Long-Run Abstinence After Narcotics Abuse: What Are the Odds?

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We consider the long-run odds that narcotics users remain abstinent after methadone treatment. A flexible split-hazard specification that allows for individual-level differences in both the long-run probability of eventual relapse and the short-run timing of relapse is developed. The model is applied to a comprehensive data set involving individual drug abuse and treatment histories for over 800 addicts. Our findings indicate (1) that the short-run success of methadone programs does not automatically translate into long-run abstinence, which suggests the need for aftercare, (2) the value of preventing a teenager or young adult from initiating, and (3) the possibility of identifying high-risk groups, both in terms of age of first daily use and in terms of ethnicity.
(*Hazard Modeling*; *Survival Analysis*; *Narcotics Abuse*; *Methadone Treatment*; *Public Policy*)

1. Introduction

Narcotics abuse, one of modern society's most persistent and costly problems, can be combatted both on the supply side (e.g., incarceration of drug dealers) and the demand side (e.g., prevention programs and/or treatment of drug addicts). To assess the latter's effectiveness, one should consider not only whether there is a decline in drug use *during* the intervention, but also long *after* treatment has ended. For example, many studies have demonstrated that addicts' narcotics use and crime rates decrease while undergoing methadone maintenance treatment (see, e.g., Anglin and Powers 1991, Hubbard et al. 1989). However, concern has been expressed over the relative costs and benefits of administering methadone, since many addicts have been shown to have difficulty remaining abstinent after treatment discharge (Maddux and Desmond 1992, Weinstein et al. 1993).

Ideally, a substantial fraction of addicts receiving treatment should refrain from ever returning to regular drug use. Estimates of such persistent treatment effects vary widely, however. Based on the two-parameter model of Maltz and McCleary (1977), Lloyd and Joe (1979) estimate the long-run abstinence probability after methadone treatment to be around 27%, while Joe et al. (1982–1983) estimate this value to be around 57%. After an extensive literature survey, Hunt and Bespalec (1974) conclude that most published relapse curves tend to stabilize around 25%. Bailey et al. (1994) classify approximately 20% of their sample as ''winners'' based on their ability to remain abstinent for 36 months, and similar percentages are reported in Hser et al. (1991) for a follow-up period of six years. A somewhat smaller number is given by Caulkins (1995), who assumes that treatment, on average, results in long-run benefits for approximately 13% of the addict population. Still other studies have argued that any treatment effects are just temporary, and that all addicts will eventually go back to narcotics use (Aspler and Harding 1991, Powers and Anglin 1993).

Overall, most previous studies seem to suggest that continuing abstinence after narcotics abuse cannot be ruled out, but that this may not be a realistic goal for some, if not most, addicts. The abstinence percentages

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above are aggregate figures, however, which do not reflect individual differences in the eventual relapse probability. An examination of such differences should allow a better ''matching'' of addicts and both treatment and prevention programs, and thereby improve the overall effectiveness of the intervention (see Hester and Miller (1988) or McClellan et al. (1982) for a discussion on the matching issue).

Survival analysis has often been used to study relapse to narcotics use, or the related topic of permanent versus temporary treatment effectiveness. Many of the earlier estimates, for example, were derived from aggregate empirical survival curves. Apart from concealing individual-level differences, these estimates are highly dependent upon the length of the follow-up period (see, e.g., Hser et al. 1991). More recently, covariates have been included into hazard-rate models to capture the impact of demographic, social and/or treatment characteristics on the timing of relapse (see, e.g., Caplehorn 1994; Hser et al. 1991, 1995). Even though these studies have successfully demonstrated the utility of survival analysis when assessing differential short-term treatment effectiveness, the adopted modelling approach implicitly assumes that *all* individuals will eventually relapse. This assumption, which precludes the detection of any long-run differences among individuals and/or treatment conditions, has been called ''ethically untenable'' (Maltz and McCleary 1977, p. 430), and many have questioned its empirical validity (see, e.g., Hubbard et al. 1989; Hunt and Bespalec 1974).

This paper adopts a split-population hazard approach which relaxes the relapse assumption, and which allows for individual-level differences in both the *long-run* probability of eventual relapse and the *short-term* timing of relapse for those who will eventually do so. As such, our research contributes to a better identification of the most vulnerable segments in the population, which should enable public-policy makers to better target these segments (Kamakura and Novak 1996). From a methodological perspective, our model is similar in spirit to the split-hazard model used by Schmidt and Witte (1989) in their study on criminal recidivism, but extends their specification in a number of ways: the baseline hazard is estimated non-parametrically, the grouped nature of the data is taken into account, and a correction for unobserved heterogeneity in the short-term relapse rates is provided. From a substantive point of view, our study is the first to systematically study both the short-run and long-run dimension of the narcotics-use relapse process using individual-level data. As indicated before, previous hazard-rate studies have focused on estimating short-term differences in the relapse timing, but have overlooked potential long-run differences. Powers et al. (1991) did quantify the short-run and long-run effectiveness of methadone maintenance and legal supervision in a multivariate time-series model, but the aggregate nature of their data precluded the detection of any individual or group differences. Joe et al. (1982–1983) and Lloyd and Joe (1979) used a two-parameter split-population model to estimate the ultimate proportion of ''recidivists'' and the short-term relapse rate. Their model did not incorporate individual-level predictors, however, and was characterized by very stringent assumptions about the nature of the relapse process (e.g., all eventual relapsers were assumed to have the same, constant relapse rate), thereby precluding some of the long-run insights offered by our approach.

The remainder of the paper is organized as follows. In §2, the split-hazard specification is developed, and it is shown how many of the earlier models are nested within our framework. Section 3 describes a longitudinal data set of over 800 narcotic addicts who received treatment at methadone maintenance clinics in California. Empirical results on their short- and long-run relapse probabilities are presented in §4. Finally, §5 concludes with policy implications and areas for future research.

