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Effectiveness of Pretrial Community-Based Diversion in Reducing Reoffending by Adult

Intrafamilial Child Sex Offenders

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INTRAFAMILIAL SEX OFFENDER RECIDIVISM AFTER DIVERSION

Abstract

To investigate whether diversion to a pretrial community-based diversion program reduced sexual recidivism in adult intrafamilial child sex offenders, 208 offenders assessed for treatment between 1989 and 2003 were monitored for periods ranging from 2.8 to 18 years. Participants accepted for treatment ($n = 88$) were compared with those declined ($n = 120$). After applying propensity score analysis to control for selection bias, Probit regression analysis and Cox proportional hazards regression were used to estimate rates of desistance between the groups. Although differences were not statistically significant, estimated rates of sexual reoffending were lower and time taken to sexually reoffend was longer in the diverted than the undiverted group. An overall effect size for treatment was large (OR = .52 [95% CI 0.18, 1.5]), and examination of the recidivists in the treatment group supported inferences of positive treatment effects. Limitations of this study are examined and future directions for intrafamilial sex offender treatment are discussed.

Keywords: diversion, intrafamilial sexual offenders, recidivism, community-based treatment, propensity analysis, sexual abuse.

44 proceed to determination if the child refuses to testify or becomes too distressed by cross-
45 examination to continue (Parkinson et al., 2002; Taylor & Gassner, 2010).

46 In light of these difficulties, many communities have explored alternatives to traditional
47 court procedures to better manage child sexual assault cases (Cossins, 2010). To deter repeat
48 sexual offenders, like their North American counterparts, Australian states have increased
49 sentences and supervision, expanded community notification and registration (e.g., Child
50 Protection [Offenders Registration] Act, 2001), and have established rehabilitation programs for
51 convicted offenders (Chung, O’Leary, & Hand, 2006; Lievore, 2004; McSherry, Keyzer, &
52 Freiberg, 2006; Wood, Grossman, & Fichtner, 2000). Rehabilitation programs operate in prison
53 and within the community (Beech, Craig, & Browne, 2009; Chung et al., 2006). This study
54 reports findings from data drawn from Cedar Cottage, a unique community-based pretrial
55 diversion program established in 1989 in Sydney, Australia, that offers treatment to intrafamilial
56 sex offenders, their victims and other family members. This treatment program was examined
57 because there exists a lack of information on intrafamilial offenders. Previous meta-analyses and
58 single case studies on the effectiveness of community-based treatment programs for sex
59 offenders, and intrafamilial sex offenders in particular, have produced inconsistent and equivocal
60 results (Collins, Peters & Lennings, 2009; Villettaz, Killias, & Zoder, 2006). The same
61 conclusion was reached over 20 years ago by Keller, Cicchinelli, and Gardner (1989) following a
62 survey of 553 child sexual abuse treatment programs in prison and community settings in the
63 United States.

64 **The dearth of information on intrafamilial offenders**

65 Comparatively little is known specifically about intrafamilial sex offenders as many
66 researchers have combined subtypes of sex offenders (e.g., rapists, extrafamilial child sex

67 offenders, voyeurs and exhibitionists) when measuring recidivism (Finkelhor, 2009; Kingston,
68 Firestone, Wexler, & Bradford, 2008; Stalans, 2004). This practice is problematic as subtypes of
69 sex offenders are known to differ in terms of offence characteristics (Craig, Browne, & Stringer,
70 2003) and recidivism rates (Harris & Hanson, 2004). For example, extrafamilial child sex
71 offenders tend to have more victims but offend only once or twice against individual victims,
72 while intrafamilial child sex offenders tend to have one or two victims but offend against them
73 repeatedly before apprehension (Brown & Brown, 1997; Smallbone et al., 2008). Other features
74 believed to differentiate intrafamilial sex offenders from rapists and extrafamilial sex offenders
75 are less use of violence or force while offending, less likelihood of a criminal record of sexual
76 offending, and cohabitation with a long-term intimate partner (Herman, 2000; Johnson, 2007).

77 In relation to sexual recidivism rates among subtypes of sex offenders, a meta-analysis of
78 ten follow-up studies of adult male sexual offenders (combined sample of 4,724), revealed that
79 overall recidivism rates (14% after 5 years, 20% after 10 years and 24% after 15 years) were
80 similar for rapists (14%, 21% and 24%) and the combined group of child molesters (13%, 18%,
81 and 23%) (Harris & Hanson, 2004). There were, however, significant differences between the
82 subtypes of child molesters, with the highest rates observed among extrafamilial boy-victim
83 child molesters (35% after 15 years) and the lowest rates observed among incest offenders (13%
84 after 15 years) (Harris & Hanson, 2004). Accordingly, researchers have recommended that
85 subtypes of sex offenders should be studied in discrete groups to better determine the specific
86 recidivism rates for each subtype of sex offender (Finkelhor, 2009; Stalans, 2004). The current
87 study reports on reoffending in a group comprised entirely of intrafamilial child sex offenders.

88 Inconsistent findings on prison and community-based sex offender treatment

89 Evaluations of cognitive behaviour therapy (CBT) treatment programs, the leading
90 treatment intervention for sex offenders (Chung, et al., 2006; Lievore, 2004), have demonstrated
91 that these programs reduce sexual recidivism rates. In a meta-analysis of 43 different studies
92 including over 9,000 offenders in North America, Canada, the United Kingdom and England,
93 sexual offence recidivism rates were significantly lower overall in offenders who received
94 treatment compared to those who did not (Hanson et al., 2002). In relation to sexual recidivism,
95 12.3% of treated offenders sexually recidivated compared to 16.8% of untreated offenders (odds
96 ratio = .81, 95% CI .70-.93) (Hanson et al., 2002). Within this meta-analysis, the therapies under
97 review operated from a combination of prison-based and community settings, and recidivism
98 rates were derived from national criminal justice records, state records, child protection records
99 and self reports, and included a follow-up period with a median of 46 months for both the
100 treatment and comparison groups (Hanson et al., 2002). Substantial variability in sexual
101 recidivism was detected across the studies ($Q = 145.02$, $df = 37$, $p < .001$) (Hanson et al., 2002).

102 Similarly, a larger-scale meta-analysis of 80 independent comparisons of a range of sex
103 offenders ($N = 22,181$) in institutional settings who were exposed to different types of treatment
104 interventions (CBT, psychosocial and organic treatment modes) yielded results showing an
105 overall positive and significant effect of sex offender treatment: 11.1% of the treated offenders
106 recidivated sexually compared to 17.5% of the offenders in the control groups (a mean odds ratio
107 of 1.70 for sexual recidivism) (Lösel & Schmucker, 2005). The mean OR of 1.70 for sexual
108 recidivism was highly significant ($z = 4.96$, $P < 0.001$). For the most part, intrafamilial
109 offenders were intermingled in samples containing other diverse types of sex offenders. In the
110 studies in which intrafamilial samples were included, the size of the samples was too small to

111 draw any definitive conclusions, a problem exacerbated by the low base rates typical in sexual
112 reoffending (Craig et al., 2003).

