Published in final edited form as:
Health Econ. 2008 August ; 17(8): 927–946. doi:10.1002/hec.1305.

SOCIAL COSTS OF ROBBERY AND THE COST-EFFECTIVENESS OF SUBSTANCE ABUSE TREATMENT

Anirban Basu[a], **A. David Paltiel**[b], and **Harold A. Pollack**[c,*]

[a]Section of General Internal Medicine, Department of Medicine, Center for Health and the Social Sciences, University of Chicago, USA

[b]Department of Epidemiology and Public Health, Yale University School of Medicine, USA

[c]School of Social Service Administration, Center for Health Administration Studies, University of Chicago, USA

SUMMARY

Reduced crime provides a key benefit associated with substance abuse treatment (SAT). Armed robbery is an especially costly and frequent crime committed by some drug-involved offenders. Many studies employ valuation methods that understate the true costs of robbery, and thus the true social benefits of SAT-related robbery reduction. At the same time, regression to the mean and self-report bias may lead pre–post comparisons to overstate crime reductions associated with SAT.

Using 1992–1997 data from the National Treatment Improvement Evaluation Study (NTIES), we examined pre–post differences in self-reported robbery among clients in five residential and outpatient SAT modalities. Fixed-effect negative binomial regression was used to examine incidence rate reductions (IRR) in armed robbery. Published data on willingness to pay to avoid robbery were used to determine the social valuation of these effects. Differences in IRR across SAT modalities were explored to bound potential biases.

All SAT modalities were associated with large and statistically significant reductions in robbery. The average number of self-reported robberies declined from 0.83/client/year pre-entry to 0.12/client/year following SAT ($p < 0.001$). Under worst-case assumptions, monetized valuations of reductions in armed robbery associated with outpatient methadone and residential SAT exceeded economic costs of these interventions. Conventional wisdom posits the economic benefits of SAT. We find that SAT is even more beneficial than is commonly assumed.

Keywords

drug treatment; robbery; contingent valuation; cost-benefit; substance abuse

SOCIAL COSTS OF ROBBERY AND THE COST-EFFECTIVENESS OF SUBSTANCE ABUSE TREATMENT

Substance abuse treatment (SAT) is critical to the well-being and social performance of individuals with substance use disorders. SAT has been linked with reduced crime, improved health, and increased employment (McGlothlin and Anglin, 1981; Gerstein and Harwood, 1990; McLellan *et al*., 1997a,b; Harwood *et al*., 1999). SAT is also credited with

*Correspondence to: School of Social Service Administration, Center for Health Administration Studies, University of Chicago, 969 East 60th Street, Chicago, IL 60637, USA. haroldp@uchicago.edu.

reducing the spread of infectious diseases, such as HIV (Metzger *et al.*, 1993; IOM, 2000). Several recent syntheses summarize the literature on the clinical benefits and economic value of SAT (Cartwright, 1998, 2000; Shepard *et al.*, 1999; Sindelar and Fiellin, 2001; French *et al.*, 2002). Most research finds that SAT is cost-effective and perhaps cost-saving (Caulkins, 1997).

Crime reduction accounts for much of the economic benefit of SAT (French *et al.*, 2002; McCollister and French, 2003; Dismuke *et al.*, 2004; Sindelar *et al.*, 2004). In one prominent analysis of cocaine-dependent clients, Flynn *et al.* examined clients' self-reported crime before and after treatment, finding that the economic value of SAT-related crime reductions far exceeded the associated treatment costs (Flynn *et al.*, 1999). Researchers are not the only observers to highlight this aspect of SAT. Histories such as Michael Massing's The *Fix* indicate that crime control provided some political impetus to the emergence of methadone maintenance, nearly four decades ago (Massing, 1998).

Such findings are doubly striking because common study methodologies easily understate the true social benefits associated with reduced crime. Most studies consider the tangible costs of crime – its direct costs to victims, to the health care and criminal justice systems, and perhaps to the perpetrators – as the empirical foundation for estimating the economic losses that criminal offending imposes on American society. The tangible cost approach provides a valuable lower bound to the benefits of crime reduction. However, tangible costs are a small fraction of the overall social costs of crime (Rajkumar and French, 1997; Cohen *et al.*, 2004). Flynn *et al.* (1999) cite tangible costs of $1304 per burglary. By contrast, Cohen *et al.* obtain estimates of $31 000 to avert the same crime using willingness-to-pay (WTP) valuation methodologies that capture a broader range of crime consequences and societal preferences.

The main contribution of this paper is to explore the economic impact of SAT on one category of crime: armed robbery. We refine existing estimates of the impact and cost-effectiveness of SAT, with a particular focus on this category of crime.

Armed robbery is an especially costly and frequent crime committed by an important, albeit small minority of drug users. The impact of SAT on armed robbery is thus especially important for economic policy analysis of treatment interventions. Along with burglary and serious assault, armed robbery imposes especially large social costs. Cohen *et al.* report that survey respondents are willing to pay more than $200 000 to avert such a crime.

The high price of heroin and cocaine provides a key motive for armed robbery, particularly when substance abuse and dependence erode possibilities for legitimate earnings (Johnson *et al.*, 1985; Allen, 2005). Substance abuse and dependence may also promote violence when robberies are committed under the influence of intoxicating substances (Kleiman, 1993). Finally, offender data indicate that people who commit one armed robbery are likely to commit many others (Blumstein and Cohen, 1987). Each offense imposes high social costs, though ironically the profits associated with the typical robbery are comparatively small. A survey of criminally active street heroin users revealed that the mean income accruing to drug users per reported robbery was less than $80 (Johnson *et al.*, 1985). This wedge between private gain and imposed social costs makes armed robbery especially costly from a social perspective.

Using a negative binomial count data regression framework, we examine pre–post differences in armed robbery incidence using self-reported data among clients in five residential and outpatient SAT program modalities from the 1992–1997 National Treatment Improvement Evaluation Study (NTIES). Examining data from both residential and outpatient care modalities, we examine reductions in offending associated with treatment interventions. We link these results to published contingent valuation (CV) studies of citizens' WTP to avoid the marginal armed robbery in their communities. We then perform threshold analysis to examine the sensitivity of our findings to WTP estimates drawn from the research literature.

Structure of the paper

In Data and Methodology section, we describe our 1992–1997 NTIES data, which provides key data regarding both robbery offending and individuals' characteristics at entry into SAT.

We then describe our negative binomial regression methodology. This section discusses how we address several potential threats, such as regression to the mean and self-report bias, to causal inference.

The subsequent section presents results from our econometric model. Linking our statistical results to published estimates of the economic costs of armed robbery, this section presents several threshold analyses to elucidate the economic import of our findings.

The last section concludes.

DATA AND METHODOLOGY

Economic policy evaluation of SAT requires information regarding both costs and outcomes. No one data source simultaneously provides comprehensive information regarding these two domains. We directly explore outcomes – changes in individual robbery offending – using individual-level NTIES data. We rely upon cost data from previous published studies which focus on cost concerns.

Client outcome data

Our individual-level data come from the 1992–1997 NTIES. NTIES provides administrative (in addition to client-provided) data for critical variables. NTIES also features a large sample size of SAT clients across five principal modalities: inpatient short-term ($N = 261$), residential short-term ($N = 1655$), residential long-term ($N = 1980$), outpatient methadone ($N = 514$), and ambulatory outpatient ($N = 2175$) treatment.[1] NTIES has a higher follow-up response rate (82%) than any comparable client-level follow-up treatment survey (Gerstein and Johnson, 2000, 2001; Flynn *et al.*, 2001). For further information on the NTIES data, see Gerstein *et al.* (1997).

NTIES was not designed to be nationally representative of treatment clients. It does not address individuals out of contact with the SAT system. The sample universe is drawn from units supported by the Center for Substance Abuse Treatment. Compared with nationally representative client surveys, NTIES included a high percentage of non-whites and criminal justice clients. It is therefore well suited to analyses of a criminally active client population (Zarkin *et al.*, 2002).

NTIES is especially useful because its baseline and post-SAT measures explore whether respondents have committed armed robbery and other offenses. If respondents report committing such a crime, NTIES explores how many robberies the individual committed. As with most SAT-related studies, crime data are self-reported. Moreover, the number of robberies is reported categorically and is top-coded at 100 per year.

[1]We dropped observations from one long-term treatment modality, which included only eight respondents.

Treatment costs

Our NTIES data also contain information regarding treatment costs. However, we draw upon other published data that are widely cited in the treatment literature to explore average costs of SAT interventions.

Studies based on the Drug Abuse Treatment Cost Analysis Program (DATCAP) methodology provide the standard benchmark for treatment cost. DATCAP-based research allows us to explore cost-effectiveness issues regarding services whose costs we cannot directly scrutinize in our specific individual-level data (French *et al.*, 1997, 2002; French and McGeary, 1997; Sindelar and Fiellin, 2001; Sindelar *et al.*, 2004).

Roebuck (2003) examines costs of inpatient and outpatient treatment interventions using this approach. Reported costs followed a skewed distribution for all the treatment modalities examined. These skewed distributions reflected wide variation in the *intensity* of provided services (such as the frequency of individual or group counseling sessions per treatment episode). These figures also reflect wide variations in the *scope* of provided services (including whether an SAT facility provides employment, legal, or family counseling, physical and mental health assessments, or HIV counseling and testing, services) (Friedmann *et al.*, 1999). These costs are similar to those obtained in analyses of the National Drug Abuse Treatment System Survey (Wheeler *et al.*, 1992).

Despite this variation, costs per treatment episode are typically below $10 000. In outpatient SAT, mean costs per treatment episode were below $7500 for all but one modality examined, including $7358 per episode in methadone maintenance, $1944 in ‘standard outpatient’ modalities, $4445 in ‘intensive outpatient’ and $3463 in drug court interventions. Inpatient costs were higher and more varied, with a mean cost of $9426 for adult residential programs. Therapeutic communities appeared most costly, with a mean cost of $18 802 per episode that far exceeded those reported in other modalities (Roebuck *et al.*, 2003; Tables I and II).

