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A random study of Staff Training Aimed at Reducing Re-arrest (STARR): using core correctional practices in probation interactions

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The recent application of the risk–need–responsivity (RNR) model, in conjunction with core correctional practices, has offered promising results. In the present study, supervision officers were trained in core correctional skills and the RNR model. Supervision officers were randomly assigned to training groups and provided audio recordings of interactions with clients to assess their use of learned skills. The current study utilizes taped interactions between officers and offenders, individual-level offender data, and outcome/recidivism data to investigate the impact of the training regimen, which is the core focus of this paper. Trained probation officers demonstrated greater use of the skills taught during training and their clients had lower failure rates. These findings suggest that providing Staff Training Aimed at Reducing Re-arrest (STARR) training to community supervision officers can impact the officers' use of important correctional skills and improve client outcomes.

Keywords: probation; RNR; officer training; recidivism

Introduction

The process of supervising offenders in the community, while undergoing many shifts in both focus and emphases over time, has had at least one constant – to provide some form of 'control' over offenders in an effort to assist them in leading a productive life while staying out of prison or some other traditional punishment. The method by which this control is exhibited has changed, depending on shifts in philosophies of punishment, new developments in knowledge or technology, and changes in offender populations or sentencing strategies. Regardless of the method or form, however, the purpose of community supervision has been to keep offenders from recidivating while under supervision, and ideally create conditions whereby they will remain crime-free after the period of supervision has ended.

Several researchers, particularly in the latter half of the twentieth century, have examined probation and parole officer roles and functioning via a number of different methodologies and rubrics. Carlson and Parks (1979), for example,

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identified four major responsibilities of probation/parole agencies including surveillance, investigation, concrete needs counseling, and emotional needs counseling. Likewise, specific role typologies have been developed in light of particular emphases or orientations held by specific officers. According to some classic research these orientations – such as 'punitive', 'protective' (of offender and/or community), or 'welfare' (pro-offender) – in turn, and in theory, dictate the actual activities officers engage in when they supervise and interact with their charges (see Ohlin *et al.* 1956). Glaser later (1969) went on to extend these three orientations, adding a fourth that he termed 'passive', to typify the officer that does the least amount possible.

Another typology of supervising officers was offered by Klockars (1972) where he identified three types that included the 'law enforcer', suggesting an emphasis on rule following and meeting requirements, the 'time server', who does not do much at all, and the 'synthetic officer' who blends their emphases between 'law enforcements' and 'treatment'. Clearly the role of the supervising officer, and the actual activities they execute as part of their role, has been viewed as paramount.

The role and function of the supervising officer has been studied from a broader vantage point as well, via the examination of 'role conflict'. It has been hypothesized that supervising officers are unique in that they are asked to fulfill two different functions that may or may not be compatible with one another – specifically that of 'surveillance' versus 'treatment'. One study in particular concluded that role conflict was hardly rare across many different professions. Further, officers are not necessarily troubled by the existence of role conflict and figure out ways to fulfill first and foremost the agency's expectations regardless of their personal preferences or orientations (Clear and Latessa 1993, see also Paparozzi and Lowenkamp 2000).

Even more broad has been classic research that examines the function of probation and parole supervision as changing via several eras, such as the 'casework', 'brokerage' and 'justice' eras (see e.g. Wallace 1974). What distinguishes these eras may be what is considered to be the 'agent of change'. For example, the casework era of supervision considers the officer to be the agent of change and therefore largely responsible for actually providing part of if not all the intervention necessary for the offender to succeed. The brokerage era recognizes the administrative function of supervision, seeing the officer as a 'broker' who refers the offender to external programming, and as such, the program is most appropriately viewed as the agent of change. The justice era places the responsibility for change largely if not solely on the offender. For example, the officer may refer, the program may provide its best, but ultimately the offender is responsible for their own success or failure.

The current research endeavors to investigate what might be yet a new era in correctional supervision practice. Ever since the casework era, the responsibility for change on the part of the offender has been viewed as lying anywhere but with the officer. In many respects, this has been regarded as an appropriate view. With ever-increasing caseloads, as well as the administrative and surveillance functions that are required by supervision agencies, it seems unrealistic to assume that an officer with a relatively limited amount of face-to-face contact could have a realistic and palpable impact on offender behavior in the long term.

The current research examines officer behavior and its impact on offender recidivism. Specifically, we examine supervision practices – interactions between officer and offender – in an effort to determine whether or not certain skill sets can impact offender recidivism. Emerging research (e.g. Bonta *et al.* 2008, Andrews *et al.* 2011) suggests that the interaction between the officer and offender may impact

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recidivism rates in ways not previously considered. We further examine the importance of the face-to-face interactions between officer and offender.

Review of the literature

Community supervision is one of the most widely imposed criminal sanctions, with approximately 5,018,900, or 69% of the correctional population, being under community supervision (Glaze 2010). Despite its popularity, researchers have limited insight into whether community supervision actually reduces recidivism. The most recent reviews of the effectiveness of community supervision (Solomon *et al*. 2005, Aos *et al*. 2006, Bonta *et al*. 2008, Green and Winik 2010) produced sobering results. More than three decades after Martinson summarized the findings of his review of rehabilitation efforts by saying, 'with few and isolated exceptions, the rehabilitative efforts that have been reported so far have had no appreciable effect on recidivism' (1974, p. 25), Bonta *et al*. (2008) have found that more recent research yields no better results: the impact of community supervision is at best limited and at worst leaves clients more likely to recidivate (Lowenkamp and Latessa 2005, Lowenkamp *et al*. 2006a).

The Urban Institute reached a similar conclusion after analyzing the impact of post-prison supervision on re-arrest outcomes. The researchers, after comparing mandatory parolees with similar prisoners released without supervision, concluded 'overall, parole supervision has little effect on re-arrest rates of released prisoners' (Solomon *et al*. 2005, p. 1). Consistent with the findings of the Urban Institute, after tracking more than 1000 offenders randomly assigned to nine judicial calendars, Green and Winik declared 'probation does not alter the probability of recidivism' (2010, p. 381). Simply put, the recidivism rate of those placed on probation was no different from that of those who were not placed on probation.

