Impact of Social-Emotional Learning Software
On Objective School Outcomes Among Diverse Adolescents:
A Summary Analyses of Six Randomized Controlled Trials

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Abstract

A substantive body of evidence suggests that development of social-emotional competence can work both to address behavior problems and to promote academic achievement. However, most effective programs require extensive training and are heavily dependent on instructor expertise. This study concerns a computer-based social-emotional learning intervention called Ripple Effects. Six randomized controlled trials evaluated the impact of the intervention under diverse, real-world, school conditions. A total of 605 ethnically diverse, rural and urban adolescents with multiple risk factors participated. The intervention was self-regulated completion of 42 multimedia tutorials over a period of eight weeks. Although intended as a social-emotional intervention, data indicate the largest significant effect across studies was on academic achievement. Independent-samples t-tests resulted in mean treatment group GPA of 2.90 compared to 2.53 for the control group, p<.01. There were significant positive differences between treatment and control group students across studies for suspensions, p<.05. There were some significant differences in the number of absences and tardies between treatment and control students for individual studies. Lower discipline referral rates for treatment group students across studies was substantively meaningful, but not statistically significant. Three studies provided one-year follow-up enrollment data. At two, treatment group students were enrolled at twice the rate of control group students, a statistically significant difference. With some studies reporting only a single year's data, and most not reporting baseline data, we cannot rule out other factors being responsible for these differences.

KEY WORDS: achievement gap; social-emotional learning; computer-based training

Background

High rates of school failure among youth with multiple risk factors is well documented, and causes for that failure have been extensively studied (Ferguson, 2002; Jencks & Phillips, 1998; McCall, Hauser, Cronin, Kingsbury, & Houser, 2006). Academic, behavioral and environmental factors all have been shown to play a role (Bennett et al., 2004; Hammond, Linton, Smink, & Drew, 2007; McEvoy & Welker 2000). A myriad of interventions have been tested, with few providing scalable solutions for real-world settings. A large funding stream has been dedicated to math and reading programs.

Despite billions of dollars in public investment, these initiatives have been largely unsuccessful in obtaining significant academic gains (Lee, 2002; McCall, 2006). Adding the use of computer-based technology as a delivery format has not produced consistent, quantifiable advantage in core academic areas (Dynarski et al., 2007).

Another approach is to focus on social-emotional instruction. A substantive body of evidence suggests that development of social-emotional competence can work both to address behavior problems and to promote academic achievement (Elias & Arnold, 2006; Zins, Weissberg, Wang & Walberg, 2004).

A growing number of social-emotional learning (SEL) interventions have been listed as promising or model programs by What Works Clearinghouse or the Substance Abuse Mental Health Services Administration (SAMHSA). Among them are *Paths* (Greenberg, Kusche, Cook, & Quamma, 1995), *Life Skills Training* (Botvin, G., Baker, Dusenbury, Botvin, E. & Diaz, 1995)*, and *Positive Action* (Flay & Allred, 2003). All have evidence of positive impact on both behavior and academic performance. However, all require extensive training and are heavily dependent on instructor expertise. Thus none are easily scalable. Recent research has focused on the important role of implementation fidelity in achieving and replicating positive results (Dane & Schneider, 1998; Devaney, O'Brien, Resnik, Keister, & Weissberg, 2006; Fixsen, Naoom, Blase, It is counter-intuitive to think that computer technology might be advantageous for delivery of social-emotional training, when computer-based training has had mixed results in impacting academic outcomes (Dynarski, et al., 2007; Kulik, 2003; Schacter, & Fagnano, 1999). Computers are unfeeling, not self-aware, often lack nuance, miss non-verbal cues, and in most case, don't provide an environment for physical rehearsal of new skills. All are factors in implementer effectiveness of SEL programs (Devaney, et al., 2006).

Nonetheless, there is a growing body of evidence that technology-based training can be effective for some psychosocial interventions.

The best evidence is for internet-delivered cognitive behavioral therapy for anxiety disorders (Carlbring et al.; 2005; Farvolden, et al. 2005) and substance abuse treatment (Carroll, Ball, Martino, et al., 2008; Brendryen & Kraft, 2008), as well as internet-delivered cognitive behavior therapy and psycho-education for depression (Clark et al. 2005; Christensen et al., 2004). Other studies show promising positive outcomes among adults, for disorders such as uncontrollable anxiety, and eating disorders (Andersson, et al., 2005; Pull, 2006; Ybarra et al., 2005; Zabinski et al., 2003). These studies examined standardized, group level protocols among adults.