Econometric methodology

Our analytic approach reflects both the strengths and the weaknesses of our NTIES data. Armed robbery poses three basic challenges that must be addressed within our econometric framework.

The first and most basic challenge is posed by client heterogeneity. The propensity to commit robbery is influenced by many characteristics of specific individuals – characteristics that may be correlated with clients’ assignment to specific modalities of SAT care. Some of these characteristics, such as age and gender, are observable to the researcher. Other characteristics remain unobserved.

The skewed distribution of armed robberies poses a second challenge. Only 6.4% of SAT clients report that they committed at least one robbery in the year prior to treatment entry, with a positively skewed distribution of offenses among those reporting at least one such crime.

The nature of armed robbery data poses a third challenge. The number of armed robberies is a non-negative integer, thus requiring a count data specification.[2] More precisely, NTIES reports the number of robberies as a categorical variable rather than as an absolute count. In the present work we assume that the underlying absolute number of robberies is generated

[2]An alternative method would have been to use an ordered choice model. The need to account for subject-level fixed effects, as described below, makes this approach infeasible (Wooldridge, 2002).

from a count distribution. We then impute the mid-point for each category range to represent the average number of robberies for that subject at that time point.

Our econometric framework addresses each of these challenges. We use a negative binomial regression specification appropriate to our available count data. NTIES includes pre-treatment and post-treatment data on robbery offending, our key outcome of interest. This NTIES design enables us to use a fixed-effect negative binomial (FENB) specification to address the important role of unobserved client characteristics (Hausman *et al.*, 1984; Cameron and Trivedi, 1998; Pollack *et al.*, 2002). Because NTIES includes self-reported robberies committed before and after treatment entry, in effect we use individuals as ‘own controls’ in examining pre–post differences in robbery offending. We then compare these pre–post differences across different treatment modalities to address potential biases in our analytic framework.

The negative binomial specification accounts for over-dispersion in the number of reported robberies. Moreover, the conditional likelihood for this model does not include idiosyncratic fixed-effect parameters. We therefore sidestep ‘incidental parameter’ problems that can arise due to the presence of many nuisance parameters (Neyman and Scott, 1948; Chamberlain, 1980).

In the FENB model, the over-dispersion (shape) parameter is modeled as a function of covariates; while the individual-level fixed effect is modeled through the scale parameter and is assumed to be fixed across time for a specific individual. Specifically, let

$$E(Y_{it} \mid X_i, Z_{it}) = \theta_i \cdot \lambda_{it}$$

and

$$\mathrm{Var}(Y_{it} \mid X_i, Z_{it}) = (1+\theta_i) \cdot E(Y_{it} \mid X_i, Z_{it})$$

Here θ_i represents the scale parameter representing the subject-specific latent characteristics. λ_{it} is the shape parameter. Y_{it} represents the robbery count for each patient, pre- and post-treatment. X_i represents time-invariant covariates; while Z_{it} represent time-variant covariates.

The mean model can be expressed as follows:

$$\ln\{E(Y_{it} \mid X_i, Z_{it})\} = \ln(\theta_i) + \ln(\lambda_{\mathrm{it}}) = \ln(\theta_i) + [\alpha + \beta^{\mathrm{T}} \cdot X_i + \gamma^{\mathrm{T}} \cdot Z_{it}] \quad (1)$$

The conditional likelihood for this model lets the θ_i parameter drop out, thereby overcoming the incidental parameter problems with fixed effects. An implication of this approach is that individuals who commit zero robberies at both baseline and the follow-up period do not contribute to the estimation.

The λ_{it} parameter is not eliminated in the conditional likelihood. Therefore, besides the coefficients on time-varying covariates (γ), and unlike conventional linear fixed effects specifications, the common intercept (α) and coefficients on time-invariant covariates (β) are all estimated in the FENB model. All the coefficients can still be interpreted in the conventional way as the effect of covariates on the mean in a log-link model. Time-varying covariates include an indicator for pre–post time point and its interaction with treatment indicators. Time-invariant covariates include all covariates listed in Table I along with the observed modality of care. IRR for a specific treatment modality is obtained from the coefficient of the interaction of that treatment indicator with time.

We use ambulatory non-methadone outpatient treatment as our reference category. If non-methadone outpatient treatment modality reduces the number of robberies in the follow-up period compared with baseline, we expect the coefficient on the pre-post indicator to be negative, implying that the *incidence rate ratio* (IRR) = $\exp(\gamma_{pre-post})$ is less than 1. IRR represents the ratio of the number of robberies per unit time in the follow-up period over baseline rate of robberies after adjusting for observed characteristics and the latent fixed-effect parameters.

The coefficients on the interaction of pre–post indicator with the other specific SAT modalities ($\gamma_{pre-post\ *SAT}$) represent the ratio of IRRs for that modality compared with ambulatory non-methadone outpatient SAT. The IRR for any specific SAT modality is then obtained by $IRR_{SAT} = \exp(\gamma_{pre-post} + \gamma_{pre-post\ *SAT})$.

IRR provides an estimate of the *proportional reduction* in the rate of robbery offending. We then use these coefficients to estimate the *Absolute Reduction* (AR) of robberies, the parameter of direct interest for public policy. Estimated AR depends upon both our estimated IRR and the pre-treatment rate of robbery offending within a particular population. An effective intervention (in the form of an IRR « 1.0) will lead to small ARs if it is applied to a low-risk population. Conversely, an intervention with IRR close to 1.0 may reduce many robberies if it is applied to a criminally active population.

To calculate treatment-specific AR we use the average number of robberies committed at baseline by all clients across all treatment modalities. We then multiply the treatment-specific estimated IRR with this baseline number of robberies and subtract this product from the baseline number:

$$\mathrm{AR} = \mathrm{Avg} \cdot \mathrm{Baseline\ no.} - (\mathrm{IRR} \times \mathrm{Avg} \cdot \mathrm{Baseline\ no.}) \quad (2)$$

This yields the potential change in the number of robberies, where all robbers who seek any treatment are assigned to this particular modality.

We compare differences in IRR and also in AR across treatment modalities. Since robbery ‘careers’ decline sharply with age (see Figure 1) (Blumstein and Cohen, 1987), we repeat our analyses stratifying by age, using a threshold of 25 years of age.[3] The standard errors for all parameters and comparisons are obtained using 1000 bootstrap replicates of the data. To assess the robustness of our results and to understand the magnitude of selection effects, we also ran a random-effect negative binomial specification and compared our results with the FENB model (see Appendix A[4]).

Our main results, which reflect our point estimates of $\gamma_{pre-post\ *SAT}$, were quite similar in the fixed-effect and random-effect specifications.

Finally, we compare the resulting social valuation of this reduced crime with reported estimates of treatment costs. Presuming that reduced robbery incidence is *the only benefit* associated with treatment receipt, we then conduct threshold analysis on the WTP for an averted robbery from a social perspective.

[3]We also stratified the sample using an age-30 cutoff. This resulted in higher sample size in the younger group and correspondingly greater statistical significance for some coefficients. However, the under-25 group was much more criminally active than the 25–30 group. An age cutoff of 25 therefore matched the key variable of policy concern.

[4]We are pleased to provide these tables as a web appendix.

Regression to the mean and reporting bias

Regression to the mean and reporting bias provide one last set of issues in our data. Clients may enter treatment ready to reduce offending, independent of SAT programs' actual effects. SAT clients may face heightened supervision or other measures that reduce their offending. Individuals may also be reluctant to divulge recent criminal offenses, particularly offenses committed during or after treatment interventions.

We bound the impact of these effects using a worst-case approach that biases our analysis against a finding of SAT treatment effectiveness. In particular, we assume that our least-intensive modality, ambulatory non-methadone outpatient treatment has *zero* causal impact on robbery offending (i.e. the true IRR equals 1.0). This approach is similar to the widely used difference-in-difference approach used in linear specifications (Wooldridge, 2002).

Any observed IRR for this SAT modality is thus posited to reflect regression to the mean or the impact of non-treatment interventions. We then subtract the observed robbery incidence change in the referent from that observed for each other modality, interpreting the difference as a lower-bound estimate of the true causal effect. To the extent that ambulatory non-methadone outpatient SAT actually reduces robbery offending, our worst-case approach understates the true robbery reduction associated with SAT intervention.

RESULTS

Table I provides descriptive statistics across SAT modalities. Almost $\frac{3}{4}$ of respondents were male, with mean age of 31.5 years. Given NTIES design, 54% of respondents were African-American, with 14.8% Hispanic/Latino and approximately 30% non-Hispanic white. Only 15.1% were employed at baseline.

Cocaine was the most common primary drug of abuse, accounting for 37% of respondents, with marijuana the next-most common listed substance, accounting for 27% of respondents. Marijuana was the most common secondary drug of abuse, accounting for 24% of responses. The distribution of primary and secondary drugs differed markedly across modalities.

At baseline, 421/6585 clients (6.4%) reported having committed at least one armed robbery in the year before treatment. Prevalence varied across modalities, ranging from 103/2175 (4.7%) in ambulatory outpatient to 186/1980 (9.4%) in long-term residential SAT. Average numbers of reported robberies reflected similar patterns, ranging from 0.58 robberies per client in short-term residential SAT to 1.20 robberies/client in long-term residential SAT.

Table II shows characteristics of clients who committed at least one robbery at baseline compared with no robberies at baseline. On average, robbers were 5 years younger than non-robbers ($p < 0.001$). They were almost twice as likely to have experienced homelessness ($p < 0.001$) and were more likely to report crack use ($p < 0.004$). Robbers were somewhat more likely to report heroin use, though the difference was statistically insignificant. Respondents who reported committing at least one robbery were much more likely to report committing other offenses, including burglary, auto theft, battery, and threats with a weapon. Among women, 34% of robbers (compared with 16% of others) reported sex work.

Table III shows estimated IRR for each treatment modality for the overall population and for the under-25 and over-25 subgroups, after adjusting for the baseline characteristics. Each SAT modality was associated with a large and statistically significant reduction (IRR < 1) in reported robbery incidence. IRRs were below 0.35 for all modalities, indicating a more than 65% pre–post predicted reduction in the rate of armed robbery.

