EFFECT OF COMPREHENSIVE PREVENTION SOFTWARE

Effect of Comprehensive Prevention Software on Risk and Protective Factors in Adolescents - A Multi-site Study

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Author Note
Co-PI, Alice Ray, is an interested party, because she designed the software being tested. She was directly involved in the intervention design and overall research design. To ensure the integrity of findings for this project, she was never present at any study site; all data were collected electronically; all data analyses were conducted by Co-PI Michael Roona and staff at the Social Capital Development Corporation. Ray was involved in the interpretation of those analyses for this report.

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Abstract

This multi-site, randomized controlled study tests two hypotheses: (1) Self-directed use of comprehensive prevention training software will result in reduced risk and enhanced protective factors in adolescents and (2) Under real-world conditions, the software will be implemented as designed with the same favorable results. Overall effects of the Ripple Effects software at sites where implementation was high and dosage-correlated effects at sites where implementation was low support the first hypothesis. Students in the intervention group had higher grade point averages, less fatalistic thinking, fewer absences, less tardiness, and fewer disciplinary referrals than students in the comparison group. They also had lower perceptions of harm from, but higher disapproval of marijuana. Implementation under real-world conditions was 60%.

Key Words: social-emotional learning, computer-based training, substance abuse prevention, youth development, program evaluation.
Effect of Comprehensive Prevention Software on Risk and Protective Introduction:

Factors in Adolescents – A Multi-Site Study

School failure, anti-social behavior and adolescent health and mental health problems, such as substance abuse, PTSD and depression, have been shown to be inter-dependent variables that can be linked to each other, as well as to common external risk factors, such as family discipline patterns, parental mental health, poverty, and community violence (Diperna and Elliott, 1999; Feshbach & Feshbach, 1987; Hawkins, Farrington & Catalano, 1998). For more than two decades school-based prevention programs have been in place to address one or more of these issues. Dozens of such programs have been shown to be effective in some ways, for some students, on some issues (Wang, Haertel and Wallberg, 1997; Zins, Weissberg, Wang and Wallberg, 2004; Wilson, Gottfredon & Najaka, 2001; Durlak, 1995; Connel, Turner & Matson, 1985; Errecart, 1991; Tobler et al., 2000; Greenberg, Domitrovich & Bumbarger, 2001; Marzek & Haggerty, 1994).

Yet prevention practitioners, researchers and educators increasingly recognize that fragmented, piecemeal prevention programs—even proven effective ones—for an ever expanding list of health and social issues are not a viable option for public schools (Elias et al., 1997; Gottfredson, 2001; Sarason, 1996; Wilson et al., 2001). There are too many to fit into the available school day. They must compete with assessed academic programs for instruction time—and most often lose. They are at the mercy of political decisions and social fads, which change from season to season. They require specialized training for a work force that turns over frequently. They are often short-term, one shot efforts, when long term repeated programs have been proven more effective (Greenberg et al., 2001).

Thus the trend is toward comprehensive prevention programs that address health, behavioral and academic issues in a coordinated way, integrated into the academic mission of schools (Elias et al., 1997; Zins et al., 2004). The most effective of such programs enhance protective factors both at the level of the individual and of the environment (Hawkins et al., 1999; Learning First Alliance, 2001; Eccles & Wells, 2002; Weissberg & Greenberg, 1998). They affect attitudes, behavior and academic performance. They focus on skill training in core social-emotional abilities that are broadly applicable to academic, as well as social and health challenges (Zins, et al., 2004; Elias,
Gara, Schuyler, Brenden-Muller, & Scyette, 1991). They allow for both universal prevention and targeted intervention. They combine strengths-based “generic” asset development with concrete problem-solving strategies for specific academic, health and behavioral challenge (Catalano, 2004). They include proven, cognitive-behavioral strategies for reducing risk and enhancing protective factors at the level of the individual. They also target the learning environment as a major protective or risk-factor for students (Gottfredson, 1986; Solomon et al., 2000; Hawkins, Catalano, Kosterman, Abott, & Hill, 1999; Reynolds, Temple, Robertson, & Mann, 2001). The most effective comprehensive programs not only reach students but reach their peers, teachers, families and communities (Catalano, Haggerty, Gainey, & Hoppe, 1997; Hawkins, 1992; Kumpfer, 1996; Osher, 2002; Weissberg & EliaS, 1993).

**Scalability & Sustainability Challenges with Comprehensive Prevention Programs**

Successful implementation of these programs is very expensive in terms of both time and money. They are rarely scalable, or sustainable over the long term. For example, materials and training for the Skills Opportunities And Recognition (SOAR) program developed through the Seattle Social Development Project (Hawkins, 1998) are $2991 per student over six years, or $499 per student per year (Channing-Bete, 2004). Yet the national per pupil average expenditure on *all* instructional materials is just $232 per year (Ave, Cohen, & Johnson, 2004).

Even when training is in place, implementation may falter. Implementation fidelity rates in real world situations are as low as 19% for a variety of programs that have been successfully implemented in academic research environments (Ennet, et al., 2003; Gottfredson & Gottfredson, 2001; Hallfors & Godette, 2002). From the implementer’s perspective, the problem is lack of adaptability to real-world, culturally specific conditions (Schinke, Brounstein, & Gardner, 2002; Brown, Roderick, Lantieri, & Aber, 2004).

Chief among the constraints limiting successful implementation of prevention programs is time. Educators have to fit more standards-based instruction, for more students, with more diverse social backgrounds, into fewer hours than at any time in history. This leaves less classroom instruction time for prevention. Where some time is available for programming, such as during in-school suspension, trained instructors are rarely available. On the other hand, educators report from 20% to as much as 80% of all instruction time goes toward dealing with behavior problems instead of academic issues. Investment of some of that time in programs designed to improve students’ social behavior could potentially result in a net gain in available instruction time.
Practitioners also decry the fact that too often they are forced to choose between programs that are proven effective but boring or culturally insensitive, and programs that are fun and/or culturally sensitive but have not demonstrated effectiveness. They note that many programs provide instructions for teachers to make programs more interesting. However that requires extra preparation time and requires teachers to be more adept at handling group interaction than many teachers are.

Even adaptable, effective programs with well-trained instructors do not have an easy way to accommodate student differences in learning styles, English language proficiency, and attention span capacity. None can be self-directed and self-paced by users based on their learning styles and preferences.

Finally, programs that depend on live instruction cannot ensure fidelity to core components of program design and content. They do not automatically keep track of dosage or implementation rates and do not have an automated, culturally unbiased way to measure student progress or outcomes (Backer, 2000, 2001).

Schools urgently need affordable, culturally inclusive and sensitive, comprehensive prevention programs that address behavior, academic and health issues concurrently, while taking into account individual learning differences. They need programs that affect both individual competency and organizational climate. They need an integrated suite of programs, not one-offs, to reach students from early childhood through high school with parallel programs for teachers, parents and other adults who work with students. They need them not to be boring. They need these programs to fit into the bits and pieces of time that educators can glean from already full schedules. They need them to lighten the burden on teachers. They need programs that enable local people to maintain control of decisions about content that affect their communities without compromising fidelity to science-based methods or accurate information. They need easy to use standardized ways to measure student progress and program effectiveness.

A Computer-based Comprehensive Program May Hold Promise

One program developed by San Francisco-based Ripple Effects purports to provide a solution to many of these challenges. It offers comprehensive prevention training software for children, adolescents and the adults who work with them. The software is designed to address academic, health and behavior issues concurrently. It targets multiple domains of individual, peers, family
and community. It is intended to work in tandem with and enhance, not substitute for, a supportive learning environment.

The storage capacity of CD-ROM format (an encyclopedia set can fit on a disk) makes the comprehensive prevention program logistically feasible. Health and mental health issues, like depression, substance abuse and PTSD are included on the same disk as training in core social-emotional abilities, such as empathy and impulse control, as well as social behavior topics from peer pressure and talking back, to bullying and hate crimes. They are all in the same program as learning-related issues, from learning styles and ADHD to teacher conflict, and risk factors such as parental addiction, homelessness and foster care. Importantly, a hyperlink structure allows users to intuitively trace the paths that link health, behavioral and academic issues to each other (as well as to shared external factors such as parental addiction) without having to understand the complex conceptual organization beneath it.

The Ripple Effects materials are offered at a district-wide cost of $8 per student, making them more affordable than many other programs. Facilitators need less training than that necessary for live delivery of content, about 3 hours, versus 1-2 days.

In terms of structure, combining a minimum number of required core tutorials with many more optional lessons available in a data base-structured program, allows for flexibility without compromising fidelity (Backer, 2001). It also offers a method to offer more targeted intervention to individual adolescents facing a myriad of diverse personal problems and mental health issues, while not neglecting the needs for universal prevention for the rest. An administrative system that allows implementers to delete those topics considered inappropriate for a particular audience or community circumvents a thorny political problem that has vexed publishers of print materials.

Multi-media micro-tutorials, with sound, text, visuals and movies all built-in, accommodate differences in learning styles, attention capacities and kinds of intelligence. The modular structure and database of tutorials lends itself to a continuum of use from individual teachable moments to curricular settings for social studies, health or language arts. The tutorials are also adaptable to situations such as detention settings where time is available, but students come in on a rolling basis and need individualized plans to deal with particular behavior problems.

Self-directed computer-based learning allows individuals to progress at their own pace. Automated progress tracking and the flexibility for students to complete the program during their
free time or as part of a computer lab or advisory period can potentially reduce the conflict with academic instruction without restricting student access to social-emotional learning. Interactivity, an important element in successful programs, is built in (Brounstein & Zweig, 2001; Tobler et al., 2000). The program includes interactive games to assess content mastery, interactive self-profiles to foster self-reflection and understanding, and assisted journal writing to tie critical thinking and writing to reflection on social-emotional experience. The programs also include role-play instructions with an emphasis on private rehearsal, not public performance, and transfer training. Besides keeping students engaged, these interactive elements provide a practical learning path for kinesthetic learners (Gardner & Krechevsky, 1993). This potentially increases the chance of program success, especially for boys. Finally, CD-ROM technology solves a practical problem: It keeps audio-visual components from getting separated from a core curriculum, something that has frustrated implementers for years.

