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EMPIRICAL RESEARCH

Learning About Love: A Meta-Analytic Study of Individually-Oriented Relationship Education Programs for Adolescents and Emerging Adults

David M. Simpson[1] · Nathan D. Leonhardt[1] · Alan J. Hawkins[1]

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Abstract Despite recent policy initiatives and substantial federal funding of individually oriented relationship education programs for youth, there have been no meta-analytic reviews of this growing field. This meta-analytic study draws on 17 control-group studies and 13 one-group/pre-post studies to evaluate the effectiveness of relationship education programs on adolescents' and emerging adults' relationship knowledge, attitudes, and skills. Overall, control-group studies produced a medium effect ($d = .36$); one-group/pre-post studies also produced a medium effect ($d = .47$). However, the lack of studies with long-term follow-ups of relationship behaviors in the young adult years is a serious weakness in the field, limiting what we can say about the value of these programs for helping youth achieve their aspirations for healthy romantic relationships and stable marriages.

Keywords Relationship education · Adolescents · Emerging adults · Youth · Meta-analysis

Introduction

Government interest in the societal benefits of stable families and healthy romantic relationships has led policy-makers to allocate more than $1.5 billion since 2005 to support relationship education programs for the purpose of helping at-risk individuals and couples form and sustain healthy relationships and marriages (Hawkins and Van-DenBerghe 2014). Relationship education refers to psychoeducational interventions that aim to provide individuals and couples with knowledge and skills that facilitate forming and maintaining healthy romantic relationships and marriages (Halford 2011). Relationship education covers a wide range of educational efforts, from teaching basic relationship literacy to youth in classroom settings, to helping young unmarried couples strengthen fragile relationships in small-group settings, to providing engaged couples with premarital education, often in religious settings, to enhancing relationship skills of married couples in large-group lecture halls as well as small-group settings (Hawkins et al. 2004). Online relationship education is an increasingly popular delivery mechanism (Georgia et al. 2016). Relationship education generally is considered preventative intervention, although many couples who participate in relationship education already are experiencing significant distress (Bradford et al. 2015). While there are many different approaches and countless different curricula, most relationship education is undergirded conceptually by behaviorism and social learning theory with its emphasis on teaching effective interaction skills that provide positive rewards and minimize costs.

There have been hundreds of studies evaluating the effectiveness of various kinds of relationship education programs on more advantaged samples, and previous meta-analytic studies have consistently documented the potential for modest, positive effects on relationship skills, attitudes, and knowledge of premarital education (Fawcett et al. 2010), relationship enrichment programs for couples in committed relationships (Blanchard et al. 2009; Hawkins et al. 2008), remarried couples (Lucier-Greer and Adler-Baeder 2012), new-parent married couples (Pinquart and Teubert 2010), and even self-guided relationship education

✉ Alan J. Hawkins
hawkinsa@byu.edu

[1] Brigham Young University, School of Family Life, 2086 JFSB, Provo, UT 84602, USA

(McAllister et al. 2012). However, recent studies of the effectiveness of relationship education for disadvantaged populations have produced mixed results so far (Hawkins and Erickson 2015; Randles 2017).

Many of the publicly funded relationship education programs have targeted disadvantaged youth and emerging adults, reaching substantial numbers of them (Scott and Karberg 2015). And scholars are increasingly aware of how important early intervention may be to helping young people avoid harmful relationship pitfalls and achieve their aspirations for healthy, stable families (Hawkins 2017). However, no meta-analysis has yet synthesized research on individually oriented relationship education for adolescents and emerging adults.

Youth and Romantic Relationships

An estimated 80% of youth experience a romantic relationship before the age of 18 (Carver et al. 2003). The prevalence of early romantic relationships is worth noting because early romantic-relationship quality is one of the strongest predictors of individual well-being, including depression, self-esteem, and even attempts at suicide (Brent et al. 1993; Joyner and Udry 2000). Research has also revealed that early romantic involvement serves as a primary context for learning interpersonal behaviors that tend to shape later relational well-being (Collins et al. 2009). Although many youth report having positive learning experiences from their early relationships (Barber and Eccles 2003; Bouchey and Furman 2003), early relational experiences also can be problematic (Bouchey and Furman 2003). Many youth consume media (Roberts 2000) that may provide confusing or even harmful messages about intimate relationships (Ward 2016). Overall, many adolescents and emerging adults may be unprepared for early romantic involvement, which may set them on problematic trajectories for eventual long-term relational success (Willoughby and James 2017).

Most youth and young adults report that marriage remains one of their highest aspirations (Willoughby and James 2017). More than 80% of adolescents expect to get married in their life, and of those who expect to get married, 90% of them expect to be married to the same person throughout their life (Wood et al. 2008). Yet, despite youthful aspirations, young people can struggle to form healthy romantic relationships (Rhoades and Stanley 2014), with more disadvantaged youth at even greater risk for poor relational outcomes (Sawhill 2014). Youthful pitfalls and a lack of relationship literacy (along with disadvantaged economic and social circumstances) help to explain the gap between aspiration and reality (Willoughby et al. 2015). Many scholars have noted that youth in our society receive little effective guidance for navigating the sometimes-dangerous shoals of romantic relationships (Regnerus and Uecker 2011; Smith et al. 2011), and most wish they had more help with this from parents and educators (Weissbourd et al. n.d.)

