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Recovery High Schools: Effect of Schools Supporting Recovery from Substance Use Disorders

Andrew J. Finch, Ph.D.,
Vanderbilt University

Emily Tanner-Smith, Ph.D.,
Vanderbilt University

Emily Hennessy, Ph.D. candidate,
Vanderbilt University

D. Paul Moberg, Ph.D.
University of Wisconsin-Madison

Abstract

Background: Recovery High Schools (RHSs) provide post-treatment education and recovery support for young people with substance use disorders (SUDs). This is the first quasi-experimental outcome study to determine RHS effectiveness relative to students in non-RHSs.

Objectives: To examine effects of RHS attendance on academic and substance use outcomes among adolescents treated for SUDs six months after recruitment to the study.

Methods: A quasi-experimental design comparing outcomes for adolescents with treated SUDs who attended RHSs for at least 28 days versus a propensity-score balanced sample of students with treated SUDs who did not attend RHSs. The sample included 194 adolescents (134 in RHSs, 60 in non-RHSs) enrolled in Minnesota, Wisconsin, or Texas schools (M age = 16; 86% White; 49% female). Multilevel linear regression models were used to examine the effect of RHS attendance on students' outcomes, after adjusting for a range of potential confounders.

Results: Adolescents attending RHSs were significantly more likely than non-RHS students to report complete abstinence from alcohol, marijuana, and other drugs at the 6-month follow-up ($OR = 4.36$, $p = .026$), significantly lower levels of marijuana use ($d = -0.51$, $p = .034$) and less absenteeism from school ($d = -0.56$, $p = .028$).

Conclusion: These results indicate that RHSs have significantly beneficial effects on substance use and school absenteeism after six months for adolescents treated for SUDs.

Keywords

adolescents; recovery schools; school success; substance use

Address for Correspondence: Andrew J. Finch (andrew.j.finch@vanderbilt.edu).

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Recovery high schools (RHSs) are a potentially important schooling innovation for providing educational and therapeutic support to a vulnerable population of students at risk for school failure or dropout and substance use relapse (1-2). Students typically enter RHSs voluntarily based on referral from treatment centers, counselors, or parents. With almost 40 recovery schools currently in operation, and approximately 90 having existed since 1979 (3), RHSs are worth closer examination to determine effectiveness. A critical question is whether RHSs do, in fact, have a positive effect relative to non-RHSs in facilitating academic achievement and improving drug use outcomes for students recovering from substance use disorders (SUDs). The primary objective of this study is to answer that question for a group of RHSs in Minnesota, Wisconsin, and Texas.

Background

Alcohol and other drug use by high school students is a recognized problem in many schools and communities (4). Drug and alcohol use not only has adverse effects on the health and safety of these youth, but also is likely to affect school performance. Although the relationship is likely bidirectional, longitudinal studies indicate that substance use during adolescence is associated with poor academic performance, drop-out, and lower college attendance (e.g., 5-8).

Approximately 1.23 million youths aged 12–17 met the criteria for SUDs in 2015 (5 percent of this population) (9). An estimated 198,000 youths aged 12–17 received treatment[i] for substance use (9) in 2015, but treatment does not produce sure cures. It is characteristic of SUDs that the progress of patients is marked by cycles of recovery and relapse (10). Abstaining from alcohol or drugs represents a major challenge for students during and after SUD treatment, and they are especially vulnerable to relapse during the period right after completion of a treatment program (11– 13).

During the particularly fragile period after SUD treatment, school is an important social environment for adolescents with SUDs. Adolescents spend more time in school than anywhere other than their homes, providing crucial social networks influencing their development. Among the most significant risk factors for substance use are association with drug-using peers, alcohol or drug availability, and academic challenges (14), all likely to be present in the school context. For an adolescent in recovery from an SUD, school is a context likely to involve interactions with peers who are actively using substances. For example, in 2015, an estimated 21.7% of high school students were offered, sold, or given an illicit drug on school property (15). One study found that virtually all adolescents returning from treatment to their old school reported being offered drugs on their first day back in school (11).

