AIDS Education and Prevention, 19(3), 258–273, 2007

REPLICATING A TEEN HIV/STD PREVENTIVE INTERVENTION IN A MULTICULTURAL CITY

Diane M. Morrison, Marilyn J. Hoppe, Elizabeth A. Wells, Blair A. Beadnell, Anthony Wilsdon, Darrel Higa, Mary Rogers Gillmore, and Erin A. Casey

Although there are now several adolescent HIV and STD preventive interventions of demonstrated efficacy in the literature, little is understood about the portability of these interventions. This study replicated Stanton's Focus on Kids intervention, developed for inner city African American adolescents, in a different population, transferring it to a multicultural city. Despite careful replication of the original study's procedures, youth in the preventive intervention condition of the replication study did not improve in attitudes, perceived norms, self–efficacy, or intentions toward sexual initiation, condom use, or abstinence compared with a carefully matched control condition. We discuss several possible reasons for this failure to replicate, concluding that the most likely reason is the lower rates of sexual activity among youth in the replication city.

Despite the widespread availability of condoms and a range of contraceptives, American adolescents continue to have high rates of sexually transmitted disease (STD) and unplanned pregnancy. Sexually transmitted infection (STI) rates are elevated among adolescents, with an estimated 9 million new infections among 15- to 24-year–olds in 2000, about twice the rate of older individuals (Weinstock, Berman, & Cates, 2004). Rates of some STIs have been demonstrated to be particularly elevated among young adolescents; for example, rates of Chlamydia are highest among females in the 15– to 19-year–old age range at about 2,800 cases per 100,000 (Centers for Disease Control and Prevention [CDC], 2005). Similarly, about half of new HIV infections are contracted by youth under 25 (Rosenberg & Biggar, 1998); given the long latency of HIV infection and diagnosis, many of these infections were likely contracted during the teen years. Rates of pregnancy are high, with 53 pregnancies per thousand among 15- to 17-years-olds in the 1990s, two to four times the rates of other developed counties (Darroch, Singh, Frost, & the Study Team, 2001).

Diane M. Morrison, Marilyn J. Hoppe, Elizabeth A. Wells, Blair A. Beadnell, Anthony Wilsdon, and Darrel Higa are with the University of Washington, Seattle. Mary Rogers Gillmore is with Arizona State University, Phoenix. Erin A. Casey is with the University of Washington, Tacoma.

This research was supported by a research grant from the National Institute of Child Health and Human Development (RO1 HD38420) and by a training grant from the National Institute of Mental Health (T32MH20010).

Address correspondence to Diane M. Morrison, PhD, University of Washington, 4101 15th Ave. NE, Seattle, WA 98105–6299; e-mail: dmm@u.washington.edu

These high rates of negative consequences of unprotected intercourse, and particularly concern about HIV/AIDS, have prompted the development of a number of sexual safety interventions targeted to adolescents. Intervening during adolescence offers promise for making significant changes in rates of sexual risk behavior. Intervening early is potentially more effective because it offers the opportunity to get youth "off on the right foot," as opposed to trying to alter behavioral patterns after they are set. Pubertal development naturally heightens adolescents' interest in sexuality, making them potentially more receptive to instruction than at older ages. Groups of adolescents are relatively easy to reach at such locations as schools, community centers, or after–school programs.

Not surprisingly, therefore, there have been an abundance of experimental tests of HIV prevention interventions in the past 2 decades (see recent reviews by Johnson, Carey, March, Levin, & Scott–Sheldon, 2003; Kirby, Laris, & Rolleri, 2005; Mullen, Ramirez, Strouse, Hedges, & Sogolow, 2002; Robin et al., 2004). All of these reviews agree that clinical intervention trials have demonstrated positive effects on condom use and contraceptive use generally, although the effects on other aspects of sexual risk taking are less consistent. Despite these promising findings, relatively little is known about the portability of these interventions. In their review, Kirby et al. (2005) discuss replications of four curricula, noting mixed findings: Similar effects were often observed, but changes in length, setting (urban vs. rural), or type of respondents (volunteers vs. school class) were often associated with failure to replicate outcome effects.

An intervention's portability is partly dependent on how much alteration is necessary to apply it in a different setting. Content and format are initially developed within a particular community and take into account the constraints and preferences of that community. This process potentially enhances effectiveness by maximizing cultural sensitivity and relevance. However, replicating any intervention requires adjustments to fit the new context, insofar as new sites vary geographically, ethnically, and culturally. Replications occur not only in different communities but also at a different time. The knowledge and skill deficits that the intervention is intended to address can change over time, necessitating updates or other alterations to didactic and experiential content, and the context within which the intervention is presented changes as a function of time, as well. These issues raise the question of how to determine how far and to what populations an intervention of demonstrated effectiveness can be generalized and still retain its effectiveness.

To begin addressing this question, the National Institute of Child Health and Human Development solicited applications and funded six studies to carefully replicate, with different populations, adolescent interventions that had been demonstrated to be successful in a controlled clinical trial. This paper describes one of these: a replication of Focus on Kids (FoK), an HIV–prevention intervention initially developed and tested by Bonita Stanton and her colleagues at the Center for Minority Health Research at the University of Maryland (Galbraith et al., 1996; Stanton et al., 1995; Stanton et al., 1996).

FOCUS ON KIDS

FoK was initially developed for African American youth in an inner–city community. It was delivered in housing project recreation centers in Baltimore, Maryland, to small groups of three to nine same–sex friends of age 9–15. All but one of eight weekly sessions was about 1.5 hours in length. Session 6 was a daylong retreat at a rural camp-

site, combining youth and facilitators from all of the groups. Sessions focus on one or more of the protection motivation theory (Rogers, 1983; Rogers & Prentice–Dunn, 1997) constructs. The initial session introduces a decision-making model, the SODA model, which teaches a four–step process of decision making (*s*top and state the problem, consider the *o*ptions, *d*ecide and choose the best solution, *a*ct on the decision). Other sessions focus on risks and values, information and decision-making skills, consequences of behavior, skill building, information about sexual health, and attitudes and skills for sexual health, respectively. The curriculum emphasizes decision making regarding a variety of risky behaviors, including sexual behavior, substance use, and violence, and used a variety of formats to deliver content (e.g., videos, games, discussions, role playing, etc.).