Ineffective results may be the rule for intensive supervision probation (ISP) as well. Taxman reviewed studies of intensive supervision and caseload size and concluded that mere supervision did not reduce recidivism: 'unless the contacts are more than check-ins it is unlikely that they will impact outcomes' (2002, p. 17). Collectively, these reviews indicate that community supervision has limited impact on the likelihood of future crime. However, none of these studies examined exactly what occurs during meetings between officers and clients on supervision. To answer this question, researchers and practitioners have started to unpack the 'black box' of supervision (Bonta *et al*. 2008) to determine which factors impact client outcomes. Results suggest that to minimize recidivism, the core of community supervision should be built upon a foundation that targets those at highest risk of engaging in criminal behavior, the areas targeted should be closely linked to future criminal behavior, barriers to treatment should be removed, and cognitive-behavioral strategies should be utilized (Andrews *et al*. 1990). Likewise, research supports the notion that the quality and nature of the relationship between the client and the supervision officer have an impact on outcomes (Paparozzi and Gendreau 2005, Skeem *et al*. 2007). Finally, Jalbert *et al*. (2011) recently completed an analysis of the impact of caseload size on supervision outcomes, and concluded that caseload size can improve outcomes only if used in combination with the effective controlling and correctional strategies described above.

The idea of moving from a 'check-in' to the use of a core skill set to increase effectiveness is supported by Andrews and Kiessling (1980), Dowden and Andrews

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(2004), Trotter (1996), and Taxman *et al.* (2008). Andrews and Kiessling (1980) introduced the five dimensions of effective correctional practice that were designed to enhance the potential of rehabilitation programs for offenders: (1) use of authority, (2) role modeling/reinforcement, (3) problem-solving strategies, (4) use of community resources, and (5) relationship factors. Dowden and Andrews (2004) provided a meta-analytic review of the core correctional practices, indicating that the use of authority, disapproval, reinforcement, modeling, teaching problem-solving skills, and structured learning are all related to the effectiveness of correctional services. While much of the research reviewed by Dowden and Andrews focused on treatment programs, other research has examined the use of these skills in community supervision settings (Trotter 1996, 1999, Taxman *et al.* 2006). Looking at community supervision, Bonta *et al.* (2010) affirmed the relationship between specific core correctional skills and the effectiveness of supervision officers, noting that those officers trained in core correctional skills utilized the skills more often, and that their clients had lower recidivism rates than clients supervised by untrained officers.

The existing research (Trotter 1996, Taxman 2006, Bonta *et al.* 2008) is encouraging and points to a need for further research on the training of community supervision officers who provide direct service to clients. Controlled research has found that offering clients more than mere check-in supervision, as noted above, yields better results. For example, Taxman (2008a) found that a *proactive* community supervision model yielded significantly better results than traditional supervision strategies. Proactive community supervision initially involved use of the Level of Service Inventory (LSI) for case classification, building case planning that centers on criminogenic traits allowing for the appropriate targeting of needs, talking actively about triggers, using behavioral techniques (incentives and consequences), timely review of progress, and emphasis on desistance from a criminal lifestyle and conduct. Relational style has also been shown to matter when determining the effect of supervision interactions. Building positive rapport with clients has shown to have a beneficial effect on outcome and an enhancing effect on service delivery (Taxman 2008a, see also Taxman *et al.* 2004). The current study uses a larger sample size than previous studies. It investigates the application of techniques that influence change. The current study also uses random assignment to control for the selection and assignment biases associated with observational studies and the use of volunteer participants.

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Method

This study used an experimental pretest–posttest design. It measured the use of core correctional skills – Staff Training Aimed at Reducing Re-arrest (STARR) – by federal probation and pretrial services officers from 10 districts. It also measured the effects that this training had on the recidivism rates of the 1163 qualifying clients whom the trained officers supervised. Officers who volunteered for the training and study were randomly assigned to the experimental (trained) and control (untrained) groups, with 67% of the officers randomly assigned to the experimental condition and 33% assigned to the control condition. Two districts would not allow their officers to be randomly assigned. Analysis with and without those officers was conducted and revealed no differences in findings. Nonetheless, in order to maintain randomization the analysis reported in this article includes only those officers that

were randomly assigned. The unequal size of the groups resulted from our concerns that the experimental group would undergo greater amounts of case attrition, since we were asking the officers assigned to the training to learn and perform new skills. This concern turned out to be largely unfounded. Chief probation officers of the participating districts submitted a list of officers who volunteered to participate in the project. Randomized group assignment was achieved by utilizing each list and randomly assigning 67% of the list (in most cases) to the experimental group and 33% to the control group.

While officers were randomly assigned to the two conditions, clients were not. Client assignment was based on geography, officer rotation, caseload size, and other factors unrelated to the officers' assignment in this study.

Participants

Officers

Eighty-eight officers were initially included in the study: 53 in the experimental group and 35 in the control group. After the skills training, 21 officers withdrew from the study for various reasons. Of the officers that dropped out of the study, many did so due to a lack of organizational support for the supervision strategy. In addition, participation was voluntary; as such, there were no administrative directives in place requiring participation, increasing the likelihood an officer would compulsorily cease their involvement. We also lost eight additional officers that were not randomly assigned. The final number of officers in the study was 59, consisting of 38 in the experimental group and 21 in the control group. The withdrawal of the 29 officers produced an attrition rate of 33%, indicating the possibility of selection bias. This concern, however, is somewhat ameliorated because the final groups retain the same general proportion of officers and a pretest–posttest design was used (Posavac and Carey 2003).

Clients

Clients were identified for inclusion in this study based on when their period of supervision began. While clients were not randomly assigned to experimental and control group officers, clients were assigned to officers independent of the officers' STARR training status. The pretrial sample is made up of cases that were assigned to pretrial supervision. Pretrial services is the front door of federal criminal justice supervision. Pretrial services supervision aims to lay the foundation for success by effective supervision practices (Administrative Office of the US Courts 2009a). Pre-training cases were those cases that began pretrial supervision during 2007 and 2008 and terminated supervision prior to the training event date (19–22 May 2009). Post-training pretrial cases were those cases that were assigned to study officers after 31 May 2009.

The post-conviction sample is made up of cases that were assigned to supervised release. The term of supervised release is a dynamic period of community supervision. During this period, officers are to keep informed of the offender's status, consistent with the conditions of release and individual circumstances, and when necessary intervene with strategies that promote public safety and provide offenders with a meaningful opportunity for change (Administrative Office of the US Courts 2009b). Post-conviction pre-training cases were identified as those cases

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that began their supervision between 31 May 2007 and 31 May 2008. This allowed for a follow-up time of at least 12 months. Post-conviction post-training cases were those cases assigned for supervision after 31 May 2009 up until 21 December 2009.[1] For both pretrial and post-conviction samples, cases whose supervision overlapped both pre- and post-training periods were dropped from the sample.

