Impact of a Self-Regulated, Computerized, Social-Emotional Learning Intervention on Disengaged and Delinquent Students at a Continuation High School

Kristin M. Bass, Ph.D. and S. Marshall Perry, Ph.D., Research Associates
Rockman et al., 49 Geary St., Ste. 530, San Francisco, CA 94108
(415) 544-0788
marshall@rockman.com, kristin@rockman.com

Alice Ray, MBA, President & CEO, and Sarah Berg, Vice President
Ripple Effects, 88 1st Street, Ste. 400, San Francisco, CA, 94105
(415) 227-1669
aray@rippleeffects.com, sberg@rippleeffects.com

Author note: Correspondence concerning this article should be addressed to Alice Ray, Ripple Effects, 88 1st St., Ste. 400, San Francisco, California. Email: aray@rippleeffects.com.

Impact of a Self-regulated, Computerized, Social-Emotional Learning Intervention on Disengaged and Delinquent Students at a Continuation High School

Abstract

Students who have previously dropped out or been involved in juvenile justice fill the "school to prison pipeline." A real world, longitudinal study of Ripple Effects computerized, social-emotional learning (SEL) intervention examined two questions: To what degree would these adolescents comply with a mandate to use the self-regulated intervention? If they complied, what would be the objective and subjective impacts? Participants were 177 mostly African American and Latino adolescents enrolled in a continuation school. Treatment group (TG) students were directed to independently complete 42 multimedia SEL skill-building tutorials, over six weeks. Fifty-nine percent were minimally compliant. Of those, 96% also addressed issues of personal interest. Post-intervention, compared to the control group (CG), TG students had significantly higher GPA, and no difference in absenteeism. The ratio of TG students enrolled in the district a year later was double that of the CG, $p<.05$. TG students had zero suspensions, compared to one for every nine CG students; an important but not significant result. There was no significant impact on attitudes about marijuana or alcohol, or locus of control. Because of insufficient baseline administrative data, we cannot rule out factors other than the intervention, such as differing levels of student motivation, being responsible for effects.
KEY WORDS: dropout; achievement gap; ripple effects; disproportionality; juvenile justice

Background

The interrelatedness of school failure, substance abuse and anti-social behavior leading to criminality is well established (Hawkins, Jenson, Catalano, & Lishner, 1988), though the causal links between them are not. In some cases, substance abuse leads to multiple problem behaviors, and problem behavior leads to truancy and school failure and/or arrest. In others, school failure leads to substance abuse, and substance abuse leads to problem behavior, and then arrest. In still others, anti-social behavior leads to school failure, which in turn leads to truancy and substance abuse, and eventually contact with the juvenile justice system.

A wide range of risk factors that operate on multiple domains–individual, peer, family, school, community and social structures–can all be precipitators for any or all three of these negative outcomes (Hawkins et al., 1998, Lipsey & Derzon, 1998). Regardless of the specific precipitating factors, their combined effect is often the same: a lifetime marked by the effects of early school failure, substance abuse and early involvement with the justice system.

Continuation schools exist to intervene in this downward spiral. The "continuation" nomenclature refers to rolling enrollment, which is a mark of these schools. They often operate in conjunction with the juvenile court system. Students frequently attend for one of three reasons: order of the court; they have been expelled from regular schools in the district; or, they are returning after dropping out for more than two years. In addition, in California and other border states, some Hispanic students attend continuation schools because their parents are seasonal workers who travel to and from Mexico or Central America. These events do not neatly coincide with semester breaks.

Ripple Effects is a comprehensive, student-centered, self-regulated, computer-based social-emotional learning (SEL) intervention designed to reduce risk and increase protective factors among youth, especially those most vulnerable to school failure, substance abuse or juvenile justice involvement. It can be configured to promote self-efficacy, as well as for other primary, secondary and tertiary interventions. It enhances protective factors at the level of the individual by providing training in core social-emotional competencies. Research has linked increased social-emotional competency to lower dropout rates, increased school engagement, reduced aggressive behavior, reduced involvement in the juvenile justice system, and greater resilience (Benard, 2004; Elias & Arnold, 2006; Lipsey & Derzon, 1998; Wilson & Lipsey, 2007; Zins, Weissberg, Wang, & Walberg, 2004).

Ripple Effects addresses risk factors in other domains by providing science-based information and skill training to enable pro-social decision-making, including the decision to ask for help. It provides intensive, individualized guidance and support through context-specific tutorials. Thousands of multimedia screens are organized into hundreds of interactive tutorials that can be mixed and matched toward these ends. It is in use in more than 500 school districts, including dozens of the largest urban districts in the United States, as well as more than 15 juvenile justice settings.