Table III also illustrates differences across modalities in IRR (a positive difference favors the columns). Examining IRR in the full sample, short-term hospital inpatient care and ambulatory non-methadone outpatient SAT were less effective than residential inpatient modalities ($p < 0.05$). We found the same pattern with respect to outpatient methadone maintenance, but this difference was statistically insignificant given our relatively small methadone sample.

Age-stratified analysis indicated similar overall patterns, but with less statistical precision within our stratified sample. Only outpatient methadone maintenance treatment was found to be statistically more effective that ambulatory non-methadone outpatient SAT within the older subgroup. Residential long-term treatment modality appears to be slightly more effective for subjects under 25 years that those over 25 years of age.

As noted in the previous section, IRRs do not fully describe the effectiveness of these treatment modalities. From a social perspective, one must examine the AR in robberies associated with SAT, rather than merely the percentage reduction as indicated by IRR. As described in Equation (2), we apply the estimated IRR to the baseline number of robberies in order to obtain a measure of the AR.

The under-25 subgroup ($N = 1688$) was markedly more prone to commit robbery, with a pre-treatment mean of 1.92 robberies/client/year and a post-treatment mean of 0.457 robberies/client/year (Figure 1). In contrast, the over-25 subgroup ($N = 4897$) reported a pre-treatment mean of 0.449 robberies/client/year and a post-treatment mean of 0.144 robberies/client/year (Figure 1).

Table IV then shows estimated ARs for the overall population and for the under-25 and over-25 subgroups. All modalities indicate a large absolute change in the predicted number of robberies, an average reduction of more than 0.50 robberies/client in every specification within the overall population, with much larger estimated gross differences within the subgroup of young clients, reflecting their higher offending rate.

The social valuation associated with reduced robbery incidence is also shown for each specification. These are based on a $232 000 CV measure to avoid robbery (Cohen *et al.*, 2004). Given the large WTP in avoiding robbery, Table IV implies especially large social gains associated with reduced robbery incidence, with extremely large estimated gains within the under-25 group. For all modalities examined, DATCAP cost estimates imply that more-intensive forms of SAT would bring positive net monetary value as long as these prevent at least 0.04 robberies per enrolled SAT client, an amount equivalent to a 5% causal pre–post reduction in robbery incidence among actual offenders.

Results (Table IV, last row for differences across treatment modalities) are most statistically significant for the entire population and for the over-25 group, given the small sample size within the youngest subgroup. For example, we find that short-term residential treatment is associated with a significant mean reduction of 0.136 robberies relative to non-methadone outpatient care.

We wish to examine the incremental costs and benefits of more-intensive and less-intensive treatment modalities. To again bias the analysis against the net benefits of intensive treatments, we presume that an episode of residential SAT (either long-term or short-term) costs $10 000 more than an episode of ambulatory (non-methadone) SAT. In a similar fashion, we presume that an episode of outpatient methadone maintenance treatment costs $6000 more than an episode of ambulatory (non-methadone) SAT. These gaps far exceed reported differences in the DATCAP literature noted in previous sections (Roebuck *et al.*, 2003).

Under these worst-case assumptions, Figure 2 shows estimated net monetary benefits of robbery reduction associated with three SAT treatment modalities: short-term and long-term residential SAT, and outpatient methadone maintenance. Here we assume that ambulatory (non-methadone) SAT has no true causal effect.

We also posit a range of WTP for averting the marginal armed robbery. Cohen and collaborators estimate citizens' WTP exceeding $232 000. This valuation was derived from CV surveys of a representative sample of Americans. These surveys were conducted using techniques originally developed to explore citizens' valuation of marginal reductions in environmental amenities and harms. CV surveys were conducted in accordance with guidelines commissioned by the National Oceanic and Atmospheric Administration (NOAA). As applied to robbery, respondents were asked questions of the following form:

> Last year, a new crime prevention program supported by your community successfully prevented one in every ten armed robberies from occurring in your community. Would you be willing to pay $**X** per year to continue this program?

The value of $**X** was randomly varied in $25 increments between $25 and $225 based on focus group retesting.

Mean WTP for a 10% reduction in the rate of armed robbery was $110/year. Given that there are 103 million US households and approximately 486 000 armed robberies per year, Cohen and colleagues calculate that, on average, respondents were willing to pay $232 000 to avert, at the margin, one armed robbery in their community.

At this marginal valuation ('WTP'), the economic benefits of SAT-related robbery reduction far exceed associated treatment costs for all the three modalities of care. Because such high WTP estimates are controversial, threshold analysis is especially informative on this variable. The point at which each curve crosses the *X*-axis indicates the WTP at which the economic benefits of robbery reduction exactly balance the estimated costs of SAT. For both residential modalities, the net monetary benefits of more-intensive SAT modalities are positive when WTP is at least at $80 000. Because outpatient SAT is cheaper than inpatient treatment, the threshold is even lower for outpatient methadone maintenance, in the range of $50 000. All of these thresholds are far below the $232 000 figure estimated by Cohen and colleagues.

Figure 3 repeats these calculations for patients under 25 years old. Given the small sample size, we only compute these curves for the two residential SAT modalities. Given high rates of reported offending in this population, both residential SAT modalities appear to bring large reductions in robbery. This pattern is reflected in the low WTP threshold (below $40 000) at which the economic benefits of reduced robberies offset the incremental cost of treatment.

Figure 4 then focuses on patients who were at least 25 years old. All three modalities are estimated to bring large net monetary benefits when the social valuation of an averted robbery exceeds $200 000. At lower WTP, we find notably different results for the three studied modalities. Outpatient methadone maintenance, given its relative effectiveness and low cost, has positive estimated net monetary benefit when WTP exceeds $60 000. Short-term and long-term residential SAT display markedly higher thresholds, both between $100 000 and $180 000.

For all three modalities examined and at the base level WTP estimate, treatment would bring positive net social benefits if one could prevent at least 0.04 robberies per enrolled SAT client, an amount equivalent to a 5% causal pre–post reduction in robbery incidence among actual offenders.

CONCLUSION

This paper performs pre–post analyses of self-reported armed robbery among SAT clients. We observed large, statistically significant declines in robbery within all examined modalities. Expected changes in robbery incidence exceed 0.4 robberies/client/year across specifications and across modalities of care. In some cases, expected reductions are much larger. Given reasonable valuations associated with averting, at the margin, a single armed robbery, this one component of benefit may be large enough to offset the economic costs of SAT intervention.

Our analysis may also shed light on differences and contrasts across modalities in average treatment effects (ATE). Short-term residential SAT incurred the largest predicted *absolute* change in incidence, 0.69 robberies/client ($p < 0.001$), with ambulatory (non-methadone) outpatient SAT incurring the smallest predicted reduction, 0.56 robberies/client.

Our results highlight the strong benefits associated with more intensive modalities that actually reduce criminal offending. Although inpatient care is more costly than outpatient care, the increased effectiveness of inpatient care (reflected in its lower IRR) more than offsets the cost difference given reasonable valuations of averted armed robbery. Our results in Table IV imply that short-term residential SAT is preferred to outpatient non-methadone modalities if one values averting one armed robbery by at least $14 705. This valuation is well below WTP estimates cited in this paper. This valuation is also below accepted estimates of victim crime costs known to understate the true social costs of robbery. For example, Table II of Rajkumar and French (1997) reports a cost to robbery victims of $17 454 in the year 1992.

Study limitations

This study has several limitations that should be considered in evaluating the results. Most of these limitations are common in policy analysis of treatment, and are thus easily neglected because they do not reflect defects in specific papers or data sets.

Our analysis reflects large WTP estimates derived from survey data. Such high valuations reflect strong aversion to interpersonal crimes with the potential to escalate into violence. Most respondents in (the small number of) WTP studies of crime have never been robbed. Fears and anxieties that underlay survey responses may thus reflect misperceptions or biases. (An estimated 0.3% of armed robberies result in homicide.) Even if such fears and anxieties reflect faulty information or bias, they still bring real disutility and may induce important changes in behavior, such as middle-class flight from urban communities.

As with any investigation that concerns rare but frightening events, responses may be sensitive to anchoring, framing and to psychological factors influenced by study design. In the surveys conducted by Cohen *et al.* (2004), such factors may have played a role. Respondents' implied mean valuation for averting the marginal crime differed across crime categories. It is concerning, however, that respondents' mean WTP for a 10% reduction *in the rate of such crimes* appeared so clustered across different crimes. Mean WTP was between $104 and $126 per year for burglary, armed robbery, serious assault, and rape/ sexual assault. Respondents expressed higher mean WTP to avert the marginal homicide, though even here the mean WTP for a 10% homicide reduction ($146/year) was similar to those reported for other crimes. Other methodologies, such as studies which capture changes in housing prices in response to changing crime rates, would bolster the credibility of CV methodologies in this area.

In addition, surveys conducted by Cohen and collaborators intentionally did not specify the nature of specific crime-control interventions. We suspect that survey respondents would be willing to pay more for SAT than they would for more punitive criminal justice policies posited to achieve the same crime-control objectives. We have no direct data on this point. A recent CV analysis of public attitudes toward juvenile offending finds higher mean WTP at the margin for rehabilitation than for increased incarceration. (Nagin *et al.*, 2006).

Because of these disadvantages, specific WTP estimates of crime costs come with large uncertainties. Some authors choose not to use WTP estimates given the known limitations of the available data. We sympathize with the underlying concern. However, the alternative to using flawed or incomplete WTP data is to use other kinds of data that neglect several critical concerns which are captured only within WTP studies: anxiety, fear, losses in well-being resulting from having to take protective measures. Studies that consider only direct or tangible crime costs implicitly assign a valuation of zero to these key effects. Threshold analysis indicates that our main results are not sensitive to the current empirical and methodological uncertainties of CV research. If one reduced available WTP valuations by half, our qualitative conclusions would remain striking and essentially unchanged: robbery prevention provides a very large economic benefit associated with SAT care.

