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Effects of a youth substance use prevention program on stealing, fighting, and weapon use

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## Abstract

Using a sample of sixth graders in 11 public schools in a large Southwestern city, this longitudinal study examined how a model substance use prevention program, *keepin' it REAL*, that was implemented in 7<sup>th</sup> grade, influenced three other problem behaviors (fighting, weapon use, stealing), measured in 8<sup>th</sup> grade. Using a non-equivalent control group design, we compared 259 students in the intervention to 322 students in a treatment-as-usual condition. At baseline, 37% of the sample reported fighting in the last 30 days; 31% reported stealing in the last 30 days, and 16% reported using a weapon in the last 30 days. Regression analyses adjusted for students nested in schools through multi-level modeling and for missing data through multiple imputation. We found that at posttest the rates of all three behaviors were lower in the intervention group than the control group at posttest: 35% versus 37% got into a fight in the last 30 days; 24% versus 31% stole something in the last 30 days; and 16% versus 25% used a weapon in the last 30 days. The program impact for fighting and stealing was not statistically significant and involved minimal effect sizes. The program impact for weapon use was not statistically significant but had an effect size comparable to that for other problem behavior interventions. Promoting positive development via life skills may be a key to broadening program impact.

**Keywords:** Prevention, Adolescence, Delinquency

## Introduction

Using a non-equivalent control group design, we explored the effect of a substance use prevention program, *keepin' it REAL*, on stealing, fighting, and weapon use. While the developers have proved the program to be efficacious in addressing substance use (Hecht et al., 2003; Hecht et al., 2008), they have not explored its effects on stealing, fighting, and weapon use – outcomes not explicitly targeted by the program. We have several reasons to believe that a problem-specific program, such as *keepin' it REAL*, may have an impact on outcomes other than the targeted problem. First, we know that different youth problem behaviors have similar etiologies. The same youths who consume substances are those most likely to engage in delinquent behavior, engage in early sexual behavior, and experience academic difficulties. (Biglan & Cody, 2003). The risk factors for different problem behaviors overlap (Eggert & Randell, 2006; Youth Justice Board, 2005). Deviant peer influence, for example, is a risk factor for substance use and youth offending (Biglan & Cody, 2003; Wright & Pemberton, 2004; Youth Justice Board, 2005). Substance use, in particular, is associated with violence perpetration (Eggert & Randell, 2006; Resnick, Ireland, & Borowsky, 2004), stealing (often for the purpose of obtaining drugs; National Institute on Drug Abuse, 2012), and weapon use (Duke, Resnik, & Borosky, 2005). Therefore, a program that reduces risk for one problem may also reduce risk for another related problem.

Second, effective prevention programs should promote competence, whether in generic life skills or skills specific to the targeted problem (Schinke, Brounstein, & Gardner, 2002). Prevention programs that develop a youth's core competencies – social and self management skills such as risk assessment, decision making, and communication (Botvin & Griffin, 2006; Hahn et al., 2007) – may positively affect multiple outcomes, including problem behaviors not

explicitly targeted. In addition to developing core competencies, substance use prevention programs commonly teach drug resistance skills (Botvin & Griffin, 2006) – skills that enable youths to refuse drugs or resist social influences to use drugs. These skills may enable youths to resist social pressure more generally. As such, the intervention may enable youths to respond effectively to social pressures regarding other problem behaviors as well.

Studies have demonstrated the capacity of problem-specific interventions to positively impact on multiple outcomes. For example, Botvin's Life Skills Training, a school-based youth substance use prevention program, positively affected participants' HIV risk behavior (Griffin, Botvin, & Nichols, 2006). Similarly, several youth violence prevention programs positively affect outcomes in addition to violence, including academic attendance and achievement, substance use, inappropriate sexual behavior, delinquency, and property crime (Hahn et al., 2007). Other evidence shows that problem-general programs, which promote youths' social-emotional and character development without focusing on specific problem behaviors, can effectively prevent multiple problem behaviors, including substance use and violence (e.g., Positive Action: Washburn et al., 2011).