Data from two prior studies indicated the program had promising but not proven positive effects on school outcomes, when used independently by students, without adult mediation of content (Ray, 1999; Stern & Repa, 2000). This article discusses one of six concurrent studies begun in 2003 to systematically examine the impacts of Ripple Effects on attitudes, behavior and academic performance among diverse groups of adolescents. National Institute on Drug Abuse was the primary funder of the series of studies.

Purpose

The purpose of this study was to twofold: to assess implementation process fidelity, and to evaluate intervention efficacy of Ripple Effects SEL software on high school students who had experienced previous school failure or involvement in the juvenile justice system.

Methods

Research Design

The school level study was a longitudinal, repeated measures, (pretest, posttest, follow-up) randomized controlled trial, with reservations, conducted under real world conditions, without any direct involvement of program developers in delivery of the intervention. Success was measured by the extent to which exposure to Ripple Effects changed students' attitudes, behavior and academic performance. Individual students were the unit of analysis. Figure 1 provides a flowchart of the research design.

We tested these hypotheses:

(1) Under real world school conditions, if given the opportunity and access to technology: a) students would comply with group level requirements for use of the software; b) with no more than three hours of training on the intervention, staff would monitor and ensure that use; and c) students would accept an invitation to explore additional tutorials of personal interest.

(2) If treatment students had three or more hours of exposure to the computerized SEL intervention, their: a) school outcomes would improve; b) perceptions of harm and norms against use of alcohol and marijuana would increase; c) internal locus of control scores would increase, all when compared with control group students.

