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Can emotional intelligence be trained? A meta-analytical investigation☆

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ABSTRACT

Human resource practitioners place value on selecting and training a more emotionally intelligent workforce. Despite this, research has yet to systematically investigate whether emotional intelligence can in fact be trained. This study addresses this question by conducting a meta-analysis to assess the effect of training on emotional intelligence, and whether effects are moderated by substantive and methodological moderators. We identified a total of 58 published and unpublished studies that included an emotional intelligence training program using either a pre-post or treatment-control design. We calculated Cohen's d to estimate the effect of formal training on emotional intelligence scores. The results showed a moderate positive effect for training, regardless of design. Effect sizes were larger for published studies than dissertations. Effect sizes were relatively robust over gender of participants, and type of EI measure (ability v. mixedmodel). Further, our effect sizes are in line with other meta-analytic studies of competency-based training programs. Implications for practice and future research on EI training are discussed.

1. Introduction

Emotional intelligence (EI) refers broadly to refers skills and/or abilities that enable awareness of the emotional states of oneself and others and the capacity to regulate or use emotions to positively affect role performance. As noted recently by Joseph, Jin, Newman, and O'Boyle (2015), since its introduction in the popular media by Goleman (1995), EI has garnered considerable attention in both mainstream culture and the business world. It "is currently considered a widely accepted practitioner tool for hiring, training, leadership development, and team building by the business community" (pg. 298). A *Fast Company* article describes Google's "insanely popular emotional intelligence course" (Giang, 2015), business blogs attribute corporate performance to the EI of its leaders (e.g., Conley, 2011), and the consulting group TalentSmart claims that 75% of Fortune 500 companies use its EI products or services.

Despite debates over emotional intelligence as a legitimate construct (see Antonakis, Ashkanasy, & Dasborough, 2009; Locke, 2005; Mayer, Salovey, & Caruso, 2008), human resource practitioners spend considerable resources selecting and training a more emotionally intelligent workforce (Fineman, 2004; Nafukho & Muyia, 2014). A December 2017 web search revealed over 200 vendors providing various forms of EI coaching or training, including those offered by large professional organizations like the American Management Association and the Association for Talent Development. Further, emotional intelligence training is increasingly being integrated into MBA training, even in prestigious universities such as Yale (Di Meglio, 2013). Although organizations and universities are investing time and money into training programs that promise to increase EI of employees and organizational

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leaders, research has yet to systematically investigate whether we successfully can train adults to be more emotionally intelligent. The primary purpose of this study addresses this gap by conducting two meta-analyses to determine the effect of training on emotional intelligence. Although prior meta-analyses have examined the relationship between EI and leadership, job performance, and health (Harms & Credé, 2010; Joseph & Newman, 2010; Joseph et al., 2015; Martins, Ramalho, & Morin, 2010; O'Boyle, Humphrey, Pollack, Hawver, & Story, 2011), this is the first time meta-analytical techniques have been used to understand the effects of formal training on EI. Our investigation addresses whether emotional intelligence can be trained, establishes an expected effect size for organizations and institutions looking to adapt EI training, and lays a groundwork for answering other practical questions about the best ways to improve EI in the workforce. Thus, our study makes an important contribution to the emotional intelligence literature by informing researchers and HR practitioners as to whether resources used to implement EI training programs are indeed well-spent.

Emotional intelligence was first introduced as a construct over 25 years ago (Salovey & Mayer, 1990) and has been popularized as a means to predict performance beyond standard measures of general intelligence (Goleman, 1995). It is worth noting that the EI literature is largely split between two primary models of the construct, either ability-based or mixed-model. Because this distinction has some implications for the potential impact of training and estimates of effect sizes, we review this distinction here. Ability-based EI is "the ability to engage in sophisticated information processing about one's own and others' emotions and the ability to use this information as a guide to thinking and behavior" (Mayer et al., 2008). Researchers in the EI-as-ability camp largely agree that emotional intelligence is the accumulation of behaviors and abilities that contribute to an individual's success at recognizing and managing the emotions of oneself and others. Mixed-model conceptualizations of EI, on the other hand, include a wide "constellation of noncognitive competencies that lead to successful coping under difficult situations…[and] incorporate motivation, personality, temperament, character, and social skills" above and beyond emotional recognition and management (Cho, Drasgow, & Cao, 2015). The breadth of this latter treatment may also be a shortcoming; mixed-models of EI are often criticized as being too all-encompassing, a kitchen sink of any construct tangentially related to emotional performance, often resulting in a lack of scientific rigor for validating these models (Joseph & Newman, 2010). The argument against ability measures is similar – whether EI can be considered a set of fundamental mental abilities distinct from existing constructs (e.g., Davies, Stankov, & Roberts, 1998; MacCann, Joseph, Newman, & Roberts, 2014).