Schools, however, can also promote recovery. Indeed, the National Institutes of Health, the Office of National Drug Control Policy, and the U.S. Department of Education have all

[i]The NSDUH defines substance use treatment as treatment received in order to reduce or stop illicit drug or alcohol use, or for medical problems associated with illicit drug or alcohol use. Location includes treatment received at any location, whether a specialty facility, such as a hospital (inpatient), rehabilitation facility (inpatient or outpatient), or mental health center; or non-specialty, such as an emergency room, private doctor's office, self-help group, or prison/jail.

published calls for more school based recovery support programs, as research (16-18) has affirmed both the chronic nature of SUDs for adolescents and the need for more continuing care programs. The annual Monitoring the Future study demonstrates a continuing decline in youth substance use since 2000 (19), suggesting a positive impact of prevention, intervention, and recovery support programs, implemented in school and community-based programs. Although the Association of Recovery Schools (ARS) was formed in 2002 to "support and inspire RHSs for optimum performance, empowering hope and access to every student in recovery" and there has been steady growth in the number of RHSs in operation since that time, there still is little research measuring the relative effect of those programs.

Theoretical Framework

RHSs support students recovering from SUDs by offering a full range of academic services in a structured environment that promotes recovery. The most complete picture of these schools and their operative program models comes from a descriptive study of 17 RHSs (17). RHSs are typically small schools that offer both academics and diverse therapeutic services, including individual and group counseling, chemical dependency education, family support, and random drug testing. Academic supports range from transitional help for students to earn credits and catch up for missed opportunities, to the provision of complete (although limited) high school curricula.

Recovery support programs enhance a person's "recovery capital," a concept that encompasses all resources related to the recovery process, including financial, human, social, and community factors (20-22). RHSs attempt to support the recovery and academic achievement of their students by creating connectedness and social capital in a context that supports the building of adolescents' recovery capital (16).

Study Aims

Given the potential benefits of RHS attendance on adolescents' outcomes, but the dearth of empirical research on the topic, this study aimed to examine the effects of RHS attendance on academic and substance use outcomes among adolescents treated for SUDs. This study is the first to use a longitudinal quasi-experimental design to examine the effects of RHS attendance on adolescents' outcomes, specifically examining whether students who have received treatment for SUDs and who subsequently attend RHSs experience significantly better behavioral outcomes (less alcohol and other drug use) and educational outcomes (higher GPA, better attendance) compared to recovering students who attend school in other settings.

Methods

Because it was neither feasible nor ethical to randomly assign adolescents to attend RHSs or non-RHSs, this study used a quasi-experimental comparison group design to evaluate the effectiveness of RHSs. A non-equivalent pretest-posttest comparison group design (23) was used to compare outcomes for adolescents who had received some formal SUD treatment and subsequently enrolled in a RHS for at least 28 days at any point during the intake

through 6-month follow-up assessment (the intervention group) and adolescents who had received some formal SUD treatment and subsequently enrolled in a non-RHS (the comparison group). This operational definition for the intervention and comparison groups was selected to correspond to an operational threshold of approximately one month of RHS attendance for the intervention group. Because baseline differences between groups are one of the primary threats to interval validity in this type of quasi-experimental design (i.e., due to potential selection bias in choice of schools), we used a propensity score based selection of cases for the comparison group and subsequent balancing procedure to ensure baseline equivalence between the intervention and comparison groups.

