentioned by 34%) and exciting (29%). Loneliness was mentioned by 2.3%. One might, of course, argue that a mere counting of words is too simplistic since many of the answers could indicate loneliness as a cause even if the term itself was never mentioned. For instance, respondents saying that they felt “alone” or that they started to use drugs after breaking with a boy- or girlfriend seem to belong under the general category of “loneliness as a cause.” Hence, to compare the relative importance of loneliness and other causes, we need to group the words into more general categories.

The creation of general categories cannot be achieved by simply adding the percentages for each word since the same respondent may have mentioned both. For instance, 10% mentioned both curiosity and excitement. If we want to arrive at the proportion of individuals who emphasised one or both we simply add the percentages for each term and subtract the percentage that gave both answers to avoid the problem of counting some respondents twice. After adjusting for the problem of double counting, the proportion of individuals who replied using terms like curiosity and excitement is 53%. In our criteria for inclusion in
Table 3: Frequency of various words mentioned when asking drug users why they started to use drugs.

<table>
<thead>
<tr>
<th>Word</th>
<th>Percentage of Drug Users Using the Term in Their Answer (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Curious, curiosity</td>
<td>34.0</td>
</tr>
<tr>
<td>Excitement, exciting</td>
<td>29.4</td>
</tr>
<tr>
<td>Environment</td>
<td>17.2</td>
</tr>
<tr>
<td>Friend</td>
<td>9.0</td>
</tr>
<tr>
<td>Older</td>
<td>8.2</td>
</tr>
<tr>
<td>Gang</td>
<td>7.3</td>
</tr>
<tr>
<td>Good</td>
<td>5.7</td>
</tr>
<tr>
<td>Moved</td>
<td>4.8</td>
</tr>
<tr>
<td>Like</td>
<td>4.6</td>
</tr>
<tr>
<td>Insecure</td>
<td>2.9</td>
</tr>
<tr>
<td>Lonely, loneliness</td>
<td>2.3</td>
</tr>
<tr>
<td>Alone</td>
<td>2.1</td>
</tr>
<tr>
<td>Escape</td>
<td>2.1</td>
</tr>
<tr>
<td>Calm</td>
<td>2.1</td>
</tr>
<tr>
<td>Angst</td>
<td>1.9</td>
</tr>
<tr>
<td>Sexual</td>
<td>1.9</td>
</tr>
<tr>
<td>Rebellion</td>
<td>1.7</td>
</tr>
<tr>
<td>Depressed</td>
<td>1.5</td>
</tr>
<tr>
<td>Addicted</td>
<td>1.3</td>
</tr>
<tr>
<td>Medication</td>
<td>0.8</td>
</tr>
<tr>
<td>Went</td>
<td>0.6</td>
</tr>
<tr>
<td>Left</td>
<td>0.2</td>
</tr>
<tr>
<td>None of the above</td>
<td>16.1</td>
</tr>
</tbody>
</table>

Source: Survey of 482 drug users in treatment organized by the Cost-Benefit Project under the National Institute for Alcohol and Drug Research in Norway. Data collection organized by Edle Ravndal and Grethe Lauritzen.

the category of loneliness, we might — in addition to loneliness — include all respondents who mentioned words like “left” (“My father left us,” “My girlfriend left me”), “went” and “alone.” Still, less than 6% of the respondents are in this group.

We could, of course, widen the net to include those mentioning “friends” and “gang.” Sometimes this sounds appropriate, for instance when respondents say they started to use drugs because “I had no friends.” However, more often replies emphasizing “friends,” “gang” and “environment” reversed the causal order. First they joined a special person or a group of friends (sometimes because they were lonely) and later they felt pressured or were introduced to the drug by the person or members of the group. That is also why the term “older” came out so high on the list (the fifth most common term, mentioned by 8.2%); many respondents answered that they started to use drugs because an older friend (often an older
boyfriend) introduced them to it. At this point the mere counting of words cannot be used because of the necessity for more contextual information to determine the classification of the answer. To solve this it is necessary to resort to manual coding of the data, but given the above-mentioned problem with this kind of coding, this option is closed. Instead, I want to discuss the more general question of whether the responses from drug users can shed any light on the real reason why people start to use drugs.

The (Limited) Usefulness of Drug Users’ Own Explanations

One prominent critic of the very possibility of learning anything about the causes of drug addiction by asking addicts themselves is John Booth Davies (1997b). The main message of his book *The Myth of Addiction* (and its sequel *Drugspeak*) is that the answers we receive in surveys are of limited use because they are primarily functional. That is, people will give an answer that is beneficial to them in a given situation and not the true answer. For instance, you are likely to be treated much more sympathetically if you report that your drug use is due to factors outside your control — like a disease (“I am an addict”) or an unhappy childhood — than if you simply say that you take drugs because you enjoy the experience. As he writes:

“From the standpoint of functional attribution, the reasons people give for their drug use are not, and can never be, hard or so-called ‘objective’ data on why drug use happens. Consequently the use of such statements as criteria against which to validate physiological or other measures, or as factual statements from which to derive diagnostic criteria, is probably misconceived. *The Myth of Addiction* argues that such explanations are primarily functional” (Davies 1997b: vii).

Not only is the answer “I take drugs because I am an addict” functional for the person using drugs, it is also functional, Davies argues, for many non-addicts to accept the answer. Many seem to believe that help and treatment can best be legitimised by labelling the use of drugs as a disease. If it is a free choice, then it seems more difficult to justify spending resources on “curing” a free choice. In this way it becomes functional for people in the treatment system to accept the addiction as a disease explanation. Finally, the addiction label is accepted by most non-professionals because it is functional in that it distinguishes the drug user from the rest of us. “They” are different from us in the sense that they have a disease that we cannot get and this is a comforting thought.