In all there were 400 pre-training cases assigned to the experimental officers and 301 pre-training cases assigned to control officers. A total of 277 post-training cases were assigned to the experimental officers and 185 post-training cases were assigned to the control officers. Table 1 provides a breakdown of these cases by pretrial versus post-conviction supervision. As shown in Table 1, more clients were under post-conviction supervision than pretrial supervision.

Table 2 presents the demographic statistics of the clients. Sixteen percent of the sample was female; 59% belonged to a racial or ethnic minority. A little over half (51%) of the clients were moderate risk (score ranging from 3 to 5 out of 9 possible points) and 49% were high risk (score ranging from 6 to 9) according to the Risk Prediction Index (RPI).[2] The average client age was approximately 35. Table 2 also shows the descriptive statistics by group (experimental versus control). None of the observed differences were statistically significant at any level. Analysis of demographic characteristics by pre and post status and pre–post status by group (pre-training control group, post-training control, pre-training experimental, and post-training experimental) revealed no statistically significant differences. The demographic statistics for the sample differ from the full population of US probationers. The only potential meaningful difference may exist for racial or ethnic minority (59% of the current sample versus 48% in the US probationer population). In addition, the sample is approximately one-half point higher risk (for all cases that score 3 points or higher) as measured by the RPI score (compared to the total

Table 1. Distribution of pre- and post-training cases by supervision type.

	Pre-training				Post-training			
	Experimental		Control		Experimental		Control	
	N	%	*N*	%	*N*	%	*N*	%
Pretrial	52	13	56	19	30	11	26	14
Post-conviction	348	87	245	81	247	89	159	86

Table 2. Descriptive statistics for demographic characteristics of clients.

Variable	All ($N = 1163$)	Experimental ($N = 677$)	Control ($N = 486$)
Female (%)	16	16	15
Minority (African American and Hispanic) (%)	59	60	58
Moderate risk (%)	51	48	53
High risk (%)	49	52	47
Mean age	34.58	34.77	34.33
Mean RPI score	5.61	5.69	5.50

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population of US probationers). Despite these differences, due to the statistical equivalence of the experimental and control groups, threats to internal validity are minimal.

Procedures

This training was built upon the research about the effects of one-on-one officer–client interactions as a means to reduce risk and thereby client recidivism. It also drew upon the literature about technology transfer and the spread of beliefs, techniques, and methods.[3]

Officers in the experimental group participated in a 3½-day classroom training (19–22 March 2009) that included a discussion of the core correctional practices supporting the development of the STARR curriculum, a demonstration of each skill, exercises, and an opportunity for officers to practice each skill and receive feedback.[4] The conceptual discussion reviewed the RNR model (see Andrews and Bonta 2003) and the research demonstrating the effectiveness of a skill-focused supervision approach. The STARR skills themselves include specific strategies for active listening, role clarification, effective use of authority, effective disapproval, effective reinforcement, effective punishment, problem solving, and teaching, applying, and reviewing the cognitive model (see Trotter 1996, Andrews and Carvell 1998, Dowden and Andrews 2004, Taxman *et al.* 2004).

For each strategy, skill cards were developed that outlined the specific activities required to successfully deliver the strategy. A fundamental focus for each skill is the internalization of strategies so that clients begin to learn and apply the strategies on their own. In addition to the skill cards, video examples of some skills were presented; others were demonstrated live. The exercises allowed officers to practice each skill. For example, after listening to a discussion about reinforcement, officers were asked to identify a behavior and a reinforcement strategy for a specific offender, then role play that interaction with another officer.

Officers (experimental and control) were asked to submit audio-recorded interactions three months before the training. Several studies have revealed the utility of tape review (both audio and video) when determining the effectiveness of various training and/or technology transfer strategies (Miller and Mount 2001, Baer *et al.* 2004, Bonta *et al.* 2008). Officers were asked to provide one from before the training event and up to 30 recordings made after the training event, at three designated intervals: during the initial meeting with the client, during an interaction with the client three months later, and three months after that (a six-month interaction). Officers recruited clients to participate in the study and made recordings with up to 10 moderate- or high-risk clients, as measured by the RPI. Officers were asked to utilize the same offenders repeatedly. Clients were given the option of participating in the research project and were informed that they would be recorded. The audio recordings were employed to better assess skill development and provide feedback to the officers. Four voluntary 'booster' trainings for officers[5] were convened over the next year to provide officers with additional training on skills where deficits were identified on the tapes. Booster trainings were delivered by telephone and included discussion of specific skills, audiotape examples of the skill, and individual feedback and coaching. The booster sessions were typically each one hour long. Participation varied, ranging from 27 to 47 participants attending one of four available sessions during each booster training which were conducted via

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video-conferencing. A consort chart providing a visual display of officer and client assignment and attrition is contained in Figure 1.

Measures

Intermediate officer measures

Officers' use of the skills taught during the training was measured by reviewing audiotape recordings of interactions with clients recruited for the study. In all, 665 audio recordings were submitted for review: 446 recordings from the experimental group and 219 from the control group. Table 3 displays the number of tapes