Subjects

The population of interest was adolescents attending high school after receipt of formal inpatient or outpatient SUD treatment. We recruited adolescent patients from 10 SUD treatment facilities in Minnesota, Wisconsin, or Texas. These locations were selected given that RHSs were known to be in operation nearby at the time of study recruitment. Adolescents and their families were recruited into the study upon discharge from the SUD treatment program (baseline data collection period), and then longitudinally followed after discharge at 3-month, 6-month, and 12-month follow-up periods. Students in the intervention and comparison conditions were recruited from the same treatment programs (i.e., catchment areas). After discharge from the SUD treatment program, families were free to enroll adolescents in any type of formal schooling; some elected to enroll in RHSs whereas others enrolled in other non-recovery oriented high schools. After the first year of recruitment, this plan yielded a smaller number of RHS enrollees than expected. Therefore, we supplemented the recruitment strategy to also identify adolescents who were newly enrolled in RHSs and had recently (within the past 12 months) discharged from a SUD treatment program. This supplemental recruitment strategy was used to increase the sample size for the intervention group only (i.e., no comparison group participants were recruited in this manner). In the final analytic sample, 18% of the RHS students and 93% of the non-RHS students had been recruited from SUD treatment programs. Thus 82% of the students in the RHS group were directly recruited from RHSs, and 7% ($n = 4$) of the students in the comparison group were recruited from RHSs but did not attend those RHSs for long enough to meet the 28 days of attendance criterion (Mean days actually attended the RHS = 15.5, $SD = 12.45$). To control for these differences in recruitment strategies, all outcome analyses statistically adjusted for recruitment site (described in more detail below).

The current report uses data from the baseline, 3-month, and 6-month follow-up responses from adolescents enrolled in the study between December 2011 and January 2016. A total of 294 adolescents were enrolled in the study during this period, 229 of whom completed 6-month follow-ups. Attrition analyses indicated that there were no significant differences between study completers and dropouts on the baseline measures of all outcomes variables (alcohol use ($t = -1.31$, $p = .19$), marijuana use ($t = 0.22$, $p = .83$), other drug use ($t = 0.73$, $p = .47$), school grades ($t = -0.57$, $p = .57$), truancy ($t = 0.19$, $p = .85$), and absenteeism ($t = -0.60$, $p = .55$).

Ethics.—Study data were collected between December 2011 and January 2017 and all data collection procedures were approved by the Institutional Review Boards at the participating universities.

Materials and Measures

The current study reports on findings from data collected during extensive youth assessments at baseline, 3-month, and 6-month follow-ups (24). Standardized assessments were collected in-person via computer assisted interviewing software using local teams of trained, mostly master's level data collectors.

Substance Use Outcomes.—We examined four outcome measures of adolescents' self-reported substance use, which were collected at baseline (defined as "…during the three months prior to attending treatment for substance abuse…") and the 6-month follow-up. Three outcomes measured days of use in the past 90 days: days used alcohol, days used marijuana, and days used drugs other than alcohol/marijuana (range 0 – 91 days). The fourth outcome was complete abstinence from alcohol, marijuana, and other drugs in the past 90 days (yes/no). These four outcomes were each assessed using the Timeline Followback method (25).

Academic Outcomes.—We examined three self-reported academic outcomes, all of which were collected at baseline and the 6-month follow-up. Self-reported grades were measured as the average of three items that asked adolescents what kind of grades they typically received in English/reading, mathematics, and all other subjects in the prior three months (range 0 [*Mostly Fs*] to 4 [*Mostly As*]; Cronbach's $\alpha = .78$). Self-reported skipping school (truancy) was measured with one ordinal item that asked adolescents when they last skipped school in the prior three months (range 0 [*Never*] to 4 [*In the past month*]). Self-reported absenteeism was measured with one item that asked adolescents how many days they were absent from school for a full-day in the prior three months (range 0 – 54).

Covariate Controls.—We collected data on a range of other adolescent characteristics, including adolescent demographics, substance use treatment histories, mental health histories, and family functioning. The following baseline measures were included in the propensity score estimation model (described in greater detail below) to control for potential differences in the intervention and comparison conditions: adolescents' self-reported age, race, gender, comorbid mental health conditions (collected using the M.I.N.I. Structured Clinical Interview for major Axis I psychiatric disorder diagnostics (26)); alcohol use, tobacco use, other substance use, and mental health treatment service receipt (all collected using the Timeline Followback method (25)); physical health treatment receipt, life satisfaction, sources of life stress, criminal justice system involvement, and school problems (all collected using the GAIN-Q3 (27)); perceived consequences of drug use (collected using the Personal Experiences Inventory (28)), interest in attending an RHS, perceived teacher support for substance use recovery, prior year school attendance and grades, negative attitudes toward school (collected using the Behavior Assessment System for Children (29)), perceived academic abilities, family income, parental education level, parental history of substance use treatment, and family history of mental health problems. Additional

measurement details and rationale for selection of control variables has been reported in prior articles (24, 30).