Prior to the beginning of this study, little formative evaluation and very few, real-world scientific studies of effectiveness of self-directed social-emotional training for children had been conducted. One early study showed that a school-based health promotion/behavior change CD-ROM-based program (*BARN*) resulted in reductions in risk-taking behavior in adolescents (Bosworth, et al., 1994). An evaluation of a kiosk based HIV/AIDS prevention program using a game format, showed increased understanding of safety issues, and modest pre to post gains in self-efficacy scores, but the study lacked a comparison group to substantiate findings (Thomas et al., 1997). A 1999 quasi-experimental pilot study of a fifty-minute computer session to build assertiveness skills (using an excerpt from the intervention examined here, Ripple Effects) showed significant, short-term increases in assertiveness and decreases in aggression, but there was no follow-up (Ray, 1999). A three-armed RCT of the impact of a computer-based intervention comprised of 24 Ripple Effects lessons stressing empathy, impulse control and anger management over 12 weeks, showed strong trends toward increased pro-social behavior, decreased aggression, and lower rates of remedial summer school. Only the scores for subscales on anti-social behaviors related to conflict and unkindness, and more respectful behavior, were significant (Stern & Repa, 2000).

Research has demonstrated that adolescents and adults are both more comfortable seeking help from a computer than a live interviewer, and are more honest in answering questions on the computer, especially about matters that may carry perceived social stigma (Karabenick & Knapp, 1988; Turner et al., 1998; Weisband et al., 1996).

Research that has been released during the course of the studies described here, shows that computerized delivery of science-based health information to children and adolescents can be effective in transferring accurate understanding related to substance abuse (Marsch, Bickel & Badger, 2006; Schinke, Schwinn & Ozanian, 2005). Computerized delivery of social skill training has been shown to be effective in promoting self-reported assertiveness and decision-making skills, the former at a level equal to or higher than, a widely validated, instructor-delivered program (Marsch et al., 2006). Several studies of computer-based training for children with autism have shown positive impacts on social-cognitive deficits related to autism (Bernard-Opitz et al., 2001; Whalen et al., 2006).

There is not published research that shows the impact of computerized health and behavioral interventions on school outcomes, especially academic performance. Nor is there research that has tested the efficacy of coupling standardized group training for children or adolescents, with self-directed individualized, therapeutic interventions to address personal risk and protective factors. This summary study is an effort to begin the fill that gap.

By 2002, the computer-delivered SEL intervention examined here, Ripple Effects, was in use in more than 100 school districts around the United States. It is a comprehensive, skill-building intervention that addresses a wide range of risk and protective factors related to health, school success and social behavior. There were compelling reasons to test its effectiveness, not the least of which is that it ensures greater implementation fidelity by keeping the content expertise "in the box," thus reducing dependence on instructor expertise. It

is also more affordable than instructor delivered SEL, which requires extensive training to prepare teachers to deliver the material with fidelity. It is also designed to enable individualized interventions, across a broad range of health, social, and behavioral subject areas.

In 2002, the National Institute on Drug Abuse (NIDA) of the National Institutes of Health funded a review of the intervention by an expert panel; completion of refinements to the content, based on that review; evaluation of the feasibility of changing from a disk-based, to a web-delivered platform; and, a test of the impact of the revised intervention on risk and protective factors among adolescents.

Expert review and revision of the program based on that review proceeded as planned. During that process it became clear that bandwidth constraints and security protocols argued against the envisioned Internet delivery of this intervention. The enterprise application was delivered on disks.

In the following year (2003-04), researchers began an evaluation of the impact of the revised Ripple Effects intervention on risk and protective factors among adolescents. The original experimental design was for a single, multi-site randomized control trial (RCT) of 600 students, under real world conditions, in schools where many students had multiple risk factors. During the recruitment phase it became apparent that differences in school structure and climate, student populations, technology capacity, and potential conditions of use, along with irreconcilable differences in how discipline data is collected across schools, all made it unlikely that meaningful results could be garnered from a single study. Methods of assignment to condition also differed across sites. In addition, the design of the Ripple Effects software (which allowed for customization of a scope and sequence to fit site-specific conditions) argued for splitting the single study into six smaller, site-based ones for more meaningful analysis.

Although the change to smaller sample sizes for analysis decreased the likelihood of detecting statistically significant effects, that risk

was offset by the possibility of analyzing multiple, simultaneous, controlled trials, where site-specific adaptation was built-in, under diverse, real-world, "business as usual" conditions.

Implementation of the original school-level interventions was completed in 2004. Administrative data was received over the following two years, and analyses completed in early 2008. We have reported results of the site-specific analysis elsewhere (Bass, Perry et al, 2008; Perry, Bass et al 2008). However, conclusion drawn from analyses of these smaller samples are vulnerable to both Type I and Type II errors. A posthoc, cross-study summary analysis of results for those variables that were standardized across schools could reduce the chance of those errors, and help separate consistent trends from singular anomalies.

PURPOSE

This article describes the results of cross-study impact analyses of Ripple Effects computerized SEL intervention on social behavior, school engagement, and academic achievement. In separate reports we consider cross-study findings related to norms and perception about alcohol and marijuana, as well as perception of locus of control.