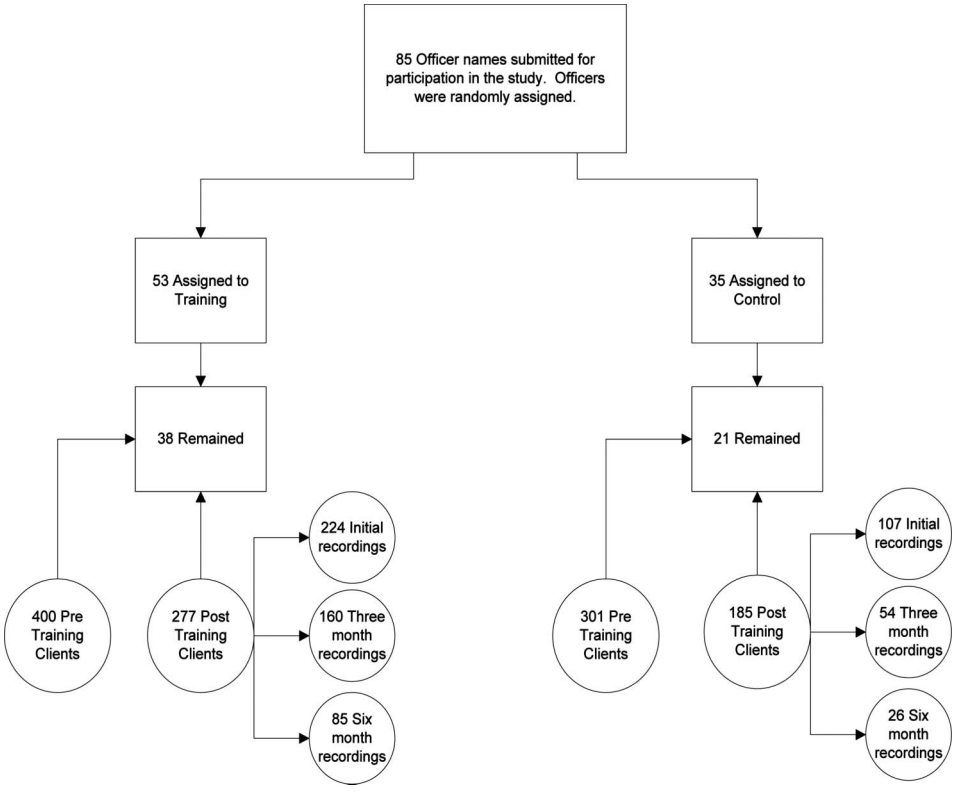

Figure 1. Consort chart indicating flow of research methodology.

Table 3. Distribution of recordings by group and timing of recording.

	Initial		Three month		Six month	
Group	*N*	%	*N*	%	*N*	%
Experimental	224	68	160	75	85	77
Control	107	32	54	25	26	23

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submitted by each of the groups of officers as well as the timing of the recording. The audio recordings were coded by trained raters,[6] who focused primarily on whether recorded behaviors were consistent with the skills introduced during STARR training. For example, when coding reinforcement or disapproval, raters evaluated whether the officer identified the specific behavior and led the client through the short- and long-term consequences of the behavior. For the cognitive model, raters coded whether officers taught, applied, or reviewed the model; in addition they coded for discussions of internal cues, consequences of internal cues, and identification of counter thoughts. Finally, raters coded what topics were discussed in the interaction. We note that all tapes submitted by officers were used in the analyses that focused on intermediate measures of skill acquisition and use. This was done because the voluntary nature of submitting tapes and of client participation in audio recording precluded any of the research benefits of random assignment.

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Client outcomes

Two different outcome measures were used in this study. For pretrial clients, the outcome measure was failure on supervision (as evidenced by failure to appear in court, revocation of supervision, or re-arrest for a new criminal charge while on pretrial supervision). We were not able to query the National Crime Information Center (NCIC) or Access to Law Enforcement Systems (ATLAS) databases for pretrial clients. For pretrial clients the time period was limited to the time they were on pretrial release. For the pre-training group, the average time on pretrial release was 229 days; for the post-training group, the average time on pretrial release was 185 days. The data for this measure was collected from the Probation/Pretrial Services Automated Case Tracking System (PACTS).[7] Because analyses of pretrial outcomes followed the same patterns as those observed with post-conviction outcomes, we combined the data and ran one set of analyses (see Lowenkamp *et al.* 2011). For post-conviction clients, the outcome measure was arrest for new criminal behavior as identified in the NCIC or ATLAS databases.[8] The follow-up time was standardized to 12 months for probationers.

Analysis

Initially, *t*-tests and chi-square analyses were utilized in order to determine whether or not the offenders assigned to the officers in the two groups were similar across a number of demographic characteristics and risk levels. We also conducted chi-square tests and calculated eta (a measure of association for ordinal scales involved in chi-square) to assess the existence and direction of the relationship between training (i.e., group membership – experimental versus control) and the use of STARR skills. We continued to use chi-square and calculate eta in order to assess the impact of STARR training on client outcomes. Finally, we conducted a series of multivariate logistic regression models to further investigate the nature of the relationships between group membership and outcome while controlling for client characteristics. In addition, multivariate logistic regression models were calculated with interaction terms testing the interaction of group assignment (experimental versus control), and pre–post training periods, as well as group membership, pre–post training periods, and risk level. We ran one additional set of multivariate models to control for

motivational interview training (for a description of motivational interviewing, see Miller and Rollnick 2002) which occurred in some districts prior to the STARR field experiment. Any time interaction terms are used there is concern about multicollinearity; however, multicollinearity between an interaction term and its components is not typically problematic even when high (Jaccard and Turrisi 2003). Nonetheless we calculated multivariate linear regression models to generate collinearity diagnostics. While it is inappropriate to use linear regression for dichotomous dependent variables, the collinearity diagnostics that result are considered valid as the diagnostics are concerned with the relationships between the independent variables and not the functional form of the dependent variable (Menard 2001). The variance inflation factors that resulted from the collinearity diagnostics were well below the threshold that indicates problems with multicollinearity (typically values over 4.00 indicate a need for further investigation while values near 10.00 indicate likely problems; none of ours reached a value of 3.00).

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Results

Length of interaction

One of the first analyses of interest involved the length of interactions after the training event. We calculated mean interaction times by group membership and by tape submission (initial, three month, and six month). Overall the average interaction for the control group was 18:04 minutes, while the average interaction for the experimental group was 20:31 minutes. The difference ($t(559) = 1.726$; $p = 0.085$) approaches significance at the $p < 0.10$ level, although the difference may not be practically meaningful. These analyses were conducted in order to demonstrate the potential that altering the content of officer–client interaction from 'standard' to STARR does not require a significantly greater amount of time. However, a different pattern emerges when examining differences in length of interaction at the three- and six-month time periods.

The next question was whether or not time taken to interact differed across groups by tape submission. These analyses indicated that pre-training and initial interactions did not differ significantly across group ($t(87) = 0.187$, $p = 0.852$ and $t(314) = 0.079$, $p = 0.937$, respectively); however, the three- and six-month interaction lengths did differ across groups ($t(156) = 2.971$, $p = 0.003$ and $t(83) = 2.257$, $p = 0.027$). The average length of the three-month interaction for the control group was 13:24 and the average for the experimental group was 17:52. The average length of the six-month interaction for the control group is 12:46 and the average for the experimental group is 20:20. Thus, interactions for the experimental group were significantly longer at the three- and six-month interval than were control group interactions.