**Grounded in Theory**

More than 300 studies have shown that one or more strategies used in this program are effective in impacting health and social issues, such as bullying, substance use and sexual behavior, when used as part of prevention programming in a live instruction format (Schinke, Brounstein & Gardner, 2002). Studies have also shown that social-emotional skill building can positively impact academic performance, again when delivered via live instruction (Zins, Weissberg, Wang, & Walberg, 2004). Many studies have shown that academic learning can happen successfully via computer. E-learning has positive effects not only on achievement but also on attitudes toward learning for all ages and across all learning domains (Schacter & Fagnano, 1999). Studies also show people are more honest and open in responding to computer-based inquiries than to live interviewers (Turner, et al., 1998). The question is whether delivering integrated training to address academic, behavioral and health issues via a computer, instead of through a live instructor, would also enable social-emotional learning to happen effectively and would result in positive behavioral, health and academic outcomes.

**Preliminary Research on Effectiveness**

The computer-based approach to comprehensive prevention programming developed by Ripple Effects is installed in more than 400 schools in 300+ districts across the country. Documented anecdotal evidence in school and after-school settings ranging from Seattle school nurses offices,
to a Navaho Indian reservation counselor’s office, to rural juvenile justice settings, to detention programs in urban, rural and suburban settings suggest the software works. Users say it reduces discipline problems, boosts academic performance, and prompts disclosure of health problems, including sexual abuse, anorexia, suicide, and substance use (Ripple Effects, 2002, 2003).

Five objective studies on effectiveness also have been done. All suggest the program may have the power to affect attitudes, behavior and academic performance, but each has been limited by some flaw in design. In addition, because of its size, the adolescent software program has never been tested in its entirety. Rather, studies have been done on the effects of extracted lessons (ranging from 5 to 42 topics) on social competencies, aggression, and academic achievement. These preliminary studies involved ethnically diverse rural and urban boys and girls in in-school and after-school programs in San Francisco, Manhattan, Kentucky, the Bronx, Trinidad and Tobago. Details about these preliminary studies are available at www.rippleeffects.com.

**Expert Panel Review**

In 2003, with a NIDA-funded SBIR Phase I grant, Ripple Effects submitted selected tutorials from the adolescent software program to an expert panel. The purpose was to ensure alignment of the intervention with components correlated with effectiveness for substance abuse prevention, prior to undertaking a larger, scientific study of its effectiveness through a NIDA-Phase II grant. The panel critiqued content according to two sets of standards: those developed by SAMSHA for effective substance abuse and mental health programs and those developed by the Department of Education for Safe and Drug Free Schools Model Program guidelines. Expert panel members included staff from Drug Strategies and faculty from New York University and Columbia University who had expertise in School Safety programs, Social-Emotional Learning (SEL) and technology design.

Incorporating the feedback from researchers and implementers entailed the addition and/or rewriting of more than 500 screens, creation of 18 interactive profiles, and production of two training postcards for parents. The result was a prototype for a new comprehensive prevention software application. This new, theory-driven, multimedia behavior-training and school achievement program for adolescents is the subject of this NIDA-funded effectiveness study.
Hypotheses

This study tests two hypotheses:
1. If used as designed, self-directed completion of assigned tutorials from the comprehensive, prevention training software will result in reduced risk and enhanced protective factors in five areas: self-efficacy, norms and perceptions about drugs, problem social behavior, school bonding/alienation and academic achievement.
2. Under real-world conditions, if teachers received training, support, and input into the program, including making decisions about content adaptation, it will be implemented as designed and have similar results, even in low performing schools.

Method

This four-month study was designed to be an experimental study in which students in each of seven schools would be randomly assigned to intervention and comparison groups, based on odd-even birth dates, and tracked individually from pretest to posttest. Data from all schools would be pooled for analysis. The study was not originally designed to assess the effect of the intervention within individual schools. As information came in about irreconcilable differences in reporting methods for some data, however, we had to analyze the effects within individual schools as well.

Participants

We initially invited more than 30 public middle and high schools in the San Francisco Bay Area to participate in the study and screened for: presence of sufficient technology to run multimedia programs, inclusion of at-risk and minority students, capacity to randomly assign students, structure that would allow students to work individually on the computer program, support of the program by principal and staff, and staff willingness to undergo a three-hour training. We invited two additional rural schools into the study after they initiated contact with us. They were screened for the same criteria. Since the original schools were heavily weighted with Black and Latino students, the mostly Caucasian make-up of the rural schools, along with an appreciable number of Native American students, contributed to the diversity of the study population as a whole. As a result of this selection process, this four-month study initially involved students in seven schools representing a diverse mix of students and teachers and a
wide range of school environments. Overall it was weighted toward socioeconomic groups that are considered at enhanced risk for substance abuse, anti-social behavior and school failure. One small school (School 4) was unable to establish a system that could simultaneously ensure the confidentiality of student data and track the individual students as needed for a longitudinal pre-post evaluation design. Hence, pretest data provided by students enrolled in this school were excluded from the study and no attempt was made to collect posttest data from these students.

**Student Demographics**

Four schools are located in a severely distressed urban area, with very high rates of substance abuse, crimes against persons, and school failure. The vast majority of the students in three of these four schools are African-American. In the fourth school, the vast majority of students are Latino. Less than 2% of the students in each of these four urban schools are Anglo. Less than 4% are Asian in three of the schools. The fourth school has a larger Asian population (12%).

The three predominantly African-American schools are alternative schools for youth who are not succeeding in regular schools. One is a middle school. One is a continuation high school. One is a gradeless school that serves students who have failed in other environments and may already be involved with the criminal justice system. Students are grouped into “houses” according to ability level, but not ranked according to grade, in an effort to avoid stigmatizing students. Their equivalent grades are purported to be 7th and 8th, but they are often older than average 7th and 8th graders. The predominantly Latino school is a new charter school that serves students in grades 6 through 11. For most of the students in this school, English is a second language. This is denoted in Table 4 as % LEP (Limited English Proficiency). The specific grades of students from whom data have been collected for this study are specified in the first row of Table 1. In all schools where data have been collected from students in more than one grade, a roughly equal proportion of data came from each grade level, except in School 1, where most of the data were provided by 8th graders and 6th graders provided only a small amount of data.

The other two schools, one elementary school and one high school, are in a rural area that is home to a thriving marijuana trade. Mistrust of government, government programs, and authority figures runs deep. The vast majority of the students in these two schools are Caucasian; less than 3% are African-American. One of the two rural schools has a relatively large (8%) Native American population. These two schools have the lowest percentage of students eligible for free
or reduced rate lunches (denoted in Table 1 by % Lunch) and the lowest percentage of Title 1
(i.e., disadvantaged) students, though still considerably above the national average.

Table 1

*School Characteristics and Demographics (Based on Pre-test Respondents)*

<table>
<thead>
<tr>
<th>All Schools</th>
<th>School 1</th>
<th>School 2</th>
<th>School 3</th>
<th>School 5</th>
<th>School 6</th>
<th>School 7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Grades</td>
<td>6 - 12</td>
<td>8, 7, &amp; 6</td>
<td>9 - 12</td>
<td>8 &amp; 9</td>
<td>7</td>
<td>8</td>
</tr>
<tr>
<td>School Type</td>
<td>Mixed</td>
<td>Alternative</td>
<td>Continuation</td>
<td>Regular</td>
<td>Regular</td>
<td>Alternative</td>
</tr>
<tr>
<td>Urbanicity</td>
<td>Mixed</td>
<td>Urban</td>
<td>Urban</td>
<td>Rural</td>
<td>Rural</td>
<td>Urban</td>
</tr>
<tr>
<td>% Black</td>
<td>40</td>
<td>58</td>
<td>71</td>
<td>2</td>
<td>2</td>
<td>77</td>
</tr>
<tr>
<td>% Latino</td>
<td>31</td>
<td>38</td>
<td>18</td>
<td>4</td>
<td>0</td>
<td>16</td>
</tr>
<tr>
<td>% Other</td>
<td>23</td>
<td>1</td>
<td>1</td>
<td>90</td>
<td>8</td>
<td>0</td>
</tr>
<tr>
<td>% Title 1</td>
<td>64</td>
<td>100</td>
<td>63</td>
<td>0</td>
<td>34</td>
<td>100</td>
</tr>
<tr>
<td>% Lunch</td>
<td>66</td>
<td>75</td>
<td>63</td>
<td>29</td>
<td>36</td>
<td>100</td>
</tr>
<tr>
<td>% LEP</td>
<td>30</td>
<td>37</td>
<td>18</td>
<td>0</td>
<td>0</td>
<td>19</td>
</tr>
<tr>
<td>% Male</td>
<td>56</td>
<td>80</td>
<td>55</td>
<td>46</td>
<td>58</td>
<td>52</td>
</tr>
</tbody>
</table>

*Sample Size and Characteristics*

Of the 731 students recruited to participate in this study, 543 (73.7%) provided pretest data.

*Missing Data*

School 4, which enrolled 28 students who had been randomly assigned to the experimental or
comparison condition based on odd-even birth dates, was unable to establish a system that could
simultaneously ensure the confidentiality of student data and track the individual student responses needed for this longitudinal design. Hence, data provided by students enrolled in that school are excluded from the study and the baseline target population number used in the following discussion is 703 (731-28=703), not 731. Because 14 students from this school had been randomly assigned to the experimental condition and 14 students had been randomly assigned to the comparison condition, and because the 28 students from this school constituted a mere 5% of the total sample of students who provided pretest data, we do not believe the decision to eliminate data provided by these students weakens the evaluation as much as would moving to a cross-sectional research design to accommodate the data from this one small school.