In short, Davies’ message is that addiction is a label that is functional both for drug users, for the experts in the treatment system and for the man in the street. It is the functionality of the label that lies behind the popularity of the addiction label and not its truth value; hence the title of the book. The question then becomes: is he right?

First of all one could argue that empirical evidence sometimes contradicts the argument that drug users mainly blame outside circumstances for their own misfortune. Many people simply say “I like it” or “It felt good” to explain their drug use — in contrast to what Davies predicts based on theoretical arguments about the functionality of answers. Somehow people
sometimes seem to give at least what they believe is the true answer, despite it being not very beneficial for themselves. This leads to the conclusion that the problem is a question of degrees and not absolutes. There are certainly factors that make respondents twist their answers, but — since they sometimes appear to give answers that are not at all self-serving — there must also be factors that prevent people from always just giving the most beneficial answer. What kind of factors could they be?

Sometimes there is little reason to lie since there are no obvious external sanctions or benefits (as in anonymous questionnaires). Admittedly, there is always the possibility that you need to lie to yourself, but removing external sanctions and benefits at least reduces the problem of possible bias. There is also a need to avoid being seen as stupid, too eager to blame others and, to the extent that norms against lying have been internalized, to avoid psychological costs associated with lying. Theoretical reasoning opens up the possibility of many types of biases, but it alone says nothing about the relative size and importance of these biases. Hence, instead of simply arguing that the above-mentioned problems render all verbal responses suspect to the point of being worthless, we should try to assess the empirical importance of the problem.

We could, for instance, ask the same question in different circumstances to examine if the mechanisms mentioned produce different answers. One such test could be to pose very sensitive questions in face-to-face encounters and compare answers received from more anonymous telephone surveys. Statistics Norway has done this and Table 4 shows the responses to three different sensitive questions — about smoking, sex and loneliness — and how they varied depending on whether the interview was done face-to-face or by telephone (Roll-Hansen 2001). As the table shows, the practical significance of the differences is not very great since the absolute differences in percentage points are small (less than 3%). In this case it seems like answers related to smoking and loneliness are reasonably consistent, even though the setting might lead one to expect different answers to be functional.

One could, of course, find empirical evidence that points in the opposite direction. For instance, Davies (1997b) cites research indicating that drug users report higher consumption when confronted by a well-dressed interviewer compared to a more roughly dressed person. There is also a wealth of evidence to suggest that people’s responses are sometimes shaped by anchoring, framing, word-switching and many other bias-creating psychological

<table>
<thead>
<tr>
<th>Question</th>
<th>Face-to-Face</th>
<th>Phone</th>
</tr>
</thead>
<tbody>
<tr>
<td>Do you have somebody close to you with whom you can talk in confidence, other than members of your family? (%)</td>
<td>90</td>
<td>88</td>
</tr>
<tr>
<td>Do you occasionally smoke? (%)</td>
<td>39</td>
<td>41</td>
</tr>
<tr>
<td>Have you had sexual intercourse during the past four weeks? (%)</td>
<td>86</td>
<td>89</td>
</tr>
</tbody>
</table>

mechanisms (Kahneman & Tversky 1982). Moreover, it seems that people are more likely to know whether they have smoked than why they did so. Hence, answers to why-questions may be less reliable than answers to questions of whether a particular form of behaviour has been performed (Nisbett & Ross 1980).

The problem is not only bias. When responding to a why-question people use a concept of explanation, and since there are competing accounts of what it is to explain something, we should not be surprised to receive a list of very diverse answers that do not fit neatly into a list of potentially important causal variables. Some answers cite a situation (e.g. loneliness); some cite an event (e.g. sexual abuse); some point to the effects of the drugs (felt good); some argue that it is best explained by a genetic weakness or some other mechanism that made the person especially vulnerable; and still others argue that they ended up as addicts because they mistakenly believed that they could experiment with drugs without becoming addicted. These answers reveal some of the same disagreements that exist among social scientists and philosophers on the nature of explanation and causation. As Rachlin shows in his contribution to the current volume (Chapter 4), some scientists seek answers by picking (sometimes literally) the brain of the person; others seek the answers in the intentions and beliefs of the individual; yet others believe that we should focus on environmental variables. There is, as Fred Dretske (1988) has argued, no necessary conflict between these perspectives since they are explanations at different levels in the reductionist chain, but, from the perspective of a brain scientist mainly concerned about the dopamine system, a qualitative survey of self-reported reasons for drug use seems of little use. If, however, we believe that causes of behaviour are best explained by assuming that people act on conscious intentions and beliefs, then surveys might be considered useful, providing that the problem of bias can be reduced.

Where does this leave us? There is conflicting evidence on the status and validity of verbal reports as a source of evidence. To some extent we can design questionnaires to avoid some of these problems and we can test the importance of the problem by examining whether we get the same answers in circumstances that are different with respect to the expected functionality of the answers. It is, however, difficult to arrive at any precise kind of estimate of the degree to which verbal reports can be relied on as evidence of why people start to use drugs. This means that it may be better not to use the data as I have done so far, that is, as evidence by counting the frequencies of various types of answers. There is, however, another way we can use the responses: reading answers about why people begin to use drugs can provide us with ideas about mechanisms that might be important. Hence, instead of using the data as evidence, we use it heuristically, as a source of ideas. This is what I will do in the next section of this paper.