We hypothesized that experimental officers had a better sense of what to 'do' with clients based on STARR skills, particularly when there were no major violations to address. For instance, when clients were in compliance, STARR-trained officers used the opportunity to positively reinforce pro-social behaviors, having the client articulate the short- and long-term benefits of compliance. Without the benefit of STARR training, the response of most officers to compliance is relatively superficial. Even in cases of violation, STARR-trained officers normally utilized the extra interaction time to fully explore the behavior, including using the cognitive model to analyze the thought process that led to non-compliant behavior. In contrast, control

group officers merely reminded the client about supervision conditions and reiterated the consequences of non-compliance. It is also important to note that, while slightly longer in duration, the interactions are still relatively short, averaging less than 20 minutes, suggesting that core correctional interventions can be used without requiring significant increases in resources. This finding is consistent with that of Bonta *et al*. (2008), who found that interactions averaging 20 minutes in length could produce significant reductions in recidivism.

Skill acquisition by officers

The 88 officers[9] participating in the study submitted 665 audio recordings for review. This included 446 (67%) from the experimental group and 219 (33%) from the control group. The range of tapes per officer submitted was 1 to 31. Half of the officers submitted five or more audio recordings. The overall mean number of audio recordings submitted was 7.47, with a standard deviation of 7.19. The average number of tapes submitted by a control group officer was 6.08 and the average number of tapes submitted by an experimental group officer was 8.42 (with standard deviations of 7.91 and 6.57, respectively). The audiotapes were coded by trained raters using a structured guide primarily focused on behaviors supported by core correctional practices. This analysis focuses on four intermediate variables: (1) the officer's use of role clarification at the initial contact; (2) the officer's use of reinforcement, disapproval, and effective authority; (3) interactions where cognitions, peers, or coping skills were discussed; and (4) the officer's use of cognitive techniques during interactions with clients.[10] Analysis of pre-training audiotapes showed no significant difference between the experimental and control groups in the use of these skills.

Table 4 displays the results of the analysis of role clarification at initial contact. We restricted the analysis of role clarification to the initial contact, as this skill is most likely to be employed during this contact, particularly since officers were trained to use role clarification in a formal and structured way during initial contacts. As seen in Table 4, officers in the experimental group used role clarification at the initial contact at more than twice the rate of untrained officers, which, theoretically, should enhance the collaborative nature of probation work (Trotter 1996). The difference between the experimental and control groups was significant and the directional measure, eta, indicates a moderate relationship between group membership and the use of role clarification.

Table 5 presents the percentage of interactions using reinforcement, disapproval, or authority by officer group membership and the sequence of the interaction (initial, three-month, or six-month taping). These skills were grouped together as they have been clustered together in the past and can be considered bridging skills (Lowenkamp *et al*. 2010), in that they can lead to enhanced relationships as well as

Table 4. Post-training use of role clarification during initial contact.

Group	*N*	%
Experimental	120	54
Control	23	22

Note: $\chi^2(1) = 30.362$, $p \leq 0.000$, $\eta = 0.303$.

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behavioral change. In this sense they are skills that bridge from relationship to intervention. Results show that the experimental group of officers was more likely to use one of these skills at each taping interval. The observed differences for initial and three-month tapings are statistically significant; however, the observed difference for the six-month taping did not attain statistical significance. In both the initial and three-month tapings, the experimental group was approximately twice as likely to use one of the skills; by the six-month taping, however, the group was only 1.25 times as likely.

Cognitions, peers, and impulsivity empirically represent some of the strongest predictors of future criminal behavior (Gendreau *et al.* 1996). A primary focus of STARR is addressing dynamic risk factors using a structured cognitive-behavioral approach. Table 6 shows post-training interactions where cognitions, peers, and impulsivity were discussed. As seen in Table 6, discussions about cognitions, peers, and impulsivity were significantly more likely to occur among officers in the experimental group than in the control group at each taping interval. Officers in the experimental group were significantly more likely to target primary risk factors than were officers in the control group. The chi-square values are significant for each taping interval and the values for eta indicate that the strength of direction of the relationship grew over time. Thus, as time progressed, the experimental officers were increasingly spending more time focusing upon those factors most likely to impact future recidivism. The control group officers also showed increases in the percentage of time during which top-tier risk factors were discussed; however, the experimental group officers outpaced the control group.

In addition to establishing a collaborative interaction, targeting dynamic risk factors, and using operant conditioning techniques, analysis suggests that officers in the experimental group were also significantly more likely to make use of the

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Table 5. Post-training use of reinforcement, disapproval, or authority by group and tape submission.

	Initial[a]		Three month[b]		Six month[c]	
Group	*N*	%	*N*	%	*N*	%
Experimental	63	25	52	49	25	42%
Control	17	16	13	24	8	31

Notes: [a]$\chi^2(1) = 5.916$, $p = 0.015$, $\eta = 0.134$. [b]$\chi^2(1) = 9.257$, $p = 0.002$, $\eta = 0.241$. [c]$\chi^2(1) = 0.969$, $p = 0.312$, $\eta = 0.110$.

Table 6. Post-training percent of interactions where cognitions, peers, or impulsivity were discussed by group and tape submission.

	Initial[a]		Three month[b]		Six month[c]	
Group	*N*	%	*N*	%	*N*	%
Experimental	77	34	56	53	39	66
Control	26	24	20	37	11	42

Notes: [a]$\chi^2(1) = 3.430$, $p = 0.064$, $\eta = 0.102$. [b]$\chi^2(1) = 3.578$, $p = 0.059$, $\eta = 0.150$. [c]$\chi^2(1) = 4.218$, $p = 0.040$, $\eta = 0.223$.

cognitive model. Table 7 displays the percentage of interactions where the cognitive model was applied by the officer. Note that there was only one interaction (an initial interaction) in which the cognitive model was used by control group officers. This finding is not surprising, since cognitive-behavioral theory and interventions are not typically taught as officer duties (Bonta *et al.* 2004). Rather, most officers view cognitive interventions as a 'treatment' issue and are likely to reserve the use of it for clients in need of mental health or substance abuse treatment. Most officers would expect treatment providers – not probation or pretrial services officers – to engage in cognitive interventions. In contrast, STARR teaches that the cognitive model explains all behavior, since any behavior is preceded by an individual's thoughts regarding the situation and how to respond. Thus, STARR focuses on a cognitive-behavioral analysis of any behavior, including pro-social behavior, so that clients begin to recognize that their thinking about situations impacts how they choose to behave. Experimental officers exposed to this training, while still using the cognitive model at a fairly low rate, showed an increase in the rate of use and used the skill more frequently than control group officers. This finding is critical, as similar investigations by Bonta *et al.* (2010) indicate that the use of the cognitive model in one-on-one probation interactions is the critical element in reducing recidivism through supervision contacts.