Given the possibility that problem-specific intervention effects may generalize to other youth problem behaviors, the present study assesses whether *keepin' it REAL*, a youth substance use prevention program, positively affects stealing, fighting, and weapon use. *keepin' it REAL* (<http://sirc.asu.edu/keepinitreal>) is a model youth substance use prevention program, recognized by the United States Department of Health and Human Services, Substance Abuse and Mental Health Services Administration and listed on the National Registry of Evidence-based Programs and Practices. In 2010, the D.A.R.E. drug education program adopted the *keepin' it REAL* curriculum, and practitioners have implemented the program throughout the 50 United States and

internationally, including the United Kingdom, Spain and Latin America. *keepin' it REAL*'s popularity is due in part to the fact that it is one of few designed with Latinos in mind. Researchers developed the program with youth and for youth, using a participatory action research model (Gosin, Dustman, Drapeau, & Harthun, 2003). While they intended the culturally grounded program to be administered in schools as a universal prevention program (Gosin, Marsiglia, & Hecht, 2003; Holleran, Reeves, Dustman, & Marsiglia, 2002), they have also adapted it for other community settings for use as a selective prevention program. Two program versions exist: a ten-lesson version for middle school students (7<sup>th</sup> graders) and a 12-lesson version for elementary school students (5<sup>th</sup> graders). The developers proved the middle school version to be efficacious in preventing substance use in randomized control trials (Hecht et al., 2003; Hecht et al., 2008; Kulis, Nieri, Yabiku, Stromwall, & Marsiglia, 2007). The program teaches four drug resistance skills (Refuse, Explain, Avoid, and Leave – hence the program acronym REAL) as well as life skills, such as decision making, assertiveness, goal setting, and self awareness. A video that demonstrates the drug resistance skills accompanies four of the lessons. Program language options include American English (with auxiliary materials in Spanish), Mexican Spanish, and Castilian Spanish.

Given the demonstrated efficacy of the *keepin' it REAL* intervention with regard to substance use, and the close relationship between factors associated with substance use and those associated with stealing, fighting and weapon use, we hypothesized that participants in the intervention would report lower post-intervention rates of stealing, fighting, and weapon use than students who did not participate in the intervention.

Our study, examining unintended but positive intervention effects, is significant because it helps to maximize the investment in prevention. We capitalize on existing data to efficiently

expand our knowledge about an existing program. Furthermore, since the list of evidence-based programs is increasing, expanding the knowledge base on existing programs will help inform choices between them and better guide the design of new programs. Finally, the middle school years are characterized by increased risk for engagement in problem behaviors (Dishion & Patterson, 2006). Stealing, fighting, and weapon use are problem behaviors with serious negative consequences for youths (Fraser, 1996; McCluskey, McCluskey, & Bynum, 2006; Moncher & Miller, 1999). Therefore, identifying an intervention that is effective in preventing these behaviors can benefit society's efforts to promote positive youth development.

## **Method**

### **Data and Sample**

This study is a secondary analysis of data from the Drug Resistance Strategies Project - 4, a six-wave randomized trial of a school-based substance use prevention intervention (Hecht et al., 2008). The institutional review board of Arizona State University approved the research. Thirty public schools participated; all were located in Phoenix, Arizona, where the resident population is over 30% Latino, and had student populations that were at least 50% Latino. We stratified the participating schools according to enrollment size and ethnicity (% Latino) and assigned them to intervention or control groups through block randomization. We recruited students ( $N = 2,084$ ) in fifth grade and followed them through eighth grade. University-trained proctors administered one-hour written surveys, available in English and Spanish, in the school classroom. We obtained active parent consent and student assent in accordance with university and school district policies and human subjects protection requirements. Parents of 82% of enrolled children gave consent for participation. Ninety-six percent of consented students (79% of enrolled) assented to participate.

Of the 2,084 students in the study, 781 participated in at least one survey wave and either received the *keepin' it REAL* intervention in the 7<sup>th</sup> grade only or were in the control condition. Of these 556 (treatment: 247, control: 309) participated in the third wave (Spring 2006) and 386 (treatment: 203, control: 183) participated in the sixth wave (Spring 2008). Of the group of 781, we included 581 students in our sample. These students provided data at either or both the third and sixth survey waves when stealing, fighting, and weapon use were measured, and when they were in the sixth and eighth grades, respectively. Thus, the analysis used a non-equivalent control group design. The sample excluded all students who had received an elementary school version of *keepin' it REAL* in the 5<sup>th</sup> grade. Teachers administered the substance use intervention in Fall 2007, during the students' 7<sup>th</sup> grade. Seventy-one percent of the sample completed a survey at wave 3, in the semester prior to the intervention, and 68% completed a survey at wave 6, approximately 14 months after the intervention and 1 month after the last of 5 booster activities.

