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Detecting Selection Bias in Community Disseminations of Universal Family-Based Prevention Programs

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Detecting Selection Bias in Community Disseminations of Universal Family-Based Prevention Programs

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Abstract

The goals of the present study were to demonstrate a method for examining selection bias in large-scale implementations of community-based family skills programs, and to explore the nature of selection bias in one such implementation. We used evaluation data from a statewide dissemination of a popular substance abuse prevention program (*N* programs = 42; *N* youth = 294). The program's evaluation measures were designed to match publicly available data on risk and protective factor scales collected in the state's schools, which enabled us to construct a comparison sample of non-participants (*N* = 20,608). We then examined the risk status of adolescents in both groups to determine whether risk and protective factor scores were related to the probability of program participation. Participation was predicted by both demographics and risk and protective factor scores. Among families with younger adolescents, program attendance was associated with lower risk; among families with older adolescents, participation was associated with both higher risk (on parental management skills) and lower risk (on substance use). Selection effects must be identified and corrected for in order to calculate valid estimates of program benefits, but in community-based disseminations, the necessary supplemental comparison sample is difficult to obtain. The evaluation design and analytic approach described here can be used in program evaluations of real-world, "bottom-up" dissemination efforts to identify who attends a program, which in turn can help to inform recruitment strategies, to pinpoint possible selection influences on measured program outcomes, and to refine estimates of program costs and benefits.

Detecting Selection Bias in Community Disseminations of Universal Family-Based Prevention Programs

The present study was designed to determine the nature of selection effects among participants attending a universal family-strengthening, substance-abuse prevention program (the Strengthening Families Program for Parents and Youth 10-14, or SFP). We briefly review the literature on selection effects in family skills prevention programs. We then describe an evaluation design and analytic method that enable detection of selection effects even when the primary evaluation sample contains observations only on program participants, as is the case in many in real-world implementations. Finally, we describe the results of applying the technique to a community-driven multisite evaluation of SFP, a successful and widely disseminated prevention program (Spoth, Redmond, & Shin, 2001). Specifically, we examined the risk status of adolescent children of families who self-selected into SFP to determine whether risk and protective factor scores were related to the probability of program participation. We discuss results of the study in terms of their implications for assessment of program benefits and provide recommendations for translational research on preventive interventions in community settings.

Self Selection into Family Interventions

Certain family patterns of interaction are strongly related to the development of substance abuse and associated risk behaviors (Biglan & Metzler, 1998). Even brief family interventions targeting those patterns can successfully improve parenting skills and decrease the likelihood of substance use and other risk behaviors; indeed, effect sizes of efficacious family interventions may increase over time (Durlak et al., 2007). In randomized, controlled trials (RCTs) of such family interventions, when risk factors and associated outcomes change in intervention but not in control groups, the program is considered efficacious. By extension, when a program is disseminated on a large scale at a community level, it is expected to affect risk and protective factor levels and long-term behavioral outcomes in the population (Brown & Liao, 1999).

In theory, clinical trials eliminate the problem of selection bias by randomizing participants to intervention and control groups, thus allowing investigators to distinguish the intervention effects from participant characteristics. However, bias due to self-selection effects may occur when programs are translated from controlled research environments to real-world circumstances. This is problematic, because it has implications for how we assess the true economic and health benefits of evidence-based family programs as they graduate from RCTs to large-scale, community-driven disseminations. If there are systematic differences between participants and nonparticipants, results attributed to the program may in fact be due to participant characteristics instead, or to an interaction of program effectiveness with family risk status (e.g. high-risk families may benefit more, or less, from intervention). Furthermore, similar rates of absolute change may have differential implications depending on a family's baseline status (e.g. a one-unit change in risk scales may decrease the likelihood of substance abuse for children in high-risk families but not for those in moderate-risk families). Thus, identification and correction of selection bias is necessary in order to make valid inferences about population-level effects of prevention programs, cost effectiveness across programs, and actual costs and benefits of real-world program implementations.