Analytic Strategies

Propensity score model.—Because adolescents self-selected into the intervention and comparison conditions, we used propensity scores to address potential baseline non-equivalence between the two conditions (30). Propensity scores were conceptualized as the predicted probability of attending an RHS, estimated with multilevel logistic regression models that included baseline values of all outcome variables as well as the covariate controls noted above. The propensity score model was built following the recommendations of Imbens and Rubin (31), using a stepwise procedure for selecting the covariates and higher-order terms included in the estimation model. We then assessed and ensured balance conditional on the estimated propensity score using multilevel linear regression models and visual inspection of Q-Q plots (31-32). The linearized propensity score, and the square of this propensity score, were then used as covariate controls in the final outcome models. We chose to balance on the propensity scores using this regression adjustment approach (versus, for example, exact matching or matching within a caliper) in order to retain the largest sample size possible. Nonetheless, 35 potential participants in the comparison condition were outside the region of common support (i.e., the region of the estimated propensity score for which intervention and comparison participants are both represented), and were dropped from the analytic sample. The final analytic sample therefore included 194 participants who were well balanced on the estimated propensity score and thus well-balanced at baseline (134 in recovery schools; 60 in non-recovery schools).

Since this study used a quasi-experimental design, an intent to treat analysis was not appropriate in that we did not have the ability to assign individuals a priori to the intervention or comparison conditions. Hence there is no “intent to treat.” Rather, we defined a threshold at which the intervention is expected to have an impact as the operational definition for the intervention group. From the remaining participants in the research study, we then selected a comparison group that was well-balanced with the intervention group (i.e., comprised of participants similar to those in the intervention group on the range of covariates included in the propensity score model), defined as those participants who were in the same range of “common support” on the estimated propensity score.

Outcome models.—We used multilevel regression models to examine the effects of RHS attendance on adolescents’ substance use and academic outcomes. Linear regression models were estimated for the continuously measured outcomes, and logistic regression models were estimated for the binary outcome measure of abstinence. All models were estimated separately for each of the seven outcomes, with statistical controls for baseline values of the outcome variable, the linearized propensity score, and the squared propensity score. Due to perfect collinearity between the baseline and follow-up measures of complete abstinence, the models predicting this outcome controlled for the baseline mean number of days used alcohol, marijuana, or other drugs (rather than baseline measures of complete abstinence).

To account for clustering of adolescents within schools, all models were estimated using two-level models that included random intercepts for schools. We also included fixed-effects for recruitment sites to account for clustering within study recruitment sites, and to statistically control for any potential differences across recruitment sites (including RHS and SUD treatment recruitment sites). The linear outcome models can be written as:

$$y_{ij} = \beta_1 + \beta_2 RHS_{ij} + \beta_3 x_{ij} + \cdots \beta_p x_{ij} + \zeta_j + \varepsilon_{ij}$$

where y_{ij} is the outcome response for adolescent i in school j; α_1 is the average intercept, β_2 is the average slope for the intervention effect across schools; $\beta_3 \ldots \beta_p$ are the effects of the p covariates included in the model (pretest measure, propensity score, and recruitment site fixed-effects); ζ_j is the random intercept for schools, which represents the deviation of school j's intercept from the mean intercept β_1; and ε_{ij} are the residuals. All results are presented as unstandardized coefficient (bs) estimates of the β parameters. We also conducted sensitivity analyses using multilevel negative binomial regression models and multilevel ordinal logistic regression models for the continuous outcomes measured as count and ordinal variables (respectively). These models yielded substantively similar findings to those from the multilevel linear regression models, so we opted to present the results from the linear models for ease of interpretation. Results were not adjusted for multiple testing given the limited number but diverse set of outcome measures examined, all of which were specified a priori.