While there is a distinction in the literature (and in measurement) between ability and mixed model treatments of emotional intelligence, the construct space is even messier. Ability definitions often reference behaviors (a type of skill), while mixed-model definitions often mention abilities "in the mix.". Thus, the current state of the literature may be better characterized as a continuum of more ability-based to more mixed-model-based. Because we will examine separate effect sizes for each approach, we distinguish between the two. We will refer to EI as either primarily *ability*-based (implying that it is an individual difference variable) or *mixed-model* (which may include abilities, but may also include other competencies that are acquired through experience). In our meta-analyses, we investigate whether the effects of training differ depending on the EI model, although we acknowledge that ability/mixed-model conceptual distinction is confounded with differences in how each construct type is measured.

Within both approaches, EI is considered an important determinant of workplace competencies such as reading the emotions of one's self or others, engaging in emotional regulation, self-awareness, and relationship management (Druskat, Sala, & Mount, 2006), and then in turn is related to effective performance in domains such as leadership, customer-service, or work teams (George, 2000; Daus & Ashkanasy, 2005; Druskat et al., 2006). Both the conceptual argument that links EI to work outcomes and empirical evidence of such relationships (Harms & Credé, 2010; Joseph & Newman, 2010; Joseph et al., 2015; Martins et al., 2010; O'Boyle et al., 2011) make the case that *developing* EI should be of interest to organizations.

There are specific reasons why organizations may seek to *improve* the emotional intelligence of its workforce. First, skills generally nested under EI constructed definitions (e.g., self-awareness and emotional regulation) are important for essential to many jobs (e.g. nurses and managers). Accordingly, needs assessments may target the value in enhancing these skills. Second, EI is so commonly thought of, particularly in the popular press and popular management literature, as something that is "good," that it organizations are likely to find ways of developing it in their workforces in any way possible. For example, Slaski and Cartwright (2003) tested the efficacy of a EI training program, hypothesizing that increased EI could also increase one's health, well-being, and performance. Other empirical studies that investigated the trainability of EI linked increased emotional intelligence with enhanced project management, sales performance, and leadership effectiveness (Cherniss, Grimm, & Liautaud, 2010; Clarke, 2010b; Gignac, Harmer, Jennings, & Palmer, 2012). As opposed to increasing a workforce's emotional intelligence via more long-term or costly talent management approaches (e.g., selection processes) EI training programs could provide a more immediate benefit to organizations by increasing EI among incumbent employees and managers. Thus, cumulative evidence that EI can be improved via training would argue for its use by organizations seeking to improve performance and affective outcomes for employees and managers.

Accordingly, we ask the empirical question as to whether emotional intelligence can be trained, and answer it by meta-analyzing studies that attempt to increase EI via training interventions. We first review prior research on direct efforts to improve EI and make the case that a meta-analysis is the best way to address the question of the trainability. We then propose several potential moderators of the relationship between training and changes in EI.

1.1. Interventions to enhance emotional intelligence

Other researchers have considered and investigated explicitly whether EI can be improved through training, albeit not at the meta-analytical level. Schutte, Malouff, and Thorsteinsson (2013) noted an increase in intervention studies targeting the emotional intelligence of participants in multiple domains including athletics, education, and organizations. EI interventions used in educational contexts are typically integrated into general undergraduate or management classes or provided to professional students entering jobs characterized by high emotional labor (Glomb & Tews, 2004). As an example of the latter, Vesely, Saklofske, and Nordstokke (2014) administered a five-week training program to 23 undergraduate teacher candidates at two Canadian universities. During the training, participants received 1.5 h of training per week, consisting of workshops, group discussion, workbook exercises, and homework assignments. Participants showed greater pre-test to post-test differences on an EI measure than did control group participants, although the differences were not significant.