Our analysis implicitly assumes that individuals' robbery offending cannot be directly observed in allocating clients to SAT care modalities. (Consistent with this assumption, less than one-third of clients who reported committing armed robbery in the year before treatment reported ever being arrested for this offense. Less than one-sixth reported a robbery arrest in the year before treatment.) If individuals could be assigned to SAT modalities based on their pre-treatment robbery offending, both SAT and incarceration would have high net benefits within the criminally involved subgroup of clients. We have no data from randomized assignment of clients across SAT interventions.

We examine pre–post differences in offending using observational data from a sample of men and women who all have contact with treatment interventions. We have no data regarding substance users who do not receive treatment and who likely commit more robberies than do users who engage SAT. In a study of criminally active street heroin users, Johnson *et al.* (1985) report that 12% of respondents had committed armed robbery. In contrast, 7.5% of heroin-using NTIES respondents reported committing the same offenses pre-treatment. Johnson and collaborators also reported higher rates of offending among self-reported robbers (Johnson *et al.*, 1985).

Our negative binomial methodology provides a powerful mechanism to adjust for fixed but unobserved individual characteristics that may be correlated with assignment to specific treatment modalities. This methodology also accommodates the count data nature of robbery offending. It cannot control for differing readiness to change, which may lead clients to seek treatment in the first place or influence clients' assignment to a particular treatment modality. If individuals initiate treatment out of a desire to reduce offending, or if treatment is accompanied by other crime-reducing interventions, our analysis will overstate the benefit of treatment.

We impute the mid-point for each response category range to represent the average number of robberies for that subject at that time point. The topmost category is top coded at 100. We believe that this provides a conservative estimate of the treatment effects since the primary reduction in the number of robberies post-treatment is brought about by curtailing the 100+ group. Similarly, within each of the middle categories, even if one assumes that the true distribution of robberies is skewed, imputing the same mid-point value for both pre- and post-treatment will provide a conservative estimate since the effect of treatment is likely to be at the higher-end tail within that category.

This analysis does not consider social valuation of serious crimes other than robbery. NTIES includes data for many other offenses, including assault and burglary, and many forms of property crime. We do not consider these crimes because the impact of different social valuation strategies is smaller than that pertains for armed robbery. Considering a mixture of heterogeneous offenses requires a more complex econometric model than is possible in the current paper. Including a broad range of offenses would likely increase the estimated net benefits associated with SAT.

We use client self-reported data regarding criminal offending. Fear of legal intervention and social stigma may lead clients to under-report recent post-treatment offending, biasing the analysis in favor of treatment interventions. Regression to the mean might also lead us to overstate the causal impact of interventions. We use an extreme worst-case analysis to provide a lower bound to address these concerns. By assuming that outpatient non-methadone treatment has no true causal effect, we gain some purchase over both potential bias. The fact that SAT appears highly beneficial under such extreme adverse assumptions gives us comfort that our findings are robust to such concerns.

Finally, we do not consider the benefits of treatment outside the domain of crime, such as the impact of SAT in HIV prevention, increased employment, or improved general health (Zaric *et al.*, 2000; Pollack, 2002).

Discussion

Despite several limitations, our analysis contributes to the existing literature in two main ways: First, we perform pre–post analysis of robbery incidence in an important cohort of drug users. Second, we show that treatment-related declines in robbery bring large economic benefits when evaluated using appropriate valuation estimates in the existing literature. These social benefits likely outweigh the economic costs of SAT within criminally active drug-using populations. This finding is especially striking, because our simple analysis only considers the benefits of SAT in one dimension – crime reduction – and for one type of crime.

Second, the net benefits of intensive SAT modalities that serve criminally active populations may be especially large. Residential modalities for populations such as young injection users or individuals with histories of homelessness and psychiatric disorders may therefore deserve high priority for funding.

Many studies identify crime reduction as a key benefit of SAT. Our analysis here suggests that conventional wisdom may even understate the social benefits of these effects. WTP methods suggest that citizens are willing to pay in excess of $200 000 to avert the marginal robbery in their communities. Although these estimates carry statistical and methodological uncertainties, WTP measures provide a coherent economic account of the true costs of crime and yield much larger benefits than the tangible cost estimates used in most economic policy analysis of SAT interventions.

Given mean estimated declines in robbery incidence exceeding 0.4 robberies per client in the past year, this corresponds to an estimated valuation exceeding $80 000 per client. Our findings are especially striking when one considers that only 6.4% of clients reported committing even one robbery. Descriptive statistics on pre-treatment offending imply that the mean NTIES respondent who committed at least one robbery imposed nearly $4 million annually on the wider society. Within this small group, if even a small fraction of estimated pre–post robbery incidence reductions can be causally attributed to SAT intervention, the accompanying reduction in social costs would greatly outweigh the economic costs of SAT intervention over the entire population.

We pursued this analysis in Figures 2–4, under the extreme worse-case assumption that all pre–post differences equivalent to those observed in ambulatory non-methadone outpatient treatment reflect regression to the mean, reporting bias, and other non-causal associations between SAT participation and reduced offending. Even under these pessimistic assumptions we find large estimated net benefits to SAT.

Our analysis also suggests pitfalls to some common strategies used to target treatment resources. SAT providers and policymakers face understandable incentives to allocate resources across clients based on clients' likely success in SAT. The associated metrics might include estimated IRR, treatment retention, or cost-effectiveness measures, such as cost per drug-free day. As described by Magura and colleagues, criteria associated with favorable client outcomes include: older ages at treatment entry, high educational attainment, no prior criminal justice involvement, no psychiatric disorders (Magura *et al.*, 1998). Such resource allocation strategies would thus lead policymakers to channel resources away from those clients with greatest propensity to commit robbery, for whom imperfectly effective SAT interventions may provide especially large social benefits. A wiser policy response would be to channel resources to expand treatment access within criminally active drug-using populations, for example, by supporting interim programs for individuals on methadone maintenance waiting lists (Schwartz *et al.*, 2006).

One potential concern is that allocating a fixed set of public resources on the basis of armed robbery may reduce funding to key populations, such as low-income women who commit few robberies but who derive other important benefits from SAT. Such high-priority populations provide a reminder that crime reduction provides only one lens through which to evaluate interventions. Economic analyses of SAT must also consider the impact of treatment on drug users themselves. Cost–utility analyses suggest that many SAT modalities are cost-effective in improving client well-being, independent of crime or other externalities. If crime reduction justifies greater SAT expenditures for particular populations, there is no inherent reason to draw these funds from competing SAT interventions rather than from other sources of public resources (Barnett and Swindle, 1997; Barnett, 1999; Cartwright, 2000; Zaric *et al.*, 2000; French *et al.*, 2002; Pollack, 2002; Mojtabai and Zivin, 2003).

Advocates who favor less punitive policies might cite our results, which are congenial to social policies that expand access to treatment services. However, the same economic logic underscores the benefits of policing and correctional interventions that deter or incapacitate individuals likely to commit future robberies. Cost–benefit analysis and related methods allow policymakers to compare specific policy choices from the standpoint of efficient resource allocation. The data do not speak for themselves in defining these policy choices or in addressing the range of normative concerns that should be considered in criminal justice policy. It is possible, for example, that exclusive application of WTP methodologies in federal sentencing guidelines would produce overly punitive sentences for some crimes.

Our analysis is also unsettling because its policy implications reflect rare but extreme outcomes. Extreme experiences and outcomes of rather atypical participants strongly influence ATE, are difficult to value and – because of their rarity – are often imprecisely linked with program effects. Such patterns are not confined to SAT. For instance, cost–benefit analyses of Job Corps are strongly influenced by marginal changes in predicted homicide offending among program participants (Donahue and Siegelman, 1998).

Influential outliers are unsettling, but they are real and cannot be set aside. They influence policy analysis because they have a large impact on American society. Considering such factors, our analysis suggests that the benefits of SAT are even greater than one would otherwise believe they are.

Acknowledgments

Peter Reuter, Jonathan Caulkins, Jens Ludwig, Ellen Meara, Richard Frank, and participants at the 2005 APPAM conference provided helpful comments. Michael French graciously shared an unpublished manuscript. Preparation of this article was facilitated by Grant 5-R01-664 DA-3272-18 from the National Institute on Drug Abuse and by a seed grant from the Chicago Center of Excellence in Health Promotion Economics.

Appendix A

Table AI shows our coefficient point estimates for log (IRR) in our negative binomial regression model. The left column shows the results of our random-effect specification. The right column shows the results of our fixed-effect specification.

As expected from our descriptive Table II, the random-effect specification indicated high propensity to robbery among young males, cocaine users, homeless and unemployed SAT clients, and those with prior criminal convictions. Also as expected, the coefficients on these stable characteristics of individuals become much smaller, and generally insignificant, in our fixed-effect specification.