Most studies of family intervention programs that examine predictors of attendance have found that families with female children, more education, and higher income levels are more likely to participate. Families with lower socioeconomic status are more likely to report privacy concerns and logistical barriers (Haggerty et al., 2002; Heinrichs et al., 2005; Spoth, Redmond, Haggerty, & Ward, 1995; Spoth, Redmond, Hockaday, & Shin, 1996). Orrell-Valente and colleagues (1999) found that parents were more likely to have a positive therapeutic alliance and thus to participate when program leaders were of the same race and of similar socioeconomic background. Some studies have found that parent perception of child behavior problems increases likelihood of attendance (Haggerty et al., 2002; Heinrichs et al., 2005), and parents who see potential benefits of a program are also more likely to attend (Spoth et al., 1996). Finally, several studies have found that positive family interactions, clear communication patterns, and family organizational skills are positively related to program participation (Bauman, Ennett, Foshee, Pemberton, & Hicks, 2001; Perrino, Gonzalez-Soldevilla, Pantin, & Szapocznik, 2000). The overall picture that emerges from this body of research is that families who voluntarily participate in parenting interventions have more years of education and higher incomes, report more positive family dynamics, perceive a benefit to the program, are less likely to have minority status, and are more likely to have female children.

Identifying Selection Bias

In order to estimate and correct for selection bias in program outcomes, and thus improve estimates of costs and benefits of a program, the nature of that selection bias must first be identified. This requires testing to determine whether systematic differences between program participants and non-participants are related to the likelihood of program participation. In clinical trials of family skills programs, demographic, attitudinal and baseline substance abuse variables are measured in both control and intervention groups. However, often in evaluation of community-based programs, we have observations only on program participants. When analysts observe only participants (a situation known as the zero variance problem), identification of selection bias is not straightforward (Humphreys, Phibbs, & Moos, 1996).

One may accurately think of this situation as an extreme case of choice-restricted samples (for a review, see Ben-Akiva et al., 1997). Various solutions have been proposed to deal with choice-restricted samples; most common is the use of a representative supplementary sample (Campbell & Fiske, 1959; Cosslett, 1981). The disadvantage of using this type of supplementary sample, however, is that it is costly to gather additional information through surveying, and, if program participants make up a small portion of the population, the needed supplementary sample may be extremely large.

Steinberg and Cardell (1992) proposed a more manageable maximum likelihood estimator to correct for selection bias. In order to apply their technique, a supplementary sample is still needed; however, the supplementary sample need not contain any information on program participation. Instead, if a program evaluation includes measures that are already available for the population at large, then the Steinberg-Cardell technique (described in detail in the Method section) can then be used in a logistic analysis to test for systematic differences in the probability of participation between participants and non-participants on measures of interest. This approach is feasible for family interventions, because data on variables of interest (e.g. youth risk and protective factors and health behaviors) are often publicly available through Departments of Health or Education and other public and private sources. These existing data can be used to construct the supplementary samples needed to test for selection effects.

The Strengthening Families Program

The rigorous clinical efficacy trial of SFP, a universal seven-week program in which parents and youth aged 10-14 spend one hour separately and a second hour together engaging in interactive exercises led by trained facilitators, produced solid evidence of long-term effectiveness in delaying onset and frequency of adolescent substance use (Foxcroft et al., 2003; Spoth et al., 2001), and because of its strong research base it has been adopted in numerous communities in the U.S. and internationally. In the clinical trial of SFP, intervention and control groups did not differ on the primary variables of interest.

In the present study, we collected data from multiple sites in a statewide dissemination of SFP. The evaluation was conducted through a collaboration of non-profit and state social service agencies, schools, faith-based organizations, and the state's land-grant university Extension system. Each site implemented the program on its own initiative and submitted evaluation data voluntarily.

To enable comparison of attendees with non-attendees, we designed our evaluation of youth attending SFP to include measures assessing risk and protective factors that are 1) targeted by the program, and 2) also collected in a biennial statewide school survey. For analysis of selection effects, we constructed a comparison sample from existing school survey data and tested for selectivity in two ways. First we compared the comparison sample to the SFP participants on mean values of risk and protective factors, and then we applied the Steinberg-Cardell technique to determine how risk and protective factor status and demographic variables affected the probability that a family would attend the program.

Method

Sample

Strengthening Families Program sample. We used archival data from an ongoing evaluation of the program at multiple sites statewide, which included 294 youth (50% female) from 42 programs in 10 counties, with an average age of 11.4 (*SD* = 1.98). Seventy-six percent of participants reported their race/ethnicity; of those, 23.1% were Latino, 8.1% American Indian, 59.3% European American, and 9.5% other or multiple race or ethnicity.