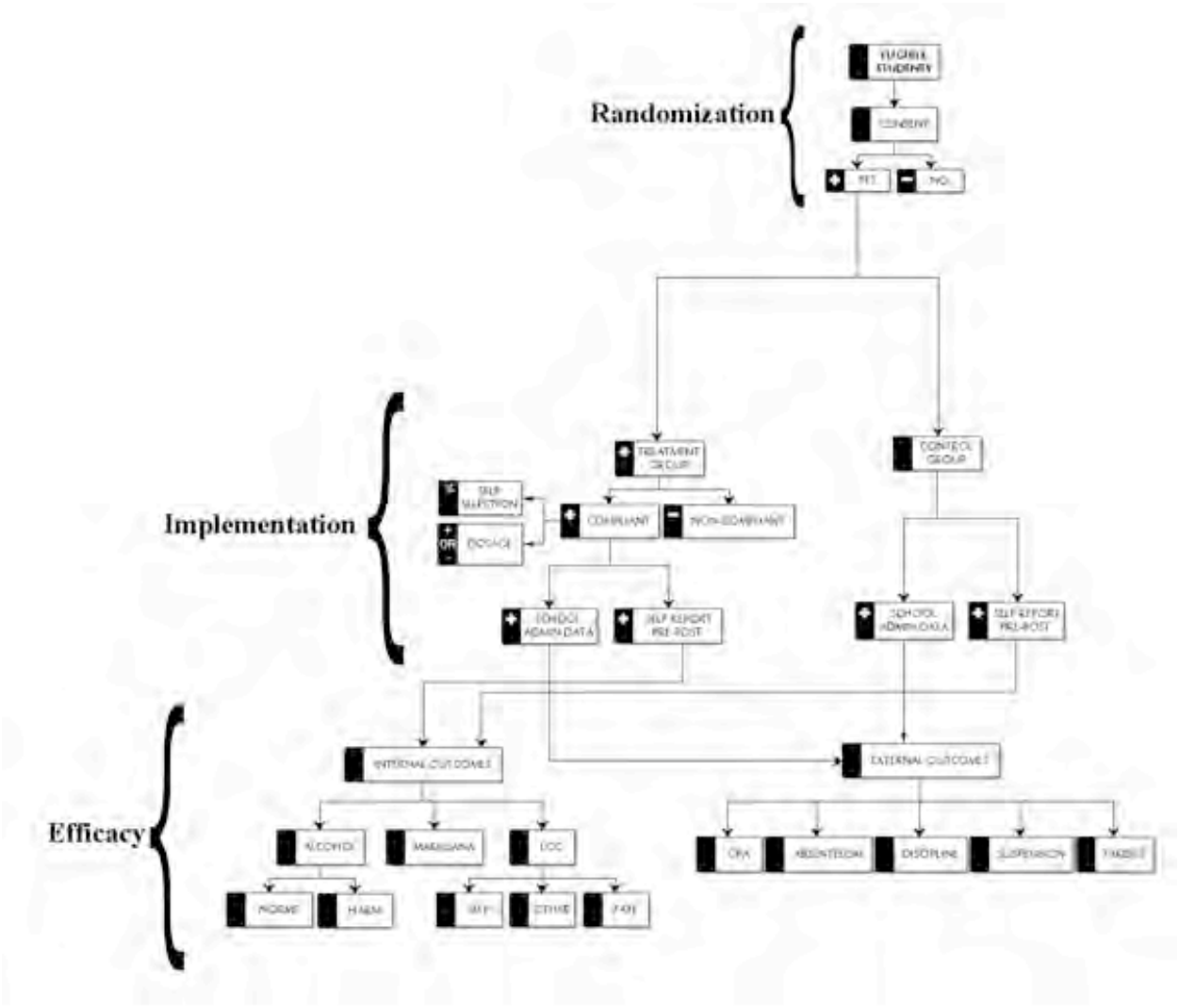

Figure 1: Flowchart of the Research Design

Setting

The setting was a continuation high school in a violence-ridden neighborhood of a major city with one of the highest homicide rates in the United States. The school is part of a district that had been taken over by a state administrator, due to financial irregularities and near insolvency. In the 2003-2004 school year, this school had 300 students. The school population reflects the population of the juvenile justice system overall. That is: poor, academically unsuccessful, and disproportionately African American and Latino. The population of this continuation school was not just comprised of students at risk of school failure and/or involvement in the juvenile justice system; it was comprised of students who had already failed. They had dropped out, or been kicked out of school, and/or had engaged in behavior that put them under the jurisdiction of the court.

Study Sample

One hundred seventy-seven students in grades 9 to 12 participated. They ranged in age from 16 to 19, and 90% were 16 or 17. Fifty-nine percent were male, 19% were Limited English Proficiency, and 66% qualified for Free or Reduced Lunch, a marker for low socioeconomic status (SES). Seventy-two percent of the students were African American, 17% Hispanic, 10% Asian/Pacific Islander, and 1% Caucasian.

Assignment to Condition

The School Administration Student Information (SASI) computer program sorted students into advisory periods. The original design was for half of the advisories to be assigned to each condition, which occurred. During the pretest period, it became apparent that technology capacity constraints required reducing the treatment group by half. Two of four classes were dropped, leaving 52 students in the treatment group. The remaining students were added to the control group. Soon after, it became clear that the same technology constraints impacted the swollen control group. The vice-principal therefore randomly selected 125 students across all grades, for completion of pretest, computerized surveys and enrollment in the study control group. This left an imbalance between group sizes (TG N=52 vs. CG N=125), which was corrected by randomly sub-sampling the control group at post to match the number of treatment group students who were exposed to the intervention (TG N=27).

Condition of Use

One advisory teacher monitored treatment group students from both advisories, for self-regulated completion of 37 tutorials (roughly nine contact hours) over seven weeks during advisory period, other free time, or whenever they had her as their teacher. All students had equal access to the program, because they all rotated through this facilitator's class equally. Some voluntarily came early or stayed late to do the program. No content was mediated by the teacher, but she was mandated to monitor electronic scorecards to track compliance. After completing their assigned tutorials, students could use remaining free time to explore any of the additional 141 tutorials of their choice.

Control Condition

Control group students participated in "business as usual" during advisory period, and during other class periods during which treatment students had access to the program.

Intervention

The intervention was a subset of tutorials from Ripple Effects SEL software. At the time of this study, Ripple Effects' teen version included 178 multimedia tutorials (390 as of 2008), designed to build protective factors, reduce risk factors, and solve problems in non-academic areas correlated with school success. The tutorials are reading-independent training modules, which take about 15 minutes each, on average, to complete. They are made up of photos, illustrations, videos, audio, peer-narrated text, and interactive exercises, with a hip-hop look and feel.

The configuration of the intervention examined here was a "self-efficacy" one. Self-efficacy is the context-specific belief in one's capacity to master what is needed to succeed (Bandura, 1997). Success in this case was defined as academic achievement, reduction in behavioral problems, and positive changes in attitudes toward alcohol, marijuana and locus of

control. A scope and sequence was designed to promote cognitive, social and emotional capacity-building toward those intended ends.

Twenty-one tutorials addressed "core components" of self-efficacy in the context of school achievement and prevention of substance abuse. Of the remaining 157, 16 that school staff considered to be most relevant to their particular student population were added. They were heavily weighted toward building strengths (13 tutorials), over addressing problems (two, disputes and quitting habits).