The intervention's efficacy was originally tested in a randomized experiment in which the FoK intervention was compared with a control group that watched films and videos about AIDS, AIDS prevention, and other risky activities (Galbraith et al., 1996; Stanton et al., 1996). A total of 383 youth participated in the study (206 who received the FoK intervention compared with 177 who did not), in single–sex groups. Sessions were led by two trained adult facilitators recruited from the community, one the same gender as the group, and most African American. The intervention's efficacy was assessed with computerized multicomponent risk assessment questionnaires at baseline, 6, 12, and 18, months postintervention. Questionnaires assessed risk behaviors in the previous 6 months, including sexual intercourse, unprotected intercourse, substance use (alcohol, cigarettes, and illegal drugs), violence (carrying guns, knives or bats, fighting), and truancy.

The FoK intervention resulted in higher rates of condom use and higher condom intentions among intervention than among control youth at 6 months, differences that were not sustained at 12 months. The intervention effect at 6 months was strongest among boys and among older (aged 13–15) rather than younger (aged 9–12) youth. Failure to maintain behavioral differences led Stanton and colleagues to add a "booster" sessions at 15 months, after which youth in the intervention group again had significantly greater condom use than those in the control group (Stanton et al., 1996).

Adaptations of FoK have also been tested in Namibia among 15- to 18-year-old youth (Fitzgerald et al., 1999; Stanton et al., 1998), in Vietnam among 15– to 20-year-olds (Kaljee et al., 2005), and in rural West Virginia among 12– to 16-year-olds (Stanton et al., 2006). In Namibia, overall differences in condom use did not occur, but intervention youth who had been sexually inexperienced at baseline were more likely than controls to remain inexperienced at 12 months. Among the youth who transitioned from being inexperienced at baseline to experienced at follow–up, condom use was higher in the postintervention period for intervention than for control youth. The intervention group showed greater increases in some cognitive variables targeted by the intervention, including knowledge, beliefs, intentions, and self–efficacy. In Vietnam, overall rates of sexual activity were low, so differences in actual behavior were difficult to detect. However, intervention youth had higher intentions to use condoms at postintervention and 6-month follow–up points than did controls and demonstrated differences in other cognitive variables. In rural West Virginia, no differences between intervention and control youth were observed in rates of sexual activity, condom use or condom intentions, although differences favoring the intervention were observed in some PMT variables (primarily coping appraisals).

Although the initial evaluation of the FoK intervention was positive, results have varied according to the population studied. It should be noted that the original and replication samples differed in cultural context, overall HIV risk in the population, age of participants, extent to which participants were sexually active at baseline and follow–up points and extent to which condom use was reported at baseline interview. It should also be noted that in keeping with its original development as a culturally tailored approach, FoK was tailored for the target population at each replication attempt. The control conditions used across studies also differed. As mentioned, in the original urban study, the control participants watched films and videos about AIDS. Group size, length of sessions, and number of sessions were not matched to the FoK intervention groups. In Namibia, a waiting list control condition was used in which participants were eligible to receive FoK after the 12–month follow–up. In Vietnam, rural communes appear to have been assigned either to intervention or control groups, and the control commune facilitators received FoK training after the 18–month follow–up. Control youth in West Virginia received an environmental health curriculum.

In this article, we describe our replication project, called Teens Take Charge (TTC). The project took place in Seattle, Washington, with an ethnically diverse population of 12– to 15-year-olds. Youth in the TTC project were similar to the youth in the original FoK study in that they are urban youth, and largely African American. However, they did not necessarily live in large subsidized housing projects or in close proximity to the other youth in their intervention groups as was the case in the original FoK study. They were more culturally heterogeneous than samples in any of the FoK studies described above: they include youth from a number of ethnic groups and with wide sociodemographic diversity. Our goal for the replication was to stay as close as possible to the original implementation of the intervention, except insofar as necessary to tailor material to our community and community centers, to examine FoK's replicability in this context.

METHODS

THE TEENS TAKE CHARGE INTERVENTION

Similar to FoK, the TTC intervention consisted of eight weekly sessions of about 2 hours in length. These sessions were followed by a 2–hour booster session at 9 months postintervention to review and reinforce the intervention material. Two trained adult facilitators, recruited from the community, led the sessions, which were conducted in same–sex groups. Facilitators followed a detailed manual, based on the FoK curriculum. Like Stanton and colleagues, we assessed the effects of the TTC intervention using a computer–administered questionnaire with audio assistance, with similar questions. Youth were assessed at baseline (preintervention), immediately following the 8 weekly sessions (posttest) and at 6– and 12–month postintervention follow–ups. The total length was similar to Stanton's, although distributed somewhat differently: For logistical reasons our weekly sessions were slightly longer and our booster session shorter. The booster session was held at 9 months to attempt to avoid the decay of intervention effects Stanton found between her 6– and 9–month follow-ups.

To adapt the original FoK curriculum to the needs of the African American, East African, and Southeast Asian communities, 10 focus groups were initially conducted with young people from the target audience in these communities. The focus groups identified issues that most concerned young people in these communities, and helped

identify attractive topics for the control group. Based on these focus groups, some material was dropped or deemphasized (substance use and drug sales), and some new material was added (information about STIs and HIV). We also made a few revisions to specific material in the intervention. The curriculum opens with a "family tree" exercise in which the group invents names and back stories for a hypothetical family that reflects the youth's family type. We revised this exercise to allow for a greater diversity of family types (e.g., immigrant families). We made changes in several places to use wording more inclusive of gay and lesbian youth, revised a story about a sexual assault to better reflect the situations our youth were likely to experience, and updated contraceptive information.

We designed a new control curriculum focused on career exploration, matched in length, style, and intensity to the HIV prevention curriculum, with similarly engaging exercises and activities. Control condition groups met for the same length of time and had equivalent attention from adult facilitators. Youth in the control condition received the same incentives as those in the experimental condition, and the control curriculum was equivalently appealing to target youth and to agency staff. Groups were randomly assigned to experimental or control condition content.