Washington State sample. We used data from the 2004 Healthy Youth Survey (HYS) (Washington State Department of Health, 2008a). The representative state sample of Washington's grade 6, 8, 10 and 12 students consisted of 191 schools, and an additional 888 schools participated in the survey as non-sampled schools in order to obtain their own results. We used data from grades 6, 8, and 10 (corresponding to the age range in the SFP dataset). Out of a total of 20,608 students, 51% were female, 17% Latino, 7.5% American Indian, 55% European American, 5% Asian, 2% African American and 18% other or multiple race or ethnicity. In 2004, school response rates ranged from 80% (6th grade) to 86% (10th grade) and individual response rate across grades was 65%. Alternative schools were significantly less likely to participate than regular schools (63% versus 82.7%). However, analysis of survey data patterns showed that school self selection did not appear to bias survey results. Eighth-grade respondents who had rates of missing data greater than 85% were significantly different from other respondents, and survey analysts advise caution on generalizability of some items. None of those items was included in scales analyzed in the present study (Washington State Department of Health, 2008b).

Data Collection Procedures

In the SFP, pretest evaluations of risk and protective factors for substance abuse were administered by program facilitators on the first night of the program, and posttests, using the same measures, on the last (7th) night. The HYS was administered by teachers in classrooms during the fall semester. Participation was optional at both the school level and the individual level. Completed evaluations were processed by the state Department of Health.

Measures

Washington State Healthy Youth Survey. The Washington Healthy Youth Survey (HYS) is conducted biennially in schools statewide in an effort to measure health risk behaviors that contribute to the morbidity, mortality and social problems of youth in Washington State. The HYS is based on the Monitoring the Future Survey, supported by the National Institute of Drug Abuse (Regents of the

University of Michigan, 2008) and the Centers for Disease Control and Prevention's Youth Risk Behavior Survey (Chronic Disease Prevention and Health Promotion Centers, 2007). It also incorporates risk and protective factor scales from the Communities that Care (CTC) survey, developed by researchers at the University of Washington (Arthur, Hawkins, Pollard, Catalano, & Baglioni, 2002). Core items on the HYS are administered to all students. Other items and scales are included based on developmental considerations; thus, the versions for older youth (8^{th}, 10^{th}, and 12^{th}-graders) are different from the version used with 6^{th}-graders.

SFP Evaluation. The SFP youth evaluation was designed to include a subset of scales from the HYS that assess risk and protective factors specifically targeted by SFP: parental monitoring, positive interactions with parents/caregivers, and peer resistance skills. Scales for younger students (6^{th} graders) assessed involvement in family activities (Opportunities for Prosocial Involvement, 3 items; $\alpha = 0.64$) and parents' rewarding of positive behaviors (Rewards for Prosocial Involvement, 2 items, $\alpha = 0.74$). Scales for older students reflect adolescents' increasing independence from parents and opportunities to engage in risk behaviors. The Family Management scale (8 items, $\alpha = 0.81$) assesses clarity of household rules and family monitoring of youth activities in and out of the home. The Peer Social Skills (4 items, $\alpha = 0.59$) assesses peer resistance skills. Peer Social Skills is constructed of four vignettes describing situations in which a peer engages in deviant behaviors and provides a selection of four response options, ranging from conformity with a peer's undesirable behavior to skilled resistance to that behavior. All other risk and protective factor scales were rated on a four-point Likert-type scale with response options indicating agreement or disagreement with each item. For purposes of this study, all scales have been scored so that higher scores indicate lower risk. The SFP evaluation also queried older youth about the frequency of their use of substances (tobacco, alcohol, marijuana, inhalants and other illegal drugs) in the past 30 days. Frequencies of use of individual substances were low and we collapsed the seven index items into a dichotomous scale, with 0 indicating "No use" and 1 indicating "Some use".

Analytic Strategy

Two analyses were used to check for selectivity bias in the SFP. First, we did a means comparison of the SFP participants to an age-corrected sample of the HYS data. Next we used the Steinberg-Cardell technique in a logistic analysis to test if the values of risk and protective factors had a systematic effect on the probability of participation in the SFP.