Methods

RESEARCH DESIGN

The six studies were longitudinal, randomized controlled trials (RCT). All six were conducted under a variety of real world conditions, with individual students as the unit of analysis. The evaluation period extended from 2003 to 2008, including baseline data collection, training, intervention, post-intervention data collection, follow-up data collection, and analysis.

Hypotheses. (1) Under real world school conditions, if given the opportunity and access to technology, treatment students would comply with group level requirements for use of the intervention; and (2) If treatment students

had three or more hours of exposure to the computerized SEL intervention, their school outcomes (GPA, attendance, tardiness, suspensions, and discipline referral rates) would improve compared with control group students.

Role of developers. In order to minimize the potential for bias of having program developers involved in the research, the role of the program developers was circumscribed. Ripple Effects staff recruited study sites, conducted a three-hour training session with facilitators at each study site, provided technical support, obtained outcome data from school and district administrators, and conducted observations and interviews with participants. They were not involved in the delivery of the intervention, nor in the statistical analysis of quantitative outcomes. An independent research firm conducted the statistical analysis of all outcome data.

Method of assignment to condition. Method of random assignment to treatment or control condition varied by study. For the five RCTs, randomization was at the level of the individual student, assigned to a group, by computer, or by odd or even date of birth. For one study, in the prior spring, two groups were hand-matched to create baseline equivalence, then in the fall, the flip of a coin selected one of them to be the treatment group, with the other becoming control. In all instances, control group students had access to the intervention at the end of the intervention study period.

Conditions of use. Treatment group students worked one-on-one on the computer to complete 42 tutorials in the intervention, during advisory, academic, or computer classes, two or more times per week, for six or more weeks, in the computer lab, library, or their regular classroom. In three schools, students took time away from core academic subjects to complete the intervention. Facilitators assigned tutorials, and monitored their completion, but otherwise played no role in delivering the content of the intervention. Control group students continued with "instruction as usual." In the three cases where students were pulled from academic subjects, instruction as usual consisted of continued

regular coursework in the academic subject. For the three sites that used it during advisory period, instruction as usual included the activities deemed appropriate to advisory at each site.

PARTICIPANTS

Recruitment and consent. Researchers recruited widely in Northern California, presenting the research opportunity to more than 30 school districts. Ultimately, a group of schools serving students with high to very high risk of school failure in one urban district, and two schools in a second rural district, met the criteria for inclusion (willingness to use random assignment, technology capacity, plan for implementation, and data collection) and chose to participate. The studies received IRB approval. Active consent was required from student participants, and passive (opt out) procedures were used to obtain consent from their parents.

Settings. Six public schools participated. Four schools (one continuation high school, two alternative middle schools, and one charter middle/high school) were in a low income, violence-ridden section of a major west coast city. Two schools (one elementary

and one high school) were in an economically depressed, rural area, where marijuana is a major cash crop.

Sample. A total of 605 students participated in the six studies: 267 in the treatment group and 338 in the control. All had multiple risks for school failure and/or use of alcohol or marijuana. For all students, those risks included being in communities where medical marijuana is legally distributed and marijuana is readily available. For all students in the urban studies it included low socioeconomic status and high neighborhood crime. For many it included multiple, family-level risks, including illegal immigrant status, single-parent family structures, parental addiction and mental health problems. For rural students, the geographic isolation is both an educational risk, and a risk for higher rates of alcohol abuse. For students in half of the urban studies, there were additional, group level behavioral or performance risk factors that increased students' chance of school failure. These included their having previously been retained in a grade, dropped out, been expelled, or become involved with the juvenile justice system. Key demographic characteristics are reported in Table 1.

Table 1. *Demographic Characteristics of Sample by Study and Overall*

Demographic Factor	Overall (n=605)	Study 1 (n=117)	Study 2 (n=177)	Study 3 (n=53)	Study 4 (n=107)	Study 5 (n=31)	Study 6 (n=120)
Grade(s)	6-12	8	9-12	7	8-9	8	6
Average age	13.5	13.7	16.6	11.9	13.4	14.3	10.8
Gender							
Female	43%	27%	41%	42%	54%	46%	51%
Ethnicity							
African American	31%	67%	72%	2%	2%	78%	17%
Asian/ Pacific Islander	6%	2%	10%	2%	1%	4%	0%
Hispanic	32%	29%	17%	0%	3%	18%	83%
Native American	1%	0%	0%	9%	2%	0%	0%
White	30%	2%	1%	87%	91%	0%	0%
English language learner							

Yes	30%	27%	24%	0%	0%	19%	83%
Free/Reduced Lunch							
Yes	60%	80%	61%	36%	31%	100%	94%

School principals used a variety of methods to select the twelve implementers who facilitated the intervention, from flip of a coin at School 3, to professional or non-professional status at School 6. At this site, non-professionals (janitor, cafeteria aide, secretary, volunteer) who supervised half of the advisory periods, were chosen to facilitate treatment group students, in the hopes mitigating potentially negative behavioral effects of not having certified teachers. Certified teachers were in charge of Advisory period for the control group students. At other schools implementers included a social worker, a math teacher, and an English teacher. None were experts in social-emotional learning.