2. Model Development

To examine the variability in the timing of relapse, §2.1. proposes a flexible hazard specification that incorporates both fixed and time-varying predictors, accounts for the grouped nature of the data, estimates the time dependence non-parametrically, and corrects for unobserved heterogeneity. In §2.2, a split-population extension of this model is introduced to explicitly allow for the fact that some individuals may remain abstinent in the long run. The resulting specification quantifies the impact of individual-level predictors on both the long-run probability of eventual relapse and the short-run timing of this relapse.

2.1. Modeling the Timing of Relapse

Following Fisher and Anglin (1987), Hser et al. (1991) and Maddux and Desmond (1992), we use the time when addicts enter their first methadone-treatment episode as the start of their time axis. All individuals in the sample stopped using narcotics regularly shortly after entering treatment, making this an appropriate starting point to measure the ''time until relapse'', where relapse is defined as return to *daily* narcotics use. This is a conservative measure, in that it classifies addicts as recidivists only when they have returned to the drug-use state that led to their original entry into treatment (Joe et al. 1982–1983, p. 372), and *not* when they only used drugs again on a sporadic basis. A similar definition is found in Hser et al. (1995), Joe et al. (1982–1983), Lloyd and Joe (1979) and Maltz and McCleary (1977).

Let T denote the random duration until relapse, with probability density function $f(t)$, cumulative distribution function $F(t)$ and hazard function $\lambda(t)$. To account for the grouped nature of the data (i.e., we know the month of relapse, but not the exact day within that month), monthly grouping intervals $[t_{k-1}, t_k)$, $k = 1, 2, \ldots, M + 1$, $t_0 = 0$ and $t_{M+1} = \infty$ are defined, and relapse in interval $[t_{k-1}, t_k)$ is recorded as duration t_k. Maximum-likelihood estimation is used to derive parameter estimates, and the contribution to the likelihood function of the ith subject differs depending on whether he/she has relapsed to daily narcotics use by the end of the observation period (i.e., the time of the interview). If the subject relapses in the kth interval, the contribution to the likelihood function is given by $S(t_{k-1}) - S(t_k)$, where the survivor function $S(t_k) = 1 - F(t_k)$ denotes the probability of no relapse after t_k months. The difference of survival functions is used rather than the density function to recognize the discrete nature of the data (i.e., we do not know the exact day of relapse). This adjustment is needed since not accounting for the discrete nature of the data has been shown to result in inconsistent parameter estimates (Kiefer 1988). If no relapse has occurred by the time of the interview, the observation is called right-censored, and the contribution to the likelihood function is given by $S(t_k - 1)$. We therefore assume that censoring occurs at the beginning of the duration interval; such an assumption is again needed given the grouped nature of the data. Hence, the contribution to the likelihood function of any individual i with observed duration t_i can be written as

$$L_i(t_i) = [S(t_i - 1) - S(t_i)]^{1-d_i}[S(t_i - 1)]^{d_i}, \qquad (1)$$

where d_i is an indicator variable equal to zero for individuals who have relapsed to daily narcotics use and equal to one for individuals with censored durations.

To incorporate covariates into the model, we first propose an expression for the hazard function, and subsequently use a general relationship between a distribution's hazard and survivor functions. Following Vanhuele et al. (1995), we write the hazard function $\lambda_i(t)$ as

$$\lambda_i(t) = \lambda_0 e^{\vec{\beta}\vec{X}_i(t)} e^{\vec{c}\vec{D}_i(t)}. \qquad (2)$$

λ_0 gives the relapse rate for addicts in the base group in their first month after admission into treatment. The base group consists of those addicts for which all covariates, given by the vector $\vec{X}_i(t)$, equal zero. Covariates may be time-invariant (e.g., ethnicity) or time-varying (e.g., treatment or legal supervision status in a given month). A positive β-coefficient implies that a positive value of the associated covariate augments the conditional probability of relapse. Specifically, when the jth covariate changes by one unit, the hazard changes by [exp $(\beta_j) - 1$)]*100 percent. When dealing with nominal (0-1) variables, this percentage refers to the impact on the hazard rate caused by the presence of the discrete characteristic. To describe how the relapse rate changes over time, a set of time-varying dummy variables $\vec{D}_i(j)$ is added. When a separate variable is used for each month, the one associated with the third month, for example, takes on the values $(0\ 0\ 1\ 0\ 0 \cdots)$. Positive (negative) c-coefficients indicate a higher (lower) relapse rate compared to the first period. This approach makes no distributional assumptions with respect to the nature of the time dependence, and is therefore called nonparametric. The only assumption made is that within a grouping interval (e.g., a month or year), the hazard remains constant. Intuitively, this consists of a piecewise approximation of an underlying, possibly very complex, continuous time-dependence pattern. A similar step-wise approximation can be found in Han and Hausman (1990), Meyer (1990) and Trussell and Richards (1985), among others. It results in consistent parameter estimates even when the true form of the baseline is unknown. In contrast, an incorrect parametric specification has been shown to result in inconsistent

parameter estimates (Meyer 1986, 1990). Given the absence of firm priors on the appropriateness of alternative distributional forms (see, e.g., Mann et al. 1984, Schmidt and Witte 1989), a non-parametric specification is preferred. Moreover, it allows us to easily deal with periods of incarceration: relapse to daily narcotics use cannot occur while the individual is incarcerated, during which his/her hazard is set to zero.