Table AI

Results for the fixed-effects negative binomial regression

	Random-effect specification coefficient (*Z*-ratio)	**Fixed-effect specification coefficient (*Z*-ratio)**
Indicator for post-intervention	−1.04*** (5.53)	−1.11*** (5.69)
Modality main-effects		
Short-term hospital-based inpatient modality	0.31 (1.14)	1.61† (1.65)
Short-term residential SAT modality	0.12 (0.77)	1.22† (1.71)
Long-term residential SAT modality	0.43*** (3.19)	0.66 (1.22)
Ambulatory methadone SAT	0.35 (1.31)	−0.61 (0.45)
Ambulatory non-methadone SAT (referent)	—	—
Interaction terms		
Short-term hospital-based inpatient modality*post	**0.12 (0.26)**	**0.025 (0.05)**
Short-term residential SAT modality*post	**−0.54† (1.71)**	**−0.70* (2.07)**
Long-term residential SAT modality*post	**−0.59* (2.28)**	**−0.61* (2.27)**
Ambulatory methadone SAT*post	**−0.59 (1.12)**	**−0.58 (1.08)**
Ambulatory non-methadone SAT (Referent)*post	—	—
Homeless	0.75*** (7.47)	0.97* (2.19)

	Random-effect specification coefficient (*Z*-ratio)	Fixed-effect specification coefficient (*Z*-ratio)
Criminal justice client	0.09 (0.87)	−0.17 (0.35)
Government paid for treatment	0.27 ** (2.58)	−0.49 (1.10)
Convicted of a prior crime	0.49 *** (4.30)	−0.42 (0.89)
Uninsured	0.06 (0.57)	0.0 (0.01)
Involved in sex work	1.01 *** (7.65)	0.25 (0.47)
Cocaine as primary drug of abuse	0.30 + (1.78)	0.46 (0.51)
Cocaine as secondary drug of abuse	0.42 ** (2.96)	−0.48 (0.73)
Heroin as primary drug of abuse	0.27 (1.34)	1.54 (1.46)
Heroin as secondary drug of abuse	−0.18 (0.65)	−14.0 (0.02)
Alcohol as primary drug of abuse	−0.29 + (1.65)	1.58 + (1.75)
Alcohol as secondary drug of abuse	0.04 (0.35)	0.12 (0.23)
Marijuana as primary drug of abuse	−0.26 (1.37)	1.43 (1.44)
Marijuana as secondary drug of abuse	−0.05 (0.37)	0.26 (0.41)
Age	−0.08 *** (12.17)	0.0045 (0.12)
Age squared	0.003 *** (4.93)	−0.0025 (0.62)
Male	0.95 *** (7.26)	−0.24 (0.42)
Black	0.18 (1.56)	0.037 (0.07)
Hispanic/Latino	0.24 + (1.80)	0.06 (0.09)
High school graduate	−0.13 (1.30)	0.048 (0.10)
Completed 9th grade	0.13 (0.86)	0.28 (0.33)
Employed	−0.49 ** (2.76)	0.19 (0.22)

*** $p < 0.001$,

** $p < 0.01$,

* $p < 0.05$,

+ $p < 0.10$.

Our main results for policy are obtained from the interaction term $\gamma_{\text{pre–post *SAT}}$. We obtain these by interacting specific modalities with our pre–post indicator, and by presuming that all pre–post declines in robbery in our reference modality, non-methadone ambulatory SAT, are non-causal. These results are marked in bold and are shaded within Table A1. As shown, point estimates were quite similar in the fixed-effect and random-effect specifications.

REFERENCES

Allen C. The links between heroin, crack cocaine, and crime. British Journal of Criminology. 2005; 45:355–372.

Barnett P. The cost-effectiveness of methadone maintenance as a health care intervention. Addiction. 1999; 94(4):479–488. [PubMed: 10605844]

Barnett P, Swindle R. Cost-effectiveness of inpatient substance abuse treatment. Health Services Research. 1997; 32(5):615–629. [PubMed: 9402904]

Blumstein A, Cohen J. Characterizing criminal careers. Science. 1987; 237(4818):985–991. [PubMed: 17837394]

Cameron, A.; Trivedi, P. Regression Analysis of Count Data. Cambridge, UK: Cambridge University Press; 1998.

Cartwright W. Cost–benefit and cost-effectiveness analysis of drug abuse treatment services. Evaluation Review. 1998; 2(5):609–636. [PubMed: 10186896]

Cartwright W. Cost–benefit analysis of drug treatment services: review of the literature. Journal of Mental Health Policy and Economics. 2000; 3(1):11–26. [PubMed: 11967433]

Caulkins, J. Mandatory Minimum Drug Sentences: Throwing Away the Key or the Taxpayers' Money?. Santa Monica: RAND Drug Policy Research Center; 1997.

Chamberlain G. Analysis of covariance with qualitative data. Review of Economic Studies. 1980; 47:225–238.

Cohen M, Rust R, et al. Willingness to pay for crime control programs. Criminology. 2004; 42(1):89–110.

Dismuke C, French M, et al. Out of touch or on the money: do the clinical objectives of addiction treatment coincide with economic evaluation results? Journal of Substance Abuse Treatment. 2004; 27:253–263. [PubMed: 15501378]

Donahue J, Siegelman P. Allocating resources among prisons and social programs in the battle against crime. Journal of Legal Studies. 1998; 27:1–43.

Flynn P, Kristiansen P, et al. Costs and benefits of treatment for cocaine addiction in DATOS. Drug and Alcohol Dependence. 1999; 57(2):167–174. [PubMed: 10617100]

Flynn P, Simpson D, et al. Comment on nonresponse and selection bias in treatment follow-up studies. Substance Use and Misuse. 2001; 36(12):1753–1757.

French M, Dunlap L, et al. A structured instrument for estimating the economic cost of drug abuse treatment. Journal of Substance Abuse Treatment. 1997; 14(5):445–455. [PubMed: 9437614]

French M, McGeary K. Estimating the economic cost of substance abuse treatment. Health Economics. 1997; 6:539–544. [PubMed: 9353658]

French M, Salomé J, et al. Benefit–cost analysis of addiction treatment: methodological guidelines and empirical application using the DATCAP and ASI. Health Services Research. 2002; 37(2):433–455. [PubMed: 12036002]

Friedmann P, Alexander J, et al. Organizational correlates of access to primary care and mental health services in drug abuse treatment units. Journal of Substance Abuse Treatment. 1999; 16(1):71–80. [PubMed: 9888124]

Gerstein D, Johnson R. Nonresponse and selection bias in treatment follow-up studies. Substance Use and Misuse. 2000; 35(6–8):971–1014. [PubMed: 10847219]

Gerstein D, Johnson R. In reply to Patrick M. Flynn and colleagues. Substance Use and Misuse. 2001; 36(12):1753–1757.

Gerstein, DR.; Datta, AR., et al. NTIES Final Report. Chicago, IL: National Opinion Research Center; 1997.

Gerstein, DR.; Harwood, HJ. Treating Drug Problems. Washington, DC: National Academy Press, Institute of Medicine; 1990.

Harwood H, Fountain D, et al. Cost estimates for alcohol and drug abuse. A Report and Commentaries. Addiction. 1999; 94(5):631–647. [PubMed: 10563025]

Hausman J, Hall B, et al. Econometric models for count data with an application to the patents-R and D relationship. Econometrica. 1984; 52:909–938.

IOM. No Time to Lose: Making the most of HIV Prevention. Washington, DC: National Academy Press; 2000.

Johnson, B.; Goldstein, P., et al. Taking Care of Business: The Economics of Crime by Heroin Users. Lexington, MA: Lexington Books; 1985.

Kleiman, M. Against Excess: Drug Policy for Results. New York: Basic Books; 1993.

Magura S, Nwakeze PC, et al. Pre- and in-treatment predictors of retention in methadone treatment using survival analysis. Addiction. 1998; 93(1):51–60. [PubMed: 9624711]

Massing, M. The Fix. New York, NY: Simon & Schuster; 1998.

McCollister K, French M. The relative contribution of outcome domains in the total economic benefit of addiction interventions: a review of first findings. Addiction. 2003; 98:1647–1659. [PubMed: 14651494]

McGlothlin WH, Anglin MD. Shutting off methadone. Costs and benefits. Archives of General Psychiatry. 1981; 38(8):885–892. [PubMed: 6789792]

McLellan A, Grissom G, et al. Problem-service ‘Matching’ in addiction treatment. Archives of General Psychiatry. 1997; 54(8):730–735. [PubMed: 9283508]

McLellan, A.; Woody, G., et al. Evaluating the effectiveness of addiction treatment: reasonable expectations, appropriate comparisons. In: Egertson, J.; Fox, D.; Leshner, A., editors. Treating Drug Abusers Effectively. Oxford: Blackwell; 1997. p. 7-40.

Metzger D, Woody G, et al. Human immunodeficiency virus seroconversion among intravenous drug users in- and out-of-treatment: an 18-month prospective follow-up. Journal of Acquired Immune Deficiency Syndromes and Human Retrovirology. 1993; 6(9):1049–1056.

Mojtabai R, Zivin J. Effectiveness and cost-effectiveness of four treatment modalities for substance disorders: a propensity score analysis. Health Services Research. 2003; 38(1):233–259. [PubMed: 12650390]

Nagin DS, Piquero AR, et al. Public preferences for rehabilitation versus incarceration of juvenile offenders: evidence from a contingent valuation survey. Criminology & Public Policy. 2006; 5(4): 627–651.

Neyman J, Scott E. Consistent estimates based on partially consistent observations. Econometrica. 1948; 16:1–32.

Pollack, H. Methadone treatment as HIV prevention: cost-effectiveness analysis. In: Kaplan, EH.; Brookmeyer, R., editors. Quantitative Evaluation of HIV Prevention Programs. New Haven, CT: Yale University Press; 2002. p. 118-143.

Pollack H, Khoshnood K, et al. The impact of needle exchange-based health services on emergency department use. Journal of General Internal Medicine. 2002; 17(5):341–348. [PubMed: 12047730]

Rajkumar A, French M. Drug abuse, crime costs, and the economic benefits of treatment. Journal of Quantitative Criminology. 1997; 13(3):291–322.

Roebuck M, French M, et al. DATStats: results from 85 studies using the Drug Abuse Treatment Cost Analysis Program (DATCAP). Journal of Substance Abuse Treatment. 2003; 25:51–57. [PubMed: 14512108]

Schwartz R, Highfield D, et al. A randomized controlled trial of interim methadone maintenance. Archives of General Psychiatry. 2006; 63(1):102–109. [PubMed: 16389204]

Shepard D, Larson M, et al. Cost-effectiveness of substance abuse services. The Psychiatric Clinics of North America. 1999; 22(2):385–400. [PubMed: 10385940]

Sindelar J, Fiellin D. Innovations in treatment for drug abuse: solutions to a public health problem. Annual Review of Public Health. 2001; 22:249–272.

Sindelar J, Jofre-Bonet M, et al. Cost-effectiveness analysis of addiction treatment: paradoxes of multiple outcomes. Drug and Alcohol Dependence. 2004; 73:41–50. [PubMed: 14687958]

Wheeler J, Fadel H, et al. Ownership and performance of outpatient substance abuse treatment centers. American Journal of Public Health. 1992; 82(5):711–718. [PubMed: 1314520]

Wooldridge, J. Econometric Analysis of Cross-Section and Panel Data. Cambridge, MA: MIT Press; 2002.