With respect to workplace training interventions, researchers often target jobs perceived as high in stress or emotional labor such as police officers, nurses, or managers. In one such study, Slaski and Cartwright (2003) assigned managers to either emotional intelligence training for one day per week for four weeks or to a control group. Paired sample t-tests showed a significant increase in a trait-based measure of EI for trained participants, but not for control group participants. Al-Faouri, Al-Ali, and Al-Shorman (2014) delivered EI training to 70 nurses in a teaching hospital for two hours per day for seven weeks. The researchers found not only an increase in nurses' EI scores, but also in their job satisfaction. In contrast, Muyia and Kacirek (2009) tested the effectiveness of an EI training programs for emerging leaders, consisting of three three-day sessions over nine months. Training consisted of lectures, case studies, feedback, and homework. Despite administering a lengthy, theoretically-driven development program, the researchers saw no significant changes in an EI measure from pre- to post-test.

As with any area of psychology, qualitative reviews on an intervention can yield positive, negative, or null effects. To provide an "initial estimate of the effect of emotional intelligence training across different populations" Schutte et al. (2013) conducted a small meta-analysis of "intervention studies using true random assignment of participants to intervention and control conditions, and which had at least 50% retention of participants from pre to post assessment" (p. 61). This resulted in six effect sizes across four studies. The researchers reported a moderate, positive effect size (Hedges'$g = 0.46$), suggesting EI can be trained. However, it should be clear that this was not a comprehensive meta-analysis, as the authors deliberately excluded a number of potentially informative study designs (e.g., quasi-experimental and/or pre-post designs). Accordingly, there is a need for a more thorough meta-analysis. A meta-analysis can provide an estimate of not only the overall effect of training, but examine the impact of methodological or substantive moderators.

In the current investigation, meta-analytic methods were used to quantitatively summarize empirical research assessing the effect of training on increasing emotional intelligence among adults. EI training was operationalized as any systematic training program (with adults) conducted with the purpose of increasing EI. An overall effect size of training on EI was obtained to answer the question: Can emotional intelligence be trained?

We expected to find a positive effect for EI training. This is based in part on prior field studies which have found EI training to be effective (e.g., (Al-Faouri et al., 2014; Slaski & Cartwright, 2003; Vesely et al., 2014). More generally, other meta-analyses consistently report positive effects on training outcomes (see Salas, Tannenbaum, Kraiger, & Smith-Jentsch, 2012). While we have yet to achieve consensus on what constitutes emotional intelligence, prior meta-analyses have shown moderate to strong effects for training on participant attitudes (Taylor, Russ-Eft, & Chan, 2005); domain-specific knowledge (Arthur, Bennett Jr., Edens, & Bell, 2003; Burke & Day, 1986; Collins & Holton, 2004; Powell & Yalcin, 2010) and resulting behavior (Arthur et al., 2003; Burke & Day, 1986; Powell & Yalcin, 2010). Thus, we predicted:

Hypothesis 1. There will be a positive effect of training on emotional intelligence.

In addition to this main effect, we examined several potential conceptual and empirical moderators of the effectiveness of EI training. We introduce these moderators below.

1.2. Ability v. Mixed-model measures as a moderator

We have noted the distinction between ability and mixed-model conceptualizations of EI. Logically, the underlying conceptual orientation of the researchers (ability v. mixed-model) should be related to the EI measure used to assess training outcomes (e.g., an ability construct focus would use an ability-based EI measure). Further, researchers' conceptual orientation should drive the training design. Therefore, examining the choice of EI measure would allow us to determine whether an ability-based or mixed-model-based approach has a greater impact on training outcomes. Explicitly, we wondered whether the effectiveness of training for EI depends on whether the construct is conceptualized (and trained) as a more ability-based or a mixed-model, with the latter more likely to include skills or competencies?

Interventions that assume EI as an ability may be structured differently than those which take a mixed-model approach. It is worth noting that other non-physical abilities can be modified via training. For example, working memory can be enhanced through training (Au et al., 2015), as can GRE scores (e.g., Mrazek, Franklin, Phillips, Baird, & Schooler, 2013) and standard IQ measures (Nisbett et al., 2012). On the other hand, EI-related competencies and attitudes such as coping, social skills, and motivation are exactly those that are considered primary outcomes of training interventions (Kraiger, Ford, & Salas, 1993; Salas et al., 2012). As we discuss below, most studies we analyzed do not provide sufficient detail to determine precisely *how* the training was structured, or whether training looked different depending upon underlying assumptions about EI. However, principles of effective training design call for different instructional methods depending on whether the learning objectives target abilities or skills and knowledge (Jonassen & Tessmer, 1996/1997).