INTERVENTION

The intervention was a subset of tutorials from Ripple Effects SEL software. Ripple Effects computerized SEL training is designed to build protective factors, reduce risk factors, and solve problems in a wide range of non-academic areas correlated with school success. The tutorials are reading-independent training modules, each consisting of from 10 to 12 different learning strategies, which take about 15 minutes, on average, to complete. Content is delivered using multiple media– photos, illustrations, videos, audio, peer voices reading aloud the text, and interactive exercises, all with a hip-hop look and feel.

At the time of this study, Ripple Effects teen version had 178 multimedia tutorials (390 as of 2008). The intervention examined here was a "self-efficacy" configuration of the software. Self-efficacy is the context-specific belief in one's capacity to master what is needed to succeed (Bandura, 1997). A scope and sequence was designed to promote cognitive, social and emotional capacity-building toward those intended ends. Students were to complete 42 tutorials, or roughly 14

contact hours, by working independently directly on the computer.

Twenty-one of the tutorials addressed self-efficacy, including social-emotional competencies that are linked to successful translation of belief in one's capacity for mastery, to actual mastery. During the three-hour, pre-intervention trainings at each site, staff collaboratively chose 21 additional tutorials to address their students' needs. All 136 remaining tutorials were available for students to privately address individual interests or risks, after completing their assigned tutorials.

Learning process. Independent of specific content, the *Whole Spectrum Self-Regulated Learning System* that powers Ripple Effects software contains instructional modalities that have been linked to successful development of self-efficacy: context-specific application, guided mastery, self-regulated learning, observational learning, systematic self-reflection, transfer training, and skill rehearsal (Bandura, 1997; Pajares & Urdan, 2006). 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, and positive reinforcement for completion of the learning process.

Implementer role and training. The role of the adult implementer was to select the site-specific tutorials, and then introduce the intervention at the first session, assign the tutorials, and check "electronic scorecards" to monitor dosage and ensure compliance. For each site, Ripple Effects staff provided implementers with a single three-hour training session to become familiar with the software, create the site-specific scope and sequence for the "implementer's choice" tutorials, and learn how to monitor student electronic scorecards for completion. They were not trained in, did

not deliver, and did not facilitate discussion of, any of the assigned content.

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 data, because they had been removed from school. We classified as "study attrition" the percentage of students who were physically enrolled in school, but did not comply with study protocols, withdrew consent to participate, or did not complete the self-report surveys both before and again after the intervention. We classified as "intervention attrition" the percentage of treatment group students who had consented to the study but did not have minimal exposure to the intervention. Minimal exposure was defined as completion of interactive exercises from at least 12 tutorials (equivalent to roughly three contact hours, or 30% of the total assigned content). We included in dosage analysis all students who had at least three hours exposure to the software program. Exposure to self-selected content was a yes or no event; we did not analyze that dosage.

Quantitative outcome measures The outcome measures used in the analyses included GPA as a measure of academic achievement, attendance (percentage of days missed) and tardiness as measures of school engagement, numbers of discipline referrals and suspensions as measures of behavior, and school enrollment rates at one-year follow-up, as a measure of persistence of gains. For efficacy analyses, we included all students who had at least three hours exposure to the software program.

DATA COLLECTION

Compliance, dosage and concept mastery. Ripple Effects software automatically collected data on compliance and dosage. 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 game-like quiz.

School data. School administrators provided data on GPA, absenteeism, tardies, suspensions, and discipline referrals at the end of the first semester following completion of the intervention. They also provide student demographic data. The school districts provided some prior year and follow-up data two years after the initial data collection period, however, due to high mobility rates, in only one study did the sample represent a large enough percentage of students to allow meaningful analysis.

METHOD OF ANALYSIS

For all data with post-Ripple Effects values only (e.g., GPA for most schools), we ran independent-samples t-tests comparing the means of the treatment and control groups. For most schools, descriptive analyses of reported number of days absent, tardies, suspensions, and behavioral referrals indicated a severe restriction of range due to the relative non-occurrence of these events (e.g., the modal value for most of these outcomes was 0). Furthermore, skewness and kurtosis values suggested that these variables did not meet the distributional assumptions of parametric tests. 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. Therefore, we used the Games-Howell test as an appropriate correction for all outcomes data except GPA. To account for the unbalanced treatment and control group sizes, we randomly sub-sampled the control group to match the treatment group size.