Missing data.—Due to survey nonresponse, there was a small amount of missing data on the academic outcomes as well as the covariates used in the propensity score estimation model. We used multiple imputation (33-34) to handle these missing data. We created 20 imputed datasets based on all key variables of interest (i.e., substance use and academic outcomes, adolescent characteristics, and all baseline covariates used in the propensity score estimation models), and then pooled values across the 20 imputed datasets to yield a single dataset with complete data for all cases.

Results

Sample Characteristics

Table 1 presents descriptive statistics for the characteristics of the propensity score balanced sample. On average, participants were age 16 and enrolled in 11th grade. The sample was split evenly by gender, but was predominantly White (86%). Students in the sample had extensive SUD treatment histories: students had an average of 3.85 (SD = 21.49) prior SUD treatment episodes, 39% had at least one outpatient SUD treatment episode, 57% had at least one intensive outpatient treatment episode, and 56% had at least one inpatient treatment episode. Based on the M.I.N.I. SCID diagnostic interview, 52% of students met criteria for an alcohol dependence diagnosis, 62% for an alcohol abuse diagnosis, 84% for a (non-alcohol) substance dependence diagnosis, and 93% for a substance abuse diagnosis. Notably, the majority of participants (93%) had at least one comorbid mental health condition (with major depression, generalized anxiety, panic, and oppositional/defiant disorders being the most prevalent). Participants reported high levels of substance use at baseline, reporting on

average 19 days of alcohol use, 55 days of marijuana use, and 30 days of other substance use in the 90 days prior to SUD treatment. Indeed, only 2% of the sample reported complete abstinence from alcohol, marijuana, and other drugs at (pre-treatment) baseline. Participants reported an average GPA of 2.42 (between mostly B's and mostly C's) and reported being absent from school 5 days in the past 90 days at baseline.

Among the 60 students in the non-RHS comparison group, at the 6-month follow-up assessment 47% were attending traditional public high schools, 23% were attending non-traditional high schools (including charter and alternative schools), 12% were attending online school, 5% were attending a technical school, 5% had graduated from high school, and 8% were not enrolled in school.

Effects of Recovery High School Attendance

Substance use outcomes.—The results indicated that adolescents who attended RHSs were significantly more likely to report being completely abstinent from alcohol, marijuana, and other drugs at the 6-month follow-up ($b = 1.47$, 95% CI [0.11, 2.69], $OR = 4.36$, 95% CI [1.19, 15.98]). The predicted probability of an adolescent being abstinent from substances was .58 for RHS students versus .30 for non-recovery school students.

At 6-month follow-up, adolescents who attended RHSs reported fewer days of marijuana consumption (*Mean* = 8.84, *SD* = 22.94 for RHS students; *Mean* = 25.82, *SD* = 34.90 for non-RHS students), and this difference was statistically significant in the adjusted regression model ($b = -14.38$, 95% CI [−27.70, −1.06]). Namely, RHS attendance was associated with one-half a standard deviation improvement in marijuana consumption ($d = -0.51$)—a statistically significant effect equivalent to 14 fewer days of marijuana use over the past 90 days.

At 6-month follow-up, adolescents who attended RHSs reported fewer days of alcohol consumption than those who attended non-RHSs (*Mean* = 2.01, *SD* = 6.82 for RHS students; *Mean* = 5.43, *SD* = 12.33 for non-RHS students). This difference was not statistically significant, however, after adjusting for the propensity score, baseline scores, and referral sites ($b = -2.07$, 95% CI [−6.56, 2.41], $d = -0.23$).

Finally, adolescents who attended RHSs also reported less frequent consumption of other substances at 6-month follow-up (*Mean* = 3.18, *SD* = 11.66 for RHS students; *Mean* = 7.08, *SD* = 20.67 for non-RHS students). This difference was not statistically significant (at the .05 level) in the adjusted regression model ($b = -6.77$, 95% CI [−14.80, 1.27], $d = -0.45$).