**Mechanisms and Alternative Views of Social Interaction**

When discussing the word-frequency approach, one problem was that we needed on occasion more contextual information to determine whether an answer with a particular word should be put into one category or another. For instance, the category for the word "friend" was particularly difficult to determine. This might be an obvious case, but as it turns out it had a particular relevance in the context of social interaction and relative
addiction. Conceptually, there is an important difference between those who said they started to use drugs because they were lonely and those who said that started to hang out with "outsiders" because they were lonely and only then started to use drugs. In the first case there is a direct causal relationship between loneliness and drug use; in the second there is an intermediate variable. As argued previously, the theory of relative addiction predicts that the relationship should be of the first type. That is, drug use should be caused by loneliness first, not by hanging out with the wrong people. The second mechanism, however, suggests that drug use may partly be caused by having the wrong kind of friends, which is very different from having no friends. Is the cause "no friends" or "wrong kind of friends?"

**Social Mechanisms: Elster and Schelling**

There is no general answer to that question. In fact, based on arguments from Elster (1998), we might doubt the very possibility of finding general answers in the form of grand theories which, within a single encompassing approach, claim to explain why people start, continue, and stop (and start again) different kinds of addictive behaviour (smoking, alcohol, illicit drugs). Given the current state of knowledge, Elster suggests that the best we can do is to explore *mechanisms* related to drug use. This may sound like a defeatist attitude, and it raises the question of exactly what a mechanism is and why it might be worth examining.

To explain what a mechanism is and why it is close to impossible to find general laws in the social sciences, Elster presents an example from the well-known alcohol researcher, George Vaillant. At issue is the question of whether there is a social law which says that children of alcoholics also tend to become alcoholics. Vaillant writes that: "Perhaps for every child who becomes alcoholic in response to an alcoholic environment, another eschews alcohol in response to the same environment" (cited in Elster 1998:45). If both effects are present, there is no general law that children of alcoholics also become alcoholics. The same condition (alcoholic parents) is causally related both to children who become teetotallers (as a reaction to their parents' abuse) and children who become alcoholics (also as a reaction). Now, the argument that there must be some kind of difference between the two that allows us to explain why some ended up as teetotallers and some as alcoholics may be true, but Elster argues that in practice our knowledge is often too limited to discover these factors. There is a multitude of small differences that could work together interactively and non-linearly and sometimes only small differences or relatively accidental events may be decisive. Hence, all we have is knowledge of a mechanism: "frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences" (Elster 1998: 45).

Social laws are useful since they can be used to predict outcomes. Mechanisms, on the other hand, do not claim to have the same predictive power. They are, however, useful in the sense that they can help us to explain an outcome after it has happened. For instance, we do not know in advance whether the child of an alcoholic will also become an alcoholic, but once we observe the outcome (alcoholic or teetotaller) we tend to accept that the alcoholism of the parents played a role in both outcomes.

In the current context Elster's approach seems to capture some of our problems. Social interaction may have both positive and negative effects on drug use depending on the
nature of the interaction. Hence to ask whether the cause of drug use is "not enough social interaction" may be too general. Instead of trying to create theories that attempt to explain why people start, continue and stop using addictive substances, we would be better off exploring mechanisms that may help explain (but seldom predict) why some people start using drugs without also claiming that this is the reason they continue or eventually stop or that the mechanism explains the majority of cases.

On the other hand, Elster's account may seem overly pessimistic. After all, it is sometimes possible to get an idea of the relative strength of the various links in a causal chain. For instance, surveys of children of alcoholics can say something about the extent to which they end up as teetotalers or alcoholics. If the tendency is clear, we may have a useful probabilistic prediction (say 70% of the children become alcoholics compared to 5% with non-alcoholic parents). This, in turn, is useful information for policy formulation in terms of suggesting which groups should be focused on to reduce alcoholism. However, the relative lack of such strong and useful relationships in the social sciences suggests that there is something to Elster's argument that we would be better off exploring mechanisms instead of proposing general theories. Hence, I shall first follow Elster's suggestion by exploring a model of two mechanisms that link social interaction to drug use. After doing this, I will discuss whether it is possible to use empirical information to arrive at conclusions about the relative strength of the links and the practical importance of the mechanisms discussed.

Exactly what kind of mechanisms should we explore? Many of the papers in this volume focus on psychological or cognitive mechanisms such as the consequences of different forms of discounting. Because this topic is so well covered I want to follow a different route. The point of departure is Thomas Schelling's definition of a mechanism as a hypothesis that seeks to explain something in terms of two types of interaction:

I propose ... that a social mechanism is a plausible hypothesis, or set of plausible hypotheses, that could be the explanation of some social phenomena, the explanation being in terms of interactions between individuals and other individuals, or between individuals and some social aggregate (Schelling 1998: 32–33).

Changing lanes when the next car is less than five feet away is an example of individual-to-individual interaction. Adjusting speed to the average speed of others in the same lane is an example of interaction between an individual and a social aggregate. The definition itself may not seem very revolutionary (and it differs somewhat from Elster's definition) but Schelling's application of it shows that it has the potential to produce original and convincing explanations of social phenomena that are considered to be paradoxical. The main reason is that the aggregate consequences of individual motivations that interact are often far from obvious. For instance, assume people get vaccinated only when they perceive the risk of a disease to be large enough to be worth the effort and cost to get vaccinated. As Schelling shows, the aggregate consequence of such a simple individual decision rule is, depending on the parameters in the model, to create ebbs and flows in the incidence of a disease. When many are observed to be ill, people will get vaccinated. Because of the increasing number of people who get vaccinated, the number of ill people will eventually fall and at some point there will be so few people who are ill that people will not find it worth the cost to
get vaccinated. But this means that the disease will start to grow again and so the ebb and
flow of the disease continues. Hence the apparently simple decision rule is able to suggest
an explanation for a larger macro pattern that at first sight looks puzzling (the ebb and flow
of a disease). Moreover, the mechanisms in this case have a very surprising implication: if
there is a lag between becoming infected and the visible signs of the disease, then one might
reduce the overall number of people who become ill by suddenly increasing the number or
visibility of ill people! This example illustrates how Schelling’s approach can produce both
original explanations and potentially important and surprising policy conclusions.