One study, School 5, provided sufficient administrative data with pre and post values (GPA and absenteeism), to enable use of repeated-measures of analysis of variance (ANOVA) to examine whether or not some of the differences between treatment and control remain after taking into account where students started. Two studies, Schools 1 and 2, provided enough baseline and follow-up data to enable independent-samples t-tests comparing the single-year means of the treatment and control groups. 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 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 performed bivariate Pearson product-moment correlations. For each set of correlations, we used the Bonferroni method to minimize the chances of making a Type I error.

Results

BASELINE EQUIVALENCE

There was insufficient baseline data on school outcomes for the year prior to the study to confirm that students randomly assigned to treatment and control groups were equivalent academically at baseline. Three studies (1, 2, and 5) provided some data to determine equivalence. Study site 5 provided sufficient data to enable a separate analysis taking into account baseline differences, reported in Table 2.

Due to high mobility and dropout rates in the district that provided some baseline data, Study sites 1 and 2 represented too small a percentage of the sample to allow ANOVA from pre to post intervention. Independent-samples t-tests on prior year data at these schools revealed no significant baseline differences between treatment and control groups on GPA or attendance, with treatment group students having somewhat lower GPA, and higher absenteeism scores at baseline.

Analysis of pretest surveys across all six schools also indicated no significant baseline differences between treatment and control groups for any self-report variable (locus of control, and attitudes towards alcohol and marijuana). The equivalence on self-report measures, randomized assignment to condition, and trends on prior year data, all suggest equivalence on school outcomes as well, but do not demonstrate it. It is possible that starting differences between control group students and those in the intervention group may be responsible for the post-intervention differences.

Table 2. *School 5 Treatment/Control Comparisons of Baseline Scores on GPA and Absenteeism for 2002-2003 School Year*

Outcome	Treatment		Control		Difference
	M	SD	M	SD	
GPA	1.10	.36	1.78	.87	-0.68
Absenteeism	0.19	.13	.15	.15	.04

Note: The sample consists of 14 students in the treatment group and 12 students in the control group.

PROCESS OUTCOMES

Enrollment attrition. Administrative data indicated that during the intervention period 5% of the treatment group and 7% of the control group moved or left the participating schools, leaving 253 in the treatment group and 314 in the control.

Study attrition. Six percent of the treatment group and 3% of the control group withdrew consent to participate, and all of these came from a single study site (Study site 4). The built-in electronic monitoring, coupled with reports by the facilitators, indicated that one control group student had contact with the intervention (.3%), and thus was dropped from the study (remaining TG N = 238, CG N = 305).

Intervention attrition. Intervention attrition was defined as failure to receive minimal exposure to the intervention, defined as 30% of the assigned tutorials or at least three contact hours. Of the 238 treatment group students remaining after enrollment and study attrition, 83 (35%) did not receive minimal exposure to the self-regulated intervention.

Dosage. Mean dosage for those who complied was 77% of total required topics, which equaled 31 tutorials and approximately 10 contact hours, depending on student pace.

Participation in self-selection option. Among students who had minimal exposure, 96% also chose to privately explore tutorials beyond those assigned.

SCHOOL PERFORMANCE OUTCOMES

According to Table 3, there are significant differences in GPA and suspensions between treatment and control group students. Additionally, there were some statistically significant differences in the number of absences and tardies between treatment and control students for individual studies.

GPA. Across study sites, students who participated in the Ripple Effects intervention had grades that were approximately 1/3 of a letter grade higher than the control students (p< .01). In three of the six studies (2, 3, and 5), Ripple Effects students had better grades than the control group students, ranging from approximately 1/2 to nearly a full letter grade higher than the control group students. There were no significant differences in academic GPA between the two groups of students in Studies 4 and 6, although there were significant differences in Study 6 for social and personal responsibility GPA (p<.01).

Attendance. In general, the rates of absences were low for both groups of students. Students in the Ripple Effects group did not have better attendance at school than did students who did not participate in the intervention. For one study (Study 1), students in the control condition had a lower rate of attendance of 0.001 (0.1%) when compared to the Ripple Effects group. This difference, while small, was statistically significant (p<.05).

Tardiness. Treatment students were less likely to come to class late than their peers in the control group, with an average of 1 tardy per student compared to 1.3 tardies per student for the control group. Five schools had reliable data to conduct the analyses for this outcome. Of those schools, only Study 5 had significantly lower tardy rates for their Ripple Effects students. Studies 1, 3, and 6 had fewer tardies for treatment students than for the control students, but these values were not significant.

Suspensions. Ripple Effects students were less likely to be suspended than their peers in the control group, with treatment group suspensions at zero compared to a mean rate of 0.14 per student for the control group (p<.05). Studies 1, 2, and 6 had data to conduct the analyses for this outcome. All had fewer suspensions for Ripple Effects students than for the control students, but only in Study 1 was the value significant (p<.05).