113 Some more recent single study evaluations of community-based sex offender programs
114 provided positive results. In a study of 175 offenders who were treated at one of the three
115 community-based programs in New Zealand (SAFE Network Inc, STOP Wellington Inc and
116 STOP Trust Christchurch), the overall sex offence recidivism rate for all clients who completed
117 one of the programs was 8.1%, compared with 16% (odds ratio = .51) in the nontreated
118 comparison group, and 21% (odds ratio = .39) in the Assessment-only group (Lambie & Stewart,
119 2003). However there was no significant difference in recidivism rates observed between these
120 groups within any one of the programs, presumably because of the diminished statistical power
121 when the data were not aggregated ($\chi^2(df = 2, N = 172) = 0.46$) (Lambie & Stewart, 2003).

122 Conversely, other recent single studies have shown no effect of treatment. For example,
123 adult male sex offenders convicted of a sexual assault offense (ranging from rape, indecent
124 sexual assault of a minor, sodomy, carnal knowledge and gross indecency) who undertook some
125 or all of the Queensland Sex Offender Treatment Program while in prison, and who were
126 released from custody between 1992 and 2001, were monitored for five years and one month
127 following release into the community (Schweitzer & Dwyer, 2003). The sample consisted of
128 445 men, 196 completers, 85 noncompleters, and 164 matched controls. No significant
129 reduction in sexual recidivism followed participation in the program: during the evaluation
130 period, 3.1% of completers, 7.1% of noncompleters, and 4.9% of controls were convicted of a
131 sexual offence ($\chi^2(3) = 2.223$; $p = .317$) (Schweitzer & Dwyer, 2003). The results were
132 compromised by extensive missing data and the use of reconvictions, a conservative criterion to

133 apply to measure relapses in sexual offending behavior given the above-mentioned difficulties in
134 securing convictions in cases of child sexual assault.

135 The foregoing examples illustrated some inconclusive and inconsistent findings from
136 treatment programs in prison and community-based settings in reducing the sexual recidivism
137 rates of adult sex offenders. The review also highlighted methodological shortfalls that pervade
138 treatment program evaluations, including difficulties in evaluating the impact of treatment across
139 different subtypes of sex offenders. The current study reports on sexual reoffending rates of
140 intrafamilial child sex offenders who were diverted from court to attend a single community-
141 based treatment program.

142 **Methodological deficits in past evaluations of sexual offender treatment programs**

143 Researchers in the criminological field have lamented the lack of scientific rigor and poor
144 methodological quality of many studies evaluating interventions designed to reduce sexual
145 reoffending (Woodrow & Bright, 2010). For example, fewer than 40% of studies included in the
146 previously-cited meta-analysis attained scores of three or more out of a possible maximum of
147 five on the Maryland Scale of Scientific Rigor (Lösel & Schmucker, 2005; Sherman et al., 1997).
148 The current study achieved Level Four on this scale.

149 A frequent criticism of methods applied to evaluate sex offender treatment programs is
150 the absence of controls for potential selection biases in the assignment of offenders to treatment
151 versus comparison groups (Rice & Harris, 2003). Selection bias is a difficult element to control
152 within studies evaluating sex offender treatment programs as in many instances participation in
153 treatment is mandated by a sentencing order (Chung et al., 2006). To overcome the documented
154 methodological shortcomings, the Campbell Collaborative Outcome Data Committee on Sexual
155 Offender Treatment Outcome Research specified a protocol (the Campbell Protocol) for sex

156 offender treatment program evaluations to increase their robustness (Beech et al., 2007). The
157 Campbell Protocol recommended the random assignment of participants to treatment and
158 comparison conditions to avoid selection bias, and where that is not possible, assessment prior to
159 treatment on risk-relevant variables, and matching on risk levels prior to assignment to treatment
160 (Beech et al., 2007). The current study met these constraints by means of propensity matching
161 analysis. The Campbell Protocol also recommended a follow-up period of a minimum of three to
162 five years to allow sufficient time to detect recidivism at a minimal level (Beech et al., 2007).
163 The current study included an extensive follow-up period.

164 **Purpose of this study**

165 Few studies on the effectiveness of community-based diversion programs that treat sex
166 offenders have included a scientifically rigorous methodology, and fewer have evaluated samples
167 of intrafamilial sex offenders. The aim of this study was to measure the effectiveness of a
168 community-based, pretrial diversion program that treats adult intrafamilial child sex offenders by
169 comparing rates of sexual recidivism in participants who were accepted for treatment (Accepted
170 group) with those of participants who were declined treatment (Declined group). To test whether
171 sexual recidivism rates were lower and periods of desistance were longer for participants in the
172 Accepted group versus the Declined group, a methodology that met the Campbell Protocol when
173 randomization was not possible was applied. The study addressed some of the methodological
174 shortcomings of previous research by use of: (a) retrospective propensity analysis to provide a
175 statistical control for selection biases in the Accepted and Declined groups, (b) a follow-up
176 period ranging from 2.8 to 18 years; and (c) a more inclusive measure of recidivism than
177 conviction rates (Payne, 2007).

178 **Cedar Cottage pretrial diversion program**

179 The community-based treatment program under review is based in Sydney, Australia
180 and administered by the New South Wales Department of Health. Known locally as Cedar
181 Cottage or the Pre-Trial Diversion Program for Child Sex Offenders (the Program) it treats
182 offenders who were in a parental role with a victim under 18 years of age at the time of the
183 offence(s).

184 Offenders charged with a sexual offence (indecent, indecent assault and penetrative
185 sexual assault) are referred to the Program by the NSW Police Force or the Director of Public
186 Prosecutions in accordance with the NSW Pre-Trial Diversion of Offenders Act (1985). To be
187 eligible for the Program, treatment candidates must be over 18 years of age, have no prior
188 conviction for a sexual offence, plead guilty to all intrafamilial sex offences with which they
189 have been charged, and the intrafamilial sex charges cannot involve overt use of force or
190 violence or have occurred in the presence of third parties. Offenders accepted into the program
191 must accept responsibility for their offending and demonstrate some insight into the impact of
192 their behaviour on the victim and the family by making statements about the offending
193 behaviour that match those provided to the police by the victim (Pratley & Goodman-
194 Delahunty, 2011).

195 Upon entry into the Program, a conviction is recorded against the offender's name and
196 registration of the sex offender proceeds under the Police Act (1990) and Child Protection
197 (Offenders Registration) Act (2001). Treatment lasts for two years, with an optional third year.
198 Offenders who do not qualify, who are not accepted into, or who breach the conditions of the
199 Program, return to court for traditional court procedures. The Program has the capacity to treat
200 25 offenders per year and an average of six family members per offender receive counseling to

201 strengthen relationships between the victim, the non-offending parent and siblings. Treatment
202 offered is based on CBT, narrative therapy, invitational practice, and insight-oriented therapy
203 with a strong emphasis on relapse prevention (Pratley & Goodman-Delahunty, 2011). Further
204 details about the diversion Program are available from Cedar Cottage at
205 <http://www.swahs.nsw.gov.au/services/cedarcottage/index.htm>.