The sample included 581 sixth graders; 54% were female. A majority (92%) participated in the free or reduced price lunch program at school. The students' ages ranged from 10 to 14 years; the average age was 11 years. In terms of race/ethnicity, 79% were Latino, 4% were non-Latino White, and 17% some other race/ethnicity. Among Latinos 63% were less linguistically acculturated (i.e., spoke a language other than English all or most of the time) and 37% were more linguistically acculturated. Just under half (45%) of the sample was in the intervention condition.

## **Measures**

We measured participation in the intervention with a dichotomous variable (1 = intervention, 0 = control). The measures of stealing, fighting, and weapon use captured the



student's report of how often in the last 30 days he or she had stolen something, gotten into a physical fight, or used or carried a weapon. The original responses ranging from "never" to "almost every day" were skewed towards "never" and therefore, dichotomized (0 = never, 1 = ever). At the sixth grade pretest 31% of the sample reported stealing, 37% reported fighting, and 16% reporting using a weapon. At the eighth grade posttest 36% of the sample reported stealing, 43% reported fighting, and 32% reporting using a weapon.

Covariates, which we measured when the students were in sixth grade, included gender, age, ethnicity by linguistic acculturation, socioeconomic status, and academic performance. Gender was dichotomous (1 = female, 0 = male). Baseline gender differences between the treatment (58% female) and control (50% female) groups were not statistically significant ( $\chi^2 = 3.34, df = 1, p = .07$ ). We measured age in years. Baseline age differences between the treatment ( $M = 11.35, SD = .55$ ) and control ( $M = 11.36, SD = .57$ ) groups were not statistically significant ( $t = .26, df = 557.18, p = .80$ ). We measured ethnicity by linguistic acculturation categorically: More acculturated Latino, Less acculturated Latino (reference group), non-Latino White, and Other ethnicity. We distinguished Latinos by acculturation since they vary greatly by acculturation and greater acculturation is associated with greater risk of problem behavior (Nieri, Lee, Kulis, & Marsiglia, 2011; Warner et al., 2006). Baseline differences in ethnicity by linguistic acculturation between the treatment (More acculturated Latino: 33%, Less acculturated Latino: 43%, non-Latino White: 2%, Other: 22%) and control (More acculturated Latino: 27%, Less acculturated Latino: 55%, non-Latino White: 5%, Other: 13%) groups were statistically significant ( $\chi^2 = 17.51, df = 3, p = .0006$ ). Participation in the school lunch program -- full priced lunch, reduced price lunch, or free lunch (reference group) -- indicated socioeconomic status. Baseline differences in participation in the school lunch program between the treatment (full:

8%, reduced: 23%, free: 69%) and control (full: 7%, reduced: 22%, free: 71%) groups were not statistically significant ( $\chi^2 = .50, df = 2, p = .78$ ).

Aside from these demographic covariates, we included academic performance as a covariate because youths with lower academic performance report more problem behavior (Bachman et al., 2008). We measured this variable as the student's report of the usual grades received in school, with responses ranging from mostly Fs to mostly As. Baseline differences in grades between the treatment ( $M = 6.68, SD = 1.70$ ) and control ( $M = 6.82, SD = 1.53$ ) groups were not statistically significant ( $t(523.96) = 1.02, p = .31$ ).

### **Analysis**

Using the GLIMMIX procedure in SAS 9.1 (2007), we employed logistic regression to predict the likelihood of each outcome at the posttest. This procedure accounted for data clustering in the form of students nested within schools (Raudenbush & Bryk, 2002). It included random intercepts in the multivariate models, which allowed different schools to have different base levels of stealing, fighting, and weapon use. Prior to conducting logistic regression, we applied multiple imputation procedures to address missing data.