Interestingly, neither emotional intelligence model assumes that adults can be taught to be more emotionally intelligent. Mayer and Cobb (2000) did not support the concept of teaching emotional intelligence because, conceptualized as an ability which is primarily shaped by genetics and how one was raised, EI merely provides people with a *capacity* to learn emotional competence. Somewhat paradoxically, the same authors do support the notion of teaching emotional knowledge and fostering emotional reasoning via social-emotional learning as well as through engagement with liberal and creative arts (Mayer & Cobb, 2000). As for mixed-models, while proponents of this EI camp emphasize the importance of competencies and skills, they also tend to conceptualize the construct as an individual difference that like a stable trait or disposition (Côté, 2014), which should therefore not be malleable to training interventions.

EI at times is conceptualized as a set of skills "concerned with the processing of emotion-relevant information" (e.g. Mayer et al., 2008; Mayer, Salovey, Caruso, & Sitarenios, 2003), even among ability-model proponents. Here, EI can be conceptualized as being able to analyze and respond to emotionally-laden situations (Nelis, Quoidbach, Mikolajczak, & Hansenne, 2009). The distinction between abilities and skills is important, as skills imply improvement potential via both maturation and interventions, while ability does not necessarily assume this malleability.

Accordingly, it is interesting to explore whether effect sizes vary depending on the type of EI measure. The moderator analysis of EI measure on the effect of training will be exploratory in nature. We do not know whether the ability or mixed-model approach is more valid (or more useful for training design), so we cannot determine a priori which training approach would be more effective. Thus, we do not hypothesize the direction of magnitude of a moderating effect of ability- or mixed-model measures on the effect of training on emotional intelligence. Thus, we also explored the following research question:

Research Question 1: Do effect sizes differ depending on the whether an ability or mixed model EI measure is used?

1.3. Publication status as a moderator

To investigate the potential publication bias that occurs when only meta-analyzing peer-reviewed journal articles (see Rothstein, Sutton, & Borenstein, 2005), we included dissertations and unpublished reports in addition to studies found in scientific journals. Because of the tendency for researchers to publish significant results with robust effect sizes and not publish null findings (Franco, Malhotra, & Simonovits, 2014), we predicted that:

Hypothesis 2. There will be a stronger effect of training on EI for studies that were published in a peer-reviewed journal compared to non-published studies.

1.4. Gender as a moderator

Women are commonly assessed as having higher EI than men (e.g., Goldenberg, Matheson, & Mantler, 2006; Schutte et al., 1998; Van Rooy, Alonso, & Viswesvaran, 2005). Accordingly, it might be anticipated that if EI is affected by training, the same training program should have a greater effect for men (who start out lower on average) than for women. By analogy, when there are differences in strength training reported for men v. women, it is the women (who start out on average lower) who benefit more (Ivey et al., 2000). The potential for the same training program to have differential impact for groups differing on an individual difference variable is a form of a selection-regression threat to internal validity (Shadish, Cook, & Campbell, 2002). In the extreme, groups scoring higher on a pre-test would show no change from the intervention; groups scoring lower would show positive gains (Campbell & Boruch, 1975).

Different investigations of whether gender moderates training effectiveness have reached different conclusions. For example, Scott, Leritz, and Mumford (2004) examined gender as a potential moderator of the effectiveness of creativity training by coding whether trainees were predominantly male, females, or split evenly. The researchers found a significant effect for gender mix, with training more effective for predominantly male samples. They attributed this finding to males' greater propensity to risk-taking, but it could also be explained that on average, men show slightly lower levels of creativity than females (Baer & Kaufman, 2008; Ma, 2009). On the other hand, it is established that males tend to score better than females on a variety of spatial tasks (Linn & Petersen, 1985; Voyer, Voyer, & Bryden, 1995), but practice, repeated testing, and training has been found to be similarly effective for men versus women (Baenninger & Newcombe, 1995; Marulis, Liu, Warren, Uttal, & Newcombe, 2007).

While the effects of gender on the effectiveness of EI training are best examined by comparing effects for males to females, we found few studies that reported the data to calculate separate effects by gender. Thus, consistent with Scott et al. (2004), we investigated the impact of gender on the training on EI by examining the relationship between the density of male participants and training effectiveness. As we anticipated greater gains for males than females in training, the proportion of men in the training program should be related to the overall effect size for that intervention. Based on the selection-regression threat and gender-based effects in other types of training, we hypothesized:

Hypothesis 3. There will be a positive correlation with the percentage of men and the effect of training on EI across samples.