In both conditions, groups were led by two trained adult facilitators who had been recruited from the community. We chose adult facilitators (male and female) on the basis of prior experience, comfort with the topics, and recommendations. Facilitation staff received 20 hours of training focused on sexuality education/HIV prevention, the experiential learning cycle, adolescent development, cultural competency, experimental protocols, mandatory reporting, crisis intervention procedures, and practice of basic facilitation and cofacilitation skills. Facilitation staff held monthly meetings to discuss problems and issues that arose during groups and to cover updates to information such as contraceptives and new information about STDs. Occasionally, facilitation staff invited guest speakers to monthly meetings on topics such as cultural competency. Facilitators followed a detailed manual that provides specific instructions for the tasks and activities for each session.

Several strategies bolstered program attendance. Youth received reminder phone calls every week before the scheduled session, transportation both to the session and back home as needed, food at each session, and $2 for each of the sessions attended.

RECRUITMENT AND PARENTAL CONSENT

Staff recruited potential study participants from 20 community centers, youth programs, and after school programs in Seattle. Recruiters explained the study to groups of youth at each site. Interested participants took home an intake form that asked for parental/guardian contact information and whether consent materials should be sent in English or another language (consent materials were translated into six different languages). Study description and consent forms were mailed to parents, and follow–up phone calls and in–home visits were conducted as needed to answer questions. Parents who consented to their children's participation returned signed consent forms before any further contact was made with youth. Consent for all participants was verified in a phone call to the parent or guardian; bilingual consent gatherers checked with non–English-speaking parents by phone or in person, as needed, to verify consent. All study procedures and protocols were reviewed and approved by the university's Human Subjects Review Board.

STUDY PARTICIPANT ENROLLMENT AND CHARACTERISTICS

More than 2,023 youth received information about the study from TTC staff. Of those youth, 84% (1,701) expressed interest in the project and filled out an intake form giving us contact information to mail consent materials to parents or guardians. About 7% (115) of these youth were not eligible because they were not within age restrictions. Of the 1,586 youth who were eligible to participate, parents/guardians of 138 youth actively declined participation, 202 parents/guardians were contacted but had not accepted or declined participation in time for their children to participate, 613 parents/guardians could not be contacted by phone in time to participate, and 99 parents/guardians did not return written consent forms although verbal parental consent had been obtained.

We obtained written parental consent for 534 youth and, of those, 454 youth were able to participate in the study. A variety of circumstances kept the remaining 80 youth from participating. These included an incompatible time at which the group was scheduled, youth changing their minds about participating, and youth being unavailable to participate at the time of baseline data collection for their particular group. The 454 participants were assigned to 54 groups, which averaged 8 participants per group. Analyses are based on the 402 (89% of the 454) who completed research assessments at baseline, and 6– and 12–month follow–ups. They did not differ from the 52 people with missing follow–up data in terms of intervention assignment, demographic characteristics (age, gender, and race), whether they were sexually active, or rates of unprotected sex. However, the remaining sample may have been somewhat biased toward lower risk teens: the 52 with missing data showed statistically significantly higher risk on cognitive variables: specifically attitudes and self–efficacy concerning abstinence and condom use, intention concerning having sex and condom use, and social norms concerning abstinence, having sex, and condom use. We also found that, on average, those without missing data had attended a larger percent of the intervention sessions (78% vs. 65%).

The mean age of the 402 youth at recruitment was 12.7 (*SD* = 1.3) years old. All were between 12 to 15 years old at the time of the baseline survey (1 youth turned 16 by the start of groups). Eighty percent of youth were 12 or 13 years old; and the majority were in Grades 6, 7, and 8 (24%, 42%, and 27%, respectively). The sample was composed of slightly more females (63%) than males. Eighty–two percent of the sample was born in the United States: 37% of the sample were African American, 14% were Asian or Pacific Islander American, 16% were of mixed race; 9% were white, 2% were Latino, and 3% were of other races/ethnicities. Eighteen percent of the participants were born outside of the United States: 8% of the youth were African immigrants, 5% were Asian immigrants, 3% were Latino immigrants, and 2% were immigrants of mixed backgrounds or immigrants from various other countries.

DATA COLLECTION

Like Stanton and colleagues, we assessed the effects of the intervention using a computer–administered questionnaire with audio assistance, using similar questions. Baseline surveys were conducted at recruitment sites or nearby community schools or agencies. Prior to administering surveys, interviewers reviewed study details with participants, answered questions, and obtained written youth assent and locator information for follow–up data collection. Participants then completed self–administered computer–assisted baseline surveys. To ensure maximum privacy and comprehen-

sion, survey questions appeared on the screen while being delivered aurally through earphones by a prerecorded voice prompt. Postintervention surveys were completed immediately after the final intervention session, in the same way. Computerized follow–up surveys were completed 6 and 12 months after the end of the intervention at the intervention sites or, if the youth had moved, at their new location. Participants received incentives for completing each survey, $5 for the baseline survey; $10 for the posttest survey, and $15 for each of the two follow–up surveys.

INTERVENTION FIDELITY

We assessed the fidelity of the intervention and control curricula using three methods. First, facilitators tape–recorded all sessions. A research team member randomly selected a tape recording from each of the 54 groups, and two research staff first independently coded one as a check for reliability and then coded the remaining individually. Activities were noted as completed or not. Results from the tapes indicated 95% of all activities were completed. Second, facilitators completed a checklist after each session on which they reported whether each activity was completed and how well they covered the activity. Overall, facilitators reported that 98% of the activities were completed. Using a 4–point scale ranging from 1 being "poor" to 4 being "very well," facilitators felt they covered the activities with a mean score of 3.4. Finally, participants completed surveys after every session indicating whether each activity occurred during the session. Decoy activities were included in the checklist. Participants reported that 92% of intervention activities occurred.

MEASURES

Questions measuring behavioral and cognitive outcomes and other variables of interest were asked as part of the self–administered, computerized survey. Variables were reverse-coded, where needed, such that higher scores represent riskier behaviors and cognitions. All variables were set to have a lowest score of zero, which allowed us to model outcomes as Poisson distributions where indicated (that is, where distributions were highly skewed toward the low end of the scale).