Content was organized into strengths (assets), problems (behavioral, academic, social), and reasons (risk factors at individual, family peer, school, community and social structure levels). Once students had completed the required tutorials, they could follow the built-in links between tutorials, to go deeper into those topics that interested them.

Learning process. Independent of specific content, the *Whole Spectrum Self-Regulated Learning System* that powers Ripple Effects SEL software (Figure 2) contains elements that have been linked to successful development of self-efficacy: guided mastery, self-regulated learning, observational learning, systematic self-reflection, transfer training, and skill rehearsal (Bandura, 1997; Pajares & Urdan, 2006). All of these modes of learning are introduced with a case study scenario (context-specific application). Additional elements of the system include continuous assessment of content mastery through interactive games; reading independence through peer narration and illustrations; narrative/story as teaching tool, including first person video true stories; and, positive reinforcement for completion of the learning process through a video game style point system.

Figure 2: Diagram of the Whole Spectrum Self-Regulated Learning System

Implementer Training

A Ripple Effects trainer provided four teachers with a single three-hour training session to orient them to the software, choose their site-specific tutorials, and prepare them to introduce the software to students, assign the tutorials, and use the built-in data management system to

monitor compliance and track student progress. Ultimately, only one of the four trained staff facilitated all student participation in the intervention.

Outcome Measures

The analysis included multiple, quantitative and qualitative, process and outcome measures.

Quantitative process measures. Quantitative process measures included enrollment attrition, study attrition, intervention attrition (compliance), dosage and self-selection of optional tutorials.

We classified as "enrollment attrition" the percentage of students for whom there was no pre or post-intervention administrative data, because their family had moved or they had been removed from school.

We classified as "study attrition" the percentage of students who were physically enrolled in school, but failed to complete the pre and/or post self-report surveys, whether because they withdrew consent, were absent, could not gain access to the technology, or were not mandated by staff to do so.

We classified as "intervention attrition" the percentage of students in the treatment group who had consented to the study but, for whatever reason, did not comply with minimal requirement of at least three hours exposure to the software. We included in efficacy and dosage analysis all students who had at least three hours exposure to the software program.

Dosage measured the level of exposure among students who complied. We defined engagement with self-selected content as a yes or no event; we did not analyze that dosage.

Quantitative outcome measures. Quantitative outcome measures included no fewer than 12 measures of concept mastery, four objective school achievement measures, and two self-report measures.

To measure concept mastery, each tutorial included a set of six multiple-choice questions, disguised as an interactive game. The tests are structured such that students cannot complete the game and earn points until every answer is correct. Students could experiment with answers until they arrived at the correct one. Compliant students had to complete at least 12 of these tests.

The four objective school achievement measures were grade point average (GPA), days absent, suspensions, and school enrollment rates at one-year follow-up.

Quantitative self-report measures included two computer-based, pre and post surveys on (1) attitudes toward alcohol and marijuana, and (2) perceived locus of control. Both self-report surveys were adaptations of previously validated instruments. The Monitoring the Future (MTF) survey measures norms and perceptions of harm about alcohol, marijuana and other drugs. The Multi-dimensional Health Locus of Control scales (MHLC) measure attribution of life events to internal (Self) or external (Fate/Other) factors. For both scales, Ripple Effects adapted the format to peer-narrated, computerized delivery, with a hip-hop look and feel, a game-like structure of reinforcement for any answer, and automated data collection. For the locus of control scales, Ripple Effects adapted the "Other" subscale to include other social forces, such as racism, as well as other powerful people.

The reliability coefficient for the REMTF scale on norms and perceptions about alcohol was 0.74, while the coefficients for marijuana norms (0.88) and risks (0.85) were sufficiently high to enable them to be analyzed separately. The RELC scales for Self and Fate both had pre and posttest alpha values of 0.70. The alpha values for the Other scale, which included the substantive content adaptations, were 0.59 for the pretest and 0.71 for the posttest. Since the

pretest did not meet the 0.70 criterion, we analyzed that posttest data alone with independent samples t-tests.

Qualitative measures. Qualitative process and outcome measures included direct observation and interview data on perception of program usage, barriers to use, and perceived value from implementer perspectives.

Data Collection

Compliance, dosage and concept mastery. Ripple Effects software automatically collected data on compliance and dosage rates. Dosage was directly tied to completion of the interactive games that measured concept mastery. If students were awarded points for a tutorial, it signified they had successfully provided all the correct answers to the quiz.

School data. School administrators provided pre-intervention demographic data, including Free or Reduced Lunch status, Limited English Proficiency (LEP), age, gender and ethnicity. They also provided enrollment attrition data, and data on GPA, absenteeism, and suspensions for the first semester of the year of the study. The school did not have a system for tracking discipline referrals, so was unable to provide this data. The school district provided prior year and follow-up year school outcome data.