Training Properties. Additionally, we examined characteristics of the training program itself. In their recent review of the science of training, Salas et al. (2012) concluded that while there is evidence that training works, "the way training is designed, delivered, and implemented matters" (p. 74). For example, we know from other meta-analyses that inclusion of lecture or discussion influences effect sizes for training in general (Arthur et al., 2003; Callahan, Kiker, & Cross, 2003), active learning promotes learning in computer-based simulation games (Sitzmann, 2011), and the presence or absence of practice and feedback moderates the effectiveness of web-based instruction relative to classroom instruction (Sitzmann, Kraiger, Stewart, & Wisher, 2006). Accordingly, we investigated the effects of training properties on effect sizes.

We used the training properties identified by Callahan et al. (2003) as well as features of training design identified by Brown and

Sitzmann (2011) to categorize and code important training properties. While it would have been more meaningful to do rich coding and analysis for specific training properties (e.g., the quality and quantity of feedback), the descriptions of training programs in most studies we reviewed allowed us merely to code whether the following training properties were described in the included study: lecture, modeling, exploration, practice, homework, feedback, coaching, assessment, and technology. Further, because there were often so few studies for many specific properties, we chose to examine the influence the training properties en masse. In other words, the more an individual study implemented lecture, modeling, learner exploration, and so forth, the more effective the training should be. Thus, we predicted that training properties, in general, would explain variability in effect sizes:

Hypothesis 4. Collectively, training properties will predict a significant amount of variance of the study-level effect size of training on emotional intelligence.

2. Method

Studies were obtained by conducting a computerized search for the words *emotional intelligence*, *training,* and *intervention* using the databases PsychInfo, Business Source Premier, Dissertation & Theses Global, and Google Scholar. We also searched for all studies referenced in papers identified in this search. To be included in the meta-analysis, a study must have included: (a) a training program with an objective to improve emotional intelligence of participants; (b) a measure of emotional intelligence administered pre- and post-training or post-test with a control group; (c) and reported means and standard deviations of the EI score, *or* enough statistical information (e.g., F, t, or p-values) to estimate effect sizes. Moreover, the participants in each sample must have been adults (i.e., over the age of 18).

We decided not to require a control group to be included in this meta-analysis, as many well-designed pre-post studies would have been excluded and valuable data lost. Morris and DeShon (2002) outlined conditions under which effect sizes from pre-post and treatment-control designs can be combined: Effect size estimates are based on the same metric and the standard deviation is drawn from the same population. In this instance, we were unwilling to assume that a pooled post-training standard deviation was drawn from the same population as the pre-training standard deviation in pre-post designs. Accordingly, we opted to conduct separate meta-analyses for pre-post and treatment-control designs, but applied both analyses to address our hypotheses and research question.

For the pre-post meta-analysis, a total of 56 samples from 50 studies met the inclusion criteria, for a total sample of 2136. The treatment-control meta-analysis included 28 samples from 26 studies, yielding 2174 participants. Of these 28 samples, 13 used an active (or alternative) treatment group, and 15 used a passive (or no) treatment group. The number of participants in the treatment-control sample exceeds the number of participants in the pre-post sample since the former includes control group participants where the latter does not. All studies were completed between 2000 and 2016. Table 1 presents a comprehensive list of studies included in both meta-analyses.

Participant roles and occupations are shown in Table 1. As can be seen in the table, working participants were in multiple roles including managers, nurses, police officers, sales, teachers, and retail staff. Additionally, student samples were drawn from undergraduate, graduate, and professional programs, with the modal sample being MBA students.

It should be noted that when student samples were used, the EI training in all cases was integrated into course or institutional programs; in other words, no studies were conducted on student participants who were engaged in the study for research credit. In addition, while several training programs were conducted on volunteer samples (e.g., Kotsou, Grégoire, Nelis, & Mikolajczak, 2011), in nearly all studies with field samples, the training was formally sanctioned by the host organizations.

2.1. Coding process

To increase accuracy and validity of the coding, both the first and second author independently coded all variables across all studies. Both authors are training researchers and well-trained in meta-analytical methodology. The two coders reached 100% reliability by engaging in multiple calibration meetings to discuss and resolve any discrepancies between individually coded items. The coders also discussed and agreed upon coding-related and inclusion rules, discussed in further detail in the below paragraphs.