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Client outcome

We were interested in identifying the difference in the failure rates of clients assigned to the officers participating in the experiment. The first column in Table 8 indicates that there was no difference in client failure rates between the groups prior to STARR training; control group officers had a failure rate of 37%, while clients assigned to the experimental officers had a 36% failure rate. The second column in Table 8 displays the post-training failure rates of the clients based on group

Table 7. Post-training percent of interactions using cognitive model by group and tape submission.

	Initial[a]		Three month[b]		Six month[c]	
Group	*N*	%	*N*	%	*N*	%
Experimental	21	9	22	25	17	29
Control	1	1	0	0	0	0

Notes: [a]$\chi^2(1) = 8.314$, $p = 0.004$, $\eta = 0.158$. [b]$\chi^2(1) = 15.815$, $p \leq 0.000$, $\eta = 0.314$. [c]$\chi^2(1) = 9.364$, $p = 0.002$, $\eta = 0.332$.

Table 8. Failure rates for moderate- and high-risk clients by group membership.

	Failure rate	
Group	Pre-training[a]	Post-training[b]
Control	110/301 (37%)	62/185 (34%)
Experimental	145/400 (36%)	73/277 (26%)

Notes: [a]$\chi^2(1) = 0.006$, $p = 0.936$. [b]$\chi^2(1) = 2.749$, $p = 0.097$.

assignment. The control group cases had a post-training failure rate of 34%, which did not significantly differ from the pre-training failure rate. The post-training failure rate for the clients assigned to experimental group officers was 26%, which is significantly lower than the pre-training experimental failure rate, and more importantly, from the post-training failure rate for the control group.

The next set of analyses focused on whether these differences in failure rates differed between moderate- and high-risk clients. Table 9 presents the failure rates by group for the moderate-risk clients (RPI score ranging from 3 to 5). The first column indicates that the pre-training failure rates for the control and experimental groups were again statistically indistinguishable from one another (30 and 29%, respectively). Post-training failure rates between the groups indicate a very different trend. The post-training failure rate for the control group was 30% (similar to the pre-training rate), while the post-training failure rate for the experimental group was 16%.

Failure rates by group for high-risk clients are reported in Table 10. The first column in Table 10 indicates, once again, that the pre-training failure rates across the two groups did not differ significantly from one another. The second column in Table 10, which displays the post-training failure rates across the two groups, also indicates no difference between the experimental and control group. However, both groups reported a decrease in failure rates from pre- to post-training. This represented a reduction in relative risk of approximately 15% and was possibly the result of other ongoing training efforts in the districts selected for this study.[11]

Indeed, additional analyses suggest that the results for the high-risk group may be confounded by additional training received by some officers in regards to interpersonal communication, particularly an officer's ability to collaborate with the client (rather than being authoritative), connect in an empathic way, and evoke from the client thoughts and feelings regarding behavior. Research suggests that these skills can produce significant reductions in recidivism (Trotter 1996). Analyses of the high-risk group using only experimental officers trained in these additional skills show that failure rates actually decreased significantly, from 44% pre-training to

Table 9. Failure rates for moderate-risk clients by group membership.

	Failure rate	
Group	Pre-training[a]	Post-training[b]
Control	49/193 (30%)	28/94 (30%)
Experimental	58/199 (29%)	20/127 (16%)

Notes: [a]$\chi^2(1) = 0.036$, $p = 0.849$. [b]$\chi^2(1) = 6.262$, $p = 0.012$.

Table 10. Failure rates for high-risk clients by group membership.

	Failure rate	
Group	Pre-training[a]	Post-training[b]
Control	61/138 (44%)	34/91 (37%)
Experimental	87/201 (43%)	53/150 (35%)

Notes: [a]$\chi^2(1) = 0.028$, $p = 0.867$. [b]$\chi^2(1) = 0.101$, $p = 0.750$.

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26% post-training. In contrast, failure rates for the experimental group of officers not trained in these additional skills remained relatively consistent, from 43% pre-training to 44% post-training.

We hypothesize that high-risk clients are the most challenging group to engage, and that officers who become particularly skilled in interpersonal communication may be able to realize results with this group, when these skills are combined with the STARR interventions.[12] As a note, when pooled together regardless of officer training status, the failure rates for high-risk clients did significantly decrease from the pre-training (44%) to the post-training (38%) time period ($\chi^2(1) = 3.339$; $p = 0.068$). This finding might indicate, as speculated above, that there were other mechanisms implemented or in place that led to high-risk clients being identified and provided with services commensurate with risk.

Since the clients were not necessarily randomly assigned to officers, we thought it was important to construct and estimate a series of multivariate logistic regression models predicting client failure. The three models are presented in Table 11. The only difference across the three models is the addition of interaction terms. More specifically, Model 2 includes an interaction term between group membership and pre–post time period that isolates the effects for clients assigned to experimental officers after the training. Model 3 includes an interaction term between group membership, pre–post time period, and high-risk status. Thus the additional interaction term allows us to better understand the effects for moderate- and high-risk clients net the effects of other variables in the model. Table 11 includes the parameter estimates, standard errors, and p values for each variable as well as the model and step chi-square statistics.

In each of the three models, the parameter estimates for minority status and female are not significant, meaning once the other factors are controlled for, minority status and being female are not associated with failure. In all three models, age is inversely associated with failure and high-risk status is positively associated with failure, meaning older clients are less likely to fail and high-risk clients are more

Table 11. Multivariate analyses of client outcomes.