Intentions. Three items measured intentions to have sex ("have sexual intercourse"), to be abstinent ("avoid [abstain from having] sexual intercourse"), and to use condoms ("use condoms if you have sex"). For each behavior, items began with the stems: "In the next 6 months do you intend to . . ." "In the next 6 months do you expect to . . ." and "In the next 6 months do you plan to . . ." For condom use, a fourth item, "In the next 6 months do you believe you will use condoms if you have sex?" was also asked. Response categories of "YES!," " yes," "no," and "NO!" were used for all items. Variables were constructed as the means of the items for each behavior. Alphas were .93, .91, and .95 for having sex, being abstinent, and using condoms, respectively. Responses ranged from 0 to 3.

Attitudes. For each behavior, respondents were asked: "Do you think if you had sexual intercourse/avoided (abstained from having) sex/used a condom if you had sex in the next 6 months it would be . . ." Responses, rated on 5–point scales, ranged from "very good" to "very bad," "very pleasant" to "very unpleasant," and "very nice" to "very awful." Means of the three items for each behavior were used as the attitude scores. Alphas were .95, .86, and .88 for having sex, being abstinent, and using condoms, respectively. Responses ranged from 0 to 4.

Norms. For each behavior, respondents were asked: "Do the people in your life (adults and other young people) think it would be OK for you to have sexual intercourse/avoid (abstain from having) sex/use a condom if you had sex in the next 6

months?" with responses ranging from "YES!" to "NO!." They were also asked, "Would the people in your life (adults and other young people) be for or against your having sexual intercourse/avoiding (abstaining from having) sex/using a condom if you had sex in the next 6 months?" with responses "They would be totally against it," "They wouldn't like it," "They wouldn't mind it," "It would be OK with them," "They would like it," and "They would be totally for it." A third item asked, "Would the people in your life (adults and other young people) approve or disapprove of your having sexual intercourse/avoiding (abstaining from having) sex/using a condom if you had sex in the next 6 months?" with responses "They would strongly disapprove," "They would slightly disapprove," "They would not approve or disapprove," They would slightly approve," They would strongly approve." Norms were computed as the item means for each behavior, and ranged from 0 to 4. Alphas were .81, .69, and .74 for having sex, being abstinent, and using condoms, respectively.

Self–Efficacy. Abstinence self–efficacy was measured with 10 items. Items all began with, "Do you think you would be able to avoid (keep from) having sex . . ." followed by: "with someone you have known for only a few days?," "with someone whose sex and drug history you don't know?," "with someone you have dated for a long time?," "with someone you want to date again?," "with someone with whom you have already had sexual intercourse?," "with someone you hope will fall in love with you?," "with someone who is pushing you to have sexual intercourse?," "after you have been drinking alcohol?," "after you have been smoking marijuana,?" and "if you really did not want to have sex?" Response categories were "definitely," "probably," "probably not," and "definitely not." The mean of these items was used as the measure. The resulting measure ranged from 0 to 3, and the alpha was .86.

Condom self–efficacy was measured with four items. The first asked, "Imagine that you decide to have sexual intercourse. Do you think you would be able to use a condom?" The responses included, "I definitely would," "I probably would," "I probably would not," and "I definitely would not." The other items were, "I could convince my girlfriend/boyfriend that we should use a condom even if she/he doesn't want to," "I could ask for condoms in a store," and "I could refuse to have sex if the other person will not use a condom." All used 5 point "strongly agree" to "strongly disagree" response scales. Items were averaged, and ranged from 0 to 4 with an alpha of .72.

Efficacy to have sex was measured with three items. "If I wanted to have sex in the next 6 months, I could find someone to have sex with." "If I wanted to have sex in the next 6 months, I could find a place to do it." "If I wanted to have sex, I could talk with my boyfriend/girlfriend about this." Responses were for each item were "definitely could," "probably could," "not sure if I could," "probably could not," and "definitely could not." The resulting scale ranged from 0 to 4, and alpha was .82.

Behavior. Participants were also asked about lifetime vaginal sex and condom use and vaginal sex and condom use in the past 6 months. (Participants were also asked about anal sex, but rates were too low to analyze.) "Have you ever had sexual intercourse (penis in vagina)?" and "In the past 6 months, have you had sexual intercourse?," both with responses of "yes" or "no" (coded as 1 or 0, respectively). Participants also were asked, "In the past 6 months, how often have you or the person you were having sex with used a condom when you have had sexual intercourse?" with responses "We've never used a condom," "We've almost never used a condom," "We've used a condom less than half the times," "We've used a condom about half the times," "We've used a condom more than half the times," "We've used a condom

nearly every time," or " We've used a condom every time." These responses were recoded as 0 (did not have sex or had only protected sex) and 1 (had at least one occasion of unprotected sex), owing to low rates of consistent condom use.

ANALYSIS STRATEGY

We analyzed the effects of the TTC experimental condition in two ways. First, we tested its effects relative to that of the career control condition on each cognitive and behavioral outcome cross–sectionally at the 6– and 12–month follow–ups. Second, to ensure that we did not miss any important effects and to estimate the effect of time, we performed repeated measures analyses. This included Condition, Time, and Condition by Time effects, the latter being the primary parameter of interest. To examine whether there were differential intervention effects owing to gender or immigration status, these were added as modifiers in supplemental cross–sectional and repeated measures analyses. Finally, we tested the direct effect of attendance, and whether it moderated the effect of experiment condition, on outcomes. For this, in the cross–sectional analyses we added the direct effect of the percentage of sessions attended and its interaction with condition.

As a supplement, we performed the cross–sectional and repeated measures analyses including only the African American participants. This allowed a comparison of the findings for this study to the original urban study being replicated for the types of participants included in that study and for whom the intervention had originally been designed.

We observed nonindependence owing to the nesting of participants within recruitment sites, with intraclass correlations ranging from .01 to .17, depending on the outcome. Because of this, we performed the analyses using mixed modeling techniques in which participants' intervention group assignment was specified as a random effect. We used SAS 9.1 PROC MIXED (for continuously distributed variables) and PROC GLIMMIX (for binomial and Poisson distributed variables).

RESULTS

PRELIMINARY ANALYSES

Intervention Attendance. Participants attended an average of 78% of sessions, with most (71%) attending at least 75% of the time. No differences were seen in attendance owing to gender, experimental condition, or the interaction of the two. Higher risk participants attended fewer sessions but only in the control condition. For them, percent of sessions attended was inversely associated with being a nonvirgin ($r = -.26$, $p < .001$), having had sex in the previous 6 months ($r = -.19$, $p < .05$), having social norms favoring having sex ($r = -.26$, $p < .001$), and having higher intentions to have sex (r. $= -.24$, $p < .01$) and to be nonabstinent ($r = -.17$, $p < .05$). Within the experimental condition, these correlations were near zero and non–significant.