Lower income youth seem particularly disadvantaged in receiving effective guidance for navigating romantic relationships (Sawhill 2014). Research estimates that 30–40% of emerging adult romantic relationships involve physical relationship violence, with lower income youth experiencing higher levels (Berger et al. 2012). Among low-income young women, higher rates of childhood abuse from caregivers and adolescent romantic partners make it harder for them to form healthy relationships and marriages in adulthood (Burton et al. 2009; Cherlin et al. 2004). Additionally, disadvantaged young women often do not place a priority on obtaining a higher education which seems to play into their willingness to begin families earlier than young women from more advantaged families (Amato and Kane 2011). Social scientists continue to explore why low-income youth are more likely to slide into unstable relationships and have a child before entering a secure long-term relationship (Hymowitz et al. 2013; McKeever and Wolfinger 2011; Sawhill 2014). In turn, these children in unstable relationships are more likely to grow up experiencing economic hardship and are less likely to see examples of stable, healthy relationships, receive relationship guidance from parents, and repeat the cycle of unstable family formation (Conger et al. 2010; Karney and Bradbury 2005). Relationship education programs may be a helpful point of intervention in helping disadvantaged youth escape this cycle.

Youth Relationship Education

Several scholar-practitioners have pioneered efforts to help adolescents and emerging adults bridge the gap between relational aspirations and reality (e.g., Adler-Baeder et al. 2007; Braithwaite and Fincham 2007; Gardner et al. 2004; Whitehead and Pearson 2006). The general focus of youth relationship education (YRE) is to provide youth information on what a healthy relationship looks like (attitudes and knowledge) and to teach them relational skills that may better prepare them to have a healthy relationship. Relationship skills have been a primary focus of most relationship education efforts for committed couples, with communication and problem-solving skills a primary target of evaluation (Blanchard et al. 2009). This is due to a robust literature suggesting that communication patterns and problem-solving behaviors are important factors in sustaining or weakening romantic relationships (Masarik et al. 2016). More recently, scholars have suggested that additional skills such as self-regulation may be critical to improving relationships (Halford 2011; Springer 2013).

The skills frequently evaluated in youth relationship education programs include interpersonal communication, problem-solving and conflict tactics, and self-regulation. These skills usually are assessed generally rather than in the context of a specific relationship.

Attitudes and knowledge about successful relationship practices also influence relational behavior (Kerpelman et al. 2009; Willoughby et al. 2015). Some beliefs and attitudes can lead to behavior that reduces the odds of successful long-term relationships. For example, increases in unrealistic expectations about soulmates (Wilcox and Dew 2010) can create challenges to forming healthy marriages and even may lower chances of marital success (Busby et al. 2010; Wilcox and Dew 2010; Willoughby et al. 2012). Many individually oriented relationship education programs for both adolescents and emerging adults use curriculum that has a focus on what the programs call "unhealthy expectations." They also teach other subject matter in regards to attitudes and knowledge that can help to counter potentially harmful relational patterns. For example, accurate knowledge can help individuals recognize what a healthy relationship looks like, recognize potentially dangerous patterns, make active decisions in relationships rather than passively sliding through relationship transitions (Cottle et al. 2014), and understand physical attraction vs. mature love and smart dating strategies (Adler-Baeder et al. 2007). Youth relationship education also may be able to reduce immature relationship beliefs, such as "love conquers all" (Kerpelman et al. 2009), decrease acceptance of relationship violence (Gardner et al. 2004), and engender more positive views about seeking relationship help in the future (Gardner et al. 2004; Williamson et al. 2014).

Current Study

The purpose of this meta-analysis, then, is to assess the potential effectiveness of relationship education programs at improving the relational skills, attitudes, and knowledge of adolescents and emerging adults. The results of this study are relevant for relationship education practitioners as well as policymakers making decisions about supporting relationship education programming for at-risk populations. As is common practice in meta-analytic studies, we make no specific hypotheses about the effectiveness of these programs.