The Steinberg-Cardell technique uses sampling rates in the choice-restricted and supplementary samples to estimate a weighted binary logistic model of participation. With the technique, each observation from the participant sample (the SFP in our application) is included in the dataset twice, once with a pseudo-dependent variable of 1 (representing participation) and once with a pseudo-dependent variable of 0 (representing non-participation). A pseudo-dependent variable of 0 (representing non-participation) is also generated for all observations in the supplementary sample (the HYS in our application). Table 1 shows how the pseudo-dependent variables compared across the two data sets.

In applying the technique for logistic estimation, observations from the supplementary sample are weighted as 1, while observations from the participant sample with a pseudo-dependent variable of 1 are weighted by the ratio of the sampling rate in the supplementary sample to the sampling rate in the participant sample (Table 1). Thus, in our application, this is the ratio of the sampling rate of the HYS to the sampling rate of SFP. The negative ratio is used as the weight for the second choice-restricted (participant) sample (those observations with a pseudo-dependent variable of 0). This weighting procedure "misclassifies" the supplementary sample and adds a correction term to the choice-restricted sample (participants) for the probability that each observation in the pseudo sample is correct or incorrect. In the end it allows us to estimate a discrete choice model using standard statistics packages (contact authors for computer code in STATA or SAS), although weighted moment matrices must be used to obtain correct standard errors.

Results

Comparison of SFP Sample with General Population

Mean differences between groups. We used two methods to investigate mean differences between the SFP group and the general population. First, we conducted a standard t-test of group differences in means for each individual variable of interest. Using the Stata sampling program, we controlled for the different age distributions in the two samples by randomly selecting an appropriate number of observations from the full HYS, by age, to simulate the age distribution of the SFP data. In Tables 2 and 3, we present descriptive statistics and tests of mean differences. As can be seen in Table 3, the only significant mean difference between groups was on Opportunities for Prosocial Involvement: scale scores for the SFP group (M = 2.90, SD = 0.69) were significantly lower than those for the population at large (M = 3.18, SD = 0.71, t =-5.4558 , p <= 0.00). We then conducted multivariate Hotelling t-tests to jointly test whether the means of the variables of interest were equal in both samples, grouping Opportunities for Prosocial Involvement with Rewards for Prosocial Involvement (for younger students) and Peer Social Skills with Family Management (for older students). The vector of means for Opportunities and Rewards for Prosocial Involvement was significantly different between the SFP and HYS groups (F(2,2097) = 27.67, p <= 0.00), but the vector of means for Peer Social Skills and Family Management was not significantly different between groups.

Logistic regression predicting participation. In Table 4 we present results of the logistic regression predicting program participation from each of the risk and protective factor scale scores (both linear and curvilinear; see Table 5 for non-linear coefficients) and controlling for youth race/ethnicity, sex, county, and age. For older youth the substance use index was also included in the regression. Our results show that in the younger group, families with female children and non-minority families were more likely to attend than families without these characteristics. In the older group, the opposite was true: families with male children were more likely to attend, as were Latino and AI/AN families. However, African-American and Asian-American families, and those from other non-White European families, were still less likely to attend. In both cases, for the age range of the children, within groups older children had lower odds ratios than younger children.

Our main interest here was to test for effects and curvilinear effects of risk/protective factors as well as interactions between risk/protective factor scores. Preliminary analysis indicated that second-order terms for protective factors belong in the regression equation for the younger group, but not for the older group. To improve efficiency the reported analysis for the older group reflects this finding and does not include estimates for curvilinear effects. In a logit estimation the regression calculates changes in the log odds of the dependent, not changes in the dependent variable itself. Thus one must be cautious about interpreting the parameter estimates (b coefficients) associated with explanatory variables, as they are estimators of the change in the logit caused by a unit change in the independent variable (see Table 5). The presence of the squared terms in the younger group means this caution is even more important, as one must compute odds ratios using linear, squared and cross-product terms; hence the odds ratios are not constant. Figures 1 and 2 show how the overall odds ratio (Figure 1) and marginal odds ratios (Figure 2) change with the indicated values for Rewards for Prosocial Involvement (Rewards) and Opportunities for Prosocial Involvement (Opportunities). Figure 1 shows that families with scores of Rewards = 4.00 and Opportunities=1.71 have the highest odds ratio, which decreases with movement in any direction from that peak within the range of scale scores, while graph 2 shows the maximum marginal odds ratios occur at Reward=4.00 and Opportunities=1.70. Both these are computed at the mean of the other variable. Since the model for the older group uses only linear estimation, odds ratios are much more straightforward; lower scores on Family Management significantly predicted participation ($OR = 0.725$, $p < .04$), and drug use was negatively associated with program attendance ($OR = 0.654$, $p < .10$).