Baseline Between–Condition Differences.

We found no significant differences on outcomes between conditions at baseline.

OUTCOME ANALYSES

Effects of Experimental Condition and Time. The results of the cross–sectional and repeated measures analyses were the same—experimental condition was not associated with differential outcomes. Tables 1 and 2 present descriptive information for cognitive and behavioral variables at each time point, by condition, and the associated statistics from the repeated measures analyses. As shown, in some cases variables

TABLE 1. Percent of Individuals Having Sex and Unprotected Sex, By Experimental Condition and Time Point

	Baseline		6–Month Follow–up		12–Month Follow–up		F_{df}		
	Control	Experimental	Control	Experimental	Control	Experimental	Condition	Time	Condition × Time
Had sex: Ever	12	10	20	21	28	29	$0.0_{1,400}$	$50.0_{2,775}$***	$0.6_{2,775}$
Last 6 months									
Had sex	08	05	10	12	16	16	$0.3_{1,400}$	$12.6_{2,797}$***	$1.2_{2,797}$
Had unprotected sex	02	01	05	02	05	04	$1.9_{1,400}$	$3.9_{2,800}$*	$0.9_{2,800}$

Note. Degrees of freedom differ owing to missing data. Higher scores represent more risky cognitions. * $p < .05$; ** $p < .01$; *** $p < .001$.

TABLE 2. Means and Standard Deviations of Cognitive Outcomes, By Experimental Condition and Time Point

	Baseline		6–Month Follow–up		12–Month Follow–up		F^{df}		
	Control	Experiment	Control	Experiment	Control	Experiment			
	M (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	Condition	Time	Condition × Time
Abstinence attitude[a]	1.1 (1.1)	1.2 (1.1)	1.2 (1.0)	1.1 (1.0)	1.3 (1.0)	1.3 (1.0)	$0.0_{1,398}$	$2.4_{2,398}$	$0.6_{2,398}$
Abstinence efficacy[b]	0.8 (0.6)	0.8 (0.6)	0.8 (0.7)	0.8 (0.6)	0.8 (0.6)	0.8 (0.6)	$0.0_{1,398}$	$0.3_{2,\ 398}$	$0.8_{2,\ 398}$
Abstinence intention[b]	0.9 (1.0)	0.9 (0.9)	0.9 (1.0)	0.9 (1.0)	1.0 (0.9)	0.9 (0.9)	$0.1_{1,400}$	$1.0_{2,789}$	$0.2_{2,789}$
Abstinence norm[a]	1.3 (1.3)	1.3 (1.3)	1.0 (1.3)	1.2 (1.3)	1.1 (1.3)	1.1 (1.3)	$0.0_{1,400}$	$5.1_{2,759}$***	$0.9_{2,759}$
Condom attitude[a]	0.6 (0.8)	0.5 (0.7)	0.7 (0.8)	0.6 (0.8)	0.7 (0.9)	0.6 (0.7)	$3.7_{1,400}$	$4.0_{2,756}$*	$0.2_{2,756}$
Condom efficacy[a]	0.6 (0.6)	0.6 (0.7)	0.4 (0.6)	0.4 (0.5)	0.4 (0.5)	0.4 (0.5)	$0.5_{1,400}$	$25.8_{2,773}$***	$0.7_{2,773}$
Condom intention[b]	0.2 (0.6)	0.2 (0.6)	0.2 (0.6)	0.2 (0.5)	0.2 (0.5)	0.2 (0.5)	$0.1_{1,400}$	$0.1_{2,770}$	$0.5_{2,770}$
Condom norm[a]	0.7 (1.2)	0.7 (1.1)	0.6 (0.9)	0.6 (1.1)	0.6 (1.0)	0.6 (1.1)	$0.1_{1,400}$	$2.4_{2,754}$	$0.2_{2,754}$
Having sex attitude[a]	1.9 (1.3)	2.0 (1.2)	3.0 (1.2)	2.0 (1.2)	2.0 (1.2)	2.2 (1.1)	$0.4_{1,396}$	$4.4_{2,396}$*	$1.1_{2,396}$
Having sex efficacy[a]	2.2 (1.2)	2.1 (1.2)	2.4 (1.2)	2.3 (1.3)	2.6 (1.2)	2.6 (1.1)	$0.4_{1,399}$	$31.2_{2,399}$***	$1.4_{2,399}$
Having sex intention[b]	0.6 (0.8)	0.6 (0.7)	0.7 (0.9)	0.7 (0.9)	0.8 (0.9)	0.8 (0.8)	$0.1_{1,400}$	$24.6_{2,783}$***	$0.3_{2,783}$
Having sex norm[a]	0.5 (0.7)	0.5 (0.7)	0.6 (0.9)	0.5 (0.8)	0.6 (0.8)	0.6 (0.8)	$0.1_{1,400}$	$6.4_{2,777}$**	$0.5_{2,777}$

Note. Higher scores represent more risky cognitions. [a]Scale ranges from 0 to 4. [b]Scale ranges from 0 to 3. * $p < .05$; ** $p < .01$; *** $p < .001$.

changed over time (toward greater risk), but these effects were not related to condition or its interaction with time. The overall rates of ever having had intercourse by 12 months follow–up, when most youth were aged 13 and 14, were 28% for experimental and 29% for control participants. This is notably higher than the overall national average for 15-year–olds, which is 16% (Alan Guttmacher Institute, 2002).