I have so far tried to justify a general approach based on mechanisms in the sense used by
Elster (1998). Within this approach I follow Schelling in focusing on mechanisms based on
interaction. I also argued that social interaction itself was too general a concept and needed
to be broken down. To impose some structure, we can use Manski (2000) who defines
and lists three general types of social interaction as follows: “Agents interact through their
chosen actions. An action chosen by one agent may affect the actions of other agents through
three channels: constraints, expectations, and preferences” (Manski 2000: 118). In the case
of drug use, the availability of drugs may be an example of the first (a user moving into a
drug-free neighbourhood may remove the availability constraint barring others); learning
about the effects of drugs (positive and negative) from existing users may be an example
of how other agents’ actions may influence somebody’s expectations; and if many of my
friends use drugs it may change my own preferences on whether or not to use drugs. The
change of preference may be shallow (a person doing something not because he likes it but
because he prefers to do what his friend is doing) or deep in the sense that you really change
your opinion about the value of something after interacting with your friend. Both types of
preference are affected by social interaction.

Note that social interaction is here used in a very different sense than in Rachlin’s article.
Psychologists and sociologists often use social interaction as a variable, trying to measure
the quality and quantity of relationships with friends and family. This, in turn, is used as
a variable to explain, for instance, drug use. When economists present a model that uses
social interaction to explain something they usually do not use interaction as an explanatory
variable in itself. Consider, for instance, a model that starts from the assumption that people
have a desire to conform and then derives the aggregate consequences of that desire for
the level of drug use in society. Moene (1999), who has modelled one such mechanism,
shows that individual desire to conform opens up the possibility of multiple equilibria and a
tendency for drug use to go up more easily than down. This model is categorized under social
interaction because the preferences of the agents are interrelated (“I want to do whatever the
others are doing”), not because social interaction is an explanatory variable in itself in the
model. There is little point in discussing who has the correct definition of social interaction,
as long as one is aware of the difference and avoids being confused.

Using the suggestion from the survey about reasons for starting to use drugs, one might
want to explore the possibility that friends influence each others’ use of drugs. As it stands,
however, this is far too general and vague. What we need is a more rigorous formulation of
the decision process and the mechanisms of how friends may affect one another’s decision
to use drugs. I shall explore two such mechanisms. First there is observational learning in
the sense that potential users learn about the dangers (and joys) of drugs by observing the
consequences of drugs on existing users. Second, I include a mechanism which implies that
the pleasure of using drugs depends on how many others are also doing drugs (a stigma mechanism).

**Microfoundation: Observational Learning, Social Stigma and Decision Making**

For the sake of argument, let us assume that each individual \( i \) in time period \( t \) decides whether to use drugs based on a comparison of his expected utility of using drugs \( [EU_{it}(D)] \) and the expected utility if he chooses to abstain from drugs \( [EU_{it}(A)] \). The underlying assumption is that the behaviour is at least influenced by some kind of calculation, as opposed to purely emotional or norm-based behaviour.

Use drugs if: \( EU_{it}(D) > EU_{it}(A) \)

What are \( EU_{it}(D) \) and \( EU_{it}(A) \)? The latter — \( EU_{it}(A) \) — is assumed to be a constant: \( U^A \).

To work out \( EU_{it}(D) \) we need to consider the possible consequences of experimenting with drugs. For the sake of simplicity I shall assume that there are only two possible outcomes for individuals who experiment with drugs: Either a junkie career (unhappy) or a yuppie career (not so unhappy). This assumption is meant to capture the fact that not all individuals who experiment with drugs end up as stereotypical junkies.

The two possible outcomes of experimenting with drugs — becoming a yuppie or a junkie — result in certain payoffs. A very simple way of formalizing this would be to say that \( U^j_{it} \) is the total (discounted) sum of utility you receive if you end up as a junkie (for individual \( i \) at time \( t \)) while \( U^y_{it} \) is the total sum of utility if you end up as a yuppie. Note that \( U^j_{it} \) does not represent annual utility as a junkie. It represents the total (discounted) sum of utilities if it turns out that experimenting with drugs makes the person a junkie for whatever time horizon the person has. Hence, the formulation is agnostic about whether the time period is considered to be the rest of the person's life or whether the agent is myopic to the extent that the coming year represents his furthest horizon. If the time period considered is the rest of his or her life then it may include some years as a yuppie, some years as a junkie and then, finally, some years as a non-user (treated or natural recovery/matured out). The same goes for \( U^y_{it} \). It does not only include years as a happy drug user, but also years as a non-user after the "happiness" has worn off.

The individual does not know whether he will end up as a junkie or a yuppie, so when working out the expected utility of experimenting with drugs, he or she has to estimate the probability of ending up as a junkie. One way of doing so would be to use the current proportion of junkies as an input in the estimation of how likely the person believes it is that he will end up as a junkie: \( p_{it}(j) \). It is as if the individual looks round to take bearings and if he sees relatively few junkies, then he or she may conclude that the danger of becoming a junkie must be quite small.

Finally, in order to capture the effects of social stigma, I introduce a moral cost of experimenting with drugs \( (m) \). One might think about this as the cost of doing something that many people dislike. A stigma parameter like this, however, should vary depending on the number of people who engage in the activity. For instance, when few people were divorced, the associated stigma was large, but in a situation where divorce is common the stigma is lower. Hence, the stigma associated with drugs is assumed to depend on the proportion of
people that are junkies \( (j) \). Moreover, individuals are assumed to differ to the extent that they care about the stigma or moral cost associated with drugs. In other words, each person has a parameter \( (\theta_t) \) that indicates the degree to which they are sensitive to social stigma (reflecting, perhaps, a desire to conform). This parameter has a cumulative density function described by \( F(\theta) \). This function simply tells us the proportion of individuals with “sensitivity to stigma” less than a certain number \( (\theta) \) and this, in turn, is the same as the proportion of the new generation that begins to experiment with drugs. We may even allow negative values of \( \theta_t \) to allow the possibility that some people enjoy deviating from the majority.