Zaric G, Brandeau M, et al. Methadone maintenance and HIV prevention: a cost-effectiveness analysis. American Journal of Public Health. 2000; 90(7):1–12.

Zarkin G, Dunlap L, et al. The effect of treatment completion and length of stay on employment and crime in outpatient drug-free treatment. Journal of Substance Abuse Treatment. 2002; 23(4):261–267. [PubMed: 12495788]

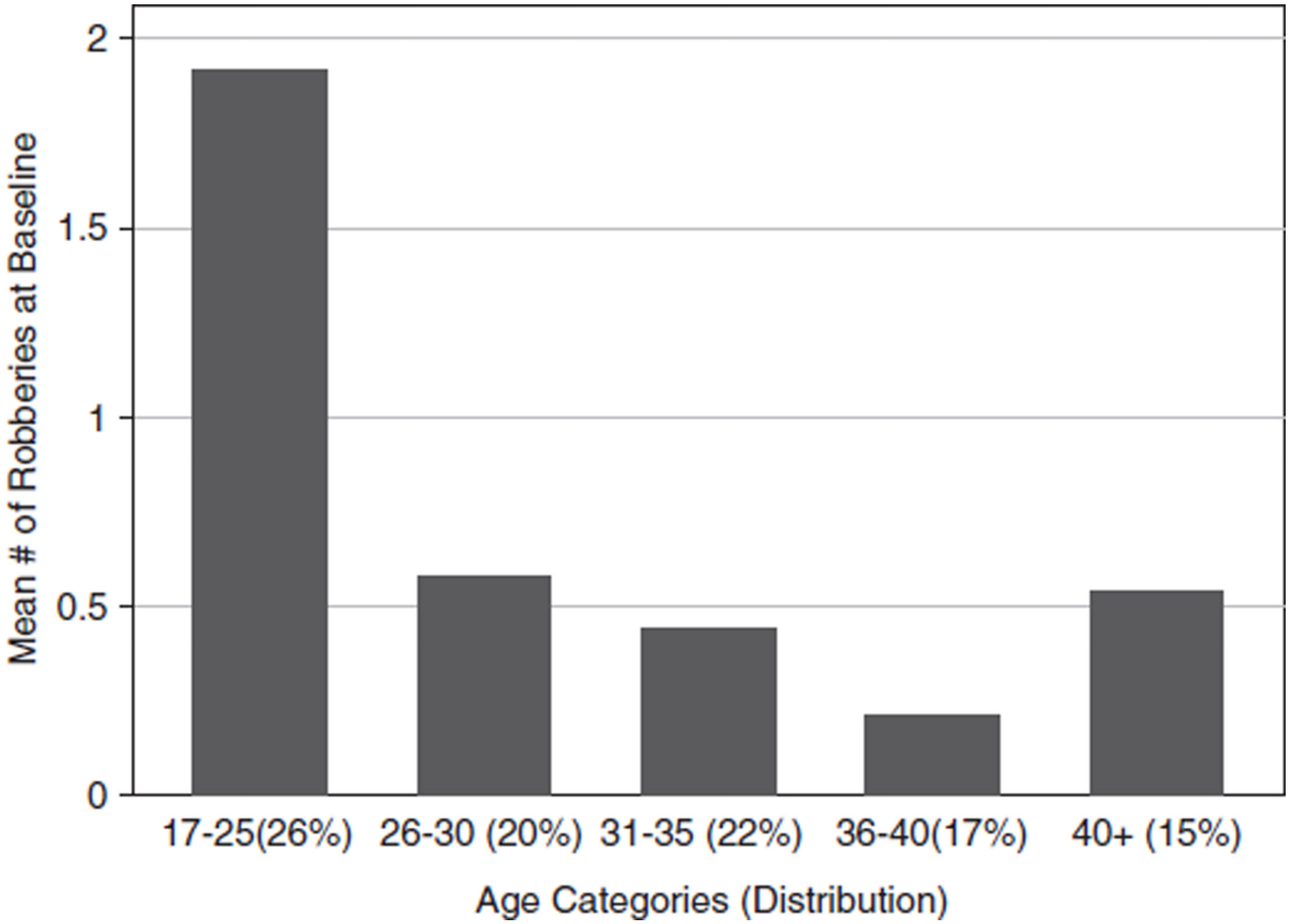

Figure 1.
Pre-treatment robbery offending by age.

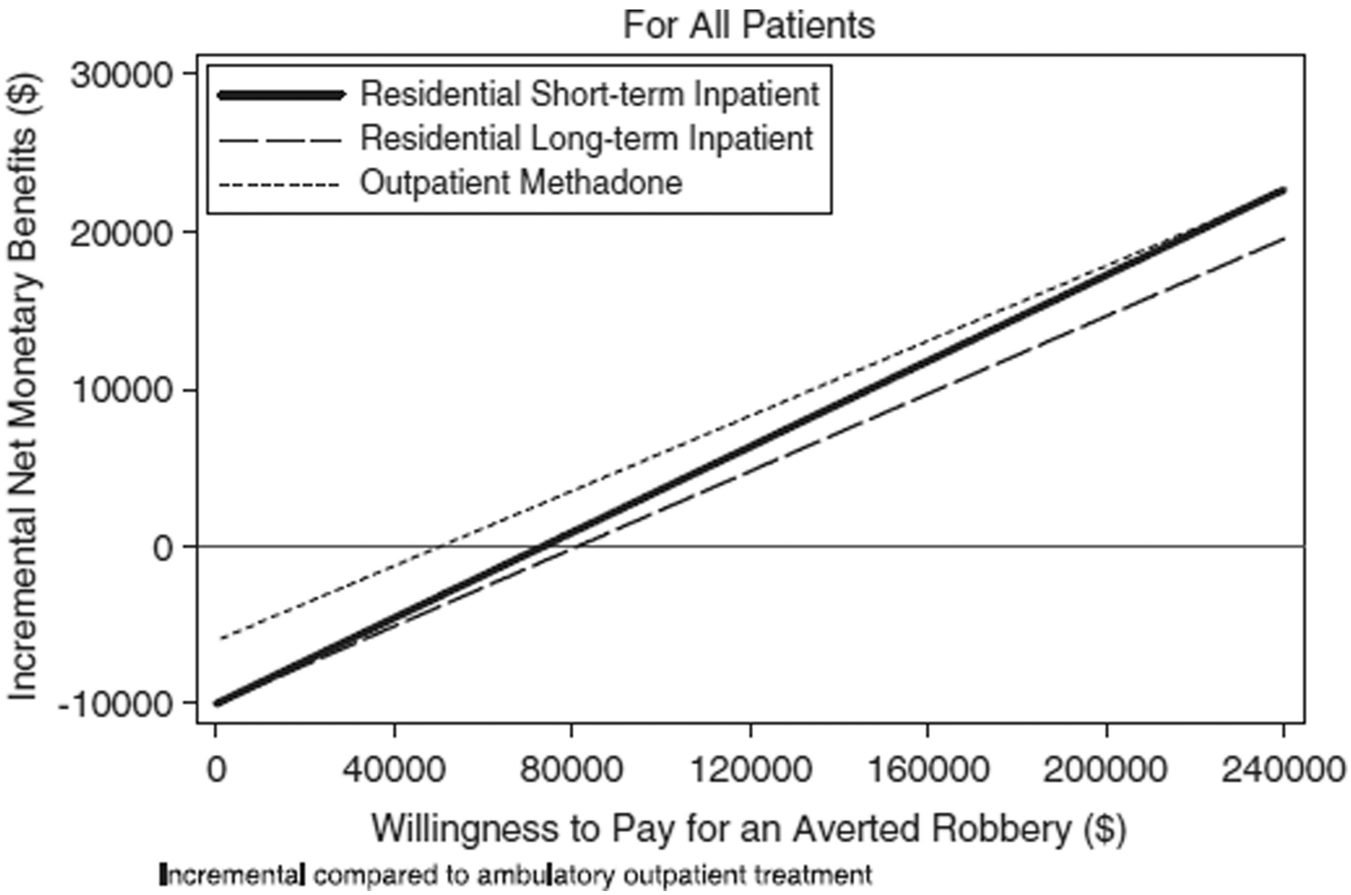

Figure 2.
Threshold analysis of net monetary value of SAT in crime reduction.