Modifiers of Efficacy. We found no interaction effects between experimental condition and either immigration status or gender. However, males typically showed overall greater risk than females. For example, at baseline males showed lower self–efficacy for abstinence ($F = 16.3_{1,330}$, $p < .001$); more risky attitudes toward having sex and abstinence ($F = 41.2_{1,331}$ and $F = 11.7_{1,342}$, respectively, both $p < .001$); higher intentions for having sex ($F = 25.8_{1,395}$, $p < .001$) and lower intentions for abstinence ($F = 28.2_{1,391}$, $p < .001$); and social norms more favorable toward having sex and less favorable toward abstinence ($F = 36.6_{1,393}$, $p < .001$; and $F = 8.4_{1,385}$, $p < .01$, respectively). Males were more likely to be nonvirgins and to have had sex in the previous 6 months ($F = 11.3_{1,339}$, $p < .0001$; and $F = 4.8_{1,346}$, $p < .05$). On the other hand, males showed greater self–efficacy for condom use ($F = 4.2_{1,390}$, $p < .05$). In terms of immigrant status, US–born participants showed more positive attitudes to having sex ($F = 5.2_{1,329}$, $p < .05$) while those born outside the U.S. showed lower intentions to be abstinent and social norms less favorable toward abstinence ($F = 4.2_{1,391}$ and $F = 5.4_{1,385}$, respectively; both $p < .05$). These baseline gender and immigrant differences were not moderated over time by experimental condition.

The percentage of sessions attended at first appeared to moderate the effect of intervention condition ($p < .05$) on condom self–efficacy ($F = 6.0_{1,337}$), sex norms ($F = 6.0_{1,336}$), being a nonvirgin ($F = 5.2_{1,338}$), and having had sex in the previous 6 months ($F = 6.7_{1,345}$). However, these interaction effects were non–significant when we controlled for the baseline value of these outcomes ($F = 0.8_{1,327}$, $F = 2.9_{1,329}$, $F = 2.0_{1,333}$, and $F = 3.5_{1,332}$, respectively). Hence, the effect appeared mainly to be owing to the fact that, as mentioned above, riskier teens attended fewer of the control than the experimental condition sessions.

Comparison to Original Study. Because these findings differed from the study being replicated (Stanton et al., 1996), we undertook two additional analyses. First, we performed cross–sectional analyses on all outcomes as above, but including only the 148 African American participants. No statistically significant differences owing to condition were seen.

Second, we compared rates of condom use among sexually active participants with available data at each time point to Stanton et al.'s (1996). Stanton's FoK trial had greater statistical power to detect differences in condom use given that more participants were sexually active (control condition $n = 31$ and 40; experimental condition $n = 40$ and 39; at 6– and 12– month follow–ups, respectively). Fewer participants in the present study were sexually active, especially at the 6-month follow–up (control condition $n = 23$ and 36; experimental condition $n = 18$ and 35; at 6– and 12– month follow–ups, respectively). Figure 1 shows that condom use rates were similar between studies. Stanton et al. found that condom use was statistically significantly different from preintervention rates only at the 6–month follow–up ($p < .05$). Although 6-month rates in the present study were similar to Stanton's, they were not statistically significant from pre–test rates ($\chi^2_{df=1} = 2.7$, $p = .10$).

DISCUSSION

In this study, we closely replicated the FoK HIV prevention intervention for early adolescents that was demonstrated to be successful by Stanton and colleagues (Galbraith

et al., 1996; Stanton et al., 1996; Stanton et al., 1995). Our goal was to determine whether the intervention was effective with a different population than the original sample of inner–city African American early adolescents. Our sample included a more heterogeneous group of African American, Asian American, and Asian and African immigrant youth. Using an experimental design in which our adaptation of the FoK intervention was compared with a career exploration condition, we found no differences on key outcome variables (sexual behaviors, intentions, norms, and attitudes) at either the 6– or the 12–month postintervention follow–up. Below we consider possible reasons for this unexpected result.

FAILURE TO REPLICATE THE INTERVENTION FAITHFULLY

There are many conceivable reasons that an intervention may not be replicated faithfully. Among the most important of these are changes in the critical elements of the intervention, inadequate facilitator training and monitoring, and failure to adhere to the intervention protocol (lack of fidelity), each of which is considered below.

It is impossible to replicate an intervention *exactly* as delivered originally and, at the same time, culturally adapt it for new populations. Based on input from our community members, we made small changes in some aspects of the intervention to make it more appropriate for the targeted youth. For example, our youths preferred the name Teens Take Charge to Focus on Kids. We adapted a few exercises (e.g., a family tree exercise) to better represent the target populations in our area, and we removed content on drug dealing because that is not a problem among the population sampled in our community. The changes necessary to adapt the curriculum for our populations were relatively minor and largely unrelated to HIV/AIDS. The only changes related to issues about sexuality were the expansion of basic STI and HIV information, updates (e.g., available methods of contraception), some changes to material related to sexual assaults, and some wording changes to make the curriculum more inclusive of gay and lesbian youth. These types of changes were similar to those reported in other settings (Fitzgerald et al., 1999; Kaljee et al., 2005; Stanton et al., 1998; Stanton et al., 2006). Moreover, the curricular content was virtually identical to that of FoK with the one exception noted. Key staff, including the project's clinical supervisor, received training in the FoK intervention from one of the original study's authors (J. Galbraith). Facilitators were extensively trained in the intervention, used a highly scripted manual, and met with the project director regularly to help ensure fidelity. In addition, the sessions were tape recorded and both facilitators and participants filled out check lists at the conclusion of each session indicating what content was and was not covered. Our analyses of these data indicate that the fidelity of the intervention was well preserved. Thus, we do not think the minor changes we made in the intervention or lack of adherence account for the results. Because there is little research on the essential components of HIV prevention interventions, however, we do not know what components are essential to retain effectiveness. This is an area worthy of continued research.

METHODOLOGICAL DIFFERENCES

There are three significant methodological differences between the FoK and TTC interventions that may have affected our results. First, groups of friends living in the same housing project were recruited to participate in the FoK intervention, and Stanton and colleagues believe that the success of the intervention was due in part to the fact that friends reinforced the intervention content. In contrast, we recruited individuals, rather than friendship groups, although some groups did indeed include