Self-report data. During the Fall of 2003, as part of their regular school activities, students completed the two computer-based surveys described above, before and within two weeks after the eight-week intervention. At least 12 weeks elapsed from teacher training to final survey.

Qualitative data. At several points along the way, the study coordinator conducted and documented phone and in-person interviews with the school administrator, and the site program facilitator. Site visits by Ripple Effects technology support staff provided observational data on implementation conditions and school climate issues.

Methods of Analysis

SPSS was used to run all of the analyses. Several methods of analysis were used, each appropriate to the kind of data being analyzed.

For administrative post intervention data with normal distribution (GPA), we ran independent-samples t-tests comparing the means of the treatment and control groups.

For administrative data factors with non-parametric distribution, such as absenteeism and suspensions, we ran the same tests, but also the Games-Howell posthoc test for pair-wise comparisons. Severely unequal variances can lead to increased Type I or Type II error, and, with smaller sample sizes, this effect can be increased. Games-Howell corrections are used when variances and group sizes are unequal.

The set of control variables included ethnicity, gender, LEP, and free or reduced lunch status, as a measure of socioeconomic status.

For the self-report data with pre and post values (the REMTF norms and risks scales, and the Fate and Self RELC scales), we ran repeated-measures ANOVAs with a between-subjects factor (study group) correction. For the Other RELC scale, since the pretest did not meet the 0.70 criterion, we analyzed that posttest data alone with independent-samples t-tests.

To establish dosage, Ripple Effects software created a password-protected file for each student and tracked completion of interactive exercises for each tutorial, assigning 100 points per exercise. This data was exported from each computer, with names decoupled from identifying numbers, and then data aggregated in centralized files. Dosage was calculated from the point

count of each student's total number of completed interactive exercises, which divided by an average completion rate of four per hour, resulted in per-student hours of exposure.

To see if the number of hours of exposure to Ripple Effects was associated with differences in outcomes, we ran bivariate Pearson product-moment correlations. In cases where there was pretest data, we ran partial correlations on the posttest data that controlled for the effect of the pretest covariate. For each set of correlations, we used the Bonferroni method to minimize the chances of making a Type I error.

To compare long term effects on students who may be dispersed among many schools, we conducted independent-samples t-tests comparing the means of the treatment and control groups of school district level enrollment data, one year post-intervention.

To account for the unbalanced treatment and control group sizes, we randomly sub-sampled the control group to match the treatment group size.

All means presented in the text and tables are the raw values unadjusted for the covariates.

Results

Baseline Equivalence

Analysis of pretest surveys indicated no significant baseline differences between treatment and control groups for any self-report variable (norms or risk related to alcohol and marijuana, or locus of control). Almost two years after the initial data collection, the school district provided administrative data on absenteeism, suspension rates and GPA from the academic year prior to the start of the intervention. That administrative baseline data covered only 7% of the total sample, with as few as three intervention-compliant students (5%) and 10 control group students (8%) with GPA data. The 10 control group students did not match the subsample of the control group that we had previously done to match group sizes, so we were unable to conduct ANOVAs. We have appended the results of independent-samples t tests from the sample we were able to obtain (Appendix A). The treatment group had a lower GPA, higher absenteeism, and lower suspension rates, compared to the control group. Thus, ANOVA may have resulted in significant differences favoring the treatment group. However, the sample size was too small to perform that test.

Process Outcomes

Technology-related delays. Several delays due to testing, computer system failures, and one power blackout shortened the duration of actual exposure to the intervention to six weeks.

Enrollment attrition. Administrative post-intervention data was not available for 13% of students: 12% of the treatment group (remaining N=46) and 14% of the control group (remaining N=108).

Intervention attrition (non-compliance). Non-compliance with at least three hours exposure to the intervention among students who remained in the study was 41%, or 19 students. Of the 19, 14 had some exposure to the software, while five had none.

The remaining 27 compliant TG students, and a randomly sub-sampled group of control group students, were included in the school outcomes efficacy analysis.

Study attrition. No students formally withdrew consent. The electronic monitoring of program usage, coupled with reports by facilitators, enabled researchers to verify that no control

group students had contact with the intervention. Pre or post self-report data was not available for 27% of students; 22% of the entire treatment group (compliant and non-compliant), and 30% of the control group. For compliant students, just 16 had completed both pre and post tests, and were included in the self-report efficacy analysis.

Dosage. Mean dosage for students who complied was 56% (20 tutorials, or roughly five contact hours).