206 **Method**

207 **Research design**

208 A retrospective quasi-experimental research design was applied in this study. The
209 treatment variable in the study had two levels: offenders accepted into the Program (coded as
210 '1') and offenders who were declined entry into the Program and experienced the regular court
211 process (coded as '0'). The Accepted group (n = 92) was defined on the basis of intention to
212 treat and comprised of participants who were offered a place in the Program without
213 distinguishing treatment completers from non-completers.

214 Of the 92 participants that comprised the Accepted group, 58% (n = 53) completed
215 treatment within the Program and 42% (n = 39) did not complete the Program (32 breached the
216 terms of the Program and 7 voluntarily withdrew from the Program before completion). The
217 Declined group (n = 120) were intrafamilial sex offenders who were referred to the Program, but
218 were declined treatment and attended traditional court and sentencing procedures. Participants
219 were declined entry if they did not meet the statutory criteria specified above or were deemed
220 unsuitable by the Program Director.

221 Before comparing the two groups on sex offending outcomes, propensity score matching
222 was conducted to control for known differences between the Accepted and Declined groups.

223 Groups were compared on outcomes related to their absolute risk of sexually re-offending, time
224 to sexually re-offend and the relative risk reduction in sexual offending.

225 While a sample of 212 intrafamilial sex offenders is relatively large in comparison to
226 prior studies of this sex offender subgroup, it is small in terms of evaluating the effectiveness of
227 a treatment program using quantitative methods. An analysis of the statistical power of the
228 research design was conducted (Rosner, 2006). Using an effect size consistent with that found
229 for similar treatment programs of a 6% point difference, with an alpha value of .05 and a sample
230 of 212, the power of the design was estimated as .45. This indicated a 45% chance of finding a
231 statistically significant difference when there is an actual difference of 6% between the treatment
232 and control groups, and is a low level of statistical power.

233 **Participant sample**

234 All 214 adult child sex offenders referred by the New South Wales Courts to the
235 Diversion Program between 1989 (the Diversion Program's inception) and 2003 were eligible
236 participants. A separate study drawing on the same sample compared 96 biological and 118
237 nonbiological fathers with respect to their demographic features, characteristics of the index
238 victim and index offence, prior offending and sexual, violent and nonviolent recidivism
239 (Titcomb, Goodman-Delahunty, & Waubert de Puiseau, 2011).

240 Two offenders were excluded from further analyses: one participant who died soon after
241 his last contact with the Program and one participant whose file lacked sufficient information
242 about variables relevant to this study. Therefore, analyses were conducted on a participant
243 sample of 212.

244 Of the participants, a relatively small proportion was Indigenous (6.6%, $n = 14$). The
245 remaining participants identified as Caucasian. At the time of the index offence, participants

246 ranged in age from 23 to 68 years ($M: 39.60$; $SD: 7.54$), with a majority (44.3%, $n = 94$) between
247 35 and 43 years of age. As displayed in Table 1, participants in the Accepted and Declined groups
248 were comparable on most demographic variables. Employment history was the sole variable that
249 differed more widely in the two groups: Accepted participants had a more stable employment
250 history than did Declined participants.

251

252

Insert Table 1 about here

253

254 Table 2 displays the proportion of each index offence-related characteristic in the
255 Accepted and Declined groups. On most index offence characteristics, the groups of participants
256 were similar; two characteristics revealed larger differences. Participants in the Declined group
257 were less varied in their range of sexual offending (committed only one type of sexual behavior,
258 e.g., sexual touching), and were more likely to have abused their victim(s) in an unplanned and
259 impulsive manner compared to participants in the Accepted group.

260

261

Insert Table 2 about here

262

263 Table 3 displays the prior offence records of the participant groups. Participants in the
264 Declined group had more extensive past offending rates than did Accepted participants.
265 Accepted participants were less likely to endorse sexual abuse myths than were their Declined
266 counterparts. Otherwise participants in both groups appeared similar in relation to prior
267 offending characteristics.

268

269

Insert Table 3 about here

270

271 **Data Sources**

272 **Diversion program records.** A database (the Program Database) containing a total of
273 116 variables for each participant offender was created by means of a manual audit of each
274 participant's case file (Goodman-Delahunty, 2009). The coders, 11 graduate-level researchers
275 affiliated with the second author, were double-blind to screen them from information regarding
276 participant recidivism. Interrater reliability was assessed by double-coding 25 randomly selected
277 cases and computing the Kappa statistic for the 60 dichotomous variables, 0.76, and Intraclass
278 Correlation Coefficients (ICCs) for the 56 continuous variables, 0.68. All Kappa statistics and
279 ICCs were significant ($p < .01$; one ICC was significant at $p < .05$).

280 The extracted data included demographic information, the offenders' criminal and violent
281 history and the type of victim of each sexual assault (adults, children, or both). Details of the
282 index offences (the number of incidents of abuse, the frequency and duration of offending, and
283 the number of charges each participant was facing at the time of referral to the Program) were
284 included along with the degree of sexual contact, the degree of planning or premeditation leading
285 to the offence, and whether threats, physical violence, or weapons were utilized. The offenders'
286 self-reported reasons for the abuse were noted, as were their employment and marital status at
287 the time of the offence. Victim details (gender, age, relationship to the offender, and the degree
288 of physical and psychological injury sustained) were recorded. Information contained in each
289 offender's file was used to derive a Static-99 score (Harris, Phenix, Hanson, & Thornton, 2003)
290 for each participant.

291 **Records of offenses.** Official records of offending by the participants were obtained
292 from three sources in New South Wales: (a) reports made to police, arrests, charges, and
293 convictions, were used to code type of offending conduct, specifically sexual offences (non-
294 contact sexual offences, indecent assault, and sexual assault), violent offences, and nonsexual
295 nonviolent offences before and after the participants contact with the Diversion Program. (b)
296 These data were cross-checked against records of offenses and outcomes maintained by the
297 NSW Bureau of Crime Statistics and Research. (c) Data were supplemented by NSW
298 Department of Corrective Services records of periods that any participants were incarcerated
299 (Goodman-Delahunty, 2009).

300 **Dependent Measures of Reoffending**

301 The outcome variable was sexual reoffenses determined by an official report to the NSW
302 Police. Reported sexual reoffenses were defined at the most inclusive level to include sexual
303 reoffenses with adults, extrafamilial and intrafamilial victims including contact and non-contact
304 offenses. Police reports were used in preference to arrest and conviction rates as report rates
305 remain verifiable, and were likely to capture with more accuracy the level of sexual reoffending
306 compared to arrest and conviction rates. While report rates offer a more accurate rate of sexual
307 reoffending compared to arrest or conviction rates, it is acknowledged that report rates may
308 include charges that are subsequently dropped (Gelb, 2007).

309 The follow-up period was operationalized as the number of days between the date of each
310 participant's final contact with the Program (on assessment completion, treatment completion,
311 breach or withdrawal), and 1 October 2007. Days when offenders were in custody and
312 unavailable to reoffend were subtracted based on information provided by the Department of
313 Corrective Services, to avoid inflating the desistance period. The follow-up period ranged from

314 2.8 to 18 years for the full participant pool ($N = 212$) with median follow-up times for the
315 Accepted Group and Declined Group of 4618 days and 3451 days respectively. Cox regression
316 was used to provide a test of treatment effect as it accounts for any impact of differences in
317 follow-up time between the groups (Prentky, Lee, Knight, & Cerce, 1997).