Method

Inclusion/Exclusion Criteria

To be included in our meta-analysis, a study needed to be a relationship education intervention evaluation focused on *individual* relationship literacy and not on *couple* relationship enhancement. We excluded clinical interventions because clinical interventions often produce larger effects (Shadish and Baldwin 2003), target different outcomes, and interventions are varied and tailored to clients rather than maualized. The average age of the sampled population needed to be between 13 and 29 years old. A few studies had participants younger or older than these parameters, but were included so long as the average sample age was between 13 and 29. A few studies of individually oriented relationship education programs included small numbers of unmarried or married couples, but so long as the curriculum focused on individual relationship literacy and not couple relationship enhancement, we included these studies in the meta-analysis. We did not include intervention studies that focused primarily on sex education. Although these interventions sometimes briefly discuss relationship knowledge, attitudes, and skills, they do not focus on general relationship literacy or skills. And a thorough meta-analysis of sex education programs was recently published (Chin et al. 2012). Similarly, we did not include studies focused on anti-violence or anti-bullying programs for youth, which is a growing and important area of youth programming (De La Rue et al. 2017). Some relationship education programs included content on sex-education, anti-violence, or anti-bullying within the context of relationship literacy; so as long as the program's primary focus was on relationship literacy, we included these studies in our meta-analysis (e.g., Braithwaite and Fincham 2007; Gardner et al. 2016). Many positive youth development programs have been evaluated, but none of the programs reviewed in a meta-analysis of this body of work (Ciocanel et al. 2017) had a focus on romantic relationship literacy. Thus, they were not included in our meta-analysis of youth relationship education programs. Studies outside the United States or in languages other than English were eligible to be included in the meta-analysis, but we found no such studies.

We included studies with various experimental designs. While randomized controlled trial (RCT) designs are considered the gold standard in intervention work, these designs are expensive and hard to implement in the field, leading many evaluators to consider other alternatives, especially early in the program evaluation process (Lipsey and Wilson 2001). For this reason, we also included quasi-experimental studies (with non-randomized control/comparison groups). Because true experimental studies may yield different results than quasi-experimental studies (Shadish and Ragsdale 1996), we tested for differences in effect sizes for these different designs before combining them. In addition, we also coded a significant number of one-group/pre-post studies that have been conducted in the field. However, best practices in meta-analysis call for these studies to be analyzed separately from control-group studies

because they employ a different formula for calculating effect sizes and because they are not strictly comparable to true experiments (Lipsey and Wilson 2001).

We searched for both published and unpublished studies. A concern in meta-analytic studies is publication bias, resulting because studies with significant findings are more likely to be submitted for review and to be published, which can inflate overall effect-size estimates (Card 2015). A thorough search for unpublished studies reduces bias in overall effect sizes because unpublished studies likely have smaller effects (Lipsey and Wilson 2001). When we found both an unpublished report (e.g., a doctoral dissertation) and a published version of the same study, we coded the published article for our analyses. In our final analyses, three unpublished studies were included.

Finally, studies had to provide sufficient data to calculate an effect size. When insufficient data were provided in the study, we attempted to contact authors to retrieve the information. Three authors responded to our requests and provided adequate data to compute an effect size.

Search Procedure

Our search for relevant studies began with searches of online databases: PubMed, Academic Search Premier, PsycINFO, PsycARTICLES, Psychology and Behavioral Sciences Collection, ProQuest Dissertation and Theses databases, and Social Work Abstracts. We searched for years 1975 through 2017. (Rigorous relationship education research began in the mid-1970s; Hawkins et al. 2008.) The following terms along with a search wildcard (*) were used to find relevant articles: relationship education, individual, couple relationship education, and specific, well-known relationship education curricula such as Relationship Smarts. In addition, we personally contacted active researchers in this field for leads on published and unpublished studies.

Through this initial search we identified 691 unduplicated potential studies. Figure 1 is a PRISMA flow-chart (Mother et al. 2009) that outlines our search procedure and study inclusion/exclusion process. In the second step of the review, we screened each study's abstract. This process lead us to remove an additional 634 studies. Next we conducted a full-text assessment on the remaining 57 studies. This full-text assessment lead to the removal of 30 studies for the following reasons: they were conceptual reviews (2) or qualitative-only studies (2); they were not focused on relationship education (1); they focused on couple rather than individually oriented education (8); the average age of the sample was too high (1); they did not assess outcomes of interest to this meta-analysis (12); we could not get adequate information to compute effect sizes (3, Hood 2016; Kilmann et al. 2006; Gardner 2001), and the study was clearly a duplicate sample from another included study (1), so it was merged with that study. (A handful of studies were conducted by the same research team and used samples from the same state, raising concerns about potentially violating the assumption of sample independence in analyses. When concerns about sample independence arose, we corresponded directly with authors to assure samples were independent.) This left 27 studies for coding. However, three articles evaluated two separate programs with independent samples in the same report, generating three additional studies. Accordingly, we coded 30 studies (17 control-group studies and 13 one-group/pre-post studies) for our meta-analysis from 27 reports. Table 1 provides a summary of these included studies and key study characteristics.

Variable Coding

Codebook

A 52-item codebook was created to code study descriptors, outcome variables, and potential moderating variables of the outcomes. (As in common in meta-analytic coding, many potentially valuable moderator variables were infrequently reported in the studies.) Coding was completed independently by the first two authors. Any coding differences were resolved by further checking study text and consulting with the third author. Using this coding procedure, we did not compute inter-coder reliability for this study.