Participation in self-selection option. Ninety-six percent of students that complied with the software intervention elected to explore unassigned tutorials related to topics of personal interest. They explored an average of 15 self-selected tutorials. Thirty-seven percent of non-compliant treatment group students also chose to use the intervention to privately explore issues of personal interest.

Quantitative Outcomes

Concept mastery. Analysis of points awarded for multiple choice games provided evidence that treatment group students demonstrated at least short term mastery of no fewer than 12 key concepts, and an average of 21.

School achievement measures. There is a significant difference of about half a grade between Ripple Effects students and control group students who did not go through the program, $p<.05$, Cohen's $d = 0.68$. The groups had no significant differences in rates of absenteeism. The treatment group had fewer suspensions than the control group. While not statistically significant, the treatment group suspension rate of zero is clinically important for this population. All values are reported in Table 1.

Table 1.

Differences in School Outcomes for Ripple Effects and Control Students

	Treatment (n=27)		Control (n=27)			
Outcome	*M*	*SD*	*M*	*SD*	Difference	Cohen's *d*
GPA	2.96	0.41	2.46	0.98	0.50*	0.68
Absenteeism	0.16	0.11	0.16 [a]	0.12	0.00	0
Suspensions	0.00 [b]	0.00	0.11	0.58	-0.11	0.30

Notes:

[a] Sample size for the control group is 21. Six students in sample were missing attendance data.

[b] Sample size for the treatment group is 26. One student was missing suspension data.

*$p < .05$

Self-report data. According to Table 2, ANOVAs indicated the treatment group had a lower score gain in perceptions of norms and risks of both alcohol and marijuana than did the control group, from pre to posttest, controlling for pretest scores. This difference is not significant.

Table 2.
Differences in Changes in Perceptions of Risk and Norms about Alcohol and Marijuana By Condition

REMTF Scales	Pre M (SD)	Post M (SD)	Change	Pre → Post Difference in Changes between Groups
Alcohol Norms & Risk				-2.06
Treatment	15.94 (3.60)	15.94 (4.12)	0.00	
Control	15.00 (4.69)	17.06 (5.27)	2.06	
Marijuana Norms				-0.99
Treatment	5.44 (2.90)	6.13 (2.16)	0.69	
Control	5.63 (2.96)	7.31 (3.42)	1.68	
Marijuana Risk				-0.19
Treatment	8.19 (3.15)	9.50 (3.31)	1.31	
Control	6.63 (2.92)	8.13 (3.90)	1.50	

Notes: Sample consists of 16 students in the treatment group and 16 students in the control group. Higher numbers represent greater perception of risk or disapproval.

As reported in Table 3, on the internal locus of control (Self) scale, the treatment-control difference in gains means that the treatment students were more likely to attribute outcomes to themselves than were the control students by the end of the study. On the Fate scale, the treatment students were more likely than the control students to attribute consequences to fate by the end of the study. On the Other scale scores, the independent-samples t test indicated the treatment students were less likely than the control students to assume that outcomes were caused by other people or structures (TG M = 33.68, SD = 6.27; CG M = 33.61, SD = 7.93). None of these differences between treatment and control groups were significant.

Table 3.
Differences in Changes in Locus of Control by Condition

RELC Scales	Pre M (SD)	Post M (SD)	Pre → Post Change	Pre → Post Difference in Changes between Groups
Self				-4.26
Treatment	25.00 (6.25)	24.24 (6.06)	-0.76	
Control	24.94 (9.47)	28.44 (9.76)	3.50	
Fate				4.00
Treatment	38.59 (5.68)	36.76 (5.64)	-1.83	
Control	34.22 (10.65)	36.39 (8.28)	2.17	

Notes: The sample consists of 17 students in treatment group and 18 students in control group. Higher numbers represent greater disagreement with the scale.

Dosage effects. As reported in Table 4, there were no significant correlations between dosage and outcomes at the .002 level.

Table 4.
Correlations Between Dosage, GPA, Absences, and Suspensions

	GPA		Absences		Suspensions	
	N	*r*	N	*r*	N	*r*
RE Group	27	0.21	27	-0.39	26	a

a: Value could not be computed because at least one of the variables is missing or constant

Twelve-month Follow-up Enrollment Data

Twelve-month follow-up data indicated 55% of treatment group students and 26% of control students were still enrolled somewhere in the school district. This does not include students from either group who were in 12th grade at the time of the intervention and were no longer enrolled. This difference in enrollment rates between the two groups was significant, $p<.05$. We cannot state with certainty whether the seven 12th graders all graduated, or some dropped out.