Discussion

The goals of the present study were 1) to demonstrate a method for examining selection bias in large-scale implementations of community-based family skills programs, and 2) to explore the nature of selection bias in one such implementation. The evaluation design and analytic approach described here can be used in program evaluations of real-world, “bottom-up” dissemination efforts to identify who attends a program, which in turn can help to inform recruitment strategies, pinpoint possible selection influences on measured program outcomes, and refine estimates of program costs and benefits.

In multivariate analyses, we found that, up to a point, higher levels of positive family interaction, having a female child, and white race/ethnicity predicted participation of families with younger adolescents. These results are consistent with those from several of the participation studies referenced earlier. However, those families with highest levels of opportunity for positive family interaction were less likely to participate. Weaker family management skills, lower levels of adolescent drug use, and Latino or AI/AN ethnicity all predicted participation of families with older youth. Taken together, these findings provide a mixed and nuanced picture: family attributes and minority status predicted attendance differently for families with younger versus older youth, and the influence of levels of opportunity for family involvement was curvilinear. For both age groups, though, selection bias that was not found in the RCT for SFP was present in community-based programs. It is also important to note that multivariate analysis using the Steinberg-Cardell technique produced different and more insightful results compared to what was found with simple tests of mean differences between groups, underscoring the utility of the technique for assessment of selection bias in community programming.

One implication of the type of selection effect reported here is that economic analysis based on program outcomes in RCTs may not translate to reliable information about population-level benefits of family interventions. Thus, analysis of effectiveness and costs and benefits of programs implemented in community settings is important, and necessary to corroborate what is found in RCTs. With non-random participation in a universal program, population benefits could be lower than those expected from the findings from RCTs, as those who need the program most may not be receiving it (Brown & Liao, 1999). On the other hand, population-level effects occur not only when individual families at highest risk are reached, but also when "systems change" occurs in community values, norms, and behaviors (Durlak et al., 2007); thus, reaching families who are not at highest risk may have significant population benefits. The main point is that we cannot quantify those benefits without an accurate understanding of the import of selection effects. The techniques for estimating and correcting for the effects of selection bias on program outcomes are relatively straightforward once selection bias has been identified.

Limitations

Some agencies in the state do not participate in the collaborative SFP evaluation, and not all programs implemented by collaborating agencies submit evaluation data. In other words, the sample of programs included in the study is itself likely to suffer from selection bias. However, the program sample represents a diversity of geographic areas (e.g. rural, small town, and urban) and participant demographics. Another limitation is that although the risk and protective factor scales used in the program evaluation have strong psychometric properties and are known to be related to behavioral outcomes such as substance abuse and delinquency, it is not known how and whether they might relate specifically to the substance abuse outcomes reported in the RCT, since the SFP efficacy trial used different measures. Our choice of youth evaluation measures was dictated by availability of public data for comparison of attendees with the population.

Conclusion

We found evidence that those attending a popular universal substance-abuse prevention program were differently at risk for substance abuse from the general population -- in some ways more at risk, in other ways less at risk. Moreover, we were able to identify to what degree these differences affected the probability of program participation. One implication is that it is important to assess and correct for selection when assessing program benefits in community implementations. These considerations are important for policy in that they affect population-level benefits. They are also important to program evaluation efforts, as selection bias may vary greatly across program sites. An understanding of site-specific selection bias may provide direction for program recruitment techniques as well as for analysis of program benefits.

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Figure Captions

Figure 1. *Probability of Participation as a Function of Risk/Protective Factor Scale Scores*.

Figure 2. *Probability of Participation as a Function of Risk/Protective Factor Scale Scores (Non-Linear Effects for Ages 11-12)*.