Six studies assessed EI using a combination of self-report and other-report measures (e.g., peers or supervisors). We did not want to include both effect sizes in our meta-analyses, as these samples would be counted twice, thus inaccurately contributing two effect sizes to the overall meta-analysis as opposed to one. In the case where the same sample yielded two effect sizes—one based on self-report and the other based on other-report data—the effect size calculated from the other-report data was used.[1] We decided to use other-report over self-report data since research shows that people commonly overestimate their own abilities in general (Mabe & West, 1982) and their emotional intelligence specifically (Brackett, Rivers, Shiffman, Lerner, & Salovey, 2006). Individuals may overestimate their EI due to a self-serving bias (i.e., wanting to perceive themselves in a more favorable light) or because they lack the emotional expertise necessary to accurately assess their overall emotional ability/capacity (Côté, 2014; Sheldon, Dunning, & Ames, 2014). Additionally, self-report measures (compared to other-report measures) tend to not correlate with other-report ratings (Joseph & Newman, 2010) although the overall impact of training on EI scores does not change in our meta-analysis by using other- or self-report scores when both are available.

[1] In the pre-post studies using both other-report and self-report measures, the correlation between effect sizes was 0.69; there was no substantive change to the mean d when using self- v. other-reported scores.

Table 1
Studies, samples, and variables included in pre-post and treatment-control meta-analyses.

Study	Publication Type	Location	Participants	EI Measure	EI Focus	Sample Pre-Post	Size T-C
Al-Faouri et al. (2014)	PR	JD	Nurses	Genos EI Inventory	Yes	52	
Avella (2008)	D	US	Restaurant servers	Bar-On EQ-I 360	Yes	9	
Beigi and Shirmohammadi (2011)	PR	IR	Bank employees	ECI-2	Yes	56	
Belyea (2011)	D	US	Graduate psychology students	SEIS	No	23	
Berking, Meier, and Wupperman (2010)	PR	SUI	Police officers	ERSQ	Yes	27	46
Borges et al. (2012)	PR	US	Medical students	WEIP	No	23	
Carrick (2010)	D	US	Managers in medicine	Bar-On EQ-I	Yes	11	
Chapman (2005)	C	UK	Property managers	Boston EI Questionnaire	Yes	7	
Cherniss et al. (2010)	PR	US	Managers, multiple industries	ECI-2	Yes	63	109
Choi, Song, and Oh (2015)	PR	SK	Nursing students	AEQT	Yes	45	87
Christopher et al. (2016)	PR	US	Police officers	SEIS	No	43	
Clark, Callister, and Wallace (2003)	PR	UK	Undergraduate students	Executive EQ Map	No	123	
Clarke (2010a)							
Sample 1	PR	UK	MBA students	MSCEIT	Yes	43	120
Sample 2	PR	UK	MBA students	MSCEIT	Yes	21	98
Clarke (2010b)	PR	UK	Project managers	MSCEIT	Yes	57	
Crombie, Lombard, and Noakes (2011)							
Sample 1	PR	SA	Cricketers	MSCEIT	Yes	12	24
Sample 2	PR	SA	Cricketers	MSCEIT	Yes	12	24
Davis (2014)							
Sample 1	D	US	Economics students	Bar-On EQ-I	Yes	35	65
Sample 2	D	US	Economics students	Bar-On EQ-I	Yes	47	81
Dulewicz and Higgs (2004)	PR	UK	Middle managers, retail	EIQ	Yes	51	
Eichmann (2009)	D	US	White collar professionals	Bar-On EQ-I	Yes	51	
Fletcher, Leadbetter, Curran, and O'Sullivan (2009)	PR	UK	Medical students	Bar-On EQ-I	Yes	17	51
Forrey (2000)	D	US	Clergy	MEIS	No	26	52
Gardner (2005)	D	AU	Mixed occupations	SUEIT	Yes	55	
Gaudet (2010)	D	CA	Pharmacy students	MSCEIT	Yes	66	135
Gignac et al. (2012)	PR	AU	Sales representatives	Genos EI Inventory	Yes	29	50
Gonzalez (2013)	D	US	Teachers, administrators, and counselors	Wong and Law EI Scale	No	12	
Grant (2007)	PR	AU	Postgraduate students	SEIS	No	23	43
Groves, McEnrue, and Shen (2008)	PR	US	Sales representatives	EISDI	Yes	75	135
Hen and Sharabi-Nov (2014)	PR	IS	Elementary school teachers	SEIS	Yes	186	
Jahangard et al. (2012)	PR	IR	Psychiatric patients	Bar-On EQ-I	Yes	15	30
Kaplan (2002)	D	US	Early childhood teachers	ESQ	No	12	
Khodayarifard, Chshmenoooshi, Nejad, and Frahani (2012)	PR	IR	MBA students	Bar-On EQ-I	Yes	20	40
Kotsou et al. (2011)	PR	BE	Mixed occupations	TEIQ-SF	Yes	48	99
Krishnamurthi and Ganesan (2008)							
Sample 1	PR	IN	MBA students	WEII	Yes	11	
Sample 2	PR	IN	MBA students	WEII	Yes	16	
Lankisch (2007)	D	US	Not specified	MSCEIT	Yes	14	31
McCarthy, Michela, and Condie (2010)	R	CA	Graduate psychology students	Wong and Law EI Scale	Yes	63	
McEnrue, Groves, and Shen (2009)	PR	US	UG business students	EISDI	Yes	75	135
Moore (2005)	D	US	Inmates	Bar-On EQ-I	Yes	31	65
Moriarty and Buckley (2003)	PR	IE	UG business students	WEIP-5	No	82	
Murray, Jordan, and Ashkanasy (2006)	R	AU	Mixed occupations	WEIP-6	Yes	81	
Muyia and Kacirek (2009)	PR	US	Managers	Bar-On EQ-I 360	Yes	43	
Nelis et al. (2009)	PR	BE	UG psychology students	TEIQ-SF	Yes	19	37
Polito (2007)	D	US	Managers	MSCEIT	Yes	10	25
Potter (2005)	D	US	Undergraduates	EDEIS	Yes	71	122
Reuben, Sapienza, and Zingales (2009)	R	US	MBA students	MSCEIT	Yes	37	72
Sala (2002)							
Sample 1	R	BR	Retail staff	ECI-2	Yes	10	
Sample 2	R	BR	Human resources staff	ECI-2	Yes	10	
Sample 3	R	US	Accountants	ECI-2	Yes	19	
Stenberg (2004)	D	CA	Resident care students	MSCEIT	Yes	5	
Vesely et al. (2014)	PR	CA	Pre-service teachers	TEIQ-SF	Yes	23	49
Wagstaff, Hanton, and Fletcher (2013)	PR	UK	Mixed occupations	MSCEIT	Yes	25	