To estimate the parameters of interest (λ_0, $\vec{\beta}$ and $\vec{c}$), an expression for the survival function associated with the hazard function in Equation (2) is needed. When the time-varying covariates are assumed to remain constant within a given month but are allowed to vary from month to month, it can be shown that (Vanhuele et al. 1995)

$$S_i(t_i) = e^{-\lambda_0 B_i(t_i)},$$

$$\text{where } B_i(t_i) = \sum_{j=1}^{t_i} e^{\vec{\beta}\vec{X}_i(j)+\vec{c}\vec{D}_i(j)}. \quad (3)$$

After appropriate substitutions, the log-likelihood function for N subjects becomes:

$$LL = \sum_{i=1}^{N} \{(1 - d_i) \log [e^{-\lambda_0 B_i(t_i-1)} - e^{-\lambda_0 B_i(t_i)}] - d_i \lambda_0 B_i(t_i - 1)\}. \quad (4)$$

Some of the factors that can impact an individual's relapse rate may be hard to quantify or may not have been available in our data set. Not accounting for these omitted factors (often referred to as unobserved heterogeneity) has been shown to cause a spurious negative duration dependence (as reflected in a downward bias in the c-coefficients), and to result in inconsistent β-estimates (Manton et al. 1992). To correct for unobserved heterogeneity, we let λ_0 be distributed according to a gamma mixing distribution with mean r/a and coefficient of variation $r^{-1/2}$ (see Dekimpe and Degraeve 1997; Gupta 1991 or Meyer 1990 for other applications of the gamma mixing distribution). This mixing distribution is quite flexible, and has been shown to result in the following closed-form solution for the log-likelihood function (Dekimpe and Degraeve 1997; Vanhuele et al. 1995):

$$LL = \sum_{i=1}^{N} \ln \left\{ (1 + d_i) \left[\frac{a}{B_i(t_i - 1) + a} \right]^r - \left[\frac{a}{B_i(t_i - 1) + (1 - d_i) e^{\vec{\beta}\vec{X}_i(t_i)+\vec{c}\vec{D}_i(t_i)} + a} \right]^r \right\}. \quad (5)$$

The average first-month relapse rate for addicts in the base group is then given by r/a, and all other coefficients can be interpreted relative to this ratio in the same way as they were interpreted vis-a-vis λ_0 in Equation (4). Jain and Vilcassim (1991) also correct for unobserved heterogeneity, but in a non-parametric way (while they estimated their baseline hazard parametrically). Recent research by Han and Hausman (1990), Manton et al. (1986) and Ridder (1986) has shown that the specification of the unobserved-heterogeneity component is not as crucial as a flexible specification of the baseline hazard. We therefore specify the baseline hazard non-parametrically, while we use a reasonably flexible mixing distribution to correct for unobserved heterogeneity. In §4.4., we will assess the robustness of our substantive long-run findings to these issues.

2.2. Split-population Models

The different hazard models discussed in §2.1. assume that all individuals will eventually relapse. To explicitly account for the possibility of persistent or long-run abstinence, we adopt a split-population specification. We assume that there are two separate populations, those who will never relapse and those who eventually will, and that the hazard-rate model discussed before applies only to the second population. According to Hunt and Bespalec (1974), the typical form of the empirical survival curve for relapse to addiction, i.e., a rapidly declining portion followed by a relatively stable asymptotic part, suggests that one may indeed be dealing with these two classes of addicts.

Following Schmidt and Witte (1989), an indicator variable A_i is defined where A_i is equal to one if subject i belongs to the group who will eventually relapse, and equal to zero otherwise. For subjects who relapsed during the observation period, we *know* $A_i = 1$. For those who did not relapse in that time span, we *do not know* the true A_i value. They may belong to the group of eventual relapses, in which case a return to daily narcotics use might have been observed if the observation period had been longer. However, it is also possible that they belong to the group that will remain abstinent in the long run, in which case no relapse would ever be observed, irrespective of the length of the observation period.

To derive the log-likelihood function, we first consider the simpler case where there is no unobserved

heterogeneity within the group of eventual relapses. Given the definition of A_i, the likelihood of observing a relapse in duration interval t_i is given by:

$$P[d_i = 0; t = t_i] = [S_i(t_i - 1 \mid A_i = 1) - S_i(t_i \mid A_i = 1)] * P(A_i = 1), \quad (6)$$

where $S(\cdot \mid A_i = 1)$ is the survival function for those subjects who will eventually relapse. It is important to note that the distribution of the durations until relapse is defined conditional on $A_i = 1$, and is irrelevant for those for whom $A_i = 0$. The likelihood of observing an individual with a censored duration of t_i, on the other hand, is given by

$$P[d_i = 1; t = t_i] = P(A_i = 0) + [P(A_i = 1) * S_i(t_i - 1 \mid A_i = 1)]. \quad (7)$$

If we define

$$\delta_i = P(A_i = 1) = 1 - P(A_i = 0), \quad (8)$$

the log-likelihood function for N addicts can be written as

$$LL = \sum_{i=1}^{N} \{\delta_i [S_i(t_i - 1 \mid A_i = 1) - S_i(t_i \mid A_i = 1)]\}^{1-d_i} * \{(1 - \delta_i) + \delta_i S_i(t_i - 1 \mid A_i = 1)\}^{d_i}. \quad (9)$$

Using a similar logic as in Equation (3), $S_i(t_i \mid A_i = 1)$ can be shown to equal $\exp[-\lambda_0 B_i(t_i)]$. To allow for the fact that some individuals may be more prone to eventual relapse than others, we model δ_i as a function of individual-level characteristics through the following logistic transformation:

$$\delta_i = \delta_i(\vec{X}_i) = \frac{1}{1 + e^{-\vec{\alpha}\vec{X}_i}}. \quad (10)$$

This formulation allows us to estimate *simultaneously* the impact of explanatory variables on the probability of eventual relapse (through the αs) and on the timing of relapse for those who will eventually do so (through the βs). However, it still assumes that all people who will eventually relapse have the same λ_0. To explicitly allow for unobserved heterogeneity in the relapse rate of the $A_i = 1$ group, we again allow λ_0 to be distributed according to a gamma mixing distribution, which results in the following closed-form solution for the log-likelihood function:

$$LL = \sum_{i=1}^{N} \ln \left\{ \frac{(\delta_i^{1-d_i} - \delta_i)(1 + d_i)a^r}{[(1 - d_i)B_i(t_i - 1) + a]^r} - \frac{(\delta_i^{1-d_i} - \delta_i)a^r}{[(1 - d_i)B_i(t_i) + a]^r} + \frac{\delta_i(1 + d_i)a^r}{[B_i(t_i - 1) + a]^r} - \frac{\delta_i a^r}{[B_i(t_i - 1) + (1 - d_i)e^{\vec{\beta}\vec{X}_i(t_i) + \vec{c}\vec{D}_i(t_i)} + a]^r} \right\}.$$