Outcome variables

During our systematic review of relevant studies, three types of outcomes emerged as common across studies: relationship knowledge, attitudes, and skills. (Because these programs teach relationship literacy to individuals, evaluation studies often did not assess relational behaviors, as is common in couple-oriented relationship education.) Outcome variables were coded and placed into one of these three categories. During the coding process, however, it became apparent that it was difficult to distinguish between outcomes that measured attitudes and those that measured knowledge, so we collapsed the two categories into a single category labeled "Attitudes." Common outcomes in the Attitude category included outomes the programs called unrealistic expectations, healthy marital beliefs, and attitudes toward seeing a marriage counselor. The relationship skills category included outcomes primarily focused on effective communication and problem-solving skills. A few studies also assessed avoidance of aggression and violence in dating relationships. To facilitate aggregated analyses

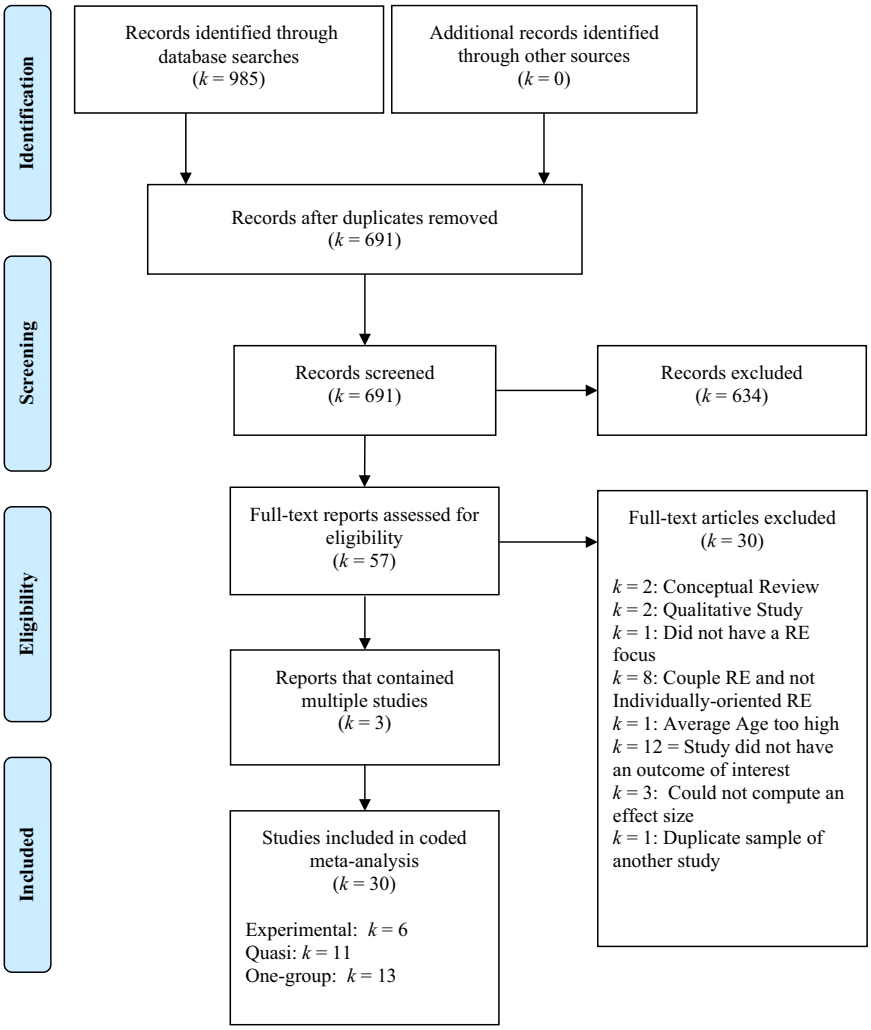

Fig. 1 PRISMA flow-diagram of the study selection process

(that also increased statistical power), all outcomes were coded in the direction that indicated a positive educational outcome.

Moderators

We coded a number of sample, methodological, and programmatic elements in the studies that could potentially moderate overall effect sizes. But due to small numbers of studies and a lack of variation or reporting on these elements in studies, only a handful of potential moderator variables could be tested fairly. We thought it was important to distinguish between programs targeted primarily to adolescents and those targeted to emerging adults. All included studies explicitly reported the average age of their sample (or that the samples were exclusively adolescents or emerging adults). Studies with a sample average age younger than 18 years old were coded as adolescent-targeted programs; studies with a sample average age of 18–29 years were coded as emerging-adult–targeted programs. In addition, there was variation in sample socioeconomic status and race/ethnicity across included studies. There has been a vigorous debate in the field about whether relationship education programs are effective for higher risk, disadvantaged individuals (Hawkins and Erickson 2015; Randles 2017). Socioeconomic status (SES) initially was coded with five categories (low-income/mixed low- and middle-income/middle-income/high-income/unreported) based on indicators such as family income or parental education level. In some cases, explicit information about sample SES was not provided, but we made best efforts to categorize the samples based on other relevant information provided (e.g., government-funded program that targeted lower income individuals). Still, six studies did not provide enough information to categorize confidently sample SES. These six studies were included in overall analyses but were not available for moderation analyses. Categories of race/ethnicity had to be collapsed to non-White/White to increase statistical power in analyses.