**Analysis of Overall and Differential Attrition**

Experimental mortality was moderate overall. It was high for Schools 1, 2, and 3, but low or very low for Schools 5, 6, and 7.

<table>
<thead>
<tr>
<th>Table 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Differential Attrition by School</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
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<tr>
<td>Target N</td>
</tr>
<tr>
<td>% Pretested</td>
</tr>
<tr>
<td>% PrePosted</td>
</tr>
</tbody>
</table>

*Note.* Int = intervention group; Comp = comparison group.

Of the 703 students recruited for this study in the six schools that could track data on individuals longitudinally while preserving confidentiality, an equal proportion of females and males were assigned to the intervention group (37.2% of the females and 37.6% of the males). However,
both males and females assigned to the comparison group were less likely to provide pretest data than males and females assigned to the intervention group, with males in the comparison group the least likely to provide pretest data.

The overall rate of differential attrition between the intervention and comparison group is driven largely by the high rate of differential attrition in School 2, a mostly Black school which had the most unbalanced design, and the largest number of study subjects initially.

Table 3

*Modules Completed by Intervention Group Students Who Provided Both Pre- and Posttests, by School*

<table>
<thead>
<tr>
<th></th>
<th>All Schools</th>
<th>School 1</th>
<th>School 2</th>
<th>School 3</th>
<th>School 5</th>
<th>School 6</th>
<th>School 7</th>
<th>Schools 1-3</th>
<th>Schools 5-7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Completed 100%</td>
<td>15%</td>
<td>6%</td>
<td>0%</td>
<td>0%</td>
<td>36%</td>
<td>69%</td>
<td>8%</td>
<td>2%</td>
<td>25%</td>
</tr>
<tr>
<td>76% - 99%</td>
<td>34%</td>
<td>22%</td>
<td>12%</td>
<td>21%</td>
<td>45%</td>
<td>31%</td>
<td>51%</td>
<td>18%</td>
<td>46%</td>
</tr>
<tr>
<td>51% - 75%</td>
<td>14%</td>
<td>0%</td>
<td>23%</td>
<td>37%</td>
<td>5%</td>
<td>0%</td>
<td>14%</td>
<td>21%</td>
<td>10%</td>
</tr>
<tr>
<td>26% - 50%</td>
<td>12%</td>
<td>11%</td>
<td>27%</td>
<td>11%</td>
<td>0%</td>
<td>0%</td>
<td>12%</td>
<td>18%</td>
<td>7%</td>
</tr>
<tr>
<td>1% - 25%</td>
<td>11%</td>
<td>6%</td>
<td>19%</td>
<td>16%</td>
<td>14%</td>
<td>0%</td>
<td>8%</td>
<td>14%</td>
<td>8%</td>
</tr>
<tr>
<td>Completed 0%</td>
<td>14%</td>
<td>56%</td>
<td>19%</td>
<td>16%</td>
<td>0%</td>
<td>0%</td>
<td>6%</td>
<td>29%</td>
<td>4%</td>
</tr>
<tr>
<td>n</td>
<td>147</td>
<td>18</td>
<td>26</td>
<td>19</td>
<td>22</td>
<td>13</td>
<td>49</td>
<td>63</td>
<td>84</td>
</tr>
<tr>
<td>M</td>
<td>0.60</td>
<td>0.30</td>
<td>0.37</td>
<td>0.48</td>
<td>0.82</td>
<td>0.98</td>
<td>0.68</td>
<td>0.38</td>
<td>0.76</td>
</tr>
<tr>
<td>SD</td>
<td>0.38</td>
<td>0.41</td>
<td>0.31</td>
<td>0.34</td>
<td>0.33</td>
<td>0.04</td>
<td>0.32</td>
<td>0.35</td>
<td>0.31</td>
</tr>
<tr>
<td>Mdn</td>
<td>0.74</td>
<td>0.00</td>
<td>0.34</td>
<td>0.59</td>
<td>0.97</td>
<td>1.00</td>
<td>0.87</td>
<td>0.34</td>
<td>0.91</td>
</tr>
</tbody>
</table>
Apparatus

Technology Infrastructure

Because computer technology is central to the intervention being tested, only schools that had multimedia capable technology were allowed into the study. That being said, the installation combinations were diverse. They included laptops, stand alone PCs, local area networks, and a peer-to-peer network. Location of the computers included mobile laptop carts, classrooms, computer labs, the library, and the discipline room. Ratios of computers to students ranged from 1:3 to 1:20. The prevention program being tested was contained on two CD's; the pre- and posttest surveys were on a third CD. Because of the rich media, all had to be installed on each computer. (Real time video cannot be widely distributed over most local area networks.) There also were wide ranges in the availability of technical support, from paid, full-time staff, to none whatsoever. Site technical support was augmented by Ripple Effects personnel, who provided in-person support to urban locations and phone support to rural locations.

The Training Software - Structure

*Ripple Effects Comprehensive Prevention Software* is built around the proprietary *Whole Spectrum Learning System*. This multi-sensory, multi-modal approach is both a navigation system and an information structure. It includes sound, illustrations, photos, videos, survey and games, as well as text. It involves 12 types of research-validated learning strategies. At least nine of these modes of learning are available within every tutorial, for every topic. The learning modes are: Problem solving through case study (Kolb, 1984; Benard, 1991; Rees & Porter, 2002); Cognitive framework - 7 W’s (Wynn, 1990; Wiig, 2000); Cognitive-behavioral training (Kendall, 1991; Ellis et al., 1994; Botvin, 1995); Narrative/true stories - video (Coles, 1989; White & Epston, 1990; Barry & Elmes, 1997; Fogg, 2003); Peer modeling of skills - video (Bandura, 1977, # 1986); Assisted journal writing (Progov, 1980); Transfer training to domains of peers and family (Yablonskey, 1976; Richey, 1992; Gambrill, 1997); Transfer training to sports domain (especially useful to boys who may be socialized to resist the “touchy-feely” content of SEL; Media analysis (Carr 1990; Weinstein, 1997); Role-plays/rehearsal (Yablonskey, 1976; Bandura, 1986, 1997); Assessment of concept mastery, consistent with No Child Left Behind (NCLB) legislation; and Self-reflection and self-examination (SAMSHA-proven
practices). Student confidentiality is assured through password-protected entrance, encryption of written entries, and a privacy screen that can shield users from unwanted intrusion.

**Training Software - Content**

In terms of content, *Ripple Effects Comprehensive Prevention Software* contains 173 complete multi-media tutorials, comprised of almost 3000 screens. These include 31 interactive self-profiles, 870 interactive assisted writing exercises, 189 interactive content assessment exercises (games), more than 100 peer modeling videos, 44 video true stories, more than 1500 hand-drawn illustrations, and an automated tracking system to monitor student progress. It is interactive, individualizable, and includes extension components and transfer training opportunities for family, peers, and community. It is intended for students aged 11-15, or grades 6-9.

The program can be accessed in two ways, through an alphabetized list, or by category. The categories are: Assets (strengths/protective factors); Problems (related to academic failure, discipline problems and substance abuse); and Reasons (risk factors—both “inside you” and “outside you”).

**Procedure**

**Intervention Description**

The intervention objective was for implementers to assign up to 42 of the 173 available tutorials to all students in each of the intervention groups for self-directed completion over a period of seven weeks. Students were required to do only the interactive parts of each tutorial (identified as core components of the learning process). Topics would take, on average, about 15 minutes to complete, for a total of 10.5 contact hours. Students would spend more or less time on the tutorials, depending on their individual pace and the depth and breadth of their inquiry. Twenty-one of the topics were identified as core components of the curriculum required for every student at every site. Twenty-one were site-specific, chosen from the remaining 150 topics. Students could choose any additional topics at will.

Because key messages are “in the box,” fidelity to core components of content was ensured. Students did not receive any of the core content from a live instructor. They did not need to use the program in a certain order, nor to complete all parts of the tutorials on any subject. They could choose whatever modes of learning were best matched to their personal aptitudes and preferences, as long as those included the three interactive exercises which have been identified as core process components of the program. The computer automatically tracked their
completion of those tasks. A built-in, automated system monitored student progress and measured fidelity to the original design. Adult supervisors did need to have both the technology infrastructure and the necessary commitment to sit students in front of the computer and monitor student progress in completing assigned tutorials.

**Implementer Training**

The three-hour training for implementers was process training; it did not include content training in any of the areas being measured. Training was focused on four things: hands-on practice using the student software; adaptation of program content for site-specific implementation; practice using the tools for monitoring students’ progress, and use of automated tools for data collection. Ripple Effects emphasized that students need to be required, not just invited, to use the program. None of the implementers were content experts in prevention of substance abuse or anti-social behavior. In fact, the eleven implementers in this study included a cafeteria aide, a school secretary, a school custodian, a classroom volunteer, a math teacher, a language arts teacher, three English/reading teachers, and a special education teacher as well as one social worker. This mixture was judged to be a reasonable real-world test of whether content expertise could be separated from the role of facilitation in prevention programming, an important factor in the scalability of the program.

**Site-specific Adaptation**

Implementers chose their site-specific additional topics (from the 150 remaining in the program) based on personal and site goals, as well as perceived characteristics and needs of students under their care. They collectively chose a total of 77 additional tutorials to become part of the 42-topic plan at one or more sites. Of these 77 topics, 43 (56%) were used at only one site. A few, such as “teacher conflict,” were chosen by several sites. Not one optional topic was chosen for use by every site. Thus implementers took full advantage of the adaptability feature of the program, without compromising the core components of a set method of delivery and 21 core curriculum topics.