Figure 1

Click here to download Figure: Figure 1 probability graph.ppt

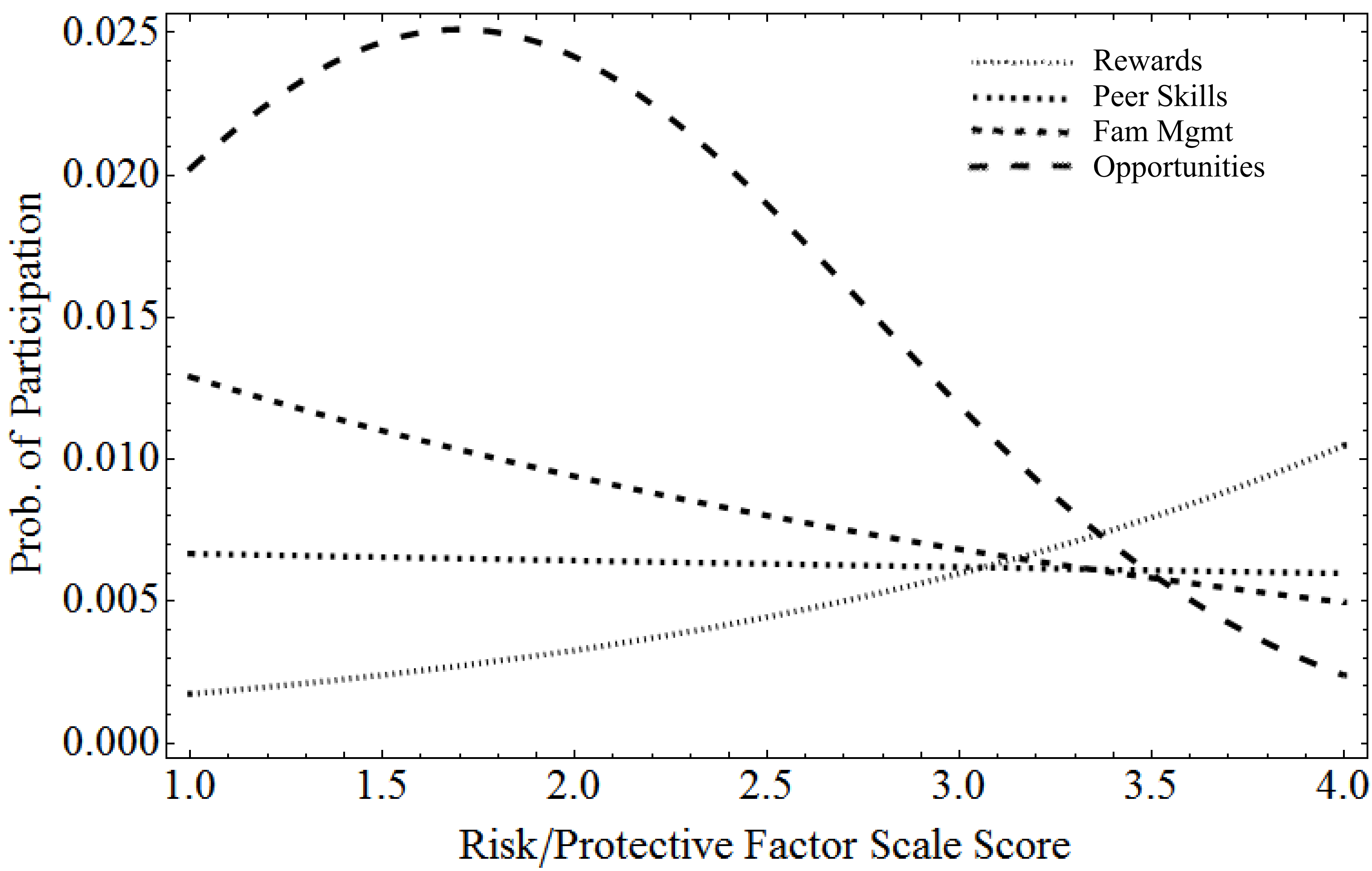

Figure 2

Click here to download Figure: Figure 2 surface graph.ppt

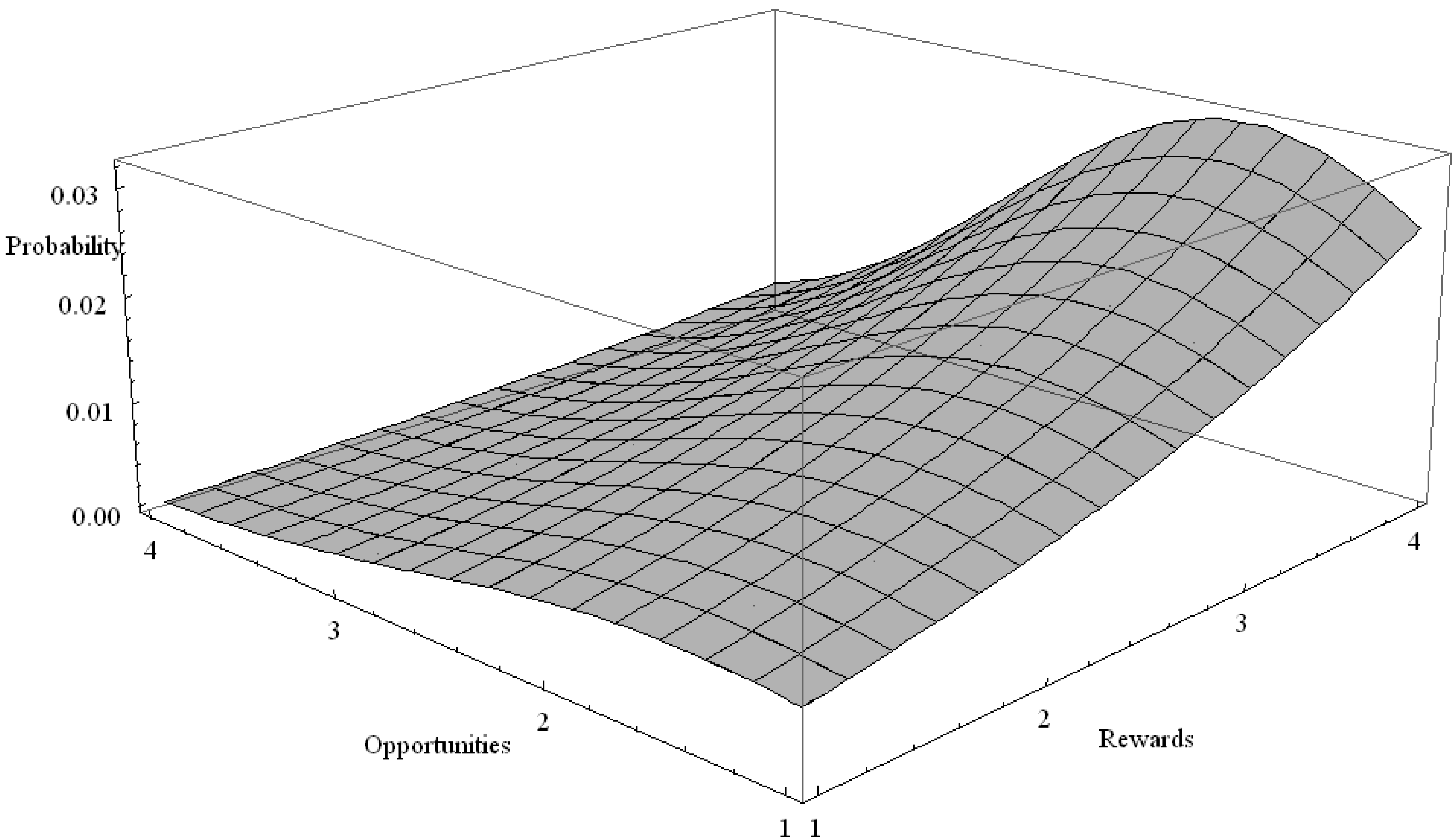

Table 1. Sampling Weights for Evaluation Sample and Supplemental Samples

Weight	Pseudo-Dependent Variable	Actual Dependent Variable	Sample
1	0	Unobserved	HYS
$\frac{Sampling\ Rate_{HYS}}{Sampling\ Rate_{SFP}}$	1	1	SFP
$-\frac{Sampling\ Rate_{HYS}}{Sampling\ Rate_{SFP}}$	0	1	SFP

Note: HYS = Healthy Youth Survey sample. SFP = Strengthening Families Program sample. Actual sampling rates are not included in the table because they vary by county.