	Model 1[a]			Model 2[b]			Model 3[c]		
Variable	*B*	SE	*p*	*B*	SE	*p*	*B*	SE	*p*
Minority	0.165	0.137	0.229	0.165	0.137	0.228	0.169	0.138	0.219
Female	−0.299	0.190	0.117	−0.299	0.191	0.116	−0.297	0.191	0.120
Age	−0.043	0.007	0.000	−0.043	0.007	0.000	−0.043	0.007	0.000
High-risk	0.606	0.134	0.000	0.608	0.134	0.000	0.508	0.149	0.001
Pre–post period	−0.352	0.134	0.008	−0.120	0.203	0.556	−0.116	0.203	0.566
Group	−0.148	0.131	0.259	0.003	0.165	0.986	0.007	0.164	0.964
Pretrial	−0.066	0.194	0.733	−0.062	0.194	0.751	−0.071	0.195	0.714
Group × Pre–post	–	–	–	−0.405	0.270	0.133	−0.725	0.347	0.037
Group × Pre–post × High risk	–	–	–	–	–	–	0.506	0.335	0.131
Constant	0.614	0.299	0.040	0.541	0.303	0.075	0.587	0.305	0.054

Notes: [a]Model $\chi^2(7) = 78.078$, $p = 0.00$. [b]Model $\chi^2(8) = 80.333$, $p = 0.00$, Step $\chi^2(1) = 2.255$, $p = 0.133$. [c]Model $\chi^2(9) = 82.676$, $p = 0.00$, Step $\chi^2(1) = 2.343$, $p = 0.126$.

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likely to fail. In each of the three models the parameter estimate for group, which captured whether an offender was assigned to a control or experimental group officer, was not significant, which is to be expected as this variable estimates the likelihood of failure by group for both the pre- and post-training periods.

The parameter estimate for the pre–post period (coded as 1 for post-training period) is significant and negative in Model 1, indicating that failure rates after the training event were lower than before. However, once the interaction term between group membership and pre–post period is introduced in Model 2, the parameter estimate for the pre–post variable is no longer significant. This indicates that whatever was driving the effect of the pre–post period in Model 1 is attributable to the reductions in the post-training experimental group. Model 3 introduces another interaction term that quantifies the impact of the training on high-risk clients. As indicated, compared to moderate-risk clients, high-risk clients in the post-training experimental group are slightly more likely to fail; however, this parameter estimate was not statistically significant. Note that in Model 3 the parameter estimate for the interaction term capturing the post-training experimental officer caseloads is now significant. The results of the multivariate model are consistent with the bivariate, indicating that the STARR training had impact on moderate-risk clients but not much impact on high-risk clients.[13]

By converting the log-odds ratios into probabilities, we are able to develop a better understanding of a particular variable while holding the other factors constant. In doing so, we estimated the probability of failure to be at 0.32 for a 35-year-old, minority, moderate-risk male under post-conviction supervision and in the control group *prior* to training. A 35-year-old, minority, moderate-risk male who was in the experimental group *prior* to training had an estimated probability of failure at 0.32. A case with those same characteristics that was on an experimental officer's caseload *after* training had a probability of failure at 0.17, while a case with the same characteristics on a control officer's caseload *after* training had a 0.32 probability of failure.

One can quickly see from these calculated numbers that the cases on the experimental officers' caseload had roughly half the failure rate. The decrease was not as large for high-risk cases; however, it was still in the expected direction. In addition, the parameter estimate, which captures the post-training difference in effect for the high-risk cases, was positive (indicating an increase in the probability of failure and thereby a decrease in effect) but not statistically significant. Figure 2 provides a visual display of the predicted probabilities of re-arrest by risk and group assignment.[14]

Discussion and conclusions

This study used an experimental design to assign federal probation and pretrial services officers to STARR training or no particular training. STARR training was designed to improve the skills of officers regarding the form and content of their officer–client interactions during supervision. We measured officer training by a convenience sample of officer tapes documenting how they used the desired skills at various points in supervision. Although there are limits to our methods in this exploratory study of officer skills, the general findings are: officers utilized effective strategies more often following training and client outcomes were affected. Incidentally, control group officers did not participate in the 3½-day training but

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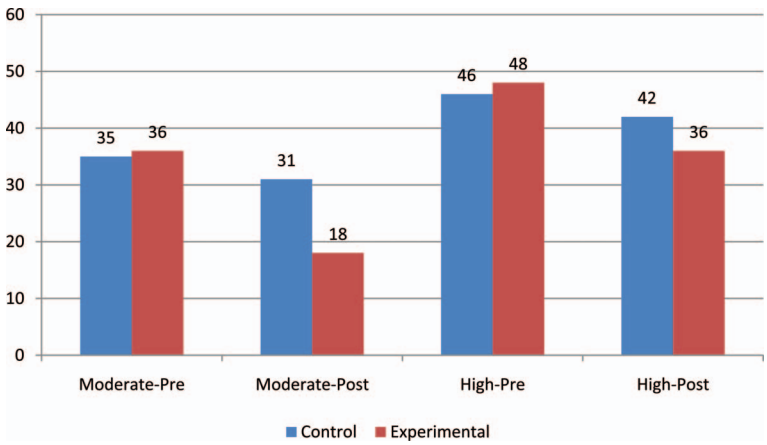

Figure 2. Predicted pre–post-training failure rates by group and risk.

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were asked to submit tapes for coding both before and after the training event. Of interest, the control group officers did increase their use of some of the skills. This could be a Hawthorne effect or the result of other training opportunities the officers had during the course of this study, or it could be the effect of some unknown factor. At any rate the growth in skill acquisition and use by the trained officers outpaces that of the control group officers.

As a whole, these results indicate that training can significantly impact strategies used by officers during supervision and that these strategies lead to lower failure rates. The overall difference in failure rates between the trained and untrained groups was nine percentage points, which equated to a reduction in relative risk of approximately 25%. This is remarkable, as these differences were achieved with approximately 40 total hours of training, no reductions in caseloads, and no additional work hours from officers.

The training impact may have plateaued at the six-month mark, or at this point perhaps the number of six-month submissions became so small that the percentages became somewhat unstable. Regardless, the findings from the initial and three-month interactions provide cause for optimism because of the demonstrated impact of operant conditioning techniques such as reinforcement and disapproval (Dowden and Andrews 2004). The data also demonstrate that officers can be trained to increase the use of these skills, but booster sessions (which in this study were voluntary and infrequently attended) may be necessary to ensure continued use of the skill.