**Student Individualization**

Students were able to individualize the program further by choosing any or all topics available after they had completed the core requirements. They had a wide range of topics to choose from.
that included school-related behavioral problems, such as skipping school and talking back; personal health and mental health challenges such as depression and PTSD; individual risk factors, such as foster care and homelessness; family and community issues including addicted parents and neighborhood violence; and substance abuse-specific topics including quitting habits, and tutorials on additional drugs (besides marijuana and alcohol). Some students chose as many as 60 additional topics during the course of the study. Facilitators monitored student completion of interactive exercises by checking the automated progress reports.

Research methods

Method of Assignment to Treatment Conditions
Two schools (Schools 3 and 6) used odd-even birth dates to assign students to treatment groups. Two schools (Schools 1 and 7) used other methods of random assignment. One school (School 2) used its pupil personnel management and room scheduling software to assign students to treatment conditions. One school (School 5) had an arguably non-random assignment, while the authors regard it as random, analysis was performed as if it were quasi-experimental for conservatism in reporting results. At that school, teachers assigned students to one of two language arts classes at the end of the previous academic year with the goal of creating two balanced classes, and then assigned students in one of the classes to the experimental condition and students in the other class to the comparison condition. Hence, while this study is considered to be a randomized experimental study by the authors, it was treated as a quasi-experimental study that equated treatment groups by statistically controlling for pretest scores on all the outcome measures for which students provided data. School administrators provided posttest scores for educational and behavior measures, but for a variety of reasons, including student mobility, could not provide pretest scores for those measures.

Method of Outcome Data Collection
The process for measuring student dosage, and by proxy, implementation rates, was an automated one. The software program creates a password-protected file for each student and tracks completion of interactive exercises, the core components of each tutorial. Adult implementers can access records of the group as a whole and monitor completion of activities for each student.
Both of the scales used in student surveys are computer-based and reading-independent so as to be accessible to English Language Learners and students with low reading ability, without compromising the privacy of participants. Vocabulary has been adapted and illustrations added to be developmentally appropriate and culturally relevant to a diverse group of contemporary adolescents. No live implementers mediate any of the content of the outcome measurement instruments. A reinforcement system is built into the data collection process, so that answering any question has a positive effect on the task of springing a fish from a cracking aquarium. The reward structure is completely independent of the content of any answer.

School administrators provided objective outcome data. GPA is the only objective outcome data that was completely standardized across all sites. Data on social behavior was collected by each school, but in diverse forms. Thus they could not be part of the multi-variate model for analysis. They were analyzed separately, school by school, and are included in the discussion.

Substantive Relevance and Psychometric Properties of Outcome Measures

The study was designed to measure the effect of the software on five risk or protective factors associated with adolescent health and personal and academic success: self-efficacy, norms and perceptions about drugs, social behavior, school bonding, and academic achievement.

The outcome measures for this study included both self-reports and objective data from administrative records at each site. Self-report outcomes included student responses on three self-efficacy scales; three measures of disapproval of alcohol (ab)use and three measures of disapproval of marijuana (ab)use (to measure norms about drugs and alcohol); three measures of perception of harm from alcohol (ab)use; and three measures of perception of harm from marijuana (ab)use. Objective data from administrative records at each site included school attendance as a proxy for school bonding; infraction incident reports, discipline referrals and citizenship grades as a measure of social behavior; and grade point average as a measure of academic performance. Only GPA was kept uniformly from site to site.

The measures of alcohol and marijuana disapproval and perception of harm were developed for the Monitoring the Future study, and adapted for computer-based delivery. Norms and perception of harm were used, rather than actual use or intent to use alcohol or marijuana. This is because the level of permissions needed to survey California students on intent to engage (or actual engagement) in illegal activity would have precluded the participation of large numbers of students at the alternative and continuation schools.
The self-efficacy scales were developed for this study. Self-efficacy measures were included in the study because self-efficacy has been linked to a wide range of health outcomes and is considered a protective factor for school success as well (Bandura, 1977, 1982, 1997; Maibach & Murphy, 1995). Fatalistic thinking was specifically identified by staff at participating urban schools as something they believed caused – or compounded – school failure. It was targeted for change and is the focus of one of the three self-efficacy subscales.

The Ripple Effects Self-Determination (RESD) scales, which measure self-efficacy, are adapted in both content and format from the Multi-dimensional Health Locus of Control scales (MHLC) that are in the public domain. MHLC scales have been used in over a thousand studies. RESD scales are like the MHLC scales (and unlike Rotter’s original scale) in that they use a Likert scoring method, instead of forced choice. This allows for the fact that people may experience BOTH a strong/weak sense of internal control AND a strong/weak sense of external control. Subjects have four options of greater or less agreement with each statement. This even number prevents them from taking a completely neutral position.

RESD scales are also like the MHLC scales in that they divide external sources of control into two parts, with a scale for each. In the MHLC scales those two parts are powerful others and chance. The RESD scales differ from the MHLC scales in that they include specific identifiable structures and processes, such as racial or gender discrimination—not just powerful persons—as a possible source of external control, separate from chance.

Thus the Ripple Effects Self-Determination scales constructed for this study to assess ‘locus of control’ measure three dimensions: Internal Locus of Control (INTERNAL), External Locus of Control Due to Luck or Fate (FATE), and External Locus of Control Due to Powerful Other People and/or Powerful Social Forces (OTHER). Each scale consists of 14 items. The scale scores are computed as the means of those 14 items, as long as at least 10 items had values. That is, if only 10 items have values the mean of those 10 items is computed and imputed to be the value of the items with no values. If a respondent failed to answer at least 10 items, the scale

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1 Adapted from Rottor’s Loucs of Control scale by Wallston to apply specifically to health matters.
2 The Drinking Related Locus of Control scale, also adapted from Rotter’s scale, relies on a forced choice method, like Rotter’s original construct.
3 Originally the MHLC external control scale was uni-dimensional. Levenson contributed the concept of separating fate from powerful others (her IPC scale). The two were combined, creating a multidimensional scale of the external locus of control.
score is not computed. Cronbach reliability coefficients for these scales are .73, .77, and .66, respectively.

Correlations between the scales indicate that they are not totally independent, but that they have enough independence to be measuring three different “things.” The strongest correlation (.60) is between FATE and OTHER and is statistically significant at the two-tailed .01 level. The weakest correlation (.01) is between FATE and INTERNAL. A strong correlation between FATE and OTHER (both measures of external locus of control) and a weak correlation between INTERNAL and FATE are both expected. However, we found a stronger than expected (though still quite modest) correlation of .25 between INTERNAL and OTHERS, which is significant at the two-tailed .01 level. This is not surprising given the fact that many of the study participants were students of color from low-income communities. Some of them may well possess both a strong sense of self-efficacy and a strong awareness of the powerful social forces, such as racism and poverty, that influence the course of their lives.

Statistical Power, Program Effect Sizes, and Sample Sizes

The ability to detect a program effect of a specific size at a specific significance level (i.e., tolerance for false positives) is a function of both the sample size (i.e., number of study subjects providing outcome data) and statistical power (i.e., tolerance for false negatives). Larger program effects are easier to detect than smaller program effects, so the power to detect program effects decreases as either the size of program effect decreases or the size of the sample decreases. Power analyses conducted during the research design process motivated us to focus on the overall effect of the intervention by pooling data from each of the schools rather than assess the effect of the intervention within individual schools. For all schools combined, there would be a 70% chance (Power = .70) of detecting a standardized mean difference effect size of .22 and an 80% chance (Power = .80) of detecting a standardized mean difference effect size of .25, if all students who completed a pretest also completed a posttest. These are reasonable effect sizes to expect from a social intervention of the sort evaluated herein. The effect sizes that could be detected within individual schools are much larger than one might reasonably expect from the intervention, so originally no plans were made to analyze program effects within each school. Unexpected variance in school reporting methods caused us to later change those plans. Nonetheless, the a priori likelihood of finding effects at individual schools by means of statistical testing is quite low, so non-significant effects cannot be interpreted as “no effect.”
Statistical Analyses

Multivariate models that allow for correlation between the measures have been estimated using the Seemingly Unrelated Regression (SUR) Method. Also known as Zellner’s Method, this method accounts for both contemporaneous correlation in the errors across equations and heteroskedasticity. When using this method, the cross-equation covariance matrix estimates are based upon parameter estimates of the unweighted system. Conducting simultaneous tests in multivariate models allows for the control of overall Type I error that would be difficult to account for in the case of multiple, correlated, individual tests. Additionally, taking account of cross-equation correlation gives rise to greater precision of estimation and more powerful testing. The statistical analysis has proceeded in three stages. First, to determine whether the intervention group and the comparison group were equivalent at baseline overall, within each school, or by gender, ethnicity, or school grade, SUR has been used to estimate multi-measure models. However, it has not been possible to use this method for one of the schools because the measures are too highly correlated in the sample, presumably because there were too few students enrolled in the school relative to the number of measures. (In this school, 31 students provided pretest data and the multi-measure model involves 41 measures: three scale scores, 19 ‘norms’ items, and 19 ‘perception of harm’ items.) For this school, ordinary least squares estimates have been computed for each equation separately.

Secondly, to test for differences among students within the treatment group, a dosage measure equal to the fraction of completed intervention components identified as core components by the program developer has been added as a variate to the measure equations. SUR has been used to estimate multi-measure models of six ordinal ‘disapproval of substance (ab)use’ variables (NORMS), six ordinal ‘perception of harm from substance (ab)use’ variables (RISK), three interval ‘locus of control’ variables (RESD), and ‘grade point average’ (GPA). Pretest scores for the six NORMS, six RISK, and three RESD variables are used as covariates in the system of

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4 Note that 19 substance (ab)use NORMS variables and 19 Risk variables were used to test for baseline equivalence, but only the six NORMS and six Risk variables related to alcohol and marijuana (ab)use were included in the multivariate tests of program effectiveness, because only the modules on alcohol and marijuana were modules all schools were required to implement. Modules on cigarettes, cocaine, etc. were optional modules.
equations to statistically control for pretest differences, and these measures are treated as ratio measures for purposes of estimation and testing. No pretest score for GPA is available, so the system of equations does not employ a pretest GPA covariate. The dosage model has been estimated seven times: once for all schools combined, and once for each of the six schools separately. To control for the fact that multiple tests of significance have been conducted, Bonferroni corrections have been applied to each of the school-specific, multivariate analyses of covariance.