This model extends Schmidt and Witte's formulation (1989), which was also used in the work of Douglas and Hariharan (1994) on the onset of smoking and Sinha and Chandrashekaran (1992) on the adoption of technological innovations, in that (1) an adjustment is made for the grouped nature of the data, (2) a correction is made for unobserved heterogeneity in the relapse rate for those who will go back to daily drug use, and (3) the nature of the time dependence is modeled non-parametrically. Each of these adjustments is needed in our application to ensure the consistency of the parameter estimates. Moreover, when $r \to \infty$ (in which case the gamma mixing distribution becomes a spike and does not pick up any unobserved heterogeneity), when $\delta = \delta_i$ (all i) and when all c and β coefficients are set to zero, the discrete version of the model in Joe et al. (1982–1983) is obtained. Our model also extends the work of Meeker (1987) and Meeker and LuValle (1995), who imposed the Weibull distribution for the baseline hazard (rather than specifying it non-parametrically) in a homogeneous split-population model (i.e., $\delta = \delta_i$) which did not correct for unobserved heterogeneity in the hazard rates.

The fact that some former addicts will never relapse could also be modeled through the use of a deficient duration distribution, such as a quadratic distribution with a significant and negative quadratic term (Helsen and Schmittlein 1993), or through the addition to the baseline hazard of a term that decreases exponentially with time (Visher et al. 1991, Visher and Linster 1990). These approaches reflect the idea that addicts who have not relapsed after a long period of time have a very small (''near zero'') probability of doing so in the near future. Apart from the fact that the above studies did not correct for unobserved heterogeneity (and therefore may have overestimated the downward slope of the

baseline hazard for long durations; see Lancaster (1990), the use of deficient distributions does not allow for a monotonically increasing base hazard for those addicts who will relapse: the hazard must eventually have a downward slope and become zero for the distribution to be deficient. Moreover, even though the impact of the covariates can vary over time in the Visher et al. (1991) specification, their model (as well as Helsen and Schmittlein's (1993)) does not allow for a different impact of the covariates on, respectively, the probability of eventual relapse and the timing of relapse.

In sum, our specification has a number of appealing properties that were only partially reflected in earlier work.

3. Data

3.1. Sample

The sample consisted of 846 male and female narcotics addicts selected from admissions to methadone-maintenance clinics in California. These subjects were interviewed during follow-up studies conducted between 1978 and 1982. A detailed description of the sampling procedure can be found in Anglin and McGlothlin (1984), and background characteristics of the sample are summarized in Table 1. The ethnic make-up was 554 whites and 292 Chicanos, with 265 females and 581 males. Other variables included family/education background, socio-economic status, and narcotics-use history. For example, the majority started using narcotics between ages 17 and 25 and more than half of them did not finish high school. (See Anglin et al. (1988), Hser et al. (1991) or Hubbard et al. (1989) for reviews of earlier research on the relationship between demographics and drug abuse/relapse.) 34.8% of the sample had not yet relapsed by the time of the interview (censored observations), and the observed duration (i.e., until relapse or until the interview) varied between 2 and 116 months.

Two cohorts could be distinguished in terms of the geographic location of the clinics. Cohort 1 referred to clinics in Central California (Bakersfield and Tulare), whereas cohort 2 included clinics in Southern California (Orange County, Riverside, San Bernardino and San Diego). We are not aware of any systematic differences in program policies across the two cohorts, but their patients differed on several background characteristics.

Table 1 Summary Statistics

A. Censored/Completed		B. Fixed Characteristics	
		Cohort	
Censored	294 (34.8%)	Cohort 1*	400 (47.3%)
Completed	552 (65.2%)	Cohort 2	446 (52.7%)
		Gender	
		Female*	265 (31.3%)
		Male	581 (68.7%)
		Ethnicity	
		White*	554 (65.5%)
		Chicano	292 (34.5%)
		Education (finished high school)	
		No*	507 (59.9%)
		Yes	339 (40.1%)
		Age first daily use	
		≤16 years*	122 (14.4%)
		17–24 years	625 (73.9%)
		≥25 years	99 (11.7%)

C. Time-Varying Descriptors	Full Sample	Completed Obs.
	($N = 846$)	($N = 552$)
% of Observed Duration Under Methadone Maintenance		
Mean	76.3	80.4
Minimum	1.0	3.0
Maximum	100.0	99.0
% of Observed Duration Under Legal Supervision		
Mean	53.6	48.8
Minimum	0.0 [$N = 226$]	0.0 [$N = 191$]
Maximum	100.0	99.0
% of Observed Duration Incarcerated		
Mean	5.3	5.3
Minimum	0.0 [$N = 663$]	0.0 [$N = 451$]
Maximum	85.0	85.0

* Defines the base group.