Altogether then, the expected utility of experimenting with drugs for an individual at a point in time is the utility he will receive in the two possible outcomes (junkie career or yuppie career) multiplied by their respective probabilities and adjusted for social stigma:

\[
EU_{it}(D) = p_{it}(j)U^J_{it} + (1 - p_{it}(j))U^Y_{it} - \theta_t m(j)
\]

One might argue that the formulation so far ignores many issues that are central to addiction. For instance, I do not explicitly model discounting, which many people argue is an important phenomenon when trying to explain addiction. I do not deny the importance of discounting, but the present focus is on something else, namely the effects of interaction through observational learning and social stigma. I want to isolate this and I do not want to bring in more complications than necessary. Moreover, there is a difference between explaining addiction at an individual level and explaining the social level of addiction. Much effort has been invested into tracing the causes of addiction — searching for a golden key variable or set of variables that would explain why some people become addicted. These efforts might lead one to assume that the individual and the aggregate are connected in a simple way: for instance, if loneliness is a major cause of drug use, then societies with much loneliness will have many addicts. Focusing on social interaction between heterogeneous agents makes us aware that this kind of inference from the individual to the aggregate is sometimes wrong and, as a consequence, the answers to “why is the level of drug use \( X \)” or “why did drug use increase by \( Y \)%?” need not always be “there was a change in the loneliness factor” or whatever is believed to explain addiction at the individual level. Instead one could point to the dynamics of the aggregate system itself. This aspect has, perhaps, received less attention than the search for a key variable and, for this reason, I find it worthwhile to focus more exclusively on aggregate mechanisms.

There is, of course, a relationship between the aggregate and the individual even if it is not of the simple “representative agent” type. Indeed, it is important to model this, which is why I have made an effort to incorporate good microfoundations. However, the important micro-features from a macro perspective need not be the same as the important features from a micro perspective. That is, discounting may be very important at the individual level, but it need not be important when considering the overall level of drug use or changes in that level. Hence, we must postpone for a later stage a more explicit modelling of discounting.

**The Aggregate Result of the Model**

So far, all I have is a very general formulation of the decision problem. What I want, however, is an expression of the aggregate result — the proportion of drug users of the total
population and how it varies — if people make their decisions based on the microfoundation just described. This requires several assumptions, both in terms of simplifying assumptions and in terms of more substantial assumptions about the mechanisms of aggregation.

In order to make it easier to get analytic results, I now make the following simplifying assumptions:

- $p_{it} = p_i \forall i$ (every individual uses the same probability of becoming a junkie)
- $U_{it}(\cdot) = U(\cdot) \forall it$ (the utility of ending up as a junkie, a yuppie or an abstainer is the same for every individual at all times)

This means that every individual uses the same probability of becoming a junkie and that the utility of ending up as a junkie or as a yuppie is the same for every individual at all times. The decision problem for the individual is then reduced to comparing $U^A$ to the following expression:

$$EU_{it}(D) = p(j)U^J + (1 - p(j))U^Y - \theta_i m(j)$$

How do we get from this individual decision problem to an aggregate result? (The following solution is based on suggestions by Jörgen Weibull on a previous paper that tried to formalize the aggregate result.) In equilibrium there is no incentive to change strategy, so, for the individual at the margin, the expected utility of using drugs must be equal to the utility of not using drugs. Hence, we solve:

$$EU(D) = U^A$$

$$p(j)U^J + (1 - p(j))U^Y - \theta_i m(j) = U^A$$

$$\theta^*_i = \frac{p(j)U^J + (1 - p(j))U^Y - U^A}{m(j)}$$

Or, in plain language, there are some people who will choose to experiment with drugs (those who are least sensitive to social stigma, $\theta_i < \theta^*$), and some people who will not experiment with drugs ($\theta_i > \theta^*$).

In equilibrium, assuming rational expectations, the expected probability of becoming a junkie must be the same as the equilibrium proportion of junkies:

$$j = p(j)F(\theta^*)$$

$$j^* = p(j)F\left(\frac{p(j)U^J + (1 - p(j))U^Y - U^A}{m(j)}\right)$$

$$j_{i+1}^n = p(j_i)F\left(\frac{p(j_i)U^J + (1 - p(j_i))U^Y - U^A}{m(j_i)}\right)$$

Depending on the properties of $p(j), F(\cdot)$ and $m(j)$, we may have one or several solutions. For instance, if the moral cost of using drugs is non-linear, we could have the situation illustrated in Figure 2. To construct this figure I assumed that the distribution of sensitivity to sanction was normally distributed. This means that the cumulative distribution $F(\theta)$ becomes non-linear (it looks like an extended $S$) and it is this non-linearity that creates the
Possible equilibria (i.e. no change in the proportion of junkies) are located along the 45 degree line (along which $j_t = j_{t+1}$). The S-shaped curve shows the number of new drug users for given values of $j_t$, i.e. the shape of $F(\cdot)$. There are three possible steady states: $j_1$, $j_2$ and $j_3$. Two of these ($j_1$ and $j_2$) are stable in the sense that you will return to these states after a small deviation. $j_2$ is unstable since small deviations will start a dynamic in which you are moved even further away. Hence, otherwise similar communities may be “locked” in very different (and self-sustaining) drug situations: $j_1$ (low drug use) or $j_3$ (high drug use).