Thirdly, to test for differences at posttest between the intervention group and the comparison group that could be attributed to the intervention, the same measures, modeling, and estimation are used as that described above, but the dosage measure used above as a ratio variate is replaced by a dichotomous variate indicating treatment condition (i.e., intervention or comparison group). As a general rule, all test statistics have been referred to the relevant asymptotic probability distribution: all multi-parameter test statistics have been referred to the appropriate chi-square distribution, while single parameter tests have been referred to the standard normal distribution. Tests are two-sided, with probability 0.05 of falsely rejecting the hypothesis of no effect.
Results

Baseline Equivalence Among Pretest Respondents

Overall, the intervention and comparison groups are equivalent at baseline on the 41 self-report measures included on the pretest. However, while there is baseline equivalence between the intervention and comparison groups for all schools combined, within five of the six schools statistically significant differences exist between the intervention and comparison groups. These differences involve a relatively small number of the 41 measures (from a low of three to a high of eight), and there does not appear to be a pattern across schools.

Substantial differences at baseline between genders, ethnicities, and grade levels is both expected and found. Ethnic differences exist on 35 (85%) of the 41 outcome measures. On the RESD scales, Black students and White/Other students are equivalent, but Latino students differ from both Black students and White/Other students on the two external locus of control scales (fate, others), but not on the internal locus of control scale. Gender differences exist at pretest on 16 (39%) of the 41 measures, including the two external locus of control scales (but not the internal locus of control scale). Grade level baseline differences exist on 23 (56%) of the 41 measures, but not on any of the three RESD scale scores.

Outcomes Based on Self-report Data

For the outcomes based on self-report data provided by the students, comparability of data across study sites is assured. In addition, because pretest measures could be used as covariates to statistically control for any inequivalences between treatment and comparison groups, there is no uncertainty about baseline equivalence for these data.

Effect of Dosage on Treatment Group, All Schools Combined

The multivariate analysis of covariance examining differences among students within the treatment group who received different intervention dosages reveals statistically significant overall differences within the system of equations (p = .000). Further, statistically significant differences are identified for four of the 16 variables: INTERNL, NORMS05, RISK07, and GPA. A unit increase in dosage (from 0% to 100%) is associated with an increase in grade point average (GPA) of 1.07 (on a scale from 0 to 4), an increase in disapproval of trying marijuana once or twice (NORMS05) of 0.47 (on a scale from 1 to 3), an increase in perception of risk of
harm from smoking marijuana regularly (RISK07) of 0.50 (on a scale from 1 to 4), and a decrease in internal locus of control (INTERNL) of 0.18 (on a scale from 1 to 4).

Implementation Fidelity (Dosage), by School

Implementation fidelity varies considerably across sites. The median percentage of modules completed in Schools 1, 2, and 3 is 34%, whereas the median percentage of modules completed in Schools 5, 6, and 7 is 91%. The median percentage of modules completed ranges from 0% in School 1 (where 56% of the intervention group students did not complete a single module) to 100% in School 6 (where 69% of the students completed all the modules and all of the students completed at least three-quarters of the modules).
Effect of Dosage on Treatment Group, by School

The multivariate analyses of covariance examining differences among students in the treatment group who received different intervention dosages reveal statistically significant differences attributable to dosage in each school after a Bonferroni correction is applied to the six significance tests (one significance test for each school). In School 1, the only statistically significant difference is for GPA (which increases 1.64 points with a unit increase in dosage, p = .00). In School 2, statistically significant differences exist for both GPA (p = .00) and INTERNL (p = .00). GPA increases 1.38 points with a unit increase in dosage (from 0% to 100%) and INTERNL decreases 0.55 points (on a scale of 1 to 4).

In School 3, several statistically significant differences attributable to different dosages received within the treatment group are found. A unit dosage increase (from 0% to 100%) is associated with an INTERNL decrease of 0.52 points (on a scale of 1 to 4), a NORMS02 (disapproval of trying one or two drinks) increase of 0.82 points (on a scale of 1 to 3), a NORMS06 (disapproval of smoking marijuana occasionally) decrease of 1.00 points (on a scale of 1 to 3), and a RISK04 (perception of risk of harm from having five drinks once or twice each weekend) increase of 1.17 points (on a scale of 1 to 4). In addition, a unit dosage increase is associated with a 0.91 increase in GPA.
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Table 5

Intervention Effect: Treatment Versus Comparison Groups, by School

<table>
<thead>
<tr>
<th>School</th>
<th>All Schools</th>
<th>School 1</th>
<th>School 2</th>
<th>School 3</th>
<th>School 5</th>
<th>School 6</th>
<th>School 7</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \chi^2 )</td>
<td>Coef 0.546</td>
<td>Coef 0.149</td>
<td>Coef 0.491</td>
<td>Coef 0.061</td>
<td>Coef 0.000</td>
<td>Coef 0.000</td>
<td>Coef 0.001</td>
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<td>Fate</td>
<td>-0.06 0.123</td>
<td>0.20 0.178</td>
<td>-0.03 0.695</td>
<td>-0.16 0.165</td>
<td>-0.09 0.239</td>
<td>0.00 0.973</td>
<td>-0.18 0.023</td>
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<tr>
<td>Others</td>
<td>-0.01 0.770</td>
<td>-0.14 0.335</td>
<td>-0.04 0.609</td>
<td>0.02 0.840</td>
<td>0.00 0.950</td>
<td>-0.05 0.603</td>
<td>0.02 0.763</td>
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<tr>
<td>Internal</td>
<td>-0.02 0.700</td>
<td>0.11 0.537</td>
<td>-0.08 0.345</td>
<td>0.11 0.296</td>
<td>0.05 0.517</td>
<td>-0.10 0.290</td>
<td>-0.07 0.281</td>
</tr>
<tr>
<td>Norms02</td>
<td>0.01 0.927</td>
<td>0.01 0.981</td>
<td>-0.04 0.819</td>
<td>-0.01 0.981</td>
<td>-0.05 0.806</td>
<td>-0.87 0.000</td>
<td>0.35 0.021</td>
</tr>
<tr>
<td>Norms03</td>
<td>0.11 0.159</td>
<td>0.24 0.285</td>
<td>-0.06 0.674</td>
<td>0.13 0.542</td>
<td>0.20 0.319</td>
<td>0.05 0.806</td>
<td>0.19 0.267</td>
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<td>Norms04</td>
<td>0.07 0.422</td>
<td>-0.23 0.377</td>
<td>-0.21 0.244</td>
<td>0.19 0.428</td>
<td>-0.12 0.548</td>
<td>0.01 0.967</td>
<td>0.28 0.073</td>
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<td>0.08 0.357</td>
<td>-0.15 0.592</td>
<td>-0.02 0.895</td>
<td>0.06 0.760</td>
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<td>0.34 0.039</td>
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<td>Norms06</td>
<td>0.08 0.345</td>
<td>0.29 0.253</td>
<td>-0.09 0.611</td>
<td>-0.04 0.861</td>
<td>0.24 0.139</td>
<td>-0.15 0.433</td>
<td>-0.02 0.906</td>
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<td>Norms07</td>
<td>0.01 0.897</td>
<td>-0.02 0.919</td>
<td>0.17 0.341</td>
<td>0.01 0.965</td>
<td>-0.27 0.100</td>
<td>-0.20 0.411</td>
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<td>Risk02</td>
<td>0.14 0.186</td>
<td>0.50 0.15</td>
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<td>0.30 0.182</td>
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<td>0.19 0.057</td>
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<td>0.19 0.346</td>
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<td>0.01 0.941</td>
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<td>0.27 0.165</td>
<td>-0.13 0.666</td>
<td>0.03 0.898</td>
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<td>-0.19 0.355</td>
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</tr>
<tr>
<td>Risk06</td>
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<td>-0.10 0.735</td>
<td>0.34 0.088</td>
<td>0.24 0.422</td>
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<td>-0.77 0.031</td>
<td>-0.37 0.036</td>
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<tr>
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<td>-0.03 0.777</td>
<td>-0.27 0.305</td>
<td>0.09 0.682</td>
<td>0.23 0.465</td>
<td>-0.18 0.429</td>
<td>-0.19 0.627</td>
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<tr>
<td>GPA</td>
<td>0.00 0.959</td>
<td>-0.05 0.849</td>
<td>-0.01 0.927</td>
<td>-0.68 0.000</td>
<td>0.92 0.001</td>
<td>0.47 0.060</td>
<td>0.16 0.046</td>
</tr>
</tbody>
</table>

Note. Coef = Coefficient
The most pronounced favorable effect of dosage on students in the intervention group is observed at School 7, where NORMS05, NORMS06, and NORMS07 (disapproval of trying marijuana, smoking marijuana occasionally, and smoking marijuana regularly, respectively) all show increased levels at higher levels of dosage, as does RISK02 (perception of risk of harm from trying one or two drinks of alcohol) and RISK07 (perception of risk of harm from smoking marijuana regularly). A unit increase in dosage (from 0% to 100%) is associated with a NORMS05 increase of 1.84 (p = .00), a NORMS06 increase of 0.69 (p = .02), and a NORMS07 increase of 0.75 (p = .01) on scales from 1 to 3, while RISK02 shows an increase of 1.00 (p = .04) and RISK07 an increase of 1.08 (p = .01) on scales of 1 to 4.