As noted above, the absolute reduction of 14 percentage points and a relative risk reduction of almost 50% indicate effectiveness of the training model. Again, this is noteworthy given the amount of training the officers were given and the fact that the experimental group officers had no additional resources or reduced caseloads. The additional training did not impact moderate-risk clients; however, failure rates dropped significantly regardless of whether officers had these additional skills. Unfortunately, data collected do not allow us to fully understand what aspects of this additional training may contribute to more positive results. Clients supervised by the experimental group of officers after the training had better outcomes, even after controlling for individual client-level characteristics, indicating promise for

future efforts toward officer skill acquisition. A sub-analysis indicated that the effects of the experimental condition were absent for high-risk clients as a whole; however, when additional training factors were considered, high-risk clients did show improvement in failure rates. In addition, the failure rates for high-risk clients dropped eight percentage points when all data on high-risk clients was pooled and pre- and post-training time periods were compared

This study, a fairly rigorous test, indicated that training in STARR was associated with reductions in 12-month failure rates of nearly 50% in relative risk for moderate-risk clients. Although STARR-trained officers exhibited significantly greater use of core correctional skills, they still used such skills in fewer than 50% of their interactions with clients. We hypothesize that the impact on outcomes might be even greater if officers used these skills in a larger proportion of interactions with clients.

The overarching purpose of the current study was to assess the impact of officer training, skill acquisition, and skill execution on offender outcome. Given the experimental nature of officer assignment (randomized), the results of the bivariate analyses, and the multivariate models, we have concluded that officers who were trained in STARR incorporate strategies that are related to offender recidivism in a suppressing fashion. Comparing the experimental group to the control group, rates of offender recidivism differed as well. While it remains true that clients were not randomly assigned to officers, we wish to highlight the fact that there were no significant differences between the offenders who were administratively assigned to the officers (regardless of the officers' status – experimental or control). As a safeguard, we ran multivariate models controlling for offender demographics and risk levels, which revealed group membership (experimental versus control officer groups) as a significant predictor of offender outcome, while demographic variables of offenders and risk levels of offenders were not. The multivariate models also revealed the potential for an interaction between additional skills training and the skills associated with the STARR model. More research is necessary in order to determine to what extent this potential interaction should guide further research, and implementation of STARR.

There were a number of limitations to this study. First, again, clients were not randomly assigned to the officers. The districts involved in this study confirmed that client assignment was based on a combination of geography, case specialty (substance abuse specialists received a higher percentage of these types of clients), and attempts to balance caseload sizes. Clients did not differ in terms of measured individual-level characteristics across the groups, but some unmeasured bias in assignment could have existed. Second, a full analysis of the intermediate measures and their relation to client outcomes has not yet been conducted. These analyses might lead to important adaptations in future training and implementation efforts. Third, while there was an attempt to minimize attrition of participants, just over 30% of the officers who began the study withdrew or were removed before its completion. Fourth, the study was conducted by the same probation administrators who developed the training program and trained the officers. Fifth, as noted above there was some officer attrition. Previously we noted that we did not consider this to be a grave concern, since attrition was fairly even across the two groups under study (note: the eight officers not randomly assigned who dropped out were not part of the study). Equal attrition rates notwithstanding, we do not know potentially critical factors about the officers regarding years of experience, age, gender, race, or prior

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training, all of which may influence attrition and in turn skill acquisition. Sixth, much of the data on officer skills for the current study came from audiotaped interactions between officers and clients. The clients were recruited by the officers (i.e., asked to volunteer) and the nature of the clients may be different from general supervision. Likewise, there could be cultural differences across the different districts from which all the participants and in turn all the data came. While we acknowledge both these issues as potential limitations, we are confident that these factors cut across the randomization that was incorporated, at least partially mitigating these concerns. Given previous findings on the relationship between demonstration studies and real-world applications, independent and external replications of this study will be important.

Notwithstanding these concerns, there are some important policy implications to take from this study. First, it appears that officers can be trained in behaviorally-based skills and they can use them in their one-on-one interactions with offenders. The data presented on the intermediate measures provides support for this conclusion. More importantly, the bivariate and multivariate analyses of outcome measures provide support for the conclusion that training probation officers in behavioral strategies can significantly reduce failure rates of clients in the correctional system. Subsequent research should focus on continuing tests of this conclusion.

Acknowledgement

Opinions or points of view expressed are those of the authors and do not reflect the official position, opinion, or policies of the Administrative Office of the US Courts.

Notes

1. Criminal record checks for the post-conviction sample were run on 21 December 2010.
2. The RPI was introduced in 1997 (see Eaglin *et al.* 1997). The RPI uses eight mostly static items to predict the likelihood of re-arrest while on supervision (Lombard *et al.* 2001).
3. The research on diffusion of innovation and implementation is voluminous. Interested readers should see Rogers (2003) and Fixsen *et al.* (2005).
4. Control group officers did not participate in the 3½-day training but were asked to submit tapes for coding both before and after the training event. Of interest, the control group officers did increase their use of some of the skills. This could be a Hawthorne effect or the result of other training opportunities the officers had during the course of this study or it could be the effect of some unknown factor. At any rate the growth in skill acquisition and use by the trained officers outpaces that of the control group officers.
5. Participation in the booster training events was voluntary.
6. Raters were trained in the curriculum (either as part of this project or another unrelated project) and then asked to rate a number of tapes. Raters had to code the tapes consistent with the accepted coding as identified by one of the authors of this study.
7. PACTS is an electronic case management tool used by probation and pretrial services officers in all 94 federal districts to track federal defendants and offenders. At the end of each month, districts submit case data into a national repository that is accessible to the Administrative Office of the US Courts, Office of Probation and Pretrial Services.
8. Data on arrests were gathered from record checks that include a search of the National Crime Information Center and the National Law Enforcement Telecommunications System; together, these include data on federal and local charges.
9. The study began with 88 officers and asked each of them to submit a taped interaction during pre-training. In addition, analysis of intermediate outcomes included officers that were not randomly assigned as well as those that were randomly assigned, given the voluntary nature of the submission of audiotapes and the voluntary nature of client participation in the recording part of this study.

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10. Twenty-four tapes that were coded by both coders showed agreement in ratings on four key items (used in analysis), ranging from 66 to 100%.
11. All of the districts included in this study were part of the federal probation and pretrial services system's Research-To-Results project, which provided funding for districts to use assessment and services for higher-risk offenders (see Alexander and VanBenschoten 2008).
12. These findings are consistent with Trotter (1999) who emphasized that relational skills alone did not bring about change in offenders. Rather it was necessary to use intervention-based skills along with functional relational styles.
13. We also estimated separate models for moderate- and high-risk clients. The findings of those models are consistent with the trend and magnitude of the results reported in Table 11.
14. The only statistically significant difference between experimental and control groups was for moderate-risk clients for the post-training period.

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