318 **Data Analysis**

319 Statistical procedures to compute the propensity score analysis were performed using
320 STATA IC11 Statistical Software (STATA) with an add-on procedure called psmatch2 that
321 enables propensity score matching, common support graphing and covariate balance testing. All
322 statistics were generated using two-tail tests at 95% confidence with α set at .05, unless
323 otherwise stated. Pre-test differences on categorical characteristics were calculated using
324 Fisher's Exact χ^2 test; differences on continuous characteristics were calculated using the Mann-
325 Whitney test.

326 **Procedure: Propensity Score Analysis**

327 Propensity score analysis is a popular technique to estimate causal treatment effects when
328 comparing groups in an observational study, while controlling for selection bias (Caliendo &
329 Kopeinig, 2005). Based on each participant's observed characteristics (e.g., pretreatment
330 demographic and clinical characteristics), statistical analysis is used to calculate the probability
331 of (or propensity for) each participant to receive treatment (Baser, 2006). This procedure
332 combines each participant's observed differences in characteristics into a single "propensity
333 score". Next, individual participants in the Accepted group are matched to individual
334 participants in the Declined group based on nearest matches of propensity scores (Rudner &
335 Peyton, 2006). After Accepted and Declined participants have been matched and covariate
336 balance established, the outcome variables can be compared--in this case sexual reoffending

337 rates and period of desistance from reoffending (Baser, 2006). Any differences in these outcome
338 measures will be attributable to the Program and not to any known and observed differences that
339 existed between the participant groups before the study began. For a full description of the
340 rationale and methods underlying the use of a propensity score analysis, see Caliendo and
341 Kopeinig (2005), Guo and Fraser (2010) and Joffe and Rosenbaum (1999).

342 Prior studies were reviewed to determine predictors of acceptance into treatment, and
343 factors contributing to sexual recidivism. The selected predictors were cross-checked against the
344 variables available in the Program Database. Sample sizes limit the number of variables that can
345 be included in logistic models (Weitzen, Lapane, Toledano, Hume, & Mor, 2004). Given a
346 sample size of 212 participants, it was not possible to include all potentially relevant variables in
347 the matching procedure. As such, only those variables that were identified as the most important
348 in the literature and that did not suffer from excessive missing data were considered for the
349 logistic regression model used in the propensity score matching analysis.

350 Using a combination of statistical significance and cross-validation approaches in an
351 iterative process (as specified by Caliendo & Kopeinig, 2005) a logistic regression model was
352 formed to generate a propensity score for each participant for selection into the Program. Unlike
353 a typical regression analysis where the goal is often to build the best predictive model, in
354 propensity score analysis, the building of the logistic model is guided primarily by the utility of
355 the model to create samples balanced across all the relevant covariates. As such, the variables
356 ultimately used in the logistic regression model as displayed in Table 4 are those that created the
357 best covariate balance across all available variables between the Accepted and Declined groups.
358 Several variables were recoded for use in the logistic model (participants' age at referral to the
359 Program, number of victims, number of index charges, employment history) either because some

360 cell sizes were too small, lacked a distribution approximating normality, or because the logistic
361 model performed better. Interactions between variables were not analyzed in this study.

362 Kernel matching (with a bandwidth of .06) was used to match participant cases in the
363 Accepted group to cases in the Declined group. To assess how well the matching procedure
364 balanced the distribution of all known relevant covariates in the Accepted and Declined groups,
365 the researchers considered descriptive statistics, t-test comparisons and Rosenbaum and Rubin's
366 (RR) standardized bias estimate (Rosenbaum & Rubin, 1985). There are, however, no clear
367 criteria in the literature to determine when balance is achieved, as common t-tests can be
368 misleading due to the impact of sample size and variability on significance. Since standardized
369 bias estimates take into consideration variability as well as difference, a standardized bias
370 estimate below 5% is regarded as sufficient to assume covariate balance (Caliendo &
371 Kopeinig, 2005).

372 After matching, sexual recidivism rates in the two groups were compared. The marginal
373 effect was calculated using probit analysis which increased the interpretability of the estimates
374 given the binary outcome measures in this research (Hill, 2008). Cox proportional hazards
375 regression (with failure rate) were performed to evaluate differences between the matched
376 groups in the time it took for participants to reoffend sexually within the follow-up period. Cox
377 regression was used rather than Kaplan Meier survival analyses due to need to include analysis
378 weights from the propensity score matching (PSM) procedure in the model. Consistent with the
379 Campbell Protocol recommendation (Villettaz, Killias, & Zoder, 2006) we also provided
380 estimates of the relative improvements in recidivism rates of the accepted versus declined groups
381 matched samples (Relative Risk Reduction or RRR). The RRR is the difference in event rates (in

382 this case, sexual recidivism rates) between two groups, expressed as a proportion of the event
383 rate in the untreated (Declined) group (Cook & Sackett, 1995).

384 **Results**

385 **Propensity score analysis.** Overall, the logistic regression model (Table 4) was
386 predictive of treatment, and pseudo- R^2 revealed that the equation explained 33.2% of variation
387 in the choice ($LR\chi^2 = 33.20, P < 0.01$). The logistic regression model fitted the data well
388 (Hosmer–Lemeshow test: $P = 0.77$), with reasonable discrimination (c statistic = 0.72). An
389 excellent discrimination of the propensity score model is not ideal as such a result could lead to
390 little or no overlap of the estimated propensity score between the groups, making it impossible
391 to conduct matching (Weitzen et al., 2004). The Kernel matching procedure left four participant
392 cases in the Accepted group that could not be matched to another participant in the Declined
393 group due to dissimilar propensity scores. These cases were discarded from the analysis,
394 leaving a sample of 208 cases: 88 in the Accepted group were matched to 120 in the Declined
395 group.

396

397 **Insert Figure 1 about here**

398

399 Figure 1 displays the percentage point differences for each variable between the
400 Accepted and Declined groups before and after matching, illustrating how pre-matching
401 differences were reduced in absolute terms. Provided in Appendix 1 are Rosenbaum and
402 Rubin’s standardized bias (RR) estimates and tests of differences before and after matching for
403 each variable. After matching, no differences between the Accepted and Declined groups were
404 statistically significant. In terms of RR estimates, all variables except four fell below the 5%

405 threshold suggested as sufficient by Caliendo and Kopeinig (2005). Of the four variables above
406 5%, all differences were relatively small (Rosenbaum & Rubin, 1985) with none above 6.8%.
407 The RR estimates for these variables were: binary Static-99 = 6.8%; poor employment history =
408 6.0%); revenge motivated offence = 5.6%; and intoxication at the time of offending = 6.5%.
409 Examination of the direction of these potential biases after matching, for all four variables,
410 revealed higher bias estimates after matching of the presence of these risk factors among more
411 participants in the Accepted than the Declined group, as shown in Figure 1. Thus, this analysis
412 suggested that in terms of known variables, if there was any remaining selection bias, the likely
413 result of this bias would predict more reoffending in the Accepted than the Declined Group.