The two statistically significant differences due to dosage within the intervention group at School 5 are both in a direction opposite to that which was hypothesized. A unit dosage increase (from 0% to 100%) is associated with a NORMS02 (disapproval of trying one or two drinks) decrease of 1.10 points (p = .05) and a NORMS07 (disapproval of smoking marijuana regularly) decrease of 0.74 points (p = .03) on scales of 1 to 4.

At School 6, lower levels of FATE and INTERNL and higher levels of NORMS04 and RISK06 are statistically associated with higher levels of dosage. A unit increase in dosage (from 0% to 100%) corresponds to a FATE decrease of 1.40 (p = .02) and an INTERNL decrease of 3.51 (p = .00) on scales of 1 to 4, while associated with a NORMS04 (disapproval of having five drinks once or twice each weekend) increase of 1.63 (p = .02) on a scale from 1 to 3 and a RISK06 (perception of risk of harm from smoking marijuana occasionally) increase of 1.91 (p = .01) on a scale of 1 to 4. However, all statistical estimates are based on variance. When there isn’t any variance, the model breaks down. Note that all but one of the students at School 6 had dosages of 97% or above. Given this limited variability in the predictor variable, the estimates (regression coefficients) generated by the model cannot reliably be extended to predict the behavior and attitudes of students at moderate and low dosage levels, so that the effect of dosage at School 6 remains uncertain. Further, the estimates actually obtained are quite sensitive to the observed measures on the one “outlier” having a dosage of 87%.

*Correlation Between Dosage, Academic Achievement and Attendance*

The association between dosage and GPA may be driven, or mediated, by other factors. One factor that might plausibly impact both dosage and GPA is attendance. To determine whether students who attend school more have both higher dosage scores and higher GPAs, the
distributions of both dosage and GPA are examined at different levels of absence for the five schools that provided ‘Days Absent’ data and for the four schools that provided ‘Periods Absent’ data. The distributions of dosage for students who did not miss any days of school and for students who missed one or more days are both bimodal with a large number of students who completed none of the intervention and a large number of students who completed all of the intervention. These bimodal distributions suggest that factors other than school absence account for dosages received by students in the treatment group. However, periods absent is negatively correlated with dosages received \((r = -.41)\), suggesting that class attendance or absence may account for some of the dosages received.

The median GPAs for students who missed zero, one, or more than one days of school, respectively, are 3.00, 2.82, and 2.70, suggesting that days absent may be associated with GPA. However, a test of equality of medians reveals these differences are not statistically significant.

**Intervention effects: treatment versus comparison groups, all schools combined**

The multivariate analysis of covariance examining differences between the treatment and comparison group for all schools combined reveals that the system of equations is not significant \((\chi^2 = 14.71, p = .545)\). There is no overall statistically significant difference between the treatment and comparison group for all schools combined when the intervention is implemented as it was in these six schools, that is when a large number of students in the intervention group were not exposed to the intervention.

**Intervention Effect: Treatment Versus Comparison Groups, by School**

The multivariate analyses of covariance examining differences between the treatment and comparison groups within each school reveal statistically significant overall differences within School 5, School 6, and School 7 (even when a Bonferroni correction is applied to the six significance tests). The observed p-value for these three sites is less than 0.008, so even under the Bonferroni correction’s most conservative statistical assumptions, the difference is significant between the treatment and comparison groups within these three schools where most students received at least a minimum dosage of the intervention.
Table 6
Pooled Treatment & Dosage Effects in Schools 5, 6, & 7

<table>
<thead>
<tr>
<th></th>
<th>Treatment Effects</th>
<th>Dosage Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coef</td>
<td>p</td>
</tr>
<tr>
<td>$\chi^2$</td>
<td>47.63</td>
<td>0.000</td>
</tr>
<tr>
<td>Fate (External LOC due to luck or fate)</td>
<td>-0.14</td>
<td>0.010</td>
</tr>
<tr>
<td>Others (External LOC due to powerful others)</td>
<td>-0.01</td>
<td>0.898</td>
</tr>
<tr>
<td>Internl (Internal locus of control)</td>
<td>-0.02</td>
<td>0.611</td>
</tr>
<tr>
<td>Norms02 (Disapproval of trying alcohol)</td>
<td>0.07</td>
<td>0.543</td>
</tr>
<tr>
<td>Norms03 (Disapproval of daily drinking)</td>
<td>0.22</td>
<td>0.064</td>
</tr>
<tr>
<td>Norms04 (Disapproval of binging on weekends)</td>
<td>0.14</td>
<td>0.238</td>
</tr>
<tr>
<td>Norms05 (Disapproval of trying marijuana)</td>
<td>0.19</td>
<td>0.107</td>
</tr>
<tr>
<td>Norms06 (Disapproval of occasional marijuana use)</td>
<td>0.08</td>
<td>0.413</td>
</tr>
<tr>
<td>Norms07 (Disapproval of regular marijuana use)</td>
<td>-0.08</td>
<td>0.470</td>
</tr>
<tr>
<td>Risk02 (From trying alcohol)</td>
<td>0.15</td>
<td>0.324</td>
</tr>
<tr>
<td>Risk03 (From daily drinking)</td>
<td>0.21</td>
<td>0.123</td>
</tr>
<tr>
<td>Risk04 (From binging on weekends)</td>
<td>-0.08</td>
<td>0.585</td>
</tr>
<tr>
<td>Risk05 (From trying marijuana)</td>
<td>-0.10</td>
<td>0.517</td>
</tr>
<tr>
<td>Risk06 (From occasionally smoking marijuana )</td>
<td>-0.39</td>
<td>0.006</td>
</tr>
<tr>
<td>Risk07 (From regularly smoking marijuana)</td>
<td>-0.11</td>
<td>0.451</td>
</tr>
<tr>
<td>GPA (Grade Point Average)</td>
<td>0.40</td>
<td>0.000</td>
</tr>
</tbody>
</table>

*Note.* Coef = coefficient.
For School 5, the only variable with a statistically significant difference is GPA (p = .00), with mean GPA being 0.92 points higher (on a scale of 0 to 4) for students in the intervention group. For School 7, which had the largest sample size but the lowest dosage of the three schools with statistically significant overall treatment group differences, statistically significant differences between the intervention group and the comparison group are found for five of the 16 variables, with GPA being 0.16 points higher in the intervention group (p = .05) and FATE being 0.18 lower in the intervention group (p = .02).

Statistically significant differences in the hypothesized direction exist for NORMS02 (disapproval of trying one or two drinks of alcohol) and NORMS05 (disapproval of trying marijuana once or twice), with alcohol disapproval being 0.35 higher (p = .02) and marijuana disapproval being 0.34 higher (p = .04) in the intervention group (on a scale of 1 to 3). The fifth statistically significant difference between the treatment and comparison group in School 7 is opposite to the hypothesized direction. RISK06 (perceived risk of harm from smoking marijuana occasionally) is 0.37 lower (p = .04) in the treatment group (on a scale of 1 to 4).

For School 6, two statistically significant differences exist, but both involve differences that are opposite to the hypothesized direction. RISK06 (perceived risk of harm from smoking marijuana occasionally) is 0.77 lower (p = .03) and NORMS02 (disapproval of trying one or two drinks of alcohol) is 0.87 lower (p = .00) in the intervention group than in the comparison group.

Differences between the intervention and comparison groups on RISK06 are opposite to the hypothesized direction in both School 6 and School 7. The difference between the intervention and comparison group on NORMS02 is in the hypothesized direction in School 7, but in the opposite direction in School 6.

*Pooled Effects in Three Schools with Adequate Implementation*

The median percentage of modules completed in Schools 5, 6, and 7 is 91%, indicating high implementation of the intervention at these sites. The median percentage of program modules completed in Schools 1, 2, and 3 is 34%, indicating low implementation at these sites. To assess the overall effect of the intervention where it was reasonably implemented, data for Schools 5, 6, and 7 have been pooled.
Table 7

School Absences and Disciplinary Actions Per Student in Schools with Adequate Implementation

<table>
<thead>
<tr>
<th></th>
<th>School 5</th>
<th></th>
<th>School 6</th>
<th></th>
<th>School 7</th>
<th></th>
<th>Schools 5-7</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Int</td>
<td>Comp</td>
<td>% diff</td>
<td>Int</td>
<td>Comp</td>
<td>% diff</td>
<td>Int</td>
<td>Comp</td>
</tr>
<tr>
<td>Days Absent</td>
<td>0.462</td>
<td>0.889</td>
<td>48%</td>
<td>0.333</td>
<td>0.750</td>
<td>56%</td>
<td>0.306</td>
<td>0.431</td>
</tr>
<tr>
<td>Days Tardy</td>
<td>0.462</td>
<td>0.630</td>
<td>27%</td>
<td>2.000</td>
<td>4.375</td>
<td>54%</td>
<td>1.323</td>
<td>1.586</td>
</tr>
<tr>
<td>Referrals</td>
<td>0.769</td>
<td>1.148</td>
<td>33%</td>
<td>9.267</td>
<td>23.313</td>
<td>60%</td>
<td>0.194</td>
<td>0.276</td>
</tr>
<tr>
<td>Detentions</td>
<td>0.769</td>
<td>1.148</td>
<td>33%</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
<td>0.177</td>
<td>0.224</td>
</tr>
<tr>
<td>Suspensions</td>
<td>0.000</td>
<td>0.000</td>
<td>0%</td>
<td>0.000</td>
<td>0.000</td>
<td>0%</td>
<td>0.016</td>
<td>0.034</td>
</tr>
<tr>
<td>Expulsions</td>
<td>0.000</td>
<td>0.000</td>
<td>0%</td>
<td>0.125</td>
<td>0.000</td>
<td>100%</td>
<td>0.000</td>
<td>0.000</td>
</tr>
</tbody>
</table>

*Note.* Int = intervention group; Comp = comparison group; % diff = percentage difference.
The multivariate analysis of covariance examining differences between the treatment and comparison groups reveals a statistically significant difference overall ($\chi^2 = 47.63$, $p = .000$), with statistically significant differences between the intervention and comparison groups on three of the 16 variables. External locus of control due to luck or fate is 0.14 lower (on a scale of 1 to 4) in the intervention group than in the comparison group ($p = .01$). Perception of harm from occasional use of marijuana is 0.39 lower (on a scale of 1 to 4) in the intervention group than in the comparison group ($p = .01$). Grade point average is 0.40 higher (on a scale of 0 to 4) in the intervention group than in the comparison group ($p = .00$).