Table 1 Study characteristics of the studies selected for the meta-analysis

Author (year)	Design	*N*-T	*N*-C	*d*	Outcome	Age	SES
Adler-Baeder et al. (2007)	QE	235	105	.150	Attitudes	AD	Mixed
				.084	Skills		
Bradford et al. (2016)	QE	682	462	.758***	Attitudes	EA	Lower
				.799***	Skills		
Braithwaite and Fincham (2007)	RCT	31	26	.084	Attitudes	EA	Insufficient
				.311	Skills		
Braithwaite and Fincham (2007)	RCT	29	26	.038	Attitudes	EA	Insufficient
				.343	Skills		
Braithwaite and Fincham (2009)	RCT	38	39	1.332***	Skills	EA	Insufficient
Gardner et al. (2004)	QE	263	147	.510***	Attitudes	AD	Mixed
				.358**	Skills		
Gardner et al. (2016)	QE	141	65	.157	Attitudes	AD	Mixed
				.143	Skills		
Halpern-Meekin (2011)	QE	67	19	.427	Skills	AD	Lower
Holt et al. (2016)	RCT	26	34	.498*	Attitudes	EA	Mixed
				.120	Skills		
Holt et al. (2016)	RCT	26	34	.342	Attitudes	EA	Mixed
				.000	Skills		
Johnson (2009)	QE	47	61	.253	Attitudes	EA	Mixed
Kerpelman et al. (2010)	RCT	767	635	.361***	Attitudes	AD	Mixed
				.059	Skills		
Ma et al. (2014)	QE	874	729	.158**	Skills	EA	Mixed
Polanchek (2014)	QE	131	120	.469***	Attitudes	EA	Insufficient
Schramm and Gomez-Scott (2012)	QE	426	197	.158	Attitudes	AD	Insufficient
				.240**	Skills		
Sharp and Ganong (2000)	QE	108	71	.435**	Attitudes	EA	Lower
Van Epp et al. (2008)	QE	120	144	.418**	Attitudes	EA	Mixed
Antle et al. (2011)	1-P/P	202	–	.154*	Attitudes	AD	Lower
				.241**	Skills		
Bradford et al. (2014)	1-P/P	1686	–	.181***	Attitudes	AD	Lower
Chan et al. (2016)	1-P/P	6984	–	.915***	Skills	AD	Lower
Cottle et al. (2014)	1-P/P	186	–	.630***	Attitudes	EA	Insufficient
				1.324***	Skills		
DuPree et al. (2016)	1-P/P	152	–	.640***	Skills	EA	Mixed
Futris et al. (2013)	1-P/P	524	–	1.033**	Attitudes	AD	Lower
				1.095***	Skills		
Halpern-Meekin (2011)	1-P/P	136	–	.095	Skills	AD	Lower
Kerpelman et al. (2009)	1-P/P	1423	–	.182***	Attitudes	AD	Lower
				.094**	Skills		
Lloyd et al. (2015)	1-P/P	211	–	.739***	Attitudes	EA	Mixed
McElwain et al. (2016)	1-P/P	1005	–	.082*	Attitudes	AD	Lower
				.143***	Skills		
Rice et. al. (2017)	1-P/P	3658	–	.901***	Attitudes	AD	Lower
				.669***	Skills		
Sparks et al. (2012)	1-P/P	114	–	.004	Attitudes	AD	Mixed
Springer (2013)	1-P/P	94	–	.178	Skills	EA	Mixed

N-T is the sample size for treatment group; N-C is the samples size for control group; Mixed is the lower and middle class sample; Insufficient is the insufficient data to record membership

RCT randomized control trial, *QE* quasi-experimental, 1-P/P = One-group/pre-post, *AD* adolescents, *EA* emerging adults

Effect sizes are significant at * $p < .05$; ** $p < .01$; *** $p < .001$

Computing and Reporting of Effect Sizes

We employed Biostats' Comprehensive Meta-Analysis III to conduct analyses. We used random effects models to calculate effect sizes, which allow for the possibility that differences in effect sizes from study to study are associated not only with participant-level sampling error but also with variations in study methods and program features, facilitating generalization of results (Lipsey and Wilson 2001). Each study effect size was weighted by the inverse variance (squared standard error) to account for differential precision in estimates of effects. For control-group studies, standardized mean group difference effect sizes were calculated. Standardized gain score effect sizes were calculated for one-group/pre-post studies (analyzed separately). Calculating the standardized gain score requires knowing the correlation between the pre- and post-test scores of outcome variables. Few studies reported the pre/post correlation of outcome variables, so a $r = .50$ correlation was imputed for studies with missing correlations. Previous research suggests a pre-post correlation of .50 is a reasonable estimate of the true correlation (Lipsey and Wilson 2001; Nowak and Heinrichs 2008). In addition, we conducted a sensitivity analysis using $r = .60$; resulting effect size estimates were virtually unchanged.