414

415

Insert Table 4 about here

416

417 **Occurrence of sexual reoffending.** Table 5 displays a comparison of mean and
418 standard deviation scores for sexual reoffending by participants before and after matching.
419 After matching, results yielded a percentage point difference of 6.0 during the follow-up period,
420 as participants who were accepted into the Diversion Program sexually reoffended at a lower
421 rate (6.8%, $n = 7.1$) than participants who did not receive treatment and who experienced the
422 regular court procedures and sanctions (12.8%, $n = 12.9$). The estimated effect size of
423 treatment, expressed as an odds ratio, was .52 (95% CI [0.18, 1.5]). In other words, treatment
424 reduced the risk of reoffending by 50%. When the incidence of outcomes is rare in the study
425 population, generally interpreted to mean less than 10%, as is the case with sexual reoffending
426 rates, the odds ratio is considered a good estimate of the risk ratio. Probit analysis showed that

427 the magnitude of the reduction in sexual reoffending did not achieve statistical significance (SE
428 = 0.05, $p = 0.22$, CI = -0.15 to 0.03).

429

430 Insert Table 5 about here

431

432 **Period of desistance from sexual reoffending.** Cox proportional hazards regression
433 analysis with failure rate (see Figure 2) showed that after matching, at a median time of 3,468
434 days (9.5 years), the failure rate for reported sexual offences in the Accepted group was
435 approximately 7% compared to an approximate failure rate of 12% in the Declined group. The
436 difference in failure rates between the two groups was not statistically significant: HR 95% CI =
437 0.60 (0.21-1.69) $p = 0.334$. There was no evidence that the proportional hazards assumption was
438 violated: $\chi^2(1, N = 208) = 1.58, p = 0.21$.

439 **Relative risk reduction.** Computation of the RRR yielded an outcome of 47% (12.8 –
440 6.8/12.8 x 100) indicating that the Program is estimated to reduce sexual recidivism in adult male
441 intrafamilial child sex offenders by 47%.

442

442 Discussion

443 This study examined whether exposure to a CBT community-based diversion program
444 reduced sexual reoffending rates in adult male intrafamilial child sex offenders by comparing
445 reoffence rates in a group accepted for treatment with those in a group declined treatment. This
446 study also examined whether exposure to the Program extended the period of desistance from
447 sexual reoffending in either participant group. A common concern in evaluations of treatment
448 programs is whether unaccounted-for selection bias is responsible for observed estimates of
449 treatment effects, rather than the treatment program itself (Rice & Harris, 2003). Selection bias

450 was explicitly managed in this study by propensity score analysis. The standardized bias
451 estimates and percentage point differences between the participant groups on selected variables
452 before and after matching (see Figure 1) confirmed that propensity score matching successfully
453 created a comparison group similar to the Accepted group on the reviewed variables. The Kernel
454 matching procedure provided assurance that the two groups were well-matched on factors that
455 commonly influence sexual reoffending. Results of these analyses demonstrated that any
456 selection bias related to any of these variables was controlled. Therefore, observed differences in
457 estimated recidivism rates in the two groups can more confidently be attributed to the effect of
458 the treatment intervention in the Program.

459 **Reduction in Sexual Reoffending**

460 A comparison of the proportion of participants who sexually reoffended before and after
461 matching (a 6% point difference) indicated that participants who were accepted into the Program
462 sexually reoffended less than participants who did not receive treatment and who experienced the
463 regular court procedures during the follow up period. This result is consistent with previous
464 research outcomes documented in meta-analyses that evaluated treatment effects in more
465 heterogeneous groups of sex offenders (Hanson et al., 2002). Although the Probit analysis
466 showed the magnitude of the reduction in sexual reoffending did not achieve statistical
467 significance, the overall effect size for treatment was large. The lack of statistical significance
468 when evaluating a single sex offender treatment program is unsurprising (Craig et al., 2003;
469 Kingston et al., 2008) given the small number of reoffenders and the low rate of sexual
470 reoffending in this study (7% in the Accepted Group; 13% in the Declined Group), producing
471 low statistical power to detect the effects of treatment. The RRR of 47% implied that in a given

472 population, if 100 sex offenders are expected to reoffend, after diversion to Cedar Cottage, the
473 number likely to sexually recidivate will be reduced to 47, or just less than one half.

474 In conformity with the principle of Intent to Treat (Hollis & Campbell, 1999),
475 participants in the Accepted group included offenders who failed to complete treatment at the
476 Program. Of the 92 participants comprising the Accepted group (before propensity score
477 analysis was applied), approximately three-fifths (58%; n = 53) completed treatment and two-
478 fifths (42%; n = 39) withdrew or were breached from the Program before completion. Since
479 participants who drop out of treatment programs often recidivate at higher rates than do
480 treatment completers (Hanson et al., 2002), the reduction in sexual recidivism rates observed in
481 the current study are likely to underestimate the effectiveness of the treatment at the Program.

482 A more fine-grained examination of the seven participants in the Accepted group who
483 sexually reoffended within the follow-up period confirmed that the Program produced positive
484 treatment effects. Of the seven recidivists four had completed treatment while three breached or
485 withdrew before completion. Three of the four completers who sexually recidivated were
486 referred to the Program in the first few years of its operation before legislative and regulatory
487 amendments were implemented in April, 1993. One re-offence recorded in that period was a
488 charge of willful and obscene exposure eight years after treatment completion against an
489 offender with Paranoid Schizophrenia, who was observed walking naked in his yard a few hours
490 he learned of the death of his victim. No conviction ensued. After 1993, more extensive
491 assessment procedures were adopted by the Program. The report of an offence in the case of the
492 participant who completed treatment after the changes in 1993 was by an adult co-worker for
493 peeping and prying when she recognized his face outside her window. This conduct involved no
494 sexual contact, did not involve a minor, and no charge, arrest or conviction ensued. In all, four

495 of the seven sexual reoffences committed by offenders referred to the Program were non-contact
496 offences; three were penetrative sexual reoffences; two by offenders who completed treatment in
497 the first few years of the Program operation, one by a noncompleter. The overall recidivism data
498 showed that in the 14 years between 1993 and the end of the follow-up period in 2007, no
499 official police reports or convictions of any sexual offence against a minor were made regarding
500 any of the 34 participants who completed treatment in the Program.

501 As a whole, this study demonstrated the usefulness of propensity score analysis to
502 overcome selection bias and produce empirically sound findings on the effectiveness of sex
503 offender treatment programs where randomized trials are infeasible. These results demonstrated
504 that the Cedar Cottage Diversion Program reduced sexual reoffending in adult male intrafamilial
505 child sex offenders, and that community-based programs to rehabilitate low risk intrafamilial
506 child sex offenders can be effective.

507 **Limitations of the study**

508 As with all studies, certain limitations in this research must be acknowledged. Foremost
509 among these was a limitation in the available information about offenders due to the
510 retrospective nature of this study.

511 Reoffending rates in this research were derived from records maintained by authorities
512 within the state of New South Wales. Therefore any reoffending on the part of participants that
513 may have occurred outside of New South Wales was not recorded and included. This omission
514 may have led to an underestimation of recidivism. Propensity score matching cannot control for
515 unobserved selection bias (Guo & Fraser, 2010).