Table 3. *Differences in Student Outcomes for Treatment and Control Students*

Study	Treatment Group			Control Group			Difference	Cohen's *d*
	N	Mean	SD	N	Mean	SD		
GPA								
All Studies	155	2.90	0.73	163	2.53	1.00	0.37**	0.42
Study 1	21	2.41	1.03	19	1.68	0.94	0.73*	0.76
Study 2	27	2.96	0.41	27	2.46	0.98	0.50*	0..68
Study 3	23	3.20	0.77	26	2.23	1.22	0.97**	0.63
Study 4	22	2.88	0.82	19	3.31	0.53	-0.44	0.96
Study 5	14	2.26	0.62	14	1.46	0.99	0.79*	1.01
Study 6	48	3.13	0.41	58	2.97	0.46	0.16	0.37
GPA Social Responsibility (School 6 only)								
Study 6	48	3.13	0.44	58	2.76	0.47	0.37**	0.82
GPA Personal Responsibility (School 6 only)								
Study 6	48	3.13	0.44	58	2.72	0.49	0.40**	0.88
Absenteeism (proportion of days absent to days enrolled)								
All Studies	156	0.03	0.08	160	0.04	0.08	0.01	0.00
Study 1	21	0.00	0.00	21	0.001	0.00	-0.001*	0.00
Study 2	27	0.16	0.11	21	0.16	0.12	0.00	0.00
Study 3	23	0.01	0.01	26	0.01	0.02	0.00	0.00
Study 4	22	0.00	0.00	19	0.00	0.00	0.00	0.00

Study 5	14	0.00	0.01	15	0.01	0.02	-0.01	0.70
Study 6	49	0.03	0.05	58	0.05	0.06	-0.02	0.40
Tardies[a]								
All Studies	129	0.93	1.70	140	1.25	1.96	-0.32	-0.32
Study 1	21	0.90	1.84	21	1.95	3.03	-1.05	-1.05
Study 3	23	0.52	0.79	26	0.65	1.36	-0.13	-0.13
Study 4	22	1.64	2.19	19	0.95	1.87	0.69	0.69
Study 5	14	0.00	0.00	16	0.44	0.73	-0.44*	-0.44*
Study 6	49	1.08	1.82	58	1.59	1.86	-0.50	-0.50
Suspensions[b]								
All Studies	96	0.00	0.00	106	0.14	0.71	-0.14*	-0.14*
Study 1	21	0.00	0.00	21	0.48	1.37	-0.48	-0.48
Study 2	26	0.00	0.00	27	0.11	0.58	-0.11	-0.11
Study 6	49	0.00	0.00	58	0.03	0.26	-0.03	-0.03

a: No data from Study 2; b: No data from Study 3, Study 4, and Study 5

$* p < 0.05$, $** p < 0.01$

Prior Grades and attendance. In order to make appropriate judgments about whether the treatment actually had an effect on student outcomes, it is important to compare treatment and control students taking into account their grades and attendance patterns prior to the intervention. To examine whether or not some of the differences between treatment and control remain after taking into account where students started, we conducted repeated-measures ANOVAs with Study 5, where we were able to obtain prior years' data.

There were statistically significant differences between the GPA gains for the treatment and control students. The treatment students increased their GPA by over one grade point, while the control students decreased their GPA by about 1/3 of a point. With respect to attendance, there were no statistically significant differences. The rates of absences were low for both groups of students. Table 4 shows the results of these analyses.

Table 4. *Pre-Post Analysis of Grades and Attendance, School 5*

Treatment Group (N = 14)					Control Group (N = 12)					Difference in Gain for the Two Groups
Pre Test		Post-Test			Pre Test		Post-Test			
M	SD	M	SD	Change	M	SD	M	SD	Change	
GPA										
1.10	0.36	2.26	0.62	1.16	1.78	0.87	1.44	1.04	-0.34	1.5**
Absences										
0.19	0.13	0.004	0.01	-0.186	0.15	0.15	0.01	0.01	-0.14	-0.046

$* p < 0.05$, $** p < 0.01$

Discipline referrals. Overall, there were no significant differences between Ripple Effects students and their peers in the control group in frequency of discipline referrals (Table 5). At most Study sites, the numbers of incidents

were small. The treatment group generally had fewer referrals than the control group in all categories, ranging from 20 to 100% fewer. In 7% of cases, control group students had fewer referrals. None of these differences were

statistically significant. Study 5 showed the largest differences between treatment and control, but the school's data also tended to have standard deviations that were double the size of the means. It is therefore not surprising that with such a wide range of responses, the differences between the treatment and control means, though large, were not statistically significant.

DOSAGE EFFECTS

Across all Studie, there were significant, small correlations between hours of Ripple Effects and absences $r(119) = -0.34$, $p = 0.002$, and between hours and GPA $r(118) = 0.28$, $p = 0.0001$ (Table 6). Among individual schools, there were no significant correlations at the 0.002 level. There were no significant correlations between hours and tardies or suspensions across the whole sample and within individual schools. When the sample was separated into dosage groups (minimum, moderate, and maximum), there were no significant correlations for GPA, tardies, absences and suspensions (Table 7).