All effect sizes come from immediate (or within 1–2 months) post-test assessments. A few studies ($k = 5$ attempted longer-term follow-up assessments, but high sample attrition rates (>50%) were common in these studies, potentially biasing results. So a fair test of longer-term effects of youth relationship education studies was not feasible.

Results

Key Study Characteristics

Half of the studies ($k = 15$) had samples with adolescents (ages <18, average age of samples = 15.8); the other half had samples of emerging adults (ages 18–29, average age of samples = 20.8). Most of the studies of emerging adult programs had samples of college students taking a course for credit; it was common for studies with adolescent samples to have students taking a high school course. The most common youth relationship education curricula studied were Relationship Smarts ($k = 11$, 37%) and PREP/Connections ($k = 7$, 23%).

Six studies did not report information on sample SES. But a little more than half of the studies ($k = 13$) that reported sample SES had mixed low-income/middle-income youth, while the remaining studies ($k = 11$) had primarily lower income youth; there were no studies with primarily higher income youth. Four studies (13%) did not report on the race/ethnicity of their samples. Of those studies that reported sample race/ethnicity, most ($k = 18$, 69%) had samples with more than one third non-White participants.

Preliminary Analyses

Standardized mean difference effect size vs. standardized mean difference of gain score effect size

All but two control-group studies reported both pre-test and post-test scores. For RCT studies, comparing post-test treatment scores and control-group scores yields an unbiased estimate of the true effect size. However, 11 control-group studies were not able to randomly assign participants to groups. If there were pre-test group differences (which has been observed in other meta-analytic studies of relationship education programs; Hawkins et al. 2008), these studies can produce biased effects. However, the overall program effect sizes estimated with the standardized mean difference effect size and the standardized mean difference of gain score effect size were virtually identical. Thus, we were comfortable in reporting the traditional standardized mean difference effect sizes for control-group studies.

RCT studies vs. quasi-experimental studies

In addition, there was not a statistically significant difference in the overall standardized mean difference effect sizes between RCT studies and non-randomized control-group studies. Thus, we combined these studies to increase statistical power in our analyses.

Publication bias/missing studies

A direct test for publication bias was not feasible because only three studies included in our meta-analysis were unpublished (two control-group studies and one one-group/pre-post study). Tests for evidence of missing studies were conducted using adjusted funnel plots and Duval and Tweedie's trim and fill method. Results (for both control-group and one-group/pre-post studies) suggested no evidence of missing studies.

Adolescent vs. emerging adult studies

Because most studies with adolescent samples involved many disadvantaged youth, while most studies with emerging adult samples involved enrolled college students, who likely were more advantaged, we report both the disaggregated effect sizes for adolescent and emerging adult samples as well as the aggregated effect sizes to deal better

with the potential confound of these two study characteristics in analyses.

Retrospective vs. standard pre-post studies

Three of 13 one-group/pre-post studies employed a retrospective-pre-post design in which participants at posttest reported on their current level of an outcome but also retrospectively reported on their level of this variable before taking the program. Some evaluators argue that this design can minimize response-shift bias that occurs when participants are inclined to overestimate their knowledge or skills before receiving an educational intervention (Duncan and Goddard 2017). Sensitive to this issue, we checked to see if studies with retrospective-pre-post designs were significantly different than studies with standard-pre-post designs and indeed they were substantially larger (retrospective pre-post $d = .89$, $p < .001$, $k = 3$; standard pre-post $d = .33$, $p < .001$, $k = 10$; $Q(1) = 39.1$, $p < .001$). Space limitations do not permit an in-depth analysis of the complex question of which method—standard pre-post or retrospective pre-post—is a better estimate of the true effect size. However, we tend to believe that the former method underestimates effect sizes in these kinds of interventions while the latter tends to overestimate them. So, the aggregated effect size actually may be a better estimate. Based on this logic, we present the aggregated effect sizes for one-group/pre-post studies, which in each case is a value higher than for standard pre-post studies and lower than for retrospective pre-post studies.

Primary Analyses: Experimentally Designed Studies

Results of our primary analyses with control-group studies are found in Table 2. We follow the standard practice of referring to studies with effects ≤.20 as small, between .20 and .79 as medium, and ≥.80 as large (Cohen 1988). Findings indicate youth relationship education programs have a moderate overall effect size ($d = .36$, $p < .001$, $k = 17$; $Q = 91.92$, $p < .001$). The effect sizes for Attitudes ($d = .36$, $p < .001$, $k = 14$) and Skills ($d = .32$, $p = .01$, $k = 13$) were similar. The Q test for heterogeneity in the distribution of effect sizes indicated considerable systematic heterogeneity, but we were only able to investigate a few moderators to explore this heterogeneity due to study coding limitations. First, although the effect size for emerging adult programs was greater than for the effect size for adolescent programs, this difference was not quite significant ($Q(1) = 3.22$, $p = .073$, $k = 17$). Second, there was evidence of moderation by SES ($Q(1) = 7.10$, $p = .008$); studies with lower income samples ($d = .60$, $p < .001$, $k = 3$) had larger effect sizes than did samples with mixed low/middle-income samples ($d = .23$, $p < .001$, $k = 9$). There was no significant moderation for race/ethnicity ($Q(2) = 3.93$, *ns*, $k = 15$).