Academic outcomes.—At 6-month follow-up, adolescents who attended RHSs reported higher grades in school (*Mean* = 2.81, *SD* = 0.76 for RHS students; *Mean* = 2.29, *SD* = 0.86 for non-RHS students), but this difference was not statistically significant in the adjusted regression model ($b = 0.21$, 95% CI [−0.17, 0.59], $d = 0.26$). Similarly, RHS students also reported lower truancy (skipping school) at follow-up (*Mean* = 2.21, *SD* = 1.40 for RHS students; *Mean* = 2.42, *SD* = 1.20 for non-RHS students), but this difference was not statistically significant in the adjusted regression model ($b = 0.01$, 95% CI [−0.17, 0.59], $d = 0.01$).

Finally, adolescents who attended RHSs reported less overall absenteeism at 6-month follow-up (*Mean* = 4.20, *SD* = 6.29 for RHS students; *Mean* = 7.35, *SD* = 12.93 for non-RHS students), and this difference was statistically significant in the adjusted regression model (*b* = 5.05, 95% CI [−9.55, −0.55], *d* = −0.56). RHS attendance was thus associated with over one-half a standard deviation improvement in absenteeism—an effect equivalent to 5 fewer absences from school over the past 90 days.

Sensitivity analyses.—We conducted post-hoc sensitivity analyses to examine the robustness of findings in Table 2. First we re-estimated the models in Table 2, additionally controlling for students' number of days of SUD treatment and mental health treatment in the past 90 days at the 6-month follow-up. The results for marijuana use were attenuated to non-significance at the .05 level, although the effect size was similar in magnitude (*b* = −12.61, 95% CI [−25.89, 0.66], *d* = −0.45). The results for all other outcomes were similar (in terms of statistical significance and effect size magnitude) to those reported in Table 2. In all outcome models reported in Table 2, RHS attendance was operationally defined as at least 28 days of enrollment in a RHS. Thus, at the 6-month follow-up, some of the students in the intervention condition (*n* = 51) were no longer attending RHSs, and had transferred to other non-RHS settings. Among those still attending RHSs at the 6-month follow-up, the mean number of days attending that RHS since the baseline study period was 159.94 (*SD* = 42.39). We therefore conducted additional sensitivity analyses to examine whether the results were robust, after adjusting for a binary measure indicating whether students were currently enrolled in a RHS at the 6-month follow-up. The results from these analyses were substantively similar to those reported in Table 2, thus providing additional assurances regarding the robustness of the results.

A final sensitivity test was conducted to confirm whether the results obtained were biased due to the supplemental recruitment of students directly from RHSs. Only students recruited directly from SUD treatment programs were included in these sensitivity analyses. Due to the resultant small sample size for RHS students, only an unadjusted comparison of outcomes was possible. The results confirmed the significant effect of RHS attendance on abstinence rates, with 58% of RHS students versus 29% of non-RHS students reporting abstinence at six months ($\chi^2_{(df1)}$ = 6.07, *p* = .014; Fisher's Exact Test, *p* = .022). Differences of means on days of alcohol, marijuana, and other drugs at 6 months all favored the RHS students, but again the contrasts were only statistically significant for the days of marijuana use outcome (*Mean* = 4.58, *SD* = 18.2 for RHS students; *Mean* = 27.6, *SD* = 35.8 for non-RHS students; *p* =.004). These results, including only students who entered RHS after recruitment to the study from treatment programs, thus provide additional assurances that the supplemental recruitment of students directly from RHSs did not unduly bias the evaluation findings.

Discussion

This study examined whether adolescents with SUDs who attend RHSs have improved substance use and academic outcomes, compared to similar adolescents who attend non-recovery-oriented high schools. We used a quasi-experimental design with propensity scores to compare substance use and academic outcomes for 134 adolescents attending RHSs and

60 adolescents in other educational settings. Whereas prior studies of RHSs with positive findings have been limited to descriptive analyses and evaluative studies devoid of comparison groups, the findings of this comparative outcomes study provide evidence of the potentially promising effect of RHSs for adolescents with histories of SUDs.