TWELVE-MONTH FOLLOW-UP ON SCHOOL ENROLLMENT

Attempts to test whether positive effects persisted over time were partially successful.

Follow-up administrative data for the 2004-05 school year, provided by the school district, allowed us to compare school enrollment rates at three Study sites, one year post-intervention. At Study site 1, 62% of treatment group students and 60% of control group students who were enrolled at the point of post-intervention data collection were still enrolled somewhere in the school district, a non-significant difference.

At both of the other Study sites, the differences were both substantive and significant. At Study site 2, 55% of treatment group students versus 26% of control students, were still enrolled somewhere in the school district one year post-intervention. This difference was significant, p<.05. This does not include students from either group who were in 12th grade at the time of the intervention and were no longer enrolled at follow-up. We cannot state with certainty whether the 12th graders all graduated, or some dropped out. At Study site 5, 71% of treatment group students and 36% of control group students who were enrolled at the time of post-intervention data collection, remained enrolled at 12-month follow-up, a substantial, significant difference, p<.05.

Table 5. *Differences in Average Number of Referrals for Ripple Effects and Control Students by Study/site*

School and Discipline Category	Treatment		Control		Difference
	Mean	SD	Mean	SD	
All Studies	N = 129		N = 142		
Fighting/starting a fight	0.02	0.12	0.03	0.17	-0.01
Defiant or disruptive	0.43	2.67	1.02	4.06	-0.59
Total number of discipline referrals	1.34	9.83	3.03	12.83	-1.69
Study 1	N = 21		N = 20		
Assault	0.00	0.00	0.05	0.22	-0.05
Defiant or disruptive	0.10	0.30	0.10	0.31	0.00
Drug use	0.00	0.00	0.05	0.22	-0.05
Fighting or starting a fight	0.00	0.00	0.05	0.22	-0.05
Threaten student	0.00	0.00	0.05	0.22	-0.05
Total number of discipline referrals	0.10	0.30	0.30	0.57	-0.20

Study 3		N = 23		N = 26	
Defiant or disruptive	0.52	0.85	0.73	0.87	-0.21
Fighting or starting a fight	0.04	0.21	0.04	0.20	0.01
Sexual harassment	0.00	0.00	0.12	0.43	-0.12
Swearing	0.09	0.29	0.08	0.39	0.01
Talking	0.13	0.34	0.19	0.49	-0.06
Threaten student	0.13	0.34	0.00	0.00	0.13
Total number of discipline referrals	0.91	1.12	1.15	1.57	-0.24
Study 4		N = 22		N = 22	
Assault	0.00	0.00	0.05	0.21	-0.05
Cut class	0.00	0.00	0.05	0.21	-0.05
Defiant or disruptive	0.18	0.40	0.09	0.29	0.09
Tardy	0.00	0.00	0.05	0.21	-0.05
Total number of discipline referrals	0.18	0.39	0.23	0.53	-0.05
Study 5		N = 14		N = 16	
Defiant or disruptive	2.14	8.02	6.94	10.46	-4.79
Fighting or starting a fight	0.07	0.27	0.06	0.25	0.01
Name calling	0.71	1.54	2.56	4.40	-1.85
Swearing	2.14	8.02	5.94	10.04	-3.79
Talking	2.29	7.99	3.50	6.78	-1.21
Threaten adult	0.00	0.00	0.13	0.34	-0.13
Threaten student	0.71	2.67	0.13	0.34	-0.59
Walk out	1.86	5.39	4.06	8.21	-2.21
Total number of discipline referrals	9.93	29.28	23.31	32.34	-13.38
Study 6		N = 49		N = 58	
Defiant or disruptive	0.14	0.46	0.19	0.61	-0.05
Fighting or starting a fight	0.00	0.00	0.02	0.13	-0.02
Swearing	0.00	0.00	0.02	0.13	-0.02
Threaten student	0.00	0.00	0.05	0.22	-0.05
Total number of discipline referrals	0.14	0.46	0.28	0.74	-0.14

$* p < 0.05, ** p < 0.01$

Table 6. *Correlations Between Dosage, GPA, Absences, Tardies, and Suspensions Across Studies and by Study*

	GPA		Absences		Tardies		Suspensions	
Study	N	r	N	r	N	r	N	r
All Studies	118	0.28**	119	-0.34**	92	0.03	118	a
Study 1	21	0.26	21	a	21	0.27	21	a
Study 2	27	0.21	27	-0.39	0	a	26	a
Study 4	22	0.34	22	a	22	-0.34	22	a
Study 6	48	0.29	49	-0.04	49	0.08	49	a

a: Value could not be computed because at least one of the variables is missing or constant
$** p < 0.002$