Table 2 Youth relationship education program outcome effect sizes for control-group and one-group/prepost studies, by age group

	Control-group ($k = 17$)			One-group/Pre-post ($k = 13$)		
Outcome	k	d	Q^a	k	d	Q^a
Overall program	17	.36***	91.92***	13	.47***	1466.02***
Adolescents	6	.23***		9	.39**	
Emerging adults	11	.44***		4	.64**	
Attitudes	14	.36***	53.48***	9	.43**	1038.64***
Adolescents	5	.28***		7	.36*	
Emerging adults	9	.42***		2	.69***	
Skills	13	.32**	114.53***	10	.53***	999.94***
Adolescents	6	.18**		7	.46**	
Emerging adults	7	.44**		3	.71*	

*$p < .05$, **$p < .01$, ***$p < .001$

[a] Test for heterogeneity of effect size distribution

Supplemental Analyses: One-group/Pre-Post Studies

While one-group/pre-post studies are more susceptible to threats to internal validity, they may have results with greater external validity when studies are conducted in the field, and they can supplement what we can learn from more controlled studies with higher levels of internal validity. Similar to control-group studies, these one-group/pre-post studies produced a medium overall effect size ($d = .47$, $p < .001$, $k = 13$; $Q = 1466.02$, $p < .001$). The effect size for Attitudes ($d = .43$, $p < .01$, $k = 9$) was slightly smaller than for Skills ($d = .53$, $p < .001$, $k = 10$). The Q test for heterogeneity in the distribution of effect sizes indicated considerable systematic heterogeneity, but again, we were only able to investigate a few moderators to explore this heterogeneity due to study coding limitations. First, paralleling control-group studies, the effect size for emerging adult programs was larger than the effect size for adolescent programs, but this difference was not statistically significant ($Q(1) = 1.50$, *ns*, $k = 17$). Second, in contrast to control-group studies, the difference between studies with lower income samples and those with mixed lower/middle-income samples was not significant ($Q(1) = .49$, *ns*, $k = 12$); There was no significant moderation for race/ethnicity ($Q(1) = .09$, *ns*, $k = 11$).

Discussion

There is a large body of literature that documents *couple* relationship education programs have significant moderate

impacts, with a handful of meta-analytic summaries (see Hawkins 2015). This study is the first to synthesize the emerging field of *individually oriented* relationship education programs targeting adolescents and emerging adults. Most of today's youth have aspirations for healthy relationships yet many fall short, often with negative consequences for themselves and their children. Relationship education programs targeted towards adolescents and emergieng adults may help set youth on a path towards future healthy relationships, but only if programs are having a positive impact.

The results of our study are relatively straightforward and reveal reasons for both optimism and concern. First, studies of youth relationship education programs ($k = 30$) yielded significant medium effect sizes (control-group-studies $d = .36$; one-group/pre-post-studies $d = .47$), including effects for attitudes and skills. These medium effect sizes are comparable to those found in meta-analytic studies of relationship education programs for committed couples (Blanchard et al. 2009; Fawcett et al. 2010; Hawkins et al. 2008; Lucier-Greer and Adler-Baeder 2012; Pinquart and Teubert 2010). The medium effect size $d = .36$ for control-group studies is larger than effect sizes found in a recent meta-analysis of RCT studies of positive youth development programs (behavioral outcomes $d = .04$; psychological outcomes $d = .17$) (Ciocanel et al. 2017). However, this difference could be the result of differences in outcomes measured in the two reviews—behavioral and psychological vs. attitudes and skills acquisition—as well as other methodological differences and less a result of overall program effectiveness.

In addition, we found that studies with emerging adult individuals produced somewhat larger effects than studies with adolescents, although these differences were not quite statistically significant, so we hesitate to speculate much on this difference. It is possible that romantic relationships are more salient and proximate for emerging adults, so that they are more attuned to and invested in youth relationship education programs than adolescents. In addition, similar to the positive youth development meta-analysis findings (Ciocanel et al. 2017), we found evidence among control-group studies that youth relationship education studies with lower income samples produced larger effects than those with mixed lower/middle income samples (although this was not replicated with the one-group/pre-post studies). A good number of studies now have found that more disadvantaged participants experience greater gains from relationship education programs than more advantaged participants (for a summary, see Hawkins et al. 2017).