The percentage of cases missing data was 18%. Although this amount is not unusually high for school-based studies of adolescent substance use (Aneshensel, Becerra, Fielder, & Schuler, 1989; Josephson & Rosen, 1978), ignoring missing data can introduce bias. Relative to other missing data techniques, multiple imputation provides more efficient estimation (Allison, 2002). One assumption of this technique is that the data are missing at random, conditional on the variables that have been observed. The assumption is not testable but can be strengthened by including all relevant variables that may be related to a case that is missing. We included in the imputation model all variables used in the analysis as well as 26 variables likely to be associated

with the process leading to missing data, such as substance use attitudes and behaviors, risk taking, decision making, educational aspirations, and reading ability. We found only four statistically significant differences between the treatment and control group on these 26 variables. Relative to the control group, the treatment group had a higher average frequency of last 30-day inhalant use, a higher average of descriptive pro-drug norms (peers), a lower average of negative decision-making skills, and higher average educational aspirations. These results, taken together with our group comparisons on demographics (above in the measures section) and baseline dependent variables (see Table 1), show no clear pattern of difference between the treatment and control groups. To the extent that there are differences, they are not in the same direction and thus, do not clearly show one group to be more at risk or protected than the other.

----- INSERT TABLE 1 ABOUT HERE -----

Using the MI and MIANALYZE procedures in SAS, we created 10 imputed datasets for the full sample (treatment and control groups together) and combined the results of the analysis to properly reflect the uncertainty in the imputed values. In keeping with recommendations by the American Psychological Association (2010; Wilkinson & Task Force on Statistical Inference, 1999) and various scholars (Cohen, 1990; LeCroy & Krysik, 2007), we report not only the alpha levels for our estimates but also effect sizes.

## **Results**

Table 1 shows the percentage of students reporting stealing, fighting, and weapon use in the last 30 days at the pretest and posttest for intervention and control groups. We conducted tests of difference of proportions to examine differences between the treatment and control groups in stealing, fighting, and weapon use. Pretest differences between the intervention and control groups were statistically significant only in the case of fighting. A higher percentage of

students in the control group reported fighting at baseline. Posttest differences between the intervention and control groups were statistically significant only in the case of using a weapon, with students in the control group reporting a higher percentage. Other differences between the intervention and control groups were not statistically significant. Of the changes across time, from the pretest to the posttest, the change in the percentage of students reporting using a weapon was largest, nearly doubling in the intervention group and more than doubling in the control group.

Table 2 shows the estimates from logistic regression analyses of the full sample. With regard to intervention effects, participation in the intervention was associated with a 34% ( $=(.658-1)*100$ ) relative decrease in the odds of weapon use, consistent with our expectation. This effect was not statistically significant at the  $p < .05$  level. It did, however, approach the margin of statistical significance ( $p = .096$ ), which is notable given our directional hypothesis. The effect size as measured by Cohen's  $d$  was .17, indicating a small effect size (Cohen, 1988). Following Rosenthal et al.'s (2000) recommendations, we also calculated the effect size in terms of the Binomial Effect Size Display (BESD) which was .0842, indicating a difference between the treatment and control groups in the rate of improvement of 8.42%.

----- INSERT TABLE 2 ABOUT HERE -----

The intervention effect on the likelihood of stealing was similar in magnitude and direction to the effect on weapon use, but it was not statistically significant ( $p = .32$ ). The Cohen's  $d$  was .10, and the BESD was 4.83%. The intervention effect on the likelihood of fighting was in the positive direction, close to zero in magnitude, and not statistically significant ( $p = .97$ ). The Cohen's  $d$  was .01, and the BESD was .25%. With regard to the covariates in the models, engagement in stealing and weapon use in 6<sup>th</sup> grade (i.e., at baseline) increased the odds of the

same behavior in 8<sup>th</sup> grade. The effect of fighting in 6<sup>th</sup> grade was not statistically significant. In addition, relative to males, females reported lower odds of all behaviors, although the effect was only statistically significant for weapon use. School lunch participation, age, school grades, and ethnicity by acculturation were not statistically significantly related to the outcomes.