**Effects on Attendance and Behavioral Problems in Three Schools with Adequate Implementation**

While differences in the way attendance and disciplinary data were recorded by administrators in each of the six schools prevented us from including these data in the multivariate analysis of covariance, among the three schools that had adequate implementation, comparable data were collected for five behavioral outcomes for all three schools and for one other behavior outcome for two of the three schools. Table 8 shows the incidence rates for days absent, days tardy, disciplinary referrals, detentions, suspensions, and expulsions for each of the three high implementation schools and for all three high implementation schools combined. (Note that these behavioral incidence rates are based on the number of students assigned to treatment conditions and are not contingent upon whether students completed a pretest or a posttest.) On every behavioral outcome, students in the intervention groups fared better than students in the comparison groups.

While each of the schools collected additional outcome data that were not comparable across schools and presenting the results for each of those noncomparable outcomes would be both unwieldy and statistically problematic, one measure used only by School 7 stands out. School 7 gives grades for social behavior, with separate grades for personal and social responsibility. The intervention group students received higher grades than the control group students on both measures. The mean personal responsibility grade for the control group was 2.72 (SD = 0.49), compared to 3.20 (SD =
0.51) for the treatment group. The mean score for social responsibility in the control group was 2.76 (SD = 0.47), versus 3.18 (SD = 0.54) for the treatment group.
Discussion

Summary

Both the overall effects of the Ripple Effects software at sites where implementation was successful and the dosage-correlated effects at sites where overall implementation was low support the hypothesis that Ripple Effects Comprehensive Prevention Software has positive effects on risk and protective factors, including attitude (reduced fatalistic thinking), behavior (discipline referrals, absenteeism, and tardiness) and performance (GPA and citizenship grades).

Relevance of Data from Individual Schools

This study was originally designed to assess the overall effect of the intervention when implemented in diverse settings with diverse populations by pooling data from all of the schools. It was not designed to assess the effect of the intervention within individual schools. Power analyses conducted during the planning phase of the study indicated that there would be sufficient power with data pooled from all the schools to detect effects of the size one might reasonably expect if the intervention is effective, but inadequate power to detect effects within individual schools.

It nonetheless seems unwise—given the size of disparities in implementation rates among sites, such that the majority of students in the intervention group at one site were not exposed to the program at all—to make a judgment about the effectiveness of the intervention by pooling all the sites' data.

In accordance with the design, we have provided analysis findings for all schools combined. Yet because these findings are limited by implementation problems, experimental mortality, and considerable differential attrition, we also have provided findings for each school individually. However, since each school is populated predominantly by one just one ethnic group, we do not believe the individual school data has the most generalizability.

Utility of Findings

The findings that we believe are most useful for assessing the effectiveness of the intervention are the findings based on the pooled data from Schools 5, 6, and 7. These findings are limited in their statistical power, but given that the median percentage of the
intervention completed by students in Schools 5, 6, and 7 is 91% (but only 34% in Schools 1, 2, and 3) and given that the experimental mortality rates in Schools 5, 6, and 7 are all less than 25% (but they all exceed 60% in Schools 1, 2, and 3), pooling data from the schools with adequate implementation of both the intervention and the research protocols while excluding data from schools with serious implementation problems at least solves the problem of trying to assess the efficacy of an intervention that was not implemented and the problem of generalizability that arises when experimental mortality is excessive. We therefore focus our discussion on findings from Schools 5, 6, and 7.

Schools 5, 6, and 7 represent a diverse mix of school types and population demographics, although there is greater homogeneity within these three schools than within Schools 1, 2, and 3. The study samples in Schools 1, 2, and 3 consist of students in at least two different grades at each school, whereas the study samples in Schools 5, 6, and 7 are drawn from a single grade within each school. In addition, the study samples in Schools 1, 2, and 3 include students in higher grades (though that was not the original intent) whereas the study samples in Schools 5, 6, and 7 include only 7th, 8th, and 6th graders, respectively. Nonetheless, the pooled sample that remains after Schools 1, 2, and 3 are excluded is still quite diverse, because it draws from a predominantly Latino, urban charter school, a predominantly Black, urban alternative school, and a predominantly White, rural traditional school. Findings based on the pooled sample, therefore, should be generalizable to a wide variety of middle school settings and populations.

**Academic Achievement**

With regard to the one outcome measure included in the multivariate analysis for which data were provided by the school administrators, rather than by student survey (i.e. GPA), statistical findings provide evidence that the intervention, when adequately implemented, can positively impact academic achievement. For the three schools combined that adequately implemented the intervention, there is a significant positive difference between the treatment and comparison groups on GPA. Furthermore, the school level analyses reveal significant positive effects on GPA at the two largest schools. We believe that the reason the positive effect on GPA is not significant at the third school is simply due to the small sample size resulting in insufficient power to detect the effect, which is large enough to be clinically important.
The school demonstrating the smallest positive intervention effect on GPA (School 7) is the one school in this group that experienced implementation problems. Half of the intervention group students in School 7 (the girls) used Ripple Effects Comprehensive Prevention Software in a computer lab. The other half (the boys) used the software with notebook computers. The computer lab group completed two topics per day, four days per week, as planned. The notebook computer group experienced a month-long delay due to technical problems before they could begin the intervention and they consistently experienced more difficulty using the program. In addition, the school did not have enough notebook computers, so the boys in the notebook computer group had to share computers. Consequently, even among the boys who were able to complete most of the intervention’s scope and sequence, the modules were not completed in a timely manner. Much of the reinforcement of ideas introduced in earlier modules provided by subsequent modules thus may have been lost.

Social Behavior

We originally intended to include a large number of behavioral outcomes in this study and did in fact collect behavioral outcome data from each of the schools. We were, however, unable to include these data in our overall multi-variate models. This was because the schools did not define and collect the behavioral outcome data in the same way and because the proportions of some kinds of incidents, such as expulsions, in the study sample during the intervention were too low to subject them to statistical analysis.

We thought we would at least be able to get comparable attendance data from each of the schools. But, because one of the schools is a continuation school for adjudicated and other troubled youth, even the attendance data are not comparable across school sites.

While differences in the way attendance and disciplinary data were recorded by administrators in each of the six schools prevented us from including these data in the multivariate analysis of covariance, among the three schools that had adequate implementation, comparable positive program effects were discerned on five behavioral outcomes for all three schools and one other behavioral outcome for two of the three schools.
Absenteeism. The three schools that had high to very high program implementation, Schools 5, 6 and 7, provided absentee data in “days absent.” Their pooled results showed 42% fewer days absent in the treatment group than in the comparison group. This is a financially, as well as clinically, important number.

Financial implications. Not only is absenteeism a marker of school bonding (or the lack of it), it is a major factor in school funding. Schools receive payment averaging at least $35 per student per day, or a minimum of $6300 per year, based on attendance. Reducing overall absenteeism by just two students per day could more than pay for the school-wide cost of the intervention. Reducing it by just 10 students per day (about a tenth of the 20% absenteeism rate of many middle schools with 500 students) could fund a full-time, school counselor. For urban high schools with 1000 students or more, where daily truancy rates are often higher than 20%, the recaptured revenue from cutting absenteeism by 40% could fund music and athletic programs—after paying for the software, training, and evaluation instruments (80 students x $6300 per year =$504,000).

Researchers often are uncomfortable discussing the financial implications of various interventions. They justifiably assert that all effective prevention pays for itself over the long term, in saved health and incarceration costs, reduced welfare payments and increased lifetime earnings. Unfortunately, schools’ zero-based budgeting does not allow for such a long-term horizon. We believe effective prevention focused on Social-Emotional Learning must—and can—show a shorter-term cost/benefit ratio to gain universal acceptance in schools.

Problem behavior. A factor that has even more direct social consequences than absenteeism, and increasingly is a measure of school safety, is problem behavior by students. Looking only at the three schools that had high levels of implementation (Schools 5, 6 and 7) the data that were collected from each school show the trend is toward reduced problem behavior at every site, on every parameter, and the magnitudes of these reductions are clinically important. Discipline referrals were down 60% overall. Detention was down more than 30%. Suspensions were cut in half. In School 6, which did not separate detentions from other discipline referrals, the treatment group had 60% fewer discipline referrals than the comparison group. Of the 139 discipline referrals in the intervention group, a single student generated 79%. This was not the case in the control
group, where about a half dozen students accounted for the majority of incidents. School 7, which had the lowest overall ratio of pre-intervention problem behavior still had 30% fewer referrals in the treatment group. It was the only school to assign citizenship grades. Both personal and social responsibility grades were higher in the treatment group, by 18% and 15% respectively. While the percentages are not so impressive, the net result was the mean grade for intervention group students for both personal and social responsibility was above 3.0—a significant achievement for Latino students with multiple risk factors.

Anecdotal Evidence

Anecdotal evidence supports the findings about academic performance as well as social behavior. A teacher at School 6 described a student who had failed at every single school undertaking, until he used the self-paced, reading-independent software intervention. Because it accommodated his low language skills, allowed him to take whatever time he needed, and enabled him to draw on his own social-emotional experience as a source of learning, he was able to complete assignments for the first time and became convinced that he was capable of learning. Another implementer commented that, “all our best students are in the Ripple Effects group,” even though these students had been randomly assigned to experimental conditions.