After balancing the intervention and comparison groups on a range of baseline characteristics included in the propensity scores, the results indicated that adolescents attending RHSs were significantly more likely to report being completely abstinent from alcohol, marijuana, and other drugs at the 6-month follow-up (OR= 4.36, p = .026) and had significantly lower levels of marijuana use (d = −0.51, p = .034) and absenteeism from school (d = −0.56 p = .028). However, we found no differences significant at the .05 level between the groups on days of alcohol use, days of other illicit drug use, school grades, or truancy.

The findings here are consistent with previous (non-controlled) studies of RHSs, thus supporting the promise of RHSs. As in the current study, prior studies have found high levels of abstinence and reduction in cannabis use among RHS students (17, 18, 35, 36); although prior studies have also reported significant reductions in alcohol and other drug use among RHS students, the results from the current study found no evidence of an effect (beneficial or harmful) for these outcomes. One previous non-controlled study also found increased academic growth among RHS students (35), but again, the current study found no evidence of an effect for academic performance outcomes. The discrepant findings between the current study and prior studies are likely due to differences in study methods, however: previous studies used different outcome measures, used different research designs (e.g., retrospective pre-test designs), had larger sample sizes due to anonymous universal surveys of all enrolled students, and had no comparison groups.

Limitations of the current study include the quasi-experimental nature of the design, which was determined to be more feasible and ecologically valid than an experimental design with random assignment. We attempted to address this limitation through the use of propensity scores to balance the intervention and comparison groups on a range of baseline variables. Another limitation to the study was that recruitment yielded a smaller sample size than anticipated, which hindered statistical power to detect effects. Indeed, the original recruitment strategy that identified adolescents at SUD treatment facilities yielded a smaller number of intervention participants than expected, which required additional recruitment efforts at actual RHS sites. We attempted to address this limitation by statistically adjusting all outcome models for the recruitment site of participants, and again, using propensity scores to ensure the intervention and comparison group participants were well-balanced at baseline. We also conducted sensitivity analyses examining outcomes only among those students initially recruited from treatment programs. Although we can only speculate as to why so few adolescents recruited at the SUD treatment facilities eventually enrolled in RHSs, possible reasons include logistical and transportation concerns among parents, fear of stigma, and simply lack of awareness of availability of RHSs. Future research is needed to probe the reasons why adolescents and parents may choose to enroll in RHSs (or not). An additional limitation of the current study is the lack of independent verification of students' reported substance use, given that urinalysis data were not available at the time of analysis to

validate the self-reported outcomes reported here. A final limitation of the study (due to limitations in how data on RHS exposure were collected) was the inability to examine whether different lengths of RHS attendance affected students' outcomes; this will be an important direction for future research.

A key strength of the study includes an innovative quasi-experimental research design, which used propensity scores to balance the RHS and non-RHS groups on a range of potentially important confounding variables. Although use of propensity scores in non-randomized designs has become common, this study was unique in the inclusion of a wide range of important correlates of outcomes selected based on meta-analytic research on adolescent treatment outcomes (30). These correlates were intentionally measured and used in the propensity analysis to balance the RHS and non-RHS groups and thus allow fair comparisons between the intervention and comparison groups.

The findings from the current study thus provide initial evidence of a promising effect of RHSs for adolescents who have received treatment for SUDs, on marijuana, substance use abstinence, and school absenteeism outcomes. In addition, there was no evidence that RHS attendance was associated with any harmful effects on adolescents' substance use and academic outcomes. Despite the promising findings from current and previous studies, however, additional research will be needed to replicate these results on larger and more diverse samples, to examine effects on other academic and social functioning outcomes, and to examine longer term follow-up to assess whether any initial benefits associated with RHS attendance continue over the course of adolescence and into early adulthood. In addition, it will be critical for further research to examine implementation barriers and facilitators, to inform discussions around the sustainability and scaling up of RHSs. Such research will help the field better understand how and why RHS attendance might improve adolescents' outcomes after SUD treatment.