We ran additional analyses on the subsamples of youths who at baseline had not engaged in stealing ( $n = 383$ ), fighting ( $n = 350$ ), or weapon use ( $n = 463$ ) (results not shown in tables). This allowed us to test whether the prevention effect found in the full sample was the prevention of behavior onset among youths who had not previously engaged in the behavior or the prevention of further behavior among youths who had previously engaged in the behavior. The coefficients for treatment were in the same direction as they were in the analyses of the full sample; however, none were statistically significant. They were negative in the models of stealing ( $b = -.47, p = .21$ ) and weapon use ( $b = -.28, p = .37$ ), suggesting a desirable treatment effect. For stealing the Cohen's  $d$  was .13, and the BESD was 6.54%. For weapon use the Cohen's  $d$  was .10, and the BESD was 5.06%. Among youths with no prior stealing, the odds of post-intervention stealing in the treatment group were 37% lower than the odds in the control group. Among youths with no prior weapon use, the odds of post-intervention weapon use in the treatment group were 25% lower than the odds in the control group.

## **Discussion**

This study examined the impact of a substance use prevention program – *keepin' it REAL* – on stealing, fighting, and weapon use among middle-school-age students. The findings suggest that the intervention had a desirable impact on participants' likelihood of using weapons. While the effect of the intervention on weapon use only demonstrated a trend at the  $p < .10$  level, it was in the hypothesized direction and had an effect size that was comparable to those of many

school-based problem-behavior prevention programs (Wilson, Gottfredson, & Najaka, 2001), including violence prevention programs (Park-Higgerson, Peruman-Chaney, Bartolucci, Grimley, & Singh, 2008).

The percentage of sample students reporting weapon use at baseline (16%) was roughly comparable to the percentages of weapon use at school found in other studies of middle-school-age students: 14% to 19% in North Carolina (Cotten et al., 1994; DuRant, Krowchuk, Kreiter, Sinal, & Woods, 1999), 17% in Maryland (Arria, Borges, & Anthony, 1997), and 15% in Illinois (Bailey, Flewelling, & Rosenbaum, 1997). The intervention effect on weapon use is notable given that there was a substantial increase in this behavior from the pretest to the posttest in the full sample. Although stealing or fighting also increased over time, their increases were relatively modest. The coincidental timing of the intervention and weapon use onset among many participants may explain the stronger pattern of intervention effectiveness with weapon use than in regards to stealing and fighting.

Using our knowledge of the schools' history during the course of the study, including information on other programs implemented in the schools during the intervention, we considered whether other activities in the schools might explain the intervention effect on weapon use. There were no differences between intervention and control schools with regard to other programming that might affect student outcomes. Furthermore, there were no unusual events or special campaigns either at the intervention schools or in their neighborhoods that would explain the prevention of weapon use among the students. Therefore, we feel confident in ruling out such alternate explanations.

Our measures of stealing, fighting, and weapon use were available from only the third and sixth waves of data collection – a full two years apart. This restriction meant that pretest data

were collected nearly six months prior to the start of the intervention and posttest data were collected over a year after the end of it. It would have been ideal to measure the pretest immediately prior to the intervention and the posttest at one or more intervals closer to the end of the intervention. Because such measures were available for substance use, we know that the intervention had both short- and long-term desirable effects on alcohol, cigarette, marijuana, and inhalant use – at the fifth wave in 7<sup>th</sup> grade and a year later at the sixth wave in 8<sup>th</sup> grade (Marsiglia, Kulis, Yabiku, Nieri, & Coleman, 2011). This evidence supports the possibility that the intervention may have had shorter term effects on stealing, fighting, and/or weapon use. In the case of stealing and/or fighting, the intervention may have had short term effects which then disappeared by the sixth wave and hence were not captured in our analysis. In the case of weapon use, the intervention may operate as it does with substance use, stemming the acceleration of the behavior over time once it has begun. Without more proximate measures, however, we cannot indicate whether the intervention had such effects.

Although in the larger study from which these data come we minimized many potential sources of bias through random assignment of schools to treatment conditions and multiple imputation of missing data, practical issues associated with conducting a field-based randomized controlled trial imposed limitations. These include substantial attrition of students due to transfers to non-participating schools, transfers of some students among study schools in different conditions, and failure to track some students successfully from elementary to middle schools. The presence of a higher percentage in the control group of less acculturated Latinos, which may include more recent immigrants, may have contributed to greater attrition due to the residential and employment mobility associated with more recent arrival (Bartel, 1989; Chiswick & Miller, 2005). While we statistically controlled for known baseline differences between the

treatment and control groups, we did not control for unknown differences. Imputation does not handle missingness that occurred before the pretest in this analysis. Thus, it is possible that unmeasured differences explain the observed effects.