Figure 2: Multiple drug use equilibria in a model of observational learning and social stigma. The number of junkies next year is a function of the number of junkies last year. Possible equilibria (i.e. no change in the proportion of junkies) are located along the 45 degree line (along which $j_t = j_{t+1}$). The S-shaped curve shows the number of new drug users for given values of $j_t$, i.e. the shape of $F(\cdot)$. There are three possible steady states: $j_1$, $j_2$ and $j_3$. Two of these ($j_1$ and $j_2$) are stable in the sense that you will return to these states after a small deviation. $j_2$ is unstable since small deviations will start a dynamic in which you are moved even further away. Hence, otherwise similar communities may be “locked” in very different (and self-sustaining) drug situations: $j_1$ (low drug use) or $j_3$ (high drug use).

multiple equilibria. We could, for instance, have three equilibria, two of which are stable (see Figure 2)

**Discussion**

One might wonder what we gain from exploring a formal mechanism in this way. It is not an attempt to explain drug addiction in general, so its utility as an explanatory model is, at best, partial. What we have are some plausible assumptions about individual behaviour and the model helps to derive the aggregate consequences of these. These aggregate consequences are not immediately obvious, so in this sense it may be worthwhile to engage in formal modelling of mechanisms. If it turns out that the model captures a mechanism that is empirically important, it may help to explain why some communities seem stuck in a situation with high drug use while others are relatively stable in a low use situation.

**Policy implications** The model may also have some surprising policy implications on how to shift from a high to a low-user equilibrium. Traditionally formal models of the spread of heroin and other drugs have used standard epidemiological models as their starting point. In
these models a drug user is always “contagious” in the sense that he spreads the habit when he interacts with friends (see, for instance, Hoppensteadt & Murray 1981; Mackintosh & Stewart 1979). The obvious policy conclusion in these models is to isolate the contagious individuals in order to reduce the spread. The problem with the traditional models is that people are linked by assumption rather than by explicit modelling of mechanisms. The models do not say exactly how drug use is contagious. And, as soon as we start to think about that, we discover that sometimes drug use may deter as much as it attracts. Watching a friend die from heroin use may sometimes scare friends away from continued use, but hanging out in a group where there is no social stigma attached to drug use (indeed, there may be pressure to conform to use) has the opposite effect. Within this new framework, isolating heavy drug users may be counterproductive since you then remove the “fright” effect. The model developed in this paper takes into account both effects and for this reason the obvious policy implication is not necessarily to isolate the drug users. On the contrary, the model developed in this paper suggests that the visibility of the worst off individuals should be increased in order to reduce the aggregate level of drug use. The implication, however, is not very strong in the sense that it depends heavily on the relative strength of the various links. Even if high visibility could increase the belief that drug use is harmful, it could also reduce the belief that drug use is limited to a few outcasts. Thus, unless we have good indications of the relative strength of the links, we are back to Elster’s argument that we can use knowledge of mechanisms to explain ex post, but not to predict or design policies.

**Empirical evidence** One of the main problems in testing theories of social interaction, as discussed by Manski (2000), is the so-called reflection problem. For instance, when students in a neighbourhood behave similarly, it is statistically often very difficult to distinguish whether they do so because they find themselves in the same environmental circumstances or through more direct influences (peer pressure and so on). Rather than looking at aggregate data, therefore, we would be better off trying to explore the empirical plausibility of the microfoundation that went into the model. For obvious reasons, few controlled experiments have been done on peer influence and illegal drugs, but there has been extensive research on the extent to which the amount of alcohol consumed is influenced by the introduction of another person (heavy or moderate drinker, male or female, high or low status). The results from these experiments strongly suggest that people are influenced by their peers (Quigley & Collins 1999). It is, however, much more difficult to pin down whether this is because of some desire to conform to what the rest of the group is doing or some other mechanism. The same goes for the mechanism of observational learning. When asked about their sources of information, many people answer that their most important source is “friends” (Hanneman 1973); there are also studies which show that those with high risk estimates are more likely to avoid smoking than those with low risk estimates (Viscusi 1991). Although suggestive and supportive of the empirical relevance of the mechanisms modelled in the last part of this paper, the evidence relies too much on correlation to allow it to be used as strong evidence for the existence of a causal relationship. The best that can be said is that the correlations at least do not appear to contradict the assumptions on which the model is built. Little can be said about the relative importance of the links at this stage.
Extensions Within the model, individuals are assumed to be influenced by everybody else. It is as if they observe all other individuals and what they are doing at all times. A much more realistic formulation would be to let people interact more locally in small groups. Instead of observing the number of junkies in the total population, they would observe the number of junkies within their small groups. And, instead of being influenced by a desire to conform to (or deviate from) the whole population, they could be influenced mainly by the members of the group to which they belong. Making social interaction local in this way would produce a much more realistic model, but one of the main results would remain (the existence of multiple equilibria and the importance of visibility of the bad effects of drugs). The same is true for more realistic formulations of how people form beliefs about the probability of ending up as a junkie. Instead of using only the proportion of junkies, they might try to work out the exact proportion of all people who have experimented with drugs who became junkies. While this revision makes the model more realistic, it does not give quantitatively different answers: it is still possible to get multiple equilibria.

Conclusion

The theory of relative addiction is an ambitious attempt to explain why people start, continue and stop/relapse smoking, alcohol and illegal drug use. I have argued that there was only weak support for the statement that loneliness and drug use are separable substitutes. Moreover, the strong version of relative addiction theory was inconsistent with many empirical facts about drug use and loneliness (not all addicts report being very lonely and vice versa). By relaxing the assumption of separable substitutes, we could create a weaker version of the theory, but then loneliness becomes only one of many possible causes. That being the case, we need comparative empirical research to justify the focus on loneliness as the most important cause of addiction. Finally, instead of using the general category of social interaction, we need to distinguish between different types of interaction and how they might affect drug use. This approach, as exemplified by a model of observational learning and stigma effects, represents a new line of research that deserves to be explored because of its potential to produce non-obvious knowledge (the aggregate consequences of several individual level structures are usually not obvious when the agents are heterogeneous). Finally, models that use social interaction as a mechanism sometimes produce policy implications that are often ignored in models that simply assume a representative agent and for this reason it is important to focus on social interaction.