Table 2: Summary Statistics for the Younger (n_{HYS}=8041, n_{SFP}=200) and Older (n_{HYS}=4413, n_{SFP}=94) Age Groups.

Variable	Range	Ages (11-12)$^{\alpha}$				Ages (13-16)$^{\beta}$			
		HYS		SFP		HYS		SFP	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD
Male	0,1	.48	.0056	.46	.035	.47	.50	.56	.50
Age	11-16	11.28	0050	11.42	.035	13.29	.48	13.57	.74
White	0,1	.522	.0056	.635	.034	.645	.48	.468	.50
Black	0,1	.020	.0016	.025	.011	.027	.16	.012	.10
Asian/Pacific Island	0,1	.048	.0024	.025	.011	.050	.22	.032	.18
Hispanic	0,1	.167	.0042	.195	.028	.169	.37	.340	.47
Native	0,1	.096	.0033	.065	.017	.056	.23	.106	.11
Other	0,1	.21	.0045	.055	.016	.093	.29	.050	.20
Reward$^{\alpha/}$ Fam. Mgmt$^{\beta}$	1-4	3.42	.0072	3.37	.045	3.31	.64	3.21	.61
Involve$^{\alpha}$/Peer Skills$^{\beta}$	1-4	3.19	.0079	2.89	.049	3.10	.73	2.86	.78
Substance Use	0,1	n/a	n/a	n/a	n/a	.209	.41	.234	.43

Note: HYS = Healthy Youth Survey sample. SFP = Strengthening Families Program sample. Reward = Rewards for Positive Involvement; Involve = Rewards for Positive Involvement; Fam. Mgmt. = Family Management; Peer Skills = Peer Social Skills.

Table 3: Mean Comparison Tests with Age Matched Samples

Variable	HYS(n=5356)	SFP (n=200)	Diff.	p-value
Rewards- Prosocial	3.41 (.648)	3.37 (.638)	.035	0.4563
Opportunities- Prosocial	3.18 (.712)	2.90 (.693)	.280	0.0000
Joint Test				0.0000

Variable	HYS (n=93)	SFP (n=94)	Diff.	p-value
Peer Skills	3.04 (.829)	2.87 (.778)	.170	0.1506
Family Mgmt.	3.20 (.673)	3.21 (.614)	.011	0.9044
Joint Test				0.1717

Table 4
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Table 4: Summary of Steinberg-Cardell Pseudo-Logistic Regression Analysis for Variables Predicting Decisions to participate in the program for children age 11-12 and children age 13-16, Controlling for Background Variables

	Ages 11-12			Ages 13-16		
Predictor	*B*	*SE B*	e^B	*B*	*SE B*	e^B
Male	-.137	.147	.872	.116	.224	1.12
Hispanic	-.342**	.203	.710	.748***	.271	2.11
Native American	-.875***	.304	.417	.566*	.383	1.76
Asian/Pacific Islander	-.698*	.459	.498	-.0969	.574	.908
Black	-.287	.463	1.33	-.634	.962	.531
Other	-1.80***	.330	.165	-1.11**	.612	.330
Rewards for Involvement$^{\Psi}$			1.75	n/a	n/a	n/a
Opportunities for Involvement$^{\Psi}$			.167	n/a	n/a	n/a
Peer Social Skills	n/a	n/a	n/a	-.0369	.184	.964
Family Management Skills	n/a	n/a	n/a	-.321**	.176	.725
Substance Use	n/a	n/a	n/a	-.424*	.321	.654

Note: *p < .10. **p < .05. ***p < .01. County effects are not reported but are included in the model and available upon request. Age was included as a curvilinear control variable; effects are available upon request. $^{\Psi}$The marginal odds ratio for these variables is calculated at the mean for the HYS participants. Because these variables enter the equation in a nonlinear way, the beta estimates and standard errors cannot be interpreted in the classical way. Beta estimates and standard errors for these non-linear terms are given below.

Table 5: Summary of Non-Linear Terms in the Steinberg-Cardell Pseudo-Logistic Regression.

	Age 11-12		
Variable	*B*	*SE B*	e^B
Reward	.538	1.03	1.71
Involve	1.36	.801	3.88
Reward Squared	-.0188	.214	0.981
Involve Squared	-.449	.167	.636
Reward*Involve	.0500	.275	1.05