Some of these characteristics, such as age of first daily use, ethnicity, gender and education were controlled for in the model, while a dummy variable was used in both the hazard and logit part of model (11) to capture the potentially confounding impact of other cohort differences such as the proportion with physical disabilities, age at admission, age of first legal supervision and

whether their mother had any alcohol problems. A dummy control variable was used to limit the number of parameters to be estimated, to avoid collinearity problems, and to maintain a large enough sample size (the values of these characteristics were missing for a number of observations). In unreported analyses, we also controlled for the subjects' socio-economic status level (44.4% poor) and occupation (67.8% semi- or unskilled), whether they had been involved in gang-related activities (28.6% yes) and whether they had been raised by both parents (38.7% yes). These variables were not included in the subsequent models, since (1) they were less likely to be time-invariant over the entire spell, and (2) they had an insignificant effect on both the relapse timing and the probability of eventual relapse. Finally, the cohort dummy variable was added to correct for the potentially confounding effect of the closure of a clinic in the Bakersfield area. (See McGlothlin and Anglin (1981) for details.) This forced termination could have had a negative impact on the relapse behavior of the affected patients (Grella et al. 1994). Unfortunately, we did not have individual-level information on the cause of discharge for all subjects. (Moreover, some individuals in the other cohort may also have been forced to stop the treatment.) Instead, we added a proxy (the cohort dummy variable) to the specification, and corrected for further unobserved heterogeneity through the gamma mixing distribution.

Subjects' time in treatment varied considerably (see Table 1), so a 0-1 time-varying dummy variable was created to indicate each subject's treatment status in any given month. Because of the time-varying nature of that variable, it could only be included into the hazard part of the model (Gupta 1991), and we used the treatment status at the end of month $t - 1$ (or similarly, at the beginning of month t) as a predictor variable for the conditional relapse probability *during* month t. Based on previous research findings (Anglin and Powers 1991, Hser et al. 1995) a significant negative effect was expected.

Information was also available on another intervention variable, legal supervision, for which a similar time-varying dummy variable was included in the hazard part of the model. On average, subjects were under legal supervision for 54% of their observed duration, but 226 were not under any legal supervision during this time period. Unlike methadone maintenance, however, the evaluation outcomes of previous studies on the effectiveness of criminal justice system interventions are less conclusive. (See Anglin and Powers (1991) for a recent review.)

Finally, information was available indicating when certain addicts were incarcerated. As indicated in Table 1, more than 75% (663/846) of the sample was not incarcerated during our observation period. Since individuals cannot relapse to daily narcotics use while they are incarcerated, their hazard was restricted to zero during these months.

3.2. Interview Procedure

The interview procedure was adapted in part from one developed by Nurco et al. (1975), and has been described in detail in another paper (McGlothlin et al. 1977). Briefly, a schematic time chart was prepared before the interview, showing all official records of arrests, intervals of incarceration, legal status, and treatment. The interviewer established the date of first narcotics use on the time chart, then augmented the time chart with respondents' self-report of other important life events (e.g., childbirths, moves, or employment) suitable to assist in recall. Starting from the time of first narcotics use, the interviewer recorded all time points when narcotics use changed from less than daily use to daily use (or vice versa), or when the respondent's legal supervision or treatment status changed. These time points were used to divide the respondent's addiction history into several intervals where his/her frequency of narcotics use, his/her legal status, and drug-treatment enrollment did not change. Self-reported data were then collected for each of these intervals on narcotics, alcohol and other drug use, drug dealing, criminal behavior, and certain other variables. In this way, the entire addiction history was recorded, from the time of first narcotics use to the time of interview.

4. Empirical Findings

Maximum-likelihood estimates for the split-population relapse model are given in Table 2. All significance levels were determined through likelihood-ratio tests (asymptotic standard errors using normal distribution theory are available from the authors upon request).

Table 2 Parameter Estimates for the Heterogenous Split-Hazard Model

Hazard Part (r/a, $\vec{\beta}$ and $\vec{c}$): Equation for Relapse Timing, Given Eventual Relapse			Logit Part ($\vec{\alpha}$): Equation for P(Eventual Relapse)		
Baseline Hazard			Intercept	0.955[a]	(6.42)
r/a (1–6 months)	0.081		Demographic Characteristics		
c_{7-12} (7–12 months)	1.174[a]	(48.71)	Cohort	1.207[a]	(25.20)
c_{13-24} (13–24 months)	1.619[a]	(60.27)	Gender	0.057	(0.04)
c_{25-42} (25–42 months)	2.056[a]	(44.95)	Ethnicity	0.486[b]	(3.77)
c_{43-48} (43–48 months)	3.097[a]	(47.15)	Education	0.023	(0.00)
c_{49-60} (49–60 months)	3.583[a]	(47.90)	Age First Daily Use		
c_{61-84} (61–84 months)	4.296[a]	(33.06)	17–24 years	−0.616[a]	(3.79)
$c_{85-\ldots}$ (≥85 months)	6.213[a]	(30.13)	≥25 years	−0.749[c]	(2.34)
Demographic Characteristics					
Cohort	−0.357[b]	(3.01)			
Gender	−0.338[c]	(2.35)			
Ethnicity	−0.045	(0.04)			
Education	0.031	(0.02)			
Age First Daily Use					
17–24 years	−0.314	(1.59)			
≥25 years	−0.840[a]	(4.94)			
Policy Control Variables					
Methadone Maintenance	−0.921[a]	(38.61)			
Legal Supervision	0.207	(0.207)			

Significance levels are based on likelihood-ratio test with $\chi^2(1)$ values reported between parentheses.

[a] $p < 0.05$.

[b] $p < 0.10$.

[c] $p < 0.15$.

4.1. Estimation of the Baseline Hazard

When applying a piece-wise approximation to an underlying continuous baseline hazard, it is important to determine *the number* and *the location* of the discrete shifts. Working with monthly durations, it clearly did not make sense to allow for a different c-parameter in every month. Instead, the following procedure was adopted. Initially, a model was estimated in which we allowed for a discrete shift after every year, i.e., we imposed the restriction that within a given year the hazard rate remained constant. In a second step, we assessed the validity of this assumption by allowing for a discrete shift in the middle of a given year. In only two cases (for year one and year four) did a likelihood-ratio test reject the more restricted model ($p < 0.1$). We did not divide these intervals any further to ensure a sufficient number of events per interval to reliably estimate the associated c-parameter. In a third and final step, likelihood-ratio tests were used to assess whether the model with these two additional shifts was overparameterized, i.e., whether two adjacent intervals could be combined without a significant loss in fit. This was the case in three instances: no discrete shift was needed at the end of year three, at the end of year six, and from year seven onwards. The location of the discrete shifts in the final model therefore resulted in the estimation of seven c-parameters (c_{7-12}, c_{13-24}, c_{25-42}, c_{43-48}, c_{49-60}, c_{61-84} and $c_{85-\ldots}$). It should be emphasized that the parameter estimates for the demographic and drug-intervention variables in both the hazard and logit part were robust to changes in the location and the number of discrete shifts (detailed results are available from the authors upon request). The slope of the baseline hazard, however, became much steeper when more shifts were allowed for. This confirms the Vanhuele et al. (1995) finding that the more unobserved heterogeneity one picks up with the model, as is now done by the additional c-parameters, the steeper the baseline hazard becomes.