References


Comments on Melberg

Howard Rachlin

I hereby abandon the strong theory of relative addiction. I did not really mean to say that all addicts are (or rather, were) lonely people. Some of my language, I admit, could be taken to imply this strong assertion. What I actually think is that relative addiction theory is a mechanism in exactly the sense that Hans Melberg uses the term. Even within relative addiction theory there are positive addictions other than social support that may substitute for negative addiction: exercising, listening to classical music, reading Trollope, doing crossword puzzles, watching soap operas, collecting stamps, gardening, and so forth.

A behavioral theory would be silent about internal events common among all of these activities and addiction, but Fisher's (1996) speculation that they all induce positive mood and reduce anxiety is at least not completely implausible.

Fisher showed that actual loss of social support was correlated with increased smoking and that actual success in treatment varied with degree of social support. This evidence is only suggestive and far from decisive. But the evidence presented by Melberg does not bear on the issue. It is not even evidence against the strong form of relative addiction theory, let alone a weak form.

First, relative addiction theory is silent on "feelings of loneliness." From the viewpoint of teleological behaviorism, feelings of loneliness would themselves be patterns of behavior. So, it would be possible to observe them. But for that we would have to ask, not the addict, but the addict's friends or relatives, people who live with him and can observe his behavior. But let us drop this idiosyncratic point of view for a moment (I will pick it up again later) and let us suppose that feelings are internal states that addicts can introspect and report on veridically. Even then, such feelings may or may not correlate with actual degree of social interaction. And, even if feelings of loneliness (accurately reported) were indeed indicative of degree of social interaction, relative addiction theory makes no prediction about absolute social interaction over a population of addicts. As correctly and lucidly described in Melberg's paper, relative addiction theory says that loss of social interaction in an individual may (if the effect of present choices on future behavior are as they are drawn in Figure 1 and if the loss of social interaction is severe enough to upset the previous equilibrium) lead to addiction, and that a given treatment will work better if it involves increased social interaction than if it does not. Neither of these predictions implies any correlation (positive or negative) over a population, at a given point in time, between social interaction and addiction.

The study in which addicts are asked to speculate about the causes of their addiction and why it is maintained also has serious flaws — but these are described much more
clearly and thoroughly by Melberg himself than I could do. From a behavioral viewpoint, it would again have been better to have asked the addicts’ friends, relatives, co-clients, co-workers, physicians, and psychologists about the addicts’ social interactions than to have asked the addicts. But, absent longitudinal tests on individual subjects, using objective measures, or at least current measures such as diaries, it is not possible to directly and decisively test the assumptions underlying relative addiction theory — even in its strong form.

As Melberg says, relative addiction theory does not answer such questions as, Why do some people smoke while others use alcohol? It is true that relative addiction theory does not make distinctions between forms of addiction, and this is a common criticism of it. However, a theory that predicts that a person will move from New York to California need not say whether he will take the plane, train, bus, or car. Such a theory might be useful nevertheless.

One area in which relative addiction theory is silent is the internal mechanisms that might mediate between lack of social support and addiction. I intended the word, “loneliness” to stand, not for an internal state — cognitive, emotional or physiological — but simply for lack of social support. If someone should eventually discover how to measure guilt other than behaviorally, it would not matter to relative addiction theory whether internal guilt, so measured, or internal feelings of loneliness were the mediating factor.

I agree with Davies (1997) that “addiction” is a functional label for people who use the word. It allows them to classify behavior patterns into those that are addictions and those that are not — and this is a very useful distinction to make. That distinction is exactly what many of us here are trying hard to make. But I disagree that this fact makes addiction a myth. If it did, we could with equal justification label as myths: sensation, perception, thinking, as well as hope, love, imagination, and all mental and emotional terms. For a teleological behaviorist, these terms are not labels for ephemeral internal states that we may introspect upon and report. They are labels for patterns of behavior, labels we apply because we find them useful. This, in my opinion, makes them more not less real.

Truth and lying about mental or emotional states therefore depend on the degree of conformity between verbal and non-verbal behavior or between one instance of verbal behavior and other instances of verbal behavior. Telling the truth about your emotions does not depend on a match between words and internal physiological events.

The addiction mechanism presented in the latter half of Melberg’s paper is an interesting one but would be even more difficult to test than relative addiction theory. It says that addiction depends on a person’s social milieu. A high number of addicts relative to non-addicts in a group will increase the probability of a member becoming an addict in two ways: via the person’s desire to conform and via decreased moral condemnation. Moral condemnation would seem to vary oppositely to social support and might be measured. But “desire to conform” is not a behavioral variable. Melberg is correct in not using this term in his utility functions. You could nevertheless ask how desire to conform would be generated and maintained. One possible candidate is by social support. Thus both of the variables said to determine addiction in this model may be traced to social support, which in turn depends on social interaction. And, social interaction can be measured. It would remain then to state how social interaction behaves in this mechanism differently from its behavior in the relative addiction mechanism.
One possible difference is the influence of the group in this mechanism and the focus on the individual in the relative addiction mechanism. A problem arises, however, in determining which of the many groups that every person belongs to determines the critical ratio of addicts to non-addicts. We are all members of our family, our neighborhood, our town, our school, our circle of friends. Ratios of addicts to non-addicts may vary widely from one of these groups to the other. Which one counts?