516 Other caveats include the fact that this research was conducted using a participant group
517 exposed to only one particular diversion program. While it is a strength of this study that all

518 offenders referred to the Program in the period 1989-2003 were included and monitored for up to
519 18 years, whether these findings may generalize to other diversionary programs or other
520 treatment program facilities is unknown. The delivery and efficacy of the Program and the
521 precise causes of the reduction in sexual reoffending observed at the Program remain unknown.
522 As time passed, the proportion of offenders referred to the Program who were accepted for
523 treatment increased. For example, in the period 2004-2008, of the 29 referrals, 8 were declined,
524 and 20 or 69% were accepted (Tolliday, D., personal communication July 7, 2008). With the
525 passage of time, the efficacy of the Program appeared to improve. For example, recidivism data
526 for all 138 offenders referred to Cedar Cottage in the period 1993-2004, of whom 64 were
527 accepted for treatment, revealed no official police reports or convictions of any sexual offence
528 against a minor by any of the 34 offenders who completed the Program. The turnover of staff
529 and legislative changes indicated that the quality of the implementation of the Program varied in
530 the years since its inception in 1989. These factors need to be investigated in future research.

531 **Future research**

532 As discussed, the areas of limitation have given rise to the recommendation of areas of
533 future research. Future research should also be implemented to address factors that fall outside
534 the scope of this study that go towards highlighting the success of the Diversion Program. For
535 example, at the Program, in addition to the rehabilitation of sex offenders, the family members of
536 each offender, including the child victim(s), receive counseling to come to terms with their
537 situation (Goodman-Delahunty, 2009). The design and implementation of the Diversion
538 Program also spares child victims the burden of testifying in legal proceedings and increases the
539 disclosure of further offenses which would otherwise have remained undetected (Pratley &
540 Goodman-Delahunty, 2011). A preliminary estimate of the cost per offender of attending the

541 Program was no different from the cost of traditional incarceration (Goodman-Delahunty, 2009).
542 These potentially positive effects should be analyzed in future research to add to the evaluation
543 of the effectiveness of the Program.

544 Offender motivation can be an important predictor of both treatment volunteerism and
545 treatment entry among incarcerated adult sex offenders who victimize children (Jones, Pelissier,
546 & Klein-Saffran, 2006). Reductions in sexual recidivism in participants who enter any sex
547 offender treatment program may reflect levels of motivation rather than the effectiveness of the
548 treatment program (Campbell, 2004). Further research is under way to assess the influence of
549 dynamic risk factors in this sample using The Violence Risk Scale: Sexual Offender Version
550 (Wong & Olver, 2010).

551 **Conclusions**

552 This study contributed to the growing body of evidence on practices for the successful
553 management and treatment of intrafamilial child sex offenders in the community, as shown by
554 the lowered sexual recidivism rates and increased desistance to sexual reoffending in the
555 offenders who completed this community-based pretrial diversion program. Given the
556 devastating impact that sexual abuse can have on child victims (AIHW, 2009a), and the dire
557 consequences that victims and their family members can face after disclosures of sexual abuse
558 (Taylor & Gassner, 2010), the reduction in the frequency of sex offending achieved by the Cedar
559 Cottage Diversion Program contributed dramatically to reduce considerable human suffering.
560 There were 3,875 substantiations of notifications of sexual abuse received during 2007-08 for
561 children aged between 0 and 16 years residing in New South Wales (AIHW, 2009b). If each
562 child was sexually abused by one offender, and those offenders had an expected sexual
563 recidivism rate of 12.8% as was observed in the group Declined treatment, some 496 children

564 would be sexually abused by these offenders. However if these potential reoffenders attended
565 Cedar Cottage, the estimates produced in this study suggested that some 232 of these 496
566 children would be spared from sexual abuse.

567 Strengths of the study included the reduced heterogeneity in sample composition by
568 focusing exclusively on intrafamilial sex offenders, a longer mean follow-up time than many
569 comparable studies, a broad official outcome source for to measure recidivism (police reports
570 rather than arrests or convictions) and a statistical control for selection bias between treated and
571 untreated groups. This study followed the Campbell Protocol and achieved Level Four on the
572 Maryland Scale of Scientific Rigor, securing its place among the small minority of studies (fewer
573 than 10% per Losel and Schmucker, 2005) that have sufficient methodological controls over
574 treated versus untreated groups to draw meaningful inferences about the impact of the diversion
575 program on sexual reoffending rates. The findings in this study contribute uniquely and
576 substantially to the Australian and international research literature on a subgroup of adult sex-
577 offenders about whom relatively little is known. These findings can inform legislators, policy-
578 makers, the judiciary, members of the legal profession and the general public of the potential
579 success of diversionary programs in managing sexual offenders in the community.

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- 725

726

Table 1.

727 *Demographic Characteristics of Participants in Accepted and Declined Groups (per cent)*

Variable	Accepted Group	Declined Group
Continuous variable (<i>m, sd</i>)		
Age in years	39.50 (7.13)	39.65 (7.90)
Categorical variables (<i>%, n</i>)		
Indigenous	Yes	5.40 (5)
	No	94.60 (87)
Employment*	Steady	73.30 (66)
	Sporadic	11.10 (10)
	Difficulty maintaining	8.90 (8)
	Unknown	6.70 (6)
Marital status at time of index offence	Single/never married	1.10 (1)
	Legal marriage	69.60 (64)
	Common law marriage	16.30 (15)
	Separated	8.70 (8)
	Divorce	4.30 (4)
	Widowed	-
	Unknown	-
History of violence against spouse/partner	No history	34.50 (29)
	Some history	38.10 (32)
	Extensive history	17.90 (15)
	Insufficient information	9.50 (8)

728 * $p < .05$

729

730 Table 2.

731 *Index Offence Characteristics of Participants in Accepted and Declined Groups (per cent)*

Variables		Accepted Group	Declined Group
<i>Continuous variables (m, sd)</i>			
Number of charges laid		5.88 (5.46)	5.52 (5.84)
Age of victim in years		8.70 (3.18)	8.96 (3.56)
Duration in years		3.13 (2.89)	3.31 (3.58)
No. of victims	Range = 1-5	1.23 (0.54)	1.23 (0.59)
<i>Categorical variables (% , n)</i>			
Relationship to victim	Biological child	46.74 (43)	43.33 (52)
	Nonbiological child	53.26 (49)	56.67 (68)
Gender of victim	Female	89.13 (82)	92.50 (111)
	Male	4.35 (4)	5.00 (6)
	Both female and male	6.52 (6)	2.50 (3)
Range of sexual offending*	Little variation	3.50 (3)	13.20 (12)
	Limited number of acts	24.40 (21)	34.10 (31)
	Variety of behaviors	70.90 (61)	49.50 (45)
	Unknown	1.20 (1)	3.30 (3)
Sexual contact	None	-	0.80 (1)
	Touching	16.30 (15)	15.00 (18)
	Penetration	83.70 (77)	84.20 (101)
Premeditation*	Unplanned/impulsive	1.10 (1)	2.50 (3)
	Some planning	20.00 (18)	32.50 (39)
	Clearly planned	77.80 (70)	60.80 (73)
	Unknown	1.10 (1)	4.20 (5)
Use of weapon	None	100.00 (92)	97.50 (117)
	Blunt object	-	1.70 (2)
	Unknown	-	0.80 (1)