At School 1, a teacher described a particularly troubled student whose “behavior was better each time after he used the computer program.” At School 6, the program facilitator commented on the fact that two of the students from the small intervention group had been selected as students of the month, while two students from the control group were expelled during the same time period. A Special Education teacher at School 3 described a student whom he had been completely unable to reach. This student had a terrible relationship with the Dean and with other classmates and was failing academically. After just a few weeks using the software, relationships with both adults and other students were transformed; the student was succeeding academically and had no further discipline referrals.

These effects did not necessarily stop at the school door. A mother who worked at School 6 where her daughter was a student in the treatment group reported that since using the program, her daughter had started communicating with her much more and
called each afternoon to check in with her, something she had never done previously. Finally, interviews with students themselves support the objective findings. One girl described being able to deal with sexual harassment after using the program. A boy described himself as smart, but a slacker at school, because his peers teased him about trying to be “all that” if he performed well. After using the program, he was able to both achieve in school and keep his friends. A girl told the interviewer, “This [using the software program] was better than winning the lotto.” She explained, “If my mom had won the lotto, we’d just have the money, then spend it, but this [Ripple Effects Comprehensive Prevention Software] is like—getting my life. I can do what I want with it.”

Findings Contrary to Hypotheses

In addition to positive outcomes, for the same three schools with adequate implementation and research protocols, there were two areas in which study findings were contrary to the original hypotheses.

Perception of Harm from Marijuana

The intervention appears to have adversely impacted perception of harm from occasional use of marijuana in particular (p = .006) and perhaps perception of harm from any use and frequent use of marijuana use as well. (Statistically significant differences are not detected for any use and frequent use with the available power in this small sample, but both show lower perception of harm in the treatment group than in the comparison group for all three schools combined and in nearly each individual case.)

There are at least two potential explanations for this finding. One involves the fact that increased sensitivity to the topic of marijuana use was coupled with extensive media coverage about marijuana arising as a result of the federal government’s decision to prosecute providers of medicinal marijuana within the study's catchment area during the period of the study. The second involves the possibility that students got the ‘wrong message’ from the full set of lessons about drugs and drug use. We shall address the latter possibility first.

Content about marijuana is addressed directly in the marijuana tutorial and indirectly in tutorials about alcohol, cocaine, methamphetamine, and other substances. The direct normative message conveyed in Ripple Effects Comprehensive Prevention
Software is: marijuana use is illegal and hence wrong. The indirect message about marijuana’s harmfulness may be more ambiguous. Because the tutorials on alcohol, methamphetamine, cocaine, and other substances ascribe more harmful potential effects to those substances than the tutorial on marijuana ascribes to marijuana, it is plausible that students concluded marijuana is less harmful relative to those other substances and hence their perception of harm from marijuana use declined.

The second plausible explanation is that the media analysis “transfer training” activity in Ripple Effects Comprehensive Prevention Software heightened student awareness of news coverage about the federal government’s prosecution of providers of medicinal marijuana who were operating in conformance with state laws and local ordinances in both of the two study catchment areas. This media coverage largely focused on the beneficial health effects of marijuana, especially in communities with high HIV rates, which includes the urban center that was one of the catchment areas in this study. During the period of the study, several “medical marijuana” clubs chartered by the city were raided by the federal government, temporarily closed, and re-opened. A highly respected local doctor was arrested for his participation in the distribution of medical marijuana. A great deal of local press covered these events, with most supporting the continuation of medical marijuana clubs, which had been created as a result of a statewide voter referendum. The doctor who was arrested became something of a local martyr. The whole process may have strengthened marijuana’s position as a legitimate, alternative medicine, especially among students who became attuned to this issue (i.e., students in the intervention group).

Anecdotal evidence that the medical marijuana issue impacted student responses to the marijuana ‘perception of harm’ items was provided by one girl during a site visit who specifically inquired about medical marijuana in response to the marijuana ‘perception of harm’ item prompts. Further study is warranted in communities where marijuana is not a major cash crop (our rural catchment area) and where medical marijuana is not a front-burner political or health care issue (our urban catchment area).

Internal Locus of Control Didn’t Go up

Reducing fatalistic thinking and increasing self-efficacy are among Ripple Effects Comprehensive Prevention Software’s key learning objectives. The pooled findings for
Schools 5, 6 and 7 do show significantly lower levels of fatalistic thinking (-.14 on a scale of 1-4) in the intervention group relative to the comparison group, after controlling statistically for any pretest differences in fatalistic thinking between the intervention and comparison groups. This provides evidence that the intervention is working as intended in this regard.

However, the reduction in fatalistic thinking was not accompanied by higher scores on the internal locus of control scale, despite the facts that (1) that was the intent and (2) GPA, a factor associated with greater self-efficacy, went up. In fact, in some schools, internal locus of control scores went down. This intervention challenged students to reconsider the role of luck or fate in their lives. It also encouraged them to think about their personal risk factors, differentiating between those factors under personal control, and those outside their immediate control, including powerful social forces such as poverty and racism. Perhaps it should not be so unexpected that these students, who were largely from minority and low-income populations with multiple risk factors, came to believe that their future was not simply a matter of fate, but also still did not see it as greatly under their personal control.

“Confronting injustice” and “confronting institutions” are two optional topics in the prevention training software. In light of these results, perhaps they should be considered mandatory—part of the suite of core social-emotional abilities needed to ensure self-efficacy in a society that is characterized by great opportunity and great personal freedom, but also powerful social forces that lead to and reinforce great social inequality. It also would be useful to test the effect of the software on levels of internal locus of control among students who were not facing so many family and societal risk factors.

**If It Works, Will They Use It? The Implementation Issue**

The second research question, “Will the computer-based training program be implemented widely in real world settings that serve students with multiple risk factors, and will it still work?” needs more investigation. The very large differences in implementation rates among sites in this study, even after care had been taken to motivate and enable implementers, suggest the need to further define what are the minimal
necessary conditions for the software to be used by students and monitored by supervisors.

The overall implementation rate for this study was 60% under real world conditions. This implementation rate is “moderate” by SAMHSA standards, but below what is needed for all students to succeed. It was carried out in publicly funded schools with high teacher turnover, serving students with multiple risk factors, and without significant financial incentives for implementers to participate. Students were not paid for participation. Teachers received only a $50 gift certificate at the end of the program.

The six schools in this study fell into a bi-modal pattern of implementation. At three schools, the median level of implementation was 91%, exceptionally high. However, each of the three schools in the 91% implementation group can be matched demographically with a school where implementation levels were unacceptably low.

The low implementation rates at three schools are especially problematic, because Ripple Effects had provided five things often considered key to implementation success: early involvement of would-be implementers in content development, so as to increase their buy-in; clear identification of core components of the program design and content; structure and planning time for site-specific adaptation, based on participants’ real world constraints and opportunities; culturally relevant and sensitive materials; and hands-on training, where participants actually used the software they would be monitoring for students.

Process: Factors Accounting for Low Implementation at These Sites

Special conditions at the three schools with very low implementation rates suggest several possible explanations for low implementation at these sites. Training, technology issues, implementer expertise, student oversight, transience, poor school record keeping, age appropriateness, sample size relative to available technology, school climate, and teacher morale are examined as possible causes of low implementation by Ray and Berg (2005).

The 34% low-end implementation rate at three schools is about the same as, not substantially lower, than the real world implementation rate for model programs such as Second Step. The 60% overall implementation rate is about double that often found among model programs in the real world (Backer, 2002). To compare either of these
implementation rates to those found in ideal research settings, especially settings where researchers have a previous and ongoing relationship with participants, where implementers are paid more than a token amount for their participation, and where student participants are facing far fewer risk factors, would be an apples-to-oranges comparison.

Conclusion

The evidence points to the fact that Ripple Effects Comprehensive Prevention Software positively affects attitudes (fatalistic thinking), behavior (absences, tardies, discipline referrals) and performance (GPA) each of which are associated with school success and good health. It does so with a smaller investment of instruction time, money, and implementer training than previously available programs. It makes tracking student progress and evaluating effectiveness easier than with live instruction programs. Although initially designed as a substance abuse prevention tool, findings suggest it may be strongest as an aid to academic achievement and a tool for changing problem behavior.

The latter results should assuage the fears of those who worry that the computer program might become a substitute for human interaction, rather than an enabler of it. Some computer-based (video) games do lead children to withdraw from a real—and uncertain—world into one they can better control. The goal of this software application is the opposite. It attempts to help students build the strengths that will enable them to actively, healthily and responsibly engage in the world. It provides a structured means for them to explore who they are and concrete skill training to help them make friends, solve problems, get closer to their families, participate in their community, and, not least, succeed in school.

While these findings are very promising, we believe there is an urgent need for further studies to replicate and expand upon what we examined here. Differences in how behavioral information is collected by schools will present a continuing problem for researchers conducting multi-site studies. Uniform reporting across schools is unlikely to be available in the near future, so either double blind, multi-site studies using behavioral observation as well as administrative data, or larger, single site replication studies are needed to confirm these results.
Two caveats are in order. These findings are based on the study of one culturally inclusive, theory-driven, youth-oriented, highly interactive, computer-based training program for adolescents (*Ripple Effects Comprehensive Prevention Software*). It includes a proprietary, media-rich learning system, specifically designed to reach learners who may have trouble with other formats. Findings from the study may be generalizable to other settings using the same program, but they are not generalizable to other kinds of computer-based training, unless they include every element of the Ripple Effects multi-modal learning system and equivalent content.

Finally, while this program shows promise, especially from the perspective of scalability and sustainability, it is not a panacea. Behavioral outcomes from this study suggest the software may partially mitigate the effects of an unprotective environment on students who use it. Fewer behavior problems from the students who use the software in turn may contribute to a safer, more disciplined environment for all. Nonetheless, the software is intended to work in tandem with—and thereby enhance—a positive, supportive learning environment. It is not intended, and should not be used, to substitute for such an environment.
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