Qualitative Data

Staff interviews revealed that the differential rate in study attrition between control and treatment groups was not directly attributable to student choice, but did correlate with student behavioral data. The 60 control group students who did not complete the posttest survey were missing from school one or more times during the two week period of testing, when they would have been pulled from class to complete the computerized survey.

Staff interviews also indicated that there was little actual direct monitoring of student electronic scorecards to ensure compliance. The fact that students could complete the

intervention when they wanted, made it difficult for one teacher to track. The high completion rate of student choice tutorials (15 on average, compared to 21 required tutorials) suggests that while the monitor may have observed students working on the intervention, they may have been completing self-selected rather than required tutorials.

Follow up interview data with staff was aligned with follow up administrative data. That is, that Ripple Effects students had lower dropout rates than treatment group students, and went on to have higher graduation rates in the following three years.

The vice-principal at the time of the study became the school's principal two years later and, based on her observations of the intervention's impact, decided to complete Ripple Effects' trainer certification course, trained her entire staff in the software, and implemented both the teen and the staff versions school-wide. She attributes her school's rise in graduation rates to the intervention, but does not have data to substantiate this belief.

Discussion

Implications for Practice

Training in social-emotional competencies, not academic content, resulted in significant, positive academic change in high school students who had previously failed. Twice as many of those students, as their control group counterparts, remained enrolled in school a year later. Thus, although proffered as a social-emotional learning intervention, Ripple Effects can as rightly be considered a dropout prevention and academic achievement intervention.

These finding are consistent with a growing body of literature about the impact on school success of live SEL instruction (Elias & Arnold, 2006; Zins et al., 2004); but there are differences from previous findings as well. The intervention was short; effective dosage was low; program implementers received minimal training (three hours); yet change was swift and enduring. The intervention occurred in two, 25 minute sessions, plus free time, over six weeks. Significant results were observed in the very first grading period after the intervention, and again at one-year follow-up. Three hours of contact was enough to produce results.

All of these things run counter to prior research findings about what works with live interventions. We are unable to explain why. It is certainly possible that there is greater emotional openness in a private, non-judging computer-based environment, than in a regular classroom. The modeling presented in the videos is faithful to proven strategies, so it may be more effective than modeling students see in the classroom. The intervention photos, images, sound, videos and games all include representations of diverse youth, so these African American and Latino students may have more closely identified with the material. Student self-regulated use of a multi-modal system provides a better chance of matching each individual's learning style, which can accelerate learning. All but one student who used the program chose to privately explore one or more topics of interest to them, effectively augmenting standardized instruction with personalized guidance and counseling. This combination may have intensified effects.

Despite the potential for positive effect, a substantial number of students who were selected to receive the intervention, failed to engage in even very minimum exposure. Taking into account the real world conditions of both the study and this particular school, the 41% intervention attrition rate is moderate. Nonetheless, it leaves many students behind. This demonstrates that, especially for students who are exposed through court order, use must be mandated, not just invited; and compliance monitored carefully, without violating the important

element of privacy. For any mandate to be effective it must be consistent with overall school climate and policy, which may not have been the case at this continuation school.

Limitations of Study

Problems with method of randomization. The school agreed to randomization and relied on its school scheduling software for advisory period to ensure it, but our discussion with the vendor suggested their confidence may have been misplaced. It is possible that the computer scheduling of advisory periods involved an algorithm to create demographically balanced classrooms. The large gaps in baseline data would have largely prohibited stratifying by academic ability, absenteeism or behavior. Technically, this is a study weakness. As a practical matter, it is likely to have ensured baseline equivalence among a population for whom little prior year data was available, and could add reason for further confidence in the results. On the other hand, reliance on the vice-principal to randomly trim the original control group to a size that technology capacity could accommodate, by randomly pulling students from class, undoubtedly biased that group somewhat toward students with better attendance, as school staff had reported. Thus absenteeism for the control group may be underreported and could account for the lack of significant differences between the two groups on that measure.

Lack of baseline school data and possible intervention attrition bias. Although we can be fairly sure there was group level equivalence at pretest, the lack of individual baseline data is a weakness. 41% percent of the assigned treatment group students did not have minimal exposure to the intervention, and so were excluded from analysis of efficacy (though not from process analysis). There may well have been baseline differences between student who complied with use of the program, and those who did not. Whatever factor was involved in that self-selection may independently account for at least part of the difference in outcomes. Although available baseline data was spotty, the little data that was available was not inconsistent with this possibility.