From a substantive point of view, the overall slope of the baseline hazard was increasing, which suggests a need for aftercare. When comparing the baseline hazard of the split-population model with the one for the regular hazard model (Equation 5), the latter was consistently lower. This is not surprising as it describes the average quitting rate for the entire population, whereas the estimates in Table 2 only apply to the subgroup that will eventually relapse.

4.2. Short-run Dynamics

The parameters in the hazard part ($\vec{\beta}$) reflect differences in the relapse timing for those individuals who would have eventually relapsed. On average, individuals in the second cohort relapsed later than those admitted to a methadone-maintenance program in the first cohort. In terms of the demographic variables, no significant differences were found for ethnicity and education. Our results do suggest, however, a significant difference for the gender ($p < 0.15$) and age ($p < 0.05$) variables. With respect to the latter variable, addicts who first used narcotics on a daily basis at age 25 or older had a 57 percent [(exp $(-0.84) - 1)*100$] lower conditional relapse probability in any given month than addicts who started at a younger age. *From a policy point of view, these findings clearly indicate the value of preventing teenagers or young adults from initiating*.

As expected, the methadone status has a significant and substantial impact on the hazard rate in a given month: individuals who were still enrolled at the end of month $t - 1$ had a 60 percent lower relapse rate during month t than those who were no longer enrolled. Their legal-supervision status, however, does not have a significant effect, which complements previous studies that questioned the effectiveness of legal supervision as a deterrent of future relapses (see, e.g., Anglin and Powers 1991).

Some of these findings are presented graphically in Figure 1, which depicts the hazard (relapse) rates for four categories of addicts during their first five years after entering into treatment. Group A consists of addicts from the Southern California cohort, who started their addiction history when they were more than 25 years old, and who were kept under methadone maintenance (for all other covariates, the value of the base group was assumed, and they were never incarcerated nor under any legal supervision). In the three other groups, we varied, respectively, the cohort (group B), the age of first daily use (less than 16 years in group C) and the methadone-treatment history (only during the first year for addicts in group D). It should be emphasized once more that these survival curves only apply to those individuals who belong to the sub-group of addicts that will eventually relapse.