732 * $p < .05$

733

734 Table 3

735 *Prior Offending by Participants in Accepted and Declined Groups*

Categorical variables (% , n)		Accepted Group	Declined Group
Static-99 scores	Zero level of risk	69.57 (64)	62.50 (75)
	Some level of risk (1-7)	30.43 (28)	37.50 (45)
Some prior offending*		46.70 (43)	60.80 (73)
Prior sexual offending		12.00 (11)	10.80 (13)
Tolerance of sexual abuse*	No endorsement of sexual abuse myths	62.00 (57)	13.80 (13)
	Agreed with some sexual abuse myths	31.50 (29)	50.00 (47)
	Endorsed many sexual abuse myths	6.50 (6)	36.20 (34)

736 * $p < .05$

737

738 Table 4

739 *Logistic Regression Model to Estimate Propensity Scores*

Variable	Categories	Coefficient	Standard Error	P > z	95% Confidence Interval	
Risk of sexual reoffending (Static-99 score)	Low/high risk	-.01	.36	0.99	-.72	.71
Indigenous	Indigenous	-.11	.65	0.87	-1.38	1.17
Employment history	Poor employment	.26	.56	0.64	-.84	1.37
	Steady employment	1.10	.49	0.03	.14	2.07
Age of participant	Under 36 years	-.19	.41	0.65	-.99	.61
	41 years and over	-.49	.38	0.20	-1.24	.26
Number of index charges	6 or more	-.05	.55	0.93	-1.13	1.04
	2 to 5	.28	.59	0.63	-.87	1.44
	1					
Number of index victims	1 to 5	-.40	.46	0.38	-1.30	.50
Any offending history	Yes	-.59	.35	0.09	-1.26	.09
Any sexual history	Yes	.29	.53	0.58	-.75	1.33
Female victims	Yes	-1.15	.60	0.06	-2.33	.05
Self-identified causes of sexual abuse:						
Do not know why abuse occurred	Yes	-1.09	.35	0.00	-1.78	-.40
Fulfill emotional need (love/loneliness)	Yes	.60	.33	0.07	-.052	1.26
Revenge	Yes	.48	.64	0.45	-.77	1.74
Rejection	Yes	-.47	.76	0.54	-1.96	1.02
Intoxication	Yes	.42	.39	0.28	-.34	1.19
Constant		.58	.94	0.54	-1.27	2.43

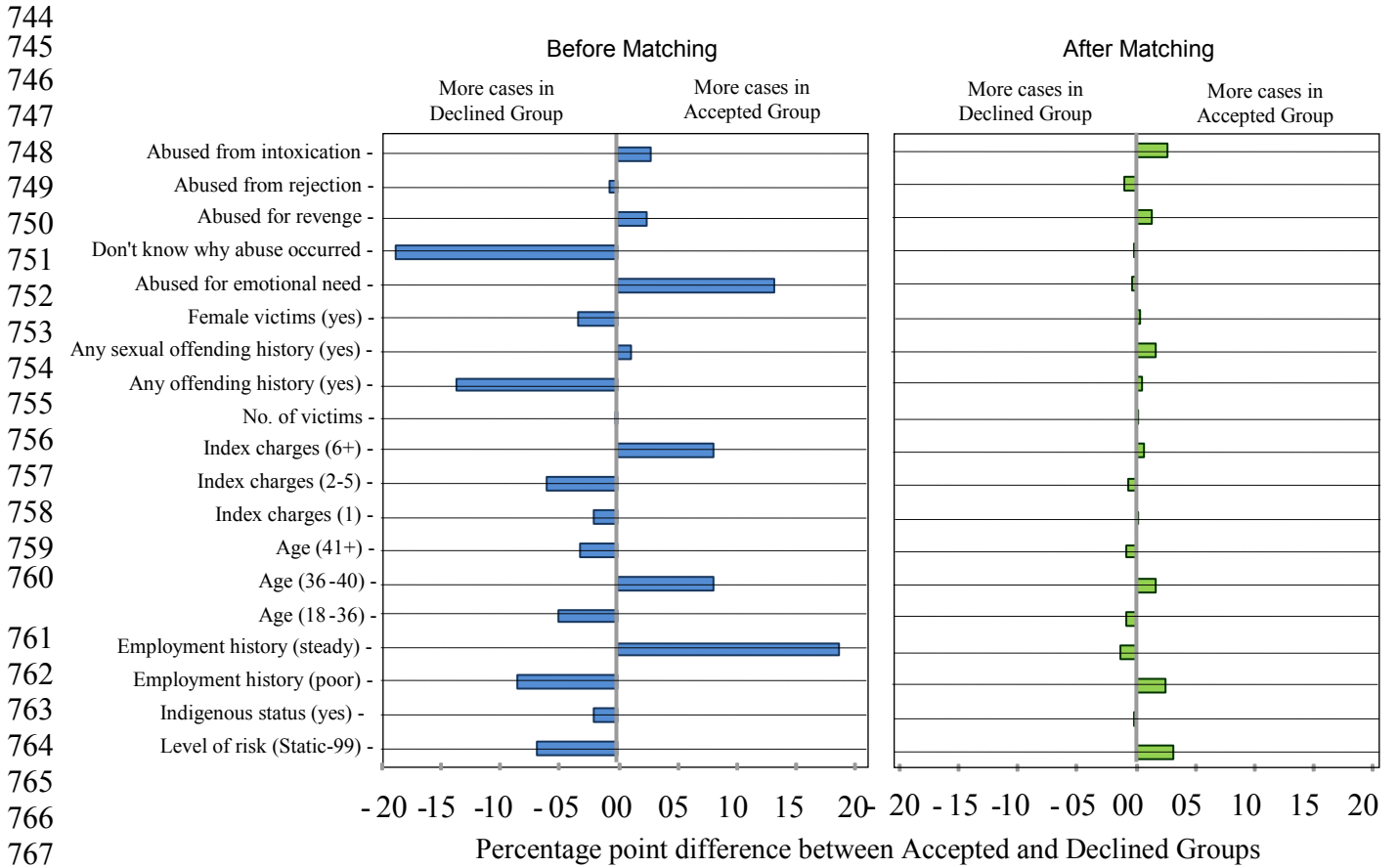
740 Table 5.