Finally, I want to express my gratitude to Hans Melberg for taking relative addiction seriously enough to put it to the test. That the tests are not definitive is not his fault; it is mine, for not specifying the theory more clearly and for using such a term as “lonely” in the title, just because it sounded good, rather than “lack of social interaction” which sounds a lot worse but better expresses what I meant to say.

References

Reply to Rachlin

Hans O. Melberg

In his comments Rachlin argues, first, that the key variable in his theory is social interaction, not loneliness, or its even less relevant counterpart, self-reported loneliness. Second, evidence on the relationship between drug use and social interaction/loneliness across a population has no relevance when it comes to testing his theory since the theory is only intended to explain individual changes and not absolute levels of substance abuse in a society. Third is the claim that the theory sets out a useful mechanism even though it does not distinguish between addiction to cigarettes, alcohol and illicit drugs. Finally, Rachlin argues that the model presented towards the end of my paper is compatible with his theory but even more difficult to test empirically. I will respond to each of these comments in turn.

Social Interaction and Loneliness

In his previous articles on the topic, Rachlin writes about both social interaction and loneliness. It is therefore conceptually useful to learn from his comments that social interaction is considered to be the main variable. Empirically speaking, however, it is actually less useful since the term is never operationalized. In order to test the theory we need some way of measuring social interaction or some variable that is related to social interaction. Based on this requirement I used self-reported feelings of loneliness and number of friends as proxies for social interaction. In response Rachlin writes that: “such feelings [loneliness] may or may not correlate with actual degree of social interaction.” Against this, I believe that a negative correlation is most likely. That is, on average people who are lonely probably have less social interaction than people who are not lonely. Whether this is actually true is impossible to know as long as Rachlin does not present an operationalized definition of social interaction.

Rachlin also argues that we would be better off having information about other people’s perceptions of an individual’s loneliness than that individual’s own feelings. Speaking in terms of cause and effect, I would argue that it is the subjective feeling of loneliness or lack of social interaction that is important; it is of little help that others believe I have lots of social interaction if I do not feel that way myself. Against this, one might argue that feelings have no place in a behavioural theory in which only observed behaviour counts. Although I disagree with this type of strict behaviouralist approach, this disagreement has far wider ramifications and Rachlin is right in correcting me for introducing non-behaviouralist elements (like feelings) into what was intended to be a strict behaviouralist theory.
A Theory of Individual Change or Levels of Drug Use?

In his comments Rachlin argues that:

... relative addiction theory says that loss of social interaction in an individual may [...] lead to addiction, and that a given treatment will work better if it involves increased social interaction than if it does not. Neither of these predictions implies any correlation (positive or negative) over a population, at a given point in time, between social interaction and addiction.

This is a much more modest and less interesting claim than the one I believed Rachlin to have made in some of his previous articles. Two of the key elements in the previous theory were, firstly, that there is a constant need for something, and second that this need can only be satisfied by drug use and social interaction (i.e. the assumption of separable substitutes). As he wrote, "A central assumption of relative addiction theory is that addictive activities and social activities are walled off in this way — they are at least moderately substitutable for each other but not for any third activity" (Rachlin 2000: 151). Interpreted in this way, the theory really is linked to aggregate implications. It is true that more assumptions are needed to make the link empirically testable — for instance, about individual differences in the substitutability of the two goods or the urgency of the need for the undefined good — but the major linking assumptions are the ones that are made by the theory itself, i.e. the assumption that there is a constant need and that this need can only be satisfied by two activities (drug use or social interaction), i.e. that they are separable. This necessarily implies that if you have a lot of one, you will have less of the other. Rachlin's suggestion that there might be many other factors apart from social interaction that might have the same effect would, as he correctly argues, eliminate the aggregate implications, but it would also mean that he has to abandon the assumption of separability which was "central" to the original theory.

Usefulness, Mechanisms and Testing

I am in complete agreement with Rachlin that a theory of addiction may be useful even if it does not predict exactly what the person is addicted to (alcohol, tobacco, illicit drugs and so on). My point was only that when trying to test such a theory one should be careful not to use evidence only from the most plausible case to support the general theory.

I would, however, disagree with the more general argument that the theory of relative addiction really is a mechanism in the sense in which I used the term. The defining feature of a mechanism, according to Jon Elster (1998), is that the same type of event — say less social interaction — may lead to both more or less drug use. In the theory of relative addiction, I do not see how less social interaction could lead to less drug use. In the model presented towards the end of my paper, however, this is possible since interaction can both scare people away from drugs (observing its bad consequences if you meet junkies) as well as attract people (peer pressure and/or observing happy and successful drug users). In this way, less social interaction could lead to both more and less drug use and the question
becomes not how much social interaction there is, but what kind of social interaction we have.

Rachlin is probably right in arguing that my model is more difficult to test than the theory of relative addiction and I do not make any strong claims about the empirical relevance of the model. Moreover, the two contributions are not on the same footing. The model does not claim to represent a general theory of addiction; the theory of relative addiction does (or at least did).

Conclusion

The theory of relative attention draws attention to a phenomenon that is potentially important both to explain and treat addiction: vicious and virtuous self-reinforcing cycles. Surely, the bad consequences of using drugs can sometimes lead you to consume even more — as when the alcoholic gets drunk to forget the misery caused by his or her alcoholism. It is also an important lesson that to find a self-sustaining treatment we should search for activities that induce positive cycles — that is, activities that become more and more enjoyable the more you do them. Although I share this general interest in the mechanisms that create positive and negative self-reinforcing cycles, I have reservations about the more specific details of the mechanisms proposed in the theory of relative addiction, such as the assumption that there is a constant need for something that only can be satisfied by drug use or social interaction.

References