(*continued on next page*)

Table 1 (*continued*)

Study	Publication Type	Location	Participants	EI Measure	EI Focus	Sample Pre-Post	Size T-C
Zammuner, Dionisio, Prandi, and Agnoli (2013)	PR	IT	Mixed occupations	ECI-2	Yes	20	
Zijlmans et al. (2015)	PR	AN	Residential care staff	Bar-On EQ-I	Yes	65	111

EI Focus refers to whether the training was designed explicitly to improve emotional intelligence. *Pre-Post* = Pre-test, post-test design.
T-C = Treatment and control group design. *D* = Dissertation, PR = Peer-Reviewed, C = Chapter, R = Report/Manuscript. *AN* = Netherlands Antilles, *AU* = Australia, *BE* = Belgium, *BR* = Brazil, *CA* = Canada, *IE* = Ireland, *IN* = India, *IR* = Iran, *IS* = Israel, *IT* = Italy, *JD* = Jordan, *SA* = South Africa, *SK* = South Korea, *SUI* = Switzerland, *UK* = United Kingdom, US = United States of America. *UG* = Undergraduate. *AEQT* = Adult Emotional Quotient Test.
ECI-1 = Emotional Competence Inventory-2, *EISDI* = EI Self-Description Inventory, *EDEIS* = Exploring and Developing EI Skills, *EIQ* = Emotional Intelligence Questionnaire, *ERSQ* = Emotional Regulation Skills Questionnaire, *ESQ* = Emotional Situations Questionnaire, *MEIS* = Multifactor Emotional Intelligence Scale, *MSCEIT* = Mayer-Salovey-Caruso EI Test, *SEIS* = Shutte Emotional Intelligence Scale, *SUEIT* = Swinburne University EI Test, *TEIQ* = Trait-EI Questionnaire, *WEII* = Weisinger's EI Instrument, *WEIP* = Workgroup EI Profile-Short Version.

Of the 56 studies coded, 45 either collected post-test data immediately at the end of training ($K = 32$) or did not specify when post-test data were collected ($K = 14$). In the 11 samples in which a time after training was specified, the lag between training and post-testing ranged from two to 104 weeks ($M = 25.82$, $SD = 29.40$). In 50 of 56 samples, only a single post-test score was available. For cases in which there were multiple post-tests, the score most distal from the pre-test was coded. The more distal measure was assumed to be more indicative of true changes in participants' EI.