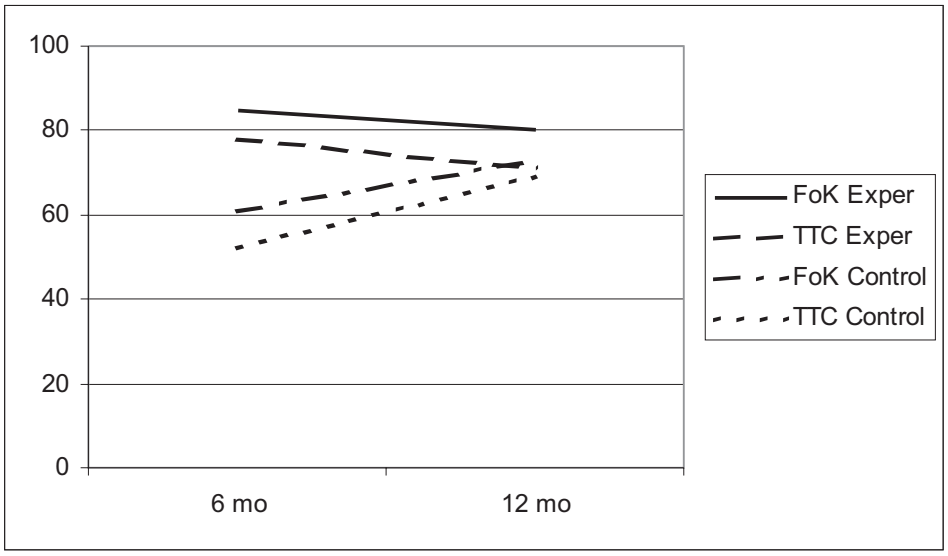

FIGURE 1. Comparison of findings concerning sexually active participants' condom use in the previous 6 months to Stanton et al.'s (1996) Focus on Kids study.
***Note*. Fok Exper = Focus on Kids experimental condition; TTC Exper = Teens Take Charge experimental condition.**

friends while others did not. We included a measure of friendship in our study and found no correlation between level of friendship in the groups and the outcomes (Beadnell et al., in press; Morrison et al., 2007), so we doubt that this difference is the reason for the failure to replicate. Stanton did not directly assess friendship, so we cannot compare friendship across the two studies. It is possible that friendship closeness was less prevalent or less salient in Seattle than in Baltimore, where youth likely saw the others in their friendship group more frequently, and so opportunity for reinforcement could have been greater.

Secondly, our control groups differed. Stanton's control group watched videos and films about AIDS prevention. We chose not to do this as we wished to control for Hawthorne effects by making the control group sessions the same number and length as the TTC sessions and equally interesting. These sessions contained no AIDS prevention or sexuality information. Participants' ratings of the two interventions (not presented) suggest that we succeeded in making the control group equally interesting. It is conceivable that the career exploration control group enticed youth to focus on their futures, which, in turn, may have affected their willingness to engage in riskier behaviors. We believe that our design represents a methodological improvement over that used in the FoK study because in the latter, Hawthorne effects cannot be completely ruled out as a reason for FoK intervention's apparent success.

Third, differences existed in characteristics of the recruited samples. For example, we narrowed the age range of the participants in our TTC intervention to 12–15 years. Stanton et al. (1996) had found that the FoK intervention was effective primarily among the teens and had little effect for younger youth (9-11 years old), so the exclusion of younger participants seems unlikely to be the source of different findings.

However, it is important to note that FoK's 12 to 15-year–old youth were more sexually experienced than the youth in our TTC intervention, making for differences in statistical power between the studies. At baseline and follow–up, 36% and 50% of the youth in the FoK study were sexually experienced, compared with only 11% and 29% of our youth. Among sexually active youth, Figure 1 shows that condom use rates among TTC HIV prevention and control condition youth were very similar to those in the original FoK study. Despite these similarities, the TTC results are based on a much smaller number of sexually active youth, with much lower statistical power, and therefore not statistically significant.

Despite condom use changes similar to Stanton's, we did not see any hints of the expected differences in the cognitive variables predicted to influence behavior; that is, intentions, attitudes, and perceived norms (including in supplemental analyses with just the sexually active subgroup). The underlying assumption of an intervention delivered before youth become sexually active is that the effects will become apparent when the youth are older, and become sexually active. Whether such differences would have occurred, or whether the information was presented too prematurely to have effects, we cannot know. Prevention research is challenging because we are trying to measure something that has not yet occurred. Therefore, unusually long follow–up periods may be necessary to detect effects. Agencies such as the Centers for Disease Control and Prevention and National Institute of Health that fund prevention research need to understand this dilemma and find ways to fund longer term longitudinal studies.

DOSAGE EFFECTS

There were marked differences in the dosage rates of the two studies. In the FoK study, 75% of youth attended at least one session, but only 40% of the youth attended five or more sessions. In contrast, 97% of the TTC participants attended at least one session and 82% five or more sessions. Our participants received higher "doses" of the intervention by virtue of attending, on average, more sessions than the FoK participants. This would lead one to expect a stronger effect, rather than no effect, ruling it out as the source of our nonsignificant findings.

HISTORICAL AND COMMUNITY DIFFERENCES

FoK was launched nearly 15 years ago, and there have been significant changes in HIV/AIDS education since that time. AIDS prevention information is widely available, we know more about what is effective and what is not (Kirby et al., 2005), and the largest school district in our region requires HIV/AIDS prevention education beginning at the 5th-grade level. Our lower rate of sexually experienced youth may suggest that these campaigns have met with some success, and that there is less risky behavior to change among younger adolescents who have been exposed to this content since 5th grade. It is notable that Stanton and her colleagues were unable to replicate the results of their original study with a different population in West Virginia (Stanton et al., 2006). They faced, and cite, many of the same challenges replicated as we found in TTC: historical changes combined with an intervention designed some time ago; lower rates of HIV and STI in the replication community; and using a previously developed intervention, as opposed to developing the intervention in collaboration with the community, as they had been able to do with the original FoK.

We conclude that the lack of effectiveness of the intervention is not owing to changes in the intervention, which were few; lack of fidelity, because fidelity was in fact very good; or dosage, which was high. It is also unlikely to be owing to the fact

that TTC was not run with close friends, because greater measured friendship within groups was not associated with greater effectiveness of the intervention in those groups (Morrison et al., 2007). It is possible that the intensity of youths' friendships differ in the Baltimore and Seattle sites: youth in Baltimore lived in very close proximity to each other, and their after–school programs were often contained in the same building in which they resided. Such close proximity may lead to closer friendships and greater peer group effects. It is also possible that the control, career exploration, intervention was indirectly effective. Although the control curriculum did not include any sexual content, it did focus on long–term goals, and this future–orientation is a key feature of FoK, as well as many youth behavior interventions across a range of potential problem behaviors. The most likely reason, however, is lower rates of sexual experience among the youth in Seattle. This affected not only the power of the test of the intervention on condom use but may also have decreased the salience of the material. The similarity of the trends in increased condoms between TTC and FoK leaves us hopeful that we were successful in effecting some change in TTC youth in the experimental arm, even though lack of power makes it impossible to verify this.