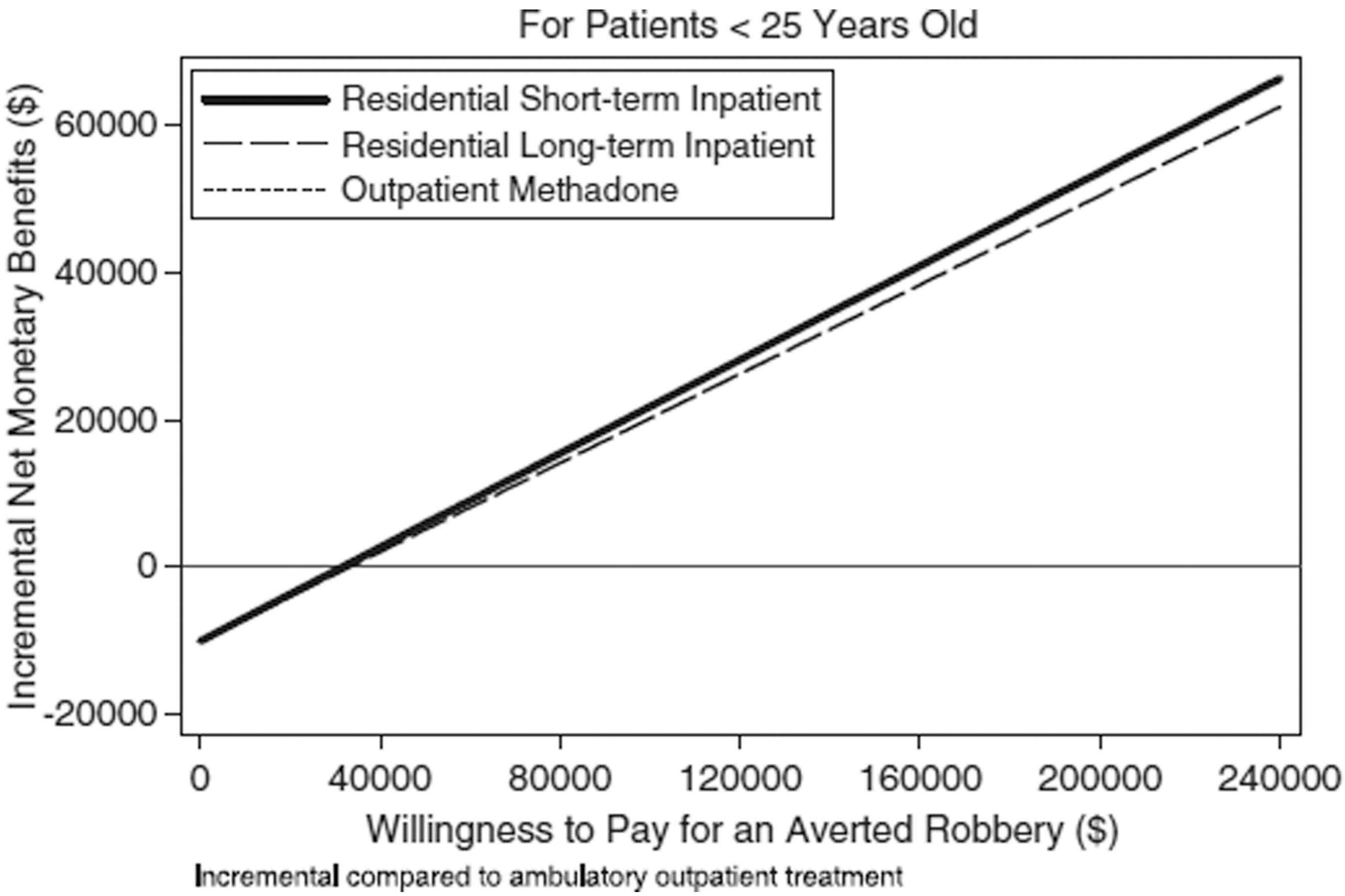

Figure 3.
Threshold analysis of net monetary value of SAT in crime reduction (patients under 25 years of age).

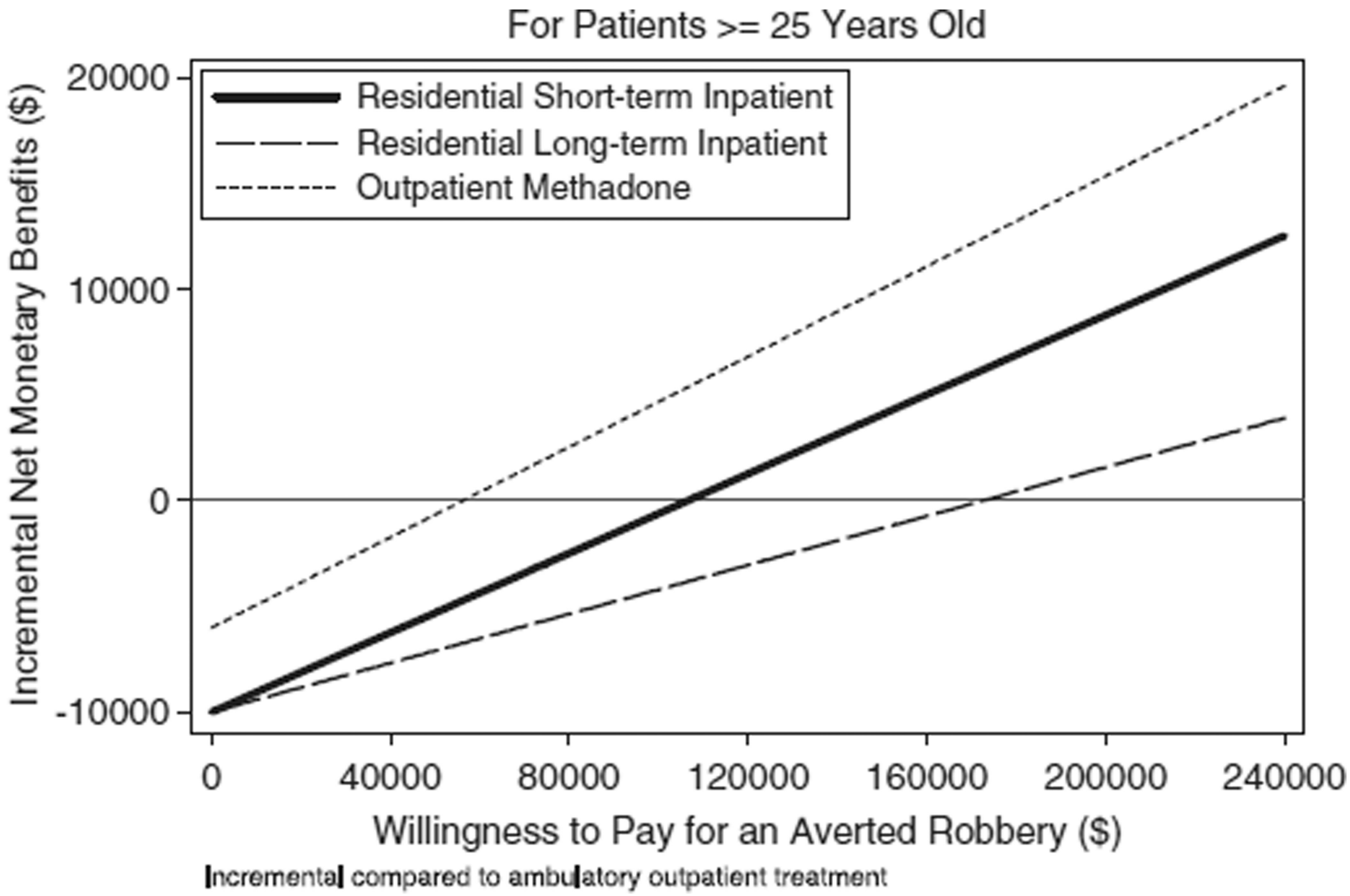

Figure 4.
Threshold analysis of net monetary value of SAT in crime reduction (patients at least 25 years of age).

Table I

Descriptive statistics by treatment

	Hospital short-term inpatient	Residential short-term inpatient	Residential long-term inpatient	Outpatient methadone	Ambulatory (non-methadone) outpatient	Overall
N	261	1655	1980	514	2175	6585
Follow-up						
Any robbery	7 (2.7%)	19 (1.2%)	40 (2.0%)	5 (1.0%)	38 (1.8%)	109 (1.7%)
Average no. of robberies|any						
1	3 (42.9%)	7 (36.8%)	8 (20.0%)	0 (0.0%)	19 (50.0%)	37 (33.9%)
3.5	3 (42.9%)	8 (42.1%)	20 (50.0%)	3 (60.0%)	13 (34.2%)	47 (43.1%)
13	1 (14.3%)	4 (21.1%)	6 (15.0%)	2 (40.0%)	6 (15.8%)	19 (17.4%)
60.5	0 (0.0%)	0 (0.0%)	6 (15.0%)	0 (0.0%)	0 (0.0%)	6 (5.5%)
100	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)
Average no. of robberies	0.10	0.05	0.26	0.07	0.07	0.123
Baseline						
Any robbery	17 (6.5%)	88 (5.3%)	186 (9.4%)	25 (4.9%)	103 (4.7%)	419 (6.4%)
No. of robberies|any						
1	6 (35.3%)	28 (31.8%)	37 (19.9%)	3 (12.0%)	27 (26.2%)	101 (24.1%)
3.5	8 (47.1%)	33 (37.5%)	75 (40.3%)	10 (40.0%)	38 (36.9%)	164 (39.1%)
13	2 (11.8%)	19 (21.6%)	53 (28.5%)	7 (28.0%)	24 (23.3%)	105 (25.1%)
60.5	0 (0.0%)	6 (6.8%)	18 (9.7%)	4 (16.0%)	10 (9.7%)	38 (9.1%)
100	1 (5.9%)	2 (2.3%)	3 (1.6%)	1 (4.0%)	4 (3.9%)	11 (2.6%)
Average no. of robberies	0.61	0.58	1.20	0.91	0.68	0.826
Age (years)	35.1 (7.2)	31.2 (7.5)	29.4 (8.4)	37.5 (7.4)	32 (9.1)	31.5 (8.6)
Male (%)	67.4	79.7	69.1	68.9	70.3	72.1
Blacks (%)	89.3	39.3	57.3	44.4	59.8	53.8
Hispanics (%)	1.1	15.0	12.7	26.7	15.4	14.8
HS (%)	46.4	63.5	46.1	57.4	53.7	53.9
≥Grade 9 (%)	92.3	94.3	89.8	92.2	91.6	91.8
Employed (%)	13.8	10.5	4.4	18.1	27.7	15.1
CrimJust (%)	10.0	36.6	17.5	4.1	30.2	25.1

	Hospital short-term inpatient	Residential short-term inpatient	Residential long-term inpatient	Outpatient methadone	Ambulatory (non-methadone) outpatient	Overall
Homeless (%)	26.4	18.5	21.1	7.6	18.7	18.8
Govpay (%)	83.1	32.3	41.4	69.5	60.2	49.2
Convict (%)	63.4	76.3	70.9	65.7	60.4	68.1
Uninsured (%)	46.3	43.9	26.1	28.8	43.0	37.2
Sexwork (%)	16.1	6.6	8.8	8.0	6.0	7.6
Primary drugs						
Coke (%)	65.1	30.8	44.7	1.2	38.8	36.7
Heroin (%)	1.9	13.4	10.3	96.3	6.9	16.3
Marijuana (%)	1.5	9.1	13.7	0.0	10.2	26.6
Alcohol (%)	31.4	29.1	19.3	0.2	37.0	9.8
Secondary drugs						
Coke (%)	18.0	16.1	13.9	36.2	13.4	16.2
Heroin (%)	0.8	3.4	4.4	1.0	2.8	3.2
Marijuana (%)	9.6	13.4	15.6	2.7	11.0	23.9
Alcohol (%)	48.7	15.9	29.7	7.2	25.8	12.3