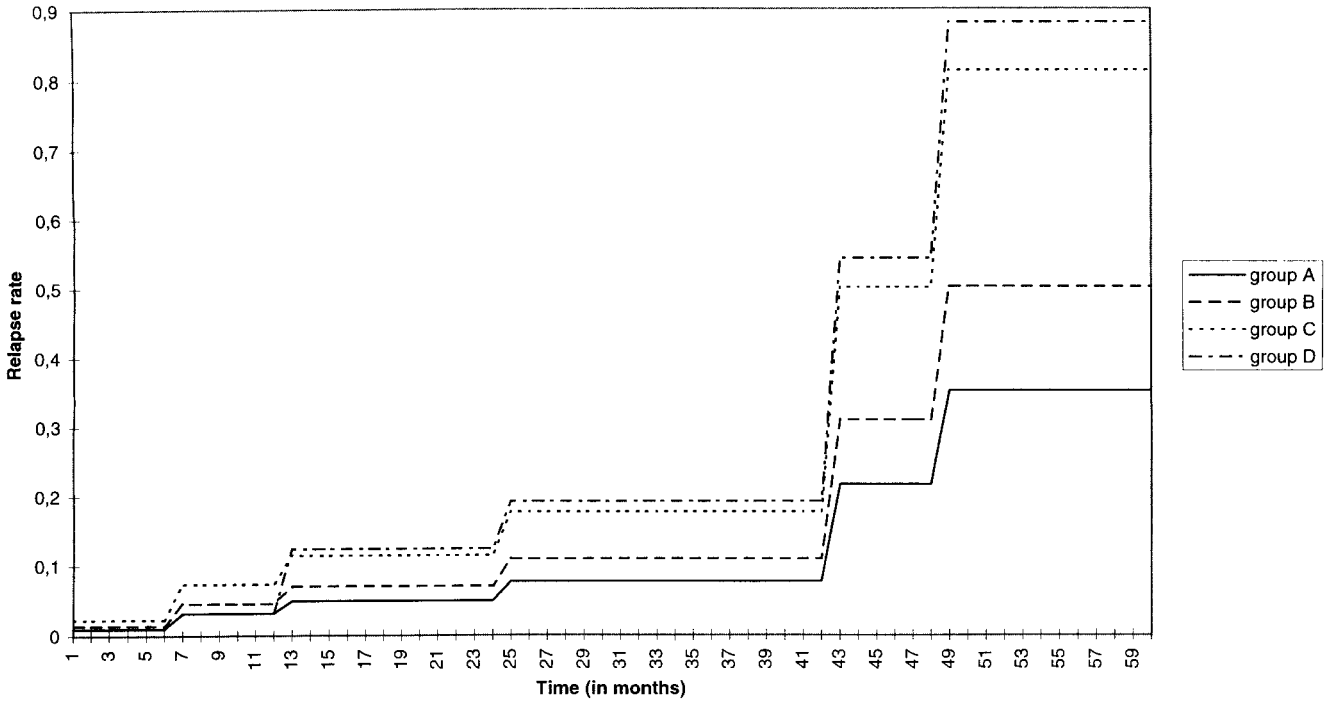
4.3. The Long-Run Relapse Probability

Comparing the log-likelihood values for the split-hazard model ($LL = -2{,}641.99$) and the regular hazard model ($LL = -2{,}659.03$), the latter's assumption that eventually all subjects would relapse is firmly rejected [$\chi^2(7) = 34.08$; $p < 0.001$]. The logit coefficients ($\vec{\alpha}$) also indicate the importance of allowing for individual differences in the long-run relapse probability. A cohort effect is again present, but in an opposite direction: a smaller fraction of the patients admitted to a program in Central California eventually relapse, even though those who relapse will, on average, do so more quickly than those in Southern California (see §4.2). It is interesting to note that the closure of a clinic in Bakersfield (see before) is unlikely to have affected the *direction* of our long-run findings for the cohort variable. If this event indeed had an impact on the post-closure performance, the difference in long-run relapse probabilities between the two cohorts might have been even bigger.

As for the demographic variables, we again found no significant differences for gender and education, but *the odds of remaining abstinent in the long run are less favorable for Chicanos than for whites*. This confirms earlier studies that minority groups face certain problems unique to their socio-cultural histories (Anglin et al. 1988, Gomez and Vega 1981), making their persistent abstinence less likely.

The findings for the age variable indicate that daily drug abuse at a young age seriously jeopardizes one's chances of remaining abstinent in the future. *The age variable therefore has a similar impact on both the long-run probability of eventually relapsing and the short-run timing of that relapse*. Combined with earlier findings that the age of first drug use is a good predictor of subsequent drug-related problems and high-risk sexual behavior (Battjes et al. 1992, Holland and Griffin 1984, Robins and Przybeck 1985), these results underline the value of preventing (or at least delaying the onset of) drug use by youngsters.

Figure 1 **Relapse Rates for Selected Groups (For Subjects Belonging to the Subpopulation of Eventual Relapsers)**



Using the logit parameters from Table 2, we present in Table 3 the long-run abstinence probability for selected groups. These estimates illustrate that the long-run relapse probability is considerably below one for most groups, and varies greatly across groups. Chicanos starting regular drug use at an early age appear to have the least favorable odds. For addicts in Central California, for example, the long-run abstinence probability for this high-risk group is almost 23 percentage points (i.e., 41.6%–19.1%) lower than for whites who started using drugs on a daily basis at a later age! Harrison and Kennedy (1994) found a comparatively higher prevalence rate among this population segment in the United States-Mexico border area. Our results indicate that they also had a smaller probability of remaining abstinent after treatment. Hence, preventing Mexican-American youths from initiating may do more to reduce drug use in the long run than preventing otherwise comparable white youths from initiating, because if they become addicted, the former are more likely to relapse.

Table 3 **Long-run Abstinence Probability for Selected Groups**

Group Description	Long-Run Abstinence Probability
Age First Daily Use $\leq$16	
White—Cohort 1	0.278
White—Cohort 2	0.103
Chicano—Cohort 1	0.191
Chicano—Cohort 2	0.066
Age First Daily Use 17–24	
White—Cohort 1	0.416
White—Cohort 2	0.176
Chicano—Cohort 1	0.305
Chicano—Cohort 2	0.116
Age First Daily Use $\geq$25	
White—Cohort 1	0.449
White—Cohort 2	0.196
Chicano—Cohort 1	0.334
Chicano—Cohort 2	0.130

4.4. Validation

At this point, one may wonder to what extent these long-run findings are pre-determined through functional form and/or distributional assumptions such as the adopted non-parametric form for the baseline hazard, or the choice of the gamma mixing distribution to correct for unobserved heterogeneity. To assess the robustness of our inferences, we computed the long-run

abstinence probability for a number of competing model specifications. Two extreme cases were considered: (1) a model with no time dependence (the exponential case) which clearly cannot capture an upward slope in the baseline hazard, and (2) a model with no correction for unobserved heterogeneity. Alternative specifications for the mixing distribution could also have been considered. However, the model without correction for unobserved heterogeneity provides a stringent test on the robustness of our findings, since it has been shown in the context of regular hazard models that the presence/absence of some correction for unobserved heterogeneity is much more crucial than the actual way in which this is implemented (see, e.g., Manton et al. 1986, Ridder 1986).

The results reported in Table 4 indicate that even though both models resulted in a considerably lower overall fit [$LL_{\text{exponential}} = -2{,}680.38$; $LL_{\text{no-gamma}} = -2{,}659.39$], the long-run inferences appeared to be robust, with a Mean Absolute Deviation (MAD) relative to the full model of less than 2%.

We also assessed the sensitivity of our long-run inferences to the length of the observation period (see Table 4, last column). Assuming that the interview date for every individual had occurred 12 months earlier, we recomputed, whenever needed, the duration and the censoring dummy variable. The estimates for the long-run relapse probability are again robust, with a MAD relative to the original estimates of about 6%.

5. Conclusions

Our paper raised the question of the long-run odds that a narcotics user abstains from daily drug use after methadone treatment. We used split-hazard methods that distinguish between short-run and long-run abstinence, and that allow for demographic and other covariates to influence each. We applied this method on a comprehensive data set involving individual drug-abuse and treatment histories for over 800 individuals. Among the many empirical results, we highlight three major findings and their implications for public policy, and we conclude with some areas for future research.

Our first important finding is that the base relapse rate among drug addicts increases over time. This suggests that there is a need for a periodic monitoring of a former drug user's abstinence, even long after treatment has ended.