These findings are encouraging considering the greater risks disadvantaged youth face for healthy relationship formation (e.g., Conger et al. 2010; Hymowitz et al. 2013). Our study provides some hope that youth relationship education could help disavantaged youth escape the cycle of unstable, unhealthy relationships leading to poor outcomes for the next generation. Perhaps youth from disadvantaged backgrounds benefit more from these programs because they provide them a positive vision of relationships (e.g., Adler-Baeder et al. 2007; Braithwaite and Fincham 2007) beyond what many may see in their everyday lives (e.g., Conger et al. 2010; Karney and Bradbury 2005; Hymowitz et al. 2013), a vision that more advantaged youth take for granted. Perhaps in learning about healthy, stable relationships, they have a heightened awareness (and more accurate understanding) of the gap between the relationships they aspire to (Willoughby et al. 2015; Wood et al. 2008) and the reality of so many relationships in their social sphere (Conger et al. 2010; Karney and Bradbury 2005; Hymowitz et al. 2013). In addition, these programs may heighten their sense of efficacy or ability through skills acquisition to achieve their relational aspirations, a crucial component of change (Snyder 2002).

Program administrators and policymakers who support relationship education programs targeted to disadvantaged youth can be encouraged by these findings. Still, there is a daunting distance between the potential effectiveness of specific youth relationship education programs (documented in this meta-analytic study) and the effectiveness of policy initiatives designed to promote these programs. Translating program potential into policy success to benefit a needy population is a long and hard process (Haskins and Margolis 2015). Moreover, there is a glaring weakness in the body of evaluation research on youth relationship education programs that must constrain optimism for now: a lack of evidence from longer term follow-up assessments of behavioral outcomes. Until we can demonstrate that immediate intervention gains in attitudes and skills are sustained and translated into positive outcomes in healthy relationship formation behaviors in early adulthood, we cannot make a strong case for the value of youth relationship educaiton programs. And from a policy standpoint, we also need to demonstrate an ability to disseminate programs widely that achieve similar effects.

Thus, large challenges lie ahead. Of course, the studies in this field have been done with youth, and it is challenging and expensive to track mobile youth over several years. Some researchers in this area have attempted to do so but experienced high rates of attrition (75%+; Kerpelman et al. 2009; Kerpelman et al. 2010). Perhaps it is worth noting that these studies did yield positive results at 1-year post-intervention, but the possibility of substantial bias from high attrition rates limits confidence in these results. Moreover, 1 year still is not a long enough period of time to gauge intervention impact on healthy relationship formation behavior. The federal Administration for Children and Families (ACF) has funded two expensive, rigorous,

multi-site, RCT evaluation studies of relationship education programs for married and unmarried *couples* with children (Lundquist et al. 2014; Williamson et al. 2016; Wood et al. 2014) and is continuing to evaluate more couple education programs, but ACF has yet to fund such a rigorous study of youth relationship education. Given the expense and skill (and patience) needed to conduct this kind of challenging research, ACF would be best suited to fund this crucial research. Given how many youth today struggle to achieve their aspirations for healthy romantic relationships and stable marriages, and the personal and societal costs of these struggles (Sawhill 2014; Scafidi 2008), the time is ripe for this research to buttress policy efforts.

Our study has both strengths and weaknesses. We were able to synthesize a growing body of research on the effectiveness of relationship education for youth. Through a thorough search process and employing best practices for meta-analytic research methods, we identified a population of 17 control-group studies yielding a moderate effect size ($d = .36$). Moreover, we also identified and analyzed 13 1-group/pre-post youth relationship education studies that together echo the moderate effect size finding for control-group studies ($d = .47$). Yet there are important limitations to our study. Due to a relatively small number of studies (17 control-group, 13 1-group/pre-post) and inconsistent reporting across studies, we were limited in our efforts to identify moderators of the significant heterogeneity of effect sizes evident in our analyses. This weakened our ability to understand why some programs may be more effective than others, an important contribution of meta-analytic research. In addition, six studies did not provide sufficient information to code sample SES. This missing information could have affected our moderation analyses of SES. Researchers should be more consistent reporting SES.

Conclusion

Our synthesis of an emerging body of research on the effectiveness of youth relationship education programs yields some encouraging signs, namely that programs are showing the ability to impact relationship knowledge, attitudes, and skills, at least in the short-run. Moreover, disadvantaged youth appear to benefit the most from these programs. This is an important finding given that these youth are at greater risk for unhealthy and unstable realtionships, and many of them have negative experiences in romantic relationships with long-term consequences (Bouchey and Furman 2003; Smith et al. 2011). Developmental research shows how early romantic involvement serves as a primary context for learning interpersonal behaviors that tend to shape later relational well-being (Collins et al. 2009). Accordingly, youth relationship education programs may serve a real need of contemporary young people, helping them achieve their aspirations for healthy relationships and stable families (Willoughby and James 2017; Wood et al. 2008). While it is too early to know the long-term impact of these programs, the early positive findings provide a rationale for continued efforts and more rigorous long-term evaluation.

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David Michael Simpson is a Ph.D. student at Brigham Young University. His research interests include program evaluation and latent variable structural equation modeling.

Nathan D. Leonhardt is a M.S. in Marriage Family and Human Development at Brigham Young University. His research focuses on bridging the gap between aspiration and reality for long-term marital/sexual relationships.

Alan J. Hawkins is the Camilla E. Kimball Chair and Professor of Family Life at Brigham Young University. His research focuses on educational and policy efforts to help couples form and sustain healthy relationships and marriages.