Table II

Baseline characteristics of robbers and non-robbers in NTIES

	Individuals with at least one self-reported robbery	Individuals with no self-reported robbery	Overall
N	419	6166	6585
Age (years)	26.3 (8.3)	31.9 (8.5)	31.5 (8.6)
Male (%)	84.1	71.3	72.1
Blacks (%)	52.4	54.0	53.8
Hispanics (%)	20.2	14.4	14.8
HS (%)	40.6	54.8	53.9
≥Grade 9 (%)	90.3	91.9	91.8
Employed (%)	6.7	15.6	15.1
CrimJust (%)	24.9	25.1	25.1
Homeless (%)	32.8	17.8	18.8
Govpay (%)	48.9	49.2	49.2
Convict (%)	80.0	67.3	68.1
Uninsured (%)	35.6	37.3	37.2
Sexwork (%)	12.35	7.24	7.6
Drug use (primary or secondary)			
Crack cocaine (%)	63.7	56.4	56.8
Heroin (%)	22.8	19.3	19.5
Marijuana (%)	34.0	18.6	19.5
Alcohol (%)	41.6	46.0	45.7
Crime history			
Burglary	53.4	8.2	11.1
Auto theft	9.7	2.1	2.5
Battery	34.9	11.1	12.6
Threat with weapon	25.4	4.8	6.1
Length of stay (months)	3.6	4.1	4.0
Ever arrested for the use of weapon/force to steal	31.5	10.7	12.0
Ever arrested for beating someone up	35.1	15.7	16.9
Ever arrested for hurting someone	41.4	24.0	25.1
Ever arrested for breaking and entering	35.8	20.9	21.9
Arrested for stealing by force in year before treatment	15.7	1.5	2.4
Arrested for beating someone up in year before treatment	16.2	3.4	4.2
Arrested for hurting someone in year before treatment	20.2	7.0	7.8
Arrested for breaking and entering in year before treatment	13.8	4.9	5.5
Arrested for selling drugs in year before treatment	21.2	5.7	6.7

Table III

Estimated treatment effectiveness: (predicted robberies at follow-up)/(predicted robberies at baseline)

Overall **(N = 6585)**	**Hospital short-term inpatient (N = 261) Mean (se)**	**Residential short-term (N = 1655) Mean (se)**	**Residential long-term (N = 1980) Mean (se)**	**Outpatient methadone (N = 514) Mean (se)**	**Ambulatory (non-methadone) outpatient (N = 2175) Mean (se)**
Ratios	0.337 (0.16)	0.164 (0.045)	0.179 (0.03)	0.184 (0.09)	0.328 (0.06)
H0 := 1[p]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]
		Differences between ratios (row–cols) : (se)			
Residential short-term	−0.173 (0.186)	—	—	—	—
Residential long-term	−0.158 (0.186)	0.015 (0.043)	—	—	—
Outpatient methadone	−0.153 (0.207)	0.020 (0.093)	0.005 (0.090)	—	—
Ambulatory non-methadone	−0.008 (0.194)	**0.165** (**0.073**)	**0.150** (**0.068**)	0.145 (0.110)	—
	Hospital short-term inpatient	Residential short-term	Residential long-term	Outpatient methadone	Ambulatory (non-methadone) outpatient
Under-25 group	(N = 20)	(N = 428)	(N = 691)	(N = 25)	(N = 524)
(N = 1688)	Mean (se)	Mean (se)	Mean (se)	Mean (se)	Mean (se)
Ratios	—	0.146 (0.07)	0.154 (0.034)	—	0.311 (0.086)
H0 := 1[p]		[0.037]	[<0.0001]		[0.0003]
		Differences between ratios (row–cols) : (se)			
Residential short-term	—	—	—	—	—
Residential long-term	—	0.008 (0.068)	—	—	—
Outpatient methadone	—	—	—	—	—
Ambulatory non-methadone	—	0.165 (0.111)	0.157 (0.091)	—	—
	Hospital short-term inpatient	Residential short-term	Residential long-term	Outpatient methadone	Ambulatory (non-methadone) outpatient
Over-25 group	(N = 241)	(N = 1227)	(N = 1289)	(N = 489)	(N = 1651)
(N = 4897)	Mean (se)	Mean (se)	Mean (se)	Mean (se)	Mean (se)
Ratios	0.232 (0.211)	0.143 (0.053)	0.223 (0.064)	0.114 (0.049)	0.351 (0.103)
H0 := 1[p]	[0.272]	[0.006]	[0.0004]	[0.020]	[0.0006]
		Differences between ratios (row–cols) : (se)			
Residential short-term	−0.089 (0.218)	—	—	—	—
Residential long-term	−0.009 (0.228)	0.080 (0.082)	—	—	—
Outpatient methadone	−0.118 (0.217)	−0.029 (0.071)	−0.109 (0.078)	—	—

Overall (N = 6585)	**Hospital short-term inpatient (N = 261) Mean (se)**	**Residential short-term (N = 1655) Mean (se)**	**Residential long-term (N = 1980) Mean (se)**	**Outpatient methadone (N = 514) Mean (se)**	**Ambulatory (non-methadone) outpatient (N = 2175) Mean (se)**
Ambulatory non-methadone	0.119 (0.236)	0.208 (0.114)	0.128 (0.117)	**0.237** (**0.111**)	—

Bold face indicates $p < 0.05$.

Table IV

Choices of baseline number of robberies and expected treatment effectiveness

Overall (*N* = 6585)	**Hospital short-term inpatient mean (se) (*N* = 261) Mean (se)**	**Residential short-term inpatient mean (se) (*N* = 1655) Mean (se)**	**Residential long-term inpatient mean (se) (*N* = 1980) Mean (se)**	**Outpatient methadone mean (se) (*N* = 514) Mean (se)**	**Ambulatory (non-methadone) outpatient mean (se) (*N* = 2175) Mean (se)**
Pre-treatment Robbery incidence: Random assignment	0.826 (0.079)	0.826 (0.079)	0.826 (0.079)	0.826 (0.079)	0.826 (0.079)
E (Change)[a]	−**0.548** (**0.166**)	−**0.691** (**0.080**)	−**0.678** (**0.079**)	−**0.674** (**0.104**)	−**0.555** (**0.093**)
(Social valuation of Robbery reduction)	$127 000	$160 000	$157 000	$156 000	$129 000
		Differences between E (Change) (row–cols) : (se)			
Residential short-term	−0.143 (0.156)	—	—	—	—
Residential long-term	−0.130 (0.155)	0.013 (0.034)	—	—	—
Outpatient methadone	−0.126 (0.171)	0.017 (0.076)	0.004 (0.074)	—	—
Ambulatory non-methadone	−0.007 (0.162)	**0.136** (**0.059**)	**0.123** (**0.055**)	0.119 (0.090)	—
	Hospital short-term inpatient mean (se)	Residential short-term inpatient mean (se)	Residential long-term inpatient mean (se)	Outpatient methadone mean (se)	Ambulatory (non-methadone) outpatient mean (se)
Under-25 group	(*N* = 20)	(*N* = 428)	(*N* = 691)	(*N* = 25)	(*N* = 524)
(*N* = 1688)	Mean (se)	Mean (se)	Mean (se)	Mean (se)	Mean (se)
Pre-treatment Robbery incidence:	1.920 (0.226)	1.920 (0.226)	1.920 (0.226)	1.920 (0.226)	1.920 (0.226)
Random assignment					
E (Change)[a]	—	−**1.640** (**0.251**)	−**1.624** (**0.230**)	—	−**1.322** (**0.282**)
(Social valuation of Robbery reduction)		$380 000	$377 000		$307 000
		Differences between E (Change) (row–cols) : (se)			
Residential short-term	—	—	—	—	—
Residential long-term	—	0.016 (0.132)	—	—	—
Outpatient methadone	—	—	—	—	—
Ambulatory non-methadone	—	0.318 (0.212)	0.302 (0.170)	—	—
	Hospital short-term inpatient mean (se)	Residential short-term inpatient mean (se)	Residential long-term inpatient mean (se)	Outpatient methadone mean (se)	Ambulatory (non-methadone) outpatient mean (se)
Over-25 group	(*N* = 241)	(*N* = 1227)	(*N* = 1289)	(*N* = 489)	(*N* = 1651)
(*N* = 4897)	Mean (se)	Mean (se)	Mean (se)	Mean (se)	Mean (se)

Overall (*N* = 6585)	**Hospital short-term inpatient mean (se) (*N* = 261) Mean (se)**	**Residential short-term inpatient mean (se) (*N* = 1655) Mean (se)**	**Residential long-term inpatient mean (se) (*N* = 1980) Mean (se)**	**Outpatient methadone mean (se) (*N* = 514) Mean (se)**	**Ambulatory (non-methadone) outpatient mean (se) (*N* = 2175) Mean (se)**
Pre-treatment Robbery incidence:	0.449 (0.065)	0.449 (0.065)	0.449 (0.065)	0.449 (0.065)	0.449 (0.065)
Random assignment					
E (Change)[a]	**−0.345** (**0.109**)	**−0.385** (**0.064**)	**−0.349** (**0.067**)	**−0.398** (**0.068**)	**−0.291** (**0.076**)
(Social valuation of Robbery reduction)	$80 000	$89 000	$81 000	$92 000	$68 000
		Differences between E (Change) (row–cols) : (se)			
Residential short-term	−0.040 (0.096)	—	—	—	—
Residential long-term	−0.004 (0.099)	0.036 (0.038)	—	—	—
Outpatient methadone	−0.053 (0.095)	−0.013 (0.032)	−0.049 (0.035)	—	—
Ambulatory non-methadone	0.054 (0.104)	0.094 (0.052)	0.058 (0.053)	**0.107** (**0.050**)	—

[a]Expected change based on ratio estimates in Table III and corresponding baseline estimates reported in this table.

Bold face indicates $p < 0.05$.