Table 4 Robustness of the Long-Run Relapse Estimates

		Sensitivity to Distributional Assumptions		Sensitivity to the Length of the Observation Period: New Interview Date = Old Date—12 Months
	Full Model	Exponential Baseline	No Gamma Heterogeneity	
≤16/White/Cohort 1	0.722	0.707	0.697	0.738
≤16/White/Cohort 2	0.897	0.905	0.878	0.961
≤16/Chicano/Cohort 1	0.809	0.797	0.792	0.788
≤16/Chicano/Cohort 2	0.934	0.939	0.923	0.970
17–24/White/Cohort 1	0.584	0.550	0.550	0.628
17–24/White/Cohort 2	0.824	0.828	0.793	0.936
17–24/Chicano/Cohort 1	0.695	0.665	0.670	0.690
17–24/Chicano/Cohort 2	0.884	0.887	0.864	0.951
≥25/White/Cohort 1	0.551	0.570	0.557	0.663
≥25/White/Cohort 2	0.804	0.839	0.798	0.945
≥25/Chicano/Cohort 1	0.666	0.683	0.676	0.723
≥25/Chicano/Cohort 2	0.870	0.894	0.867	0.958
MAD*	–	0.017	0.017	0.064

* MAD refers to the Mean Absolute Deviation (across the twelve considered groups) between the long-run relapse probability estimated from the full model in column 2 and the estimates in the other columns.

Our second major finding is that the long-run abstinence probability depends on the age of the earliest incidence: early-age daily narcotics users are less likely to permanently recover from their drug habit than later-age first users. Established addictive behavior is hard to change in general, but our results indicate that this behavior is even more difficult to abandon when it has become entrenched at an early age. This clearly demonstrates the value of programs that are successful in preventing (or delaying) youngsters from initiating drug use. Indeed, such programs, if effective, result in ''hidden'' long-term beneficial effects in that the drug use they prevent in the short run would have been more difficult to overcome at a later age. (See also Battjes et al. (1992) for a more elaborate discussion of these hidden benefits in the context of HIV prevention efforts, and Douglas and Hariharan (1994) for an application to smoking prevention.) The observed differences are substantial. For example, first drug use at age 25 or older results in a long-run relapse probability that is almost half that of first drug use among teenagers.

Third, drug relapse odds are significantly different across socio-demographic groups, making it possible to define at-risk populations and targeting scarce prevention resources to them. We have found these differences to exist both in the short run and in the long run, though our data did not always identify important demographic drivers. The key public policy implication is that effective prevention programs should be targeted to specific socio-demographic segments, as opposed to the population at large. The targeting of narcotics treatment to specific population segments, especially youths, raises important ethical and legal questions about the use of public funds to improve the lives of all people in need, versus only a subset. While answering these questions is beyond the scope of our research, we observe the analogy with prioritization in medical treatments, which is often based on a metric of saving (quality-adjusted) life *years*, as opposed to saving lives (see, e.g., Carr-Hill 1991, Johannesson and Gerdtham 1996, Nordenfelt 1993). In our case, an effective prevention and treatment of young addicts creates more additional productive life years than the treatment of addicts in general. Ultimately, the question is one of efficient allocation of scarce public resources, and we hope that our empirical research has shed light on at least one major aspect of the question, i.e., the efforts' long-run abstinence implications.

Even though our findings have some clear cost-benefit implications, they do not offer a full answer to this complex issue. Indeed, a complete cost-benefit analysis would require much more detailed information on both the cost side (not only in terms of the actual costs of providing methadone, but also in terms of the other costs often encountered by society after relapse) and the performance side (where one should have more detailed information on the different segments' responsiveness across a broad range of dimensions, such as relapse, employment and relational stability). While the actual implementation of such a cost-benefit analysis involves a lot of subjective judgments which are beyond the scope of the current research (see, e.g., French (1995) or Zarkin et al. (1994) for a recent conceptual discussion), a first step in such a study is to understand the actual short-run and long-run *relapse* patterns of addicts. This is where our research has provided a number of new insights. Indeed, while a number of authors (Anglin and Fisher 1987; Poikolainen 1983) have made the case for considering multiple outcome measures (relapse, criminal behavior, employment status, . . .), relapse control remains the primary treatment-evaluation criterion, and has been shown to highly correlate with those other performance measures (see, e.g., Powers 1990, Speckart and Anglin 1986).

Our paper has adopted a methodology which removes many of the limitations inherent in previous narcotics-treatment effectiveness research. For example, we were able to relax the restrictive assumption that all drug users eventually relapse, and found several insightful results. However, a few limitations remain, which should be addressed in future research. First, it is important to note that when using an observational design, one cannot completely preclude the possibility of spurious correlations due to omitted variables. Second, the adopted proportional-hazard specification assumed that the effect of the included demographic covariates remains constant over the considered abstinence spell. For example, no education differences were found to exist in the short-run relapse probabilities, but one could conceive the existence of certain time intervals (e.g., the first few

months after entering into treatment) where the level of education has a differential impact. A varying-parameter hazard model as discussed in Sharma (1993) or Visher et al. (1991) should be a useful extension in this respect. In a similar vein, we did not consider how different addicts could react differently to (successive) methadone-treatment spells. One (average) methadone-response parameter was estimated, and we did not consider variations over time and/or across population segments. The number of a priori, higher-order interaction effects one could potentially consider quickly becomes excessive, which makes a latent-class segmentation approach a useful alternative. (See Wedel and DeSarbo 1995 for a review.) The split-population approach advocated in our paper involves a two-group latent segmentation in terms of the probability of belonging to, respectively, the group that will eventually relapse to daily narcotics use and the group that will remain abstinent in the long run. A fruitful area for future research would be to extend the current split-hazard segmentation on the long-run relapse probability with a latent-class segmentation on the short-run responsiveness to successive episodes of methadone treatment and/or legal supervision. Finally, it is also important to replicate our findings on a geographically different sample (e.g., East Coast or non-US), to incorporate other important policy-criterion variables such as the reduction of drug-related property crime, and to validate our correlational findings in an experimental design. We hope that this and future research will contribute to the successful execution of effective narcotics prevention and treatment programs.[1]

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