While we need additional research to understand why *keepin' it REAL* worked as it did, this study added to the evidence of the potential benefits of life skills training (Botvin, Griffin, & Nichols, 2006). The life skill components of *keepin' it REAL*, which aims to improve communication competence, risk assessment, and decision-making, may have provided students with positive alternatives for handling risky or dangerous situations and for resolving conflicts without resorting to weapons. Furthermore, this study showed that when problem behaviors have similar etiologies, problem-specific strategies may apply to related problems. The drug resistance strategies taught in the intervention—refuse, explain, avoid, and leave—may be applicable to weapon use and help students navigate peer pressure to use weapons. It appears that students in the experimental group who did not want to engage in the additional risk behavior may have benefited from the REAL strategies they learned. Students in the control group who were not exposed to the strategies did not have the tools to effectively resist the risk. In summary, these results suggest that *keepin' it REAL* is efficacious in preventing weapon use among middle-school-age students as an ancillary program benefit. To be clear, they do not suggest that *keepin' it REAL* is a weapon prevention program or may substitute for one. That said, universal prevention programs like *keepin' it REAL*, which have ancillary prevention benefits, may be especially attractive to providers who are seeking to maximize limited resources and must choose between programs to address multiple problem behaviors among the youths they serve.



## **Statement of Conflict of Interest**

The authors have no conflicts of interest.

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Table 1

*Percentage Reporting Stealing, Fighting, and Weapon Use in Last 30 Days*

Behavior	Intervention ( $n = 259$ )		Control ( $n = 322$ )	
	Pretest	Posttest	Pretest	Posttest
Stealing	30.54%	33.47%	31.28%	38.14%
Fighting <sup>a</sup>	33.32%	42.86%	39.81%	43.11%
Using or carrying a weapon <sup>b</sup>	16.60%	26.29%	16.49%	34.10%

<sup>a</sup> Statistically significant group differences at pretest:  $z = -1.62$ ,  $p = .02$  (two tailed). <sup>b</sup> Statistically significant group differences at posttest:  $z = -2.02$ ,  $p = .01$  (two tailed).

Table 2

*Logistic Regression Analysis of Effect of keepin' it REAL in 7<sup>th</sup> Grade on Stealing, Fighting, and Weapon Use in 8<sup>th</sup> Grade*

Variable	Stealing			Fighting			Weapon Use		
	<i>B</i>	<i>SE B</i>	<i>OR</i>	<i>B</i>	<i>SE B</i>	<i>OR</i>	<i>B</i>	<i>SE B</i>	<i>OR</i>
7 <sup>th</sup> grade intervention	-.26	.26	.77	.01	.21	1.01	-.42+	.25	.66
6 <sup>th</sup> grade behavior	.84**	.24	2.32	.28	.24	1.32	.92*	.33	2.51
Full-priced lunch <sup>a</sup>	-.53	.48	.59	-.80	.56	.45	-.42	.53	.66
Reduced-price lunch <sup>a</sup>	-.23	.27	.79	-.14	.24	.87	-.14	.34	.87
Female <sup>b</sup>	-.54*	.27	.58	-.16	.24	.85	-.56*	.24	.57
Age	.20	.23	1.22	.25	.17	1.28	-.11	.21	.90
Usual grades	-.11	.07	.90	-.12+	.06	.89	-.12+	.07	.89
More acculturated Latino <sup>c</sup>	.34	.28	1.40	.01	.29	1.01	.25	.26	1.28
Non-Latino White <sup>c</sup>	.18	.75	1.20	.71	.58	2.03	-.10	.63	.90
Non-Latino other ethnicity <sup>c</sup>	.34	.31	1.40	.15	.33	1.16	.17	.37	1.19
Constant	-2.10	2.68	.12	-2.27	2.06	.10	1.46	2.65	4.31

<sup>a</sup> Reference group: Free lunch. <sup>b</sup> Reference group: Male. <sup>c</sup> Reference group: Less acculturated Latino.

+  $p < .10$ . \*  $p < .05$  (two tailed). \*\*  $p < .01$  (two tailed). \*\*\*  $p < .001$  (two tailed).