741 *Observed Rates of Sexual Reoffending 1989-2007 by Group Before and After Matching*

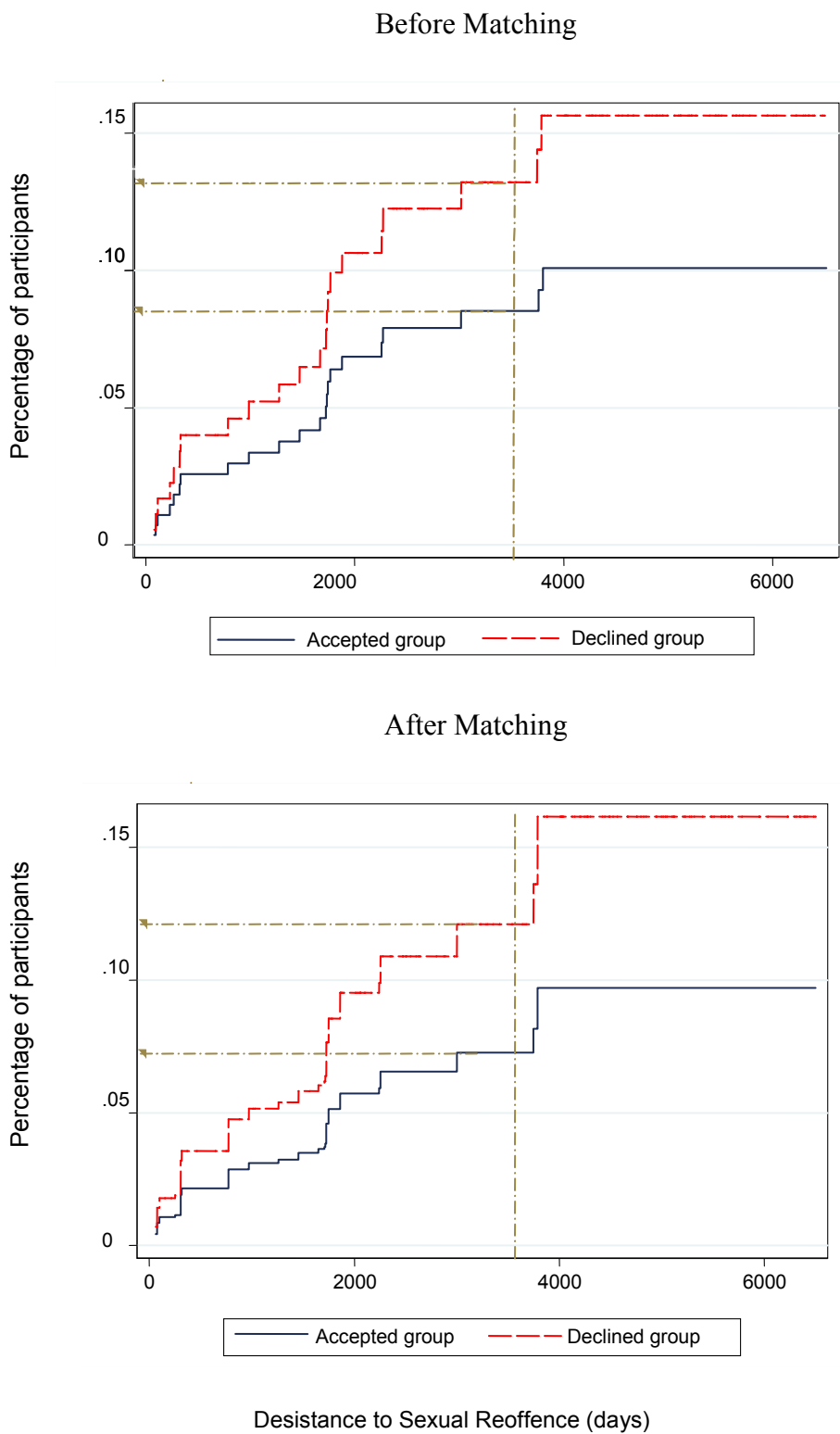
	Accepted Group (%, n)	Declined Group (%, n)	Percentage Point Difference (95% CI)	Standard Error	<i>p</i>	Odds ratio (95% CI)
Before matching	7.6 (6.0)	13.3 (16.0)	-5.7 (-13.9 -2.4)	.04	0.18	.48 (.18- 1.3)
After matching	6.8 (7.1)	12.8 (12.9)	- 6.0 (-14.6 -3.5)	.05	0.22	.52 (.18- 1.5)

742

743 *Figure 1. Covariate Balance Achieved Before and After Kernel Matching*



771 *Figure 2. Cox Proportional Hazards Analysis of Sexual Recidivism in Accepted and Declined Groups*
772 *Before and After Matching (1989-2007).*
773
774



Appendix 1. Variables before and after Propensity Score Matching

Variable	Before/after matching	Accepted Group <i>M</i>	Declined Group <i>M</i>	Bias (%)	Reduction in bias (%)	<i>t</i>	<i>p</i> > <i>t</i>
Risk of reoffending (Static-99 score)	before	0.30	0.38	-14.9		-1.07	0.29
	after	0.31	0.28	6.8	54.5	0.47	0.64
Indigenous status	before	0.05	0.08	-8.4		-0.60	0.55
	after	0.06	0.06	-0.4	94.6	-0.03	0.98
Poor employment history	before	0.20	0.28	-20.6		-1.47	0.14
	after	0.21	0.18	6.0	71.0	0.43	0.67
Steady employment	before	0.72	0.53	40.3		2.89	0.00*
	after	0.71	0.72	-2.8	93.1	-0.19	0.85
< 36 years of age**	before	0.28	0.33	-11.0		-0.79	0.43
	after	0.30	0.30	-1.9	82.3	-0.13	0.90
Age 36-40 years**	before	0.36	0.28	18.0		1.30	0.19
	after	0.33	0.31	3.7	79.5	0.24	0.81
Over 40 years**	before	0.36	0.39	-6.8		-0.49	0.63
	after	0.38	0.38	-1.7	75.2	-0.11	0.91
6 or more index charges	before	0.09	0.11	-7.2		-0.51	0.61
	after	0.09	0.09	0.1	98.8	0.01	0.99
2 to 5 index charges	before	0.52	0.58	-12.4		-0.89	0.37
	after	0.53	0.54	-1.5	87.8	-0.10	0.92
1 index charge	before	0.39	0.31	17.4		1.26	0.21
	After	0.36	0.37	1.5	91.3	0.10	0.92
Number of index victims	before	0.17	0.18	-0.3		-0.02	0.98
	after	0.18	0.18	0.3	-16.2	0.02	0.98
Any prior offending	before	0.47	0.61	-28.4		-2.05	0.04*
	after	0.48	0.47	0.9	97.0	0.06	0.96
Prior sexual offending	before	0.12	0.11	3.5		0.25	0.80
	after	0.13	0.11	4.9	-40.1	0.32	0.75
Female index victims	before	0.90	0.93	-11.6		-0.85	0.40
	after	0.91	0.91	1.0	91.0	0.07	0.95
Fulfill emotional need	before	0.40	0.27	28.9		2.10	0.04*
	after	0.39	0.40	-0.8	97.3	-0.05	0.96
Don't know/can't explain	before	0.21	0.40	-42.9		-3.05	0.00*
	after	0.22	0.22	-0.5	98.8	-0.04	0.97
Revenge	before	0.08	0.05	10.7		0.78	0.44
	after	0.08	0.07	5.5	48.4	0.34	0.73
Rejection	before	0.04	0.05	-3.1		-0.22	0.83
	after	0.05	0.06	-4.8	-57.5	-0.31	0.76
Intoxicated	before	0.23	0.20	6.9		0.50	0.62
	after	0.23	0.20	6.5	4.8	0.43	0.67

Note: * $p < .05$, independent samples t-test, two-tailed.

** Refers to age of participant at referral to the Diversion Program.