Table 7. *Correlations Between Dosage, GPA, Absences, Tardies, and Suspensions, by Level of Dosage*

| | GPA | | Absences | | Tardies | | Suspensions | |
Level	N	r	N	r	N	r	N	r
Minimum	39	0.30	40	-0.13	24	-0.15	36	a
Moderate	43	-0.24	43	-0.09	33	0.13	29	a
Maximum	36	0.00	36	0.23	35	0.42	31	a

a: Value could not be computed because at least one of the variables is missing or constant
** p < 0.002

Discussion

The data from this series of real-world studies suggest: a) If you make a technology-based SEL training option available and direct students to use it, according to their own learning style, a substantial majority will comply; b) If they minimally comply with the assignment, they will proactively use it to get private, individualized guidance in areas they select; and c) This combined use of the intervention is significantly correlated with higher GPA and fewer suspensions in the short term, and higher rates of continued school enrollment in the long term.

The mean compliance rate of 65% is valid for the studies overall, but rates were somewhat bi-modal. At three of the study sites, 80% or more of eligible students at least minimally complied with the intervention protocol; at the other study sites, from 37% to 61% did. We hypothesize that school climate, as well as individual motivation and ease of access to the technology, may be factors in compliance, and address these issues in another paper on implementation fidelity and compliance (Ray, Berg, 2009).

Among students who were minimally exposed to the intervention, the data indicate almost all (96%) took advantage of the opportunity to privately explore areas of individual interest or concern. In fact, the Study/site with the lowest average compliance rate, Study 1, had the highest rate of individual use of the intervention for self-selected topics. Essentially, student choice transformed a group-level, secondary, preventive intervention, into an individualized, intensive tertiary intervention. This is a meaningful finding because this student population with multiple risk factors frequently has high rates of family and community-related trauma, but low rates of voluntary use of school counselors, or mental health professionals.

Third, across studies, as little as three hours exposure to the combination of teacher-assigned and self-selected skill-building tutorials resulted in positive differences in academic performance when compared to control group students. The greatest effects were among the students with the most risks. That from three to ten hours of independent use of a computerized, social-emotional learning intervention was correlated with substantively and significantly better grades among diverse adolescents with multiple risk factors is startling. We do not currently have a way to tease out causal mechanisms, including the relative value of the self-chosen, versus teacher-assigned components.

The intervention's apparent significant positive impact on suspensions, and trends toward positive impact on attendance, tardiness, and discipline referrals, suggests that the intervention may also offer a scalable means to increase school engagement, and improve school climate. Although reductions in

discipline referrals were not statistically significant, the combination of large effects sizes and reduction to zero in the treatment group of many infractions that were also low in the control group, is substantively meaningful for practitioners. More research is needed to explore these findings further.

The fact that one year post-intervention, district level enrollment data for two of the three schools that tracked it, indicated treatment group students had continued enrollment rates more than twice as high as their control group counterparts suggest that this very short term, relatively inexpensive intervention may be a valuable tool for dropout prevention among students with multiple risk factors who currently account for the largest portion of the achievement gap.

LIMITATIONS

Insufficient baseline data. Only three of the six studies had prior year school data, and even that data was insufficient to enable useful analysis, limiting the ability to interpret the impact of the intervention on outcomes. In three of the six cases, the paucity of school baseline and follow up data was a function of the extreme transience of these student populations. Many have previously dropped out, move in and out of the juvenile justice system, and/or are from undocumented families that move frequently either to find work, elude immigration authorities, or/and because they get behind in their rent and have to move. It is possible that the differences in outcomes can be attributable to starting differences between students. However, none of the data we could gather suggests that is the case.

Attrition bias. Thirty-five percent of treatment group students did not have minimal exposure to the intervention, and so were excluded from analysis of efficacy. It is possible that students who were not exposed were lower performing students overall and thus raised the average performance for the remaining treatment group students. However, those students who were exposed and for whom we had baseline data, were also almost universally

low performing, so it is doubtful that could fully explain the effect.

Small sample sizes. The small sample sizes were a function of the decision to allow site-specific adaptation of content and conditions of use as a real world test of effectiveness, despite the negative impact on the overall available sample size and scope of the research effort. Although these conditions increase the chance of both Type I and Type II error, they also increase the probability that the implementation and study can be replicated in diverse real world settings. The small sample sizes, coupled with large variances with behavioral data, also made it difficult to detect effects of the intervention on discipline referrals. The latter is indicative of a near universal school condition in which a few students account for much of the disruptive behavior.

Conclusion

Given recent findings on the failures of much educational technology to positively impact outcomes in the domains of math and reading, the potential positive impacts of social-emotional learning software are particularly promising. Further research is urgently needed to replicate these studies with larger samples, and see whether the promise shown here by computerized SEL training can be further validated. If so, it offers a more widely usable and easily scalable new tool in the effort to address one of the most pressing issues facing educators today: how to improve outcomes for youth with multiple risk factors for school failure, within the real-world constraints faced by our nation's public schools.

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