Assignment of instructor to condition. The assignment of one teacher to the treatment condition may not have been random. However, that teacher had no role in mediating any content. Based on experience with other schools in parallel studies and beyond, we consider the choice of teacher to be relevant to study attrition rates, but not to intervention effects related to student exposure. For all of these reasons, we submit this study as a randomized controlled trial with reservations.

Small sample size. Finally, the smaller sample size leaves open the possibility of Type 1 error, even with the Games-Howell correction. For instance, treatment group suspension rates went to zero, a substantive, but not significant result. Since there are no negative suspension rates, it was mathematically impossible with the control group rates so low, to find a significant difference, even if it were there.

Conclusions

The evidence supports several conclusions. Some students with multiple risk factors and a history of non-compliance and/or disengagement, will voluntarily engage in self-regulated use of this kind of computerized SEL intervention, but as many will not. Those who use it are likely to experience positive and enduring academic and behavioral outcomes, including significantly higher grades and reduced dropout rates, and substantively lower suspension rates. They are unlikely to have significant gains in attitudes about marijuana, alcohol or locus of control, which

previous research has indicated are associated with school success. We are unable to determine from data in this study how much any of these outcomes are caused by exposure to Ripple Effects intervention, and how much to factors that prompted students to engage in using it, from personal qualities, to technology access, to relationship with the adult implementer or other authority figures. Much more study is needed to clarify causal mechanisms for change. For students with so many high-risk strikes against them, that clarification cannot come soon enough.

Appendix A

Appendix A. Table

Baseline School Outcome Data for 2002-2003 School Year, by Condition

	Treatment Group			Control Group				
School Outcome	N	Mean	SD	N	Mean	SD	Difference	p value
GPA	3	0.39	0.35	10	2.29	0.76	-1.90	0.002
Days Absent	4	34	25	12	25	18	9.00	0.475
Days Suspended	4	0.0	0.0	12	0.17	0.58	-0.17	0.582

References

Author names withheld. (2008). Summary of findings from six studies on effectiveness of a computerized social-emotional learning program to reduce risk and increase protective factors among adolescents. Manuscript in preparation.

Bandura, A. (1997). *Self-efficacy: The exercise of control.* New York: W. H. Freeman.

Benard, B. (2004). *Resiliency: What we have learned*. San Francisco: WestEd.

Elias, M.J., & Arnold, H. (2006). *The educator's guide to emotional intelligence and academic achievement: Social-emotional learning in the classroom.* Thousand Oaks, CA: Corwin Press.

Hawkins, J.D., Jenson, J.M., Catalano, R.F. & Lishner, D.M. (1988). Delinquency and Drug Abuse: Implications for Social Services. *Social Service Review*, 62(2), 258-284.

Lipsey, M.W., & Derzon. J.H. (1998) Predictors of violent or serious delinquency in adolescence and early adulthood: A synthesis of longitudinal research. In R. Loeber & D.P. Farrington (Eds.). *Serious and violent juvenile offenders: Risk factors and successful interventions*. Thousand Oaks, CA: Sage.

Pajares, F., & Urdan, T. (Eds.). (2006). *Self-efficacy beliefs of adolescents.* Greenwich, CT: Information Age Publishing.

Ray, A. (1999). *Impact on passivity-assertiveness-aggression of short term, computer-based, skill building in assertiveness: a pilot study.* San Francisco: Ripple Effects.

Stern, R., & Repa, J. T. (2000). *The study of the efficacy of computerized skill building for adolescents: Reducing aggression and increasing pro-social behavior.* Unpublished manuscript.

Wilson, S. J., & Lipsey, M. W. (2007). School-based interventions for aggressive and disruptive behavior: Update of a meta-analysis. *American Journal of Preventive Medicine, 33* (Supplement 2), S130-S143.

Zins, J. E., Weissberg, R. P., Wang, M. C., & Walberg. H. J. (Eds.). (2004). *Building academic success on social and emotional learning: What does the research say?* New York: Teachers College Press.

Acknowledgements

This study was funded by the National Institute on Drug Abuse of the National Institutes of Health, SBIR Fast Track Grants R44 DA13325-01A1, and R44 DA013325-03. It is one of a series of collaborative projects between the program developers (Ripple Effects), Oakland and Southern Humboldt county schools and school districts, and research analysts (Rockman et al). As CEO of Ripple Effects, Principal Investigator Alice Ray is an interested party. We gratefully acknowledge the early contribution of Michael Roona in framing theoretical questions to be addressed. Preliminary summary findings of the group of studies were presented as a poster at the May 2007 Annual Meeting of the Society for Prevention Research. Correspondence concerning this article should be addressed to Alice Ray, Ripple Effects, San Francisco, California. Email: aray@rippleeffects.com.