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Table 1

Sample Characteristics – Propensity Score Balanced Sample

	Recovery High School (*n* = 134)		Non-Recovery High School (*n* = 60)		Total Sample (*n* = 194)		
	Mean	**(SD)**	**Mean**	**(SD)**	**Mean**	**(SD)**	**Range**
Linearized propensity score	3.21	2.79	−0.86	1.42	1.95	3.09	−3.22-11.56
Student Demographics							
Age	16.49	(1.01)	16.25	(1.00)	16.41	(1.01)	14-19
Grade in school	11.09	(0.91)	11.00	(0.96)	11.06	(0.92)	9-12
Male (%)	50.00		51.67		50.51		0-100
White (%)	85.07		86.67		85.57		0-100
African-American (%)	7.46		6.67		7.22		0-100
Other race (%)	7.47		6.66		7.21		0-100
Lifetime number of SUD treatment episodes	4.81	(25.82)	1.68	(1.11)	3.85	(21.49)	1-300
Prior outpatient SUD treatment (%)	41.79		33.33		39.18		0-100
Prior intensive outpatient SUD treatment (%)	55.97		60.00		57.22		0-100
Prior inpatient SUD treatment (%)	60.45		46.67		56.19		0-100
Alcohol dependence diagnosis (%)	52.99		48.33		51.55		0-100
Alcohol abuse diagnosis (%)	65.67		55.00		62.37		0-100
Other (non-alcohol) drug dependence diagnosis (%)	86.57		78.33		84.02		0-100
Other drug abuse diagnosis (%)	94.03		91.67		93.30		0-100
Co-morbid mental health condition (%)	95.52		88.33		93.30		0-100
Substance Use (90 days pre-treatment for SUD)							
Days used alcohol	19.26	(25.50)	17.65	(24.32)	18.76	(25.09)	0-91
Days used marijuana	53.58	(35.57)	57.20	(34.38)	54.70	(35.16)	0-91
Days used other drugs	34.16	(36.86)	20.88	(29.47)	30.05	(35.21)	0-91
Abstinence (%)	2.99		0.00		2.06		
Substance Use (past 90 days at 6-month follow-up)							
Days used alcohol	2.01	(6.82)	5.43	(12.33)	3.07	(9.00)	0-91
Days used marijuana	8.84	(22.94)	25.82	(34.90)	14.09	(28.23)	0-91
Days used other drugs	3.18	(11.66)	7.08	(20.67)	4.39	(15.08)	0-91
Abstinence (%)	58.96		30.00		50.00		
Academics (pre-treatment)							
Grades	2.48	(1.01)	2.29	(1.14)	2.42	(1.05)	0-4
Skipping school (truancy)	2.12	(1.29)	2.40	(1.09)	2.21	(1.23)	0-4
Absenteeism	4.98	(8.04)	4.57	(7.65)	4.85	(7.90)	0-54
Academics (6-month follow-up)							
Grades	2.81	(0.76)	2.29	(0.86)	2.65	(0.82)	0-4
Skipping school (truancy)	2.21	(1.40)	2.42	(1.20)	2.27	(1.34)	0-4
Absenteeism	4.20	(6.29)	7.35	(12.93)	5.18	(8.97)	0-54

Note. Means and Standard Deviations (SDs) are for unadjusted baseline data.

Table 2

Effects of Recovery High School Attendance on Adolescents' Substance Use and Academic Outcomes

Outcome	*b*	95% CI	*d*	*OR*
Substance Use				
Days used alcohol	−2.07	[−6.56, 2.41]	−0.23	
Days used marijuana	−14.38*	[−27.70, −1.06]	−0.51	
Days used other drugs	−6.77†	[−14.80, 1.27]	−0.45	
Abstinence	1.47*	[0.18, 2.77]		4.36
Academics				
Grades	0.21	[−0.17, 0.59]	0.26	
Skipping school (truancy)	0.01	[−0.65, 0.67]	0.01	
Absenteeism	5.05*	[−9.55, −0.55]	−0.56	

Notes. *b* = unstandardized regression coefficient indexing difference in means (or log odds ratios) after adjusting for propensity scores and baseline scores. *d* = Cohen's *d* standardized mean difference effect size, estimated as the difference in adjusted means divided by unadjusted pooled standard deviation. OR = odds ratio effect size, adjusted for propensity scores and baseline scores.

† p < .10.

* *p* < .05.