Our inability to follow youth until they became sexually active leaves open the question of how youth use sexual safety material when it is presented before they become sexually active: Is the immediate effect of the intervention the sole effect, or do the information and skills imparted reactivate when youth begin sexual activity at some later time? Does the intervention shape youth to receive or seek future information differently (e.g., inoculation against disparagement of condoms' effectiveness)? The significant time effects that we found in both conditions in cognitions about abstinence and having sex demonstrated youths' attitudes and norms toward intercourse becoming more positive over time. Although it seems unlikely that any intervention will alter this general trend (nor should it), successful sexual safety interventions may alter the slope of the change, or of changes in attitudes and efficacy about condom use. Further research focused on understanding more about how sexual safety interventions work will enable us to design more effective interventions of lasting value.

REFERENCES

Alan Guttmacher Institute. (2002). *Teenager's sexual and reproductive health in developed countries*. Retrieved January 29, 2007, from http://www.guttmacher.org/pubs/fb_teens.html

Beadnell, B., Carlisle, S. K., Hoppe, M. H., Mariano, K. A., Wilsdon, A., Morrison, D. M., et al. (in press). The reliability and validity of a group–based measure of adolescents' friendship closeness. *Research on Social Work Practice*.

Centers for Disease Control and Prevention. (2005). Trends in reportable sexually transmitted disease rates in the United States, 2005. Retrieved Janurary 29, 2007, from http://www.cdc.gov/std/stats/trends2005.htm

Darroch, J. E., Singh, S., Frost, J. J., & the Study Team. (2001). Differences in teenage pregnancy rates among five developed countries: The roles of sexual activity and contraceptive use. *Family Planning Perspectives, 33,* 244–250, 281.

Fitzgerald, A. M., Stanton, B. F., Terreri, N., Shipena, H., Li, X., Kahihuata, J., et al. (1999). Use of Western–based HIV risk–reduction interventions targeting adolescents in an African setting. *Journal of Adolescent Health, 25*, 52–61.

Galbraith, J., Ricardo, I., Stanton, B., Black, M., Feigelman, S., & Kaljee, L. (1996). Challenges and rewards of involving community in research: An overview of the "Focus on

Kids" AIDS–prevention program. *Health Education Quarterly, 23*, 383–394.

Johnson, B. T., Carey, M. P., Marsh, K. L., Levin, K. D., & Scott–Sheldon, L. A. J. (2003). Interventions to reduce sexual risk for human immunodeficiency virus in adolescents, 1985–2000: A research synthesis. *Archives of Pediatric Adolescent Medicine, 157*, 381–388.

Kaljee, L. M., Genberg, B., Riel, R., Cole, M., Tho, L. H., Thoa, L. T. K., et al. (2005). Effectiveness of a theory–based risk reduction HIV prevention program for rural Vietnamese adolescents. *AIDS Education and Prevention, 17*, 185–199.

Kirby, D., Laris, B. A., & Rolleri, L. (2005). *Impact of sex and HIV education programs on sexual behaviors of youth in developing and developed countries.* Research Triangle Park, NC: Family Health International.

Morrison, D. M., Casey, E. A., Beadnell, B. A., Hoppe, M. J., Gillmore M. R., Wilsdon, A., et al. (2007). Effects of friendship closeness in an adolescent group intervention. Unpublished article.

Mullen, P. D., Ramirez, G., Strouse, D., Hedges, L. V., & Sogolow, E. (2002). Meta–analysis of the effects of behavioral HIV prevention interventions on the sexual risk behavior of sexually experienced adolescent in controlled studies in the United States. *Journal of Acquired Immune Deficiency Syndromes, 30*, S94–S105.

Robin, L., Dittus, P., Whitaker, D., Crosby, R., Ethier, K., Mezoff, J., et al. (2004). Behavioral interventions to reduce incidence of HIV, STD, and pregnancy among adolescents: A decade in review. *Journal of Adolescent Health, 34*, 3–26.

Rogers, R. W. (1983). Cognitive and physiological processes in fear appeals and attitude change: A revised theory of protection motivation. In T. Cacioppi & R. E. Petty (Eds.), *Social psychology* (pp. 156–176). New York: Guilford.

Rogers, R. W., & Prentice–Dunn, S. Protection motivation theory. (1997). In D. S. Gochman (Ed.), *Handbook of health behavior research 1: Personal and social determinants*. (pp. 113–132). New York: Plenum.

Rosenberg, P. S., & Biggar, R. J. (1998) Trends in HIV incidence among young adults in the United States. *Journal of the American Medical Association, 279*, 1894–1899.

Stanton, B., Black, M., Feigelman, S., Ricardo, I., Galbraith, J., Li, X., et al. (1995). Development of a culturally, theoretically, and developmentally based survey instrument for assessing risk behaviors among inner city African American early adolescents. *AIDS Education and Prevention, 7,* 160–177.

Stanton, B., Harris, C., Cottrell, L., Li, X., Galbraith, J., Pendleton, S., et al. (2006). Trial of an urban adolescent sexual risk–reduction intervention for rural youth: A promising but imperfect fit. *Journal of Adolescent Health, 38*, 55.e25–55.e36.

Stanton, B., Li, X., Kahihuata, J., Fitzgerald, A.M., Neumbo, S., Kanduuombe, G., et al. (1998). Increased protected sex and abstinence among Namibian youth following a HIV risk–reduction intervention: A randomized, longitudinal study. *AIDS, 12,* 2473–2480.

Stanton, B., Li, X., Ricardo, I., Galbraith, J., Feigelman, S., & Kaljee, L. (1996). A randomized controlled effectiveness trial of an AIDS prevention program for low–income African American youth. *Archives of Pediatric Adolescent Medicine, 150*, 363–372.

Weinstock, H., Berman, S., & Cates, W., Jr. (2004). Sexually transmitted disease among American youth: Incidence and prevalence estimates, 2000. *Perspectives on Sexual and Reproductive Health, 36,* 6–10.