When possible, we tried to distinguish participant gender by treatment condition. In cases in which the authors did not specify gender separately by treatment condition, the overall frequencies were recorded for both groups since this was the best estimate available. Although this study assessed whether EI in general can be trained, EI is conceptualized as a multidimensional construct and is often measured as such. When possible, we recorded and aggregated an EI total score. When studies reported subscale scores, and not overall EI (e.g., Borges, Kirkham, Deardorff, & Moore, 2012), we calculated an effect size for the study by averaging the calculated effect sizes for each subscale.

2.2. Moderator variables

Moderator variables were identified as factors that were common across studies that may also significantly reduce or increase the effect size of training on EI. The first moderator variable was publication status. To examine potential publication-bias, we coded studies according to whether they were published in a peer-reviewed journal, as a dissertation, or as a book chapter/manuscript/report.

Additionally, we coded each of the following variables as dichotomous variables: Lecture, modeling, exploration, practice, homework, feedback, coaching, assessment, and technology. While it would have been preferable to code in ways that better captured the richness and quality of the training practice (e.g., the type and frequency of feedback), those studies that did include information that allowed us to code for training properties typically did so in a level of detail that allowed us to judge no more than "present" or "not present." Individual moderators were coded as follows. Lecture was coded a "1" whenever an instructor explained the content relevant to the learning objectives. Lectures could be presented in a variety of ways, including through computer conferencing, an audio or video recording, or a live lecture. Modeling was coded a "1" whenever the following conditions were followed in the training: (a) describing a set of well-defined skills, (b) showing a model or models displaying the use of those skills, (c) practicing using those skills, and (d) providing feedback and reinforcement (Decker & Nathan, 1985). Exploration referred to a variety of forms of active or experiential learning by participants by offering learning resources and allow learners to practice and explore. Examples of exploration include diaries, self-directed learning, and reflection. Practice was coded as present if trainees were explicitly instructed to practice emotional intelligence strategies. Homework was coded a "1" whenever participants were given assignments to complete between training assessments. Coaching was recorded when participants met one-on-one with the trainer/facilitator or another expert to discuss topics such as handling problems on-the-job or responding to emotional challenges. Feedback was coded a "1" when participants were given feedback by the instructor or peers anytime throughout the training. While all studies included assessment at least at post-test, for purposes of this investigation, assessment was coded a "1" when participants completed an EI measure before or during training and received information about how they scored (and perhaps what it meant). Finally, technology was scored a "1" whenever the description of the program mentioned the use of *any* form of technology-distributed instruction including computer-based training, video players, or audio recordings.

The final moderator variable was the EI measure used to assess emotional intelligence. Fifteen different EI measures were used to assess emotional intelligence of trainees. The most commonly used measure was the Mayer-Salovey-Caruso Emotional Intelligence Test (MSCEIT) ($K = 12$), followed by the Bar-On EQ-I ($K = 10$). We coded each EI measure as taking either an ability-model or mixed-model approach to measurement, according to theoretical underpinnings and reported construct validity. We used the MSCEIT as the prototypical EI measure to assess EI as an ability, as this is most commonly referenced ability-model measure in the EI literature (Côté, 2014). Therefore, any other measure that used the Mayer and Salovey (1997) definition of the emotional intelligence construct as the basis of their measure was included in the group of EI-ability measures. All other measures were coded as mixed-model measures.

2.3. Effect-size calculation

Effect sizes were calculated by the authors from the coded data. When means and standard deviations were available, we calculated the d-value using a variant of Cohen's *d*-statistic formula. There are a variety of choices of standard deviation terms available for calculating *d* (Schmidt & Hunter, 2015). We chose the weighted average of the treatment and control groups for those designs, and the pre-score SD for pre-post designs. Our rationale was that assuming training *works*, the SD for the post-test should be lower than at pre-test (Alliger & Katzman, 1997), thus the pre-test SD is the best estimate of variability in the population. To calculate the d-statistic for our treatment and control studies, we imputed change scores instead of post-test means in order to account for inequivalent pre-test scores between the treatment and control groups that could potentially mask or inflate the overall effect size between the two groups. Our primary d-statistic equations were as follows:

Repeated measures:

$$d_{RM} = \frac{(M_{post} - M_{pre})}{SD_{pre}}$$

Independent-groups:

$$d_{IG} = \frac{[(M_{Tpost} - M_{Tpre}) - (M_{Cpost} - M_{Cpre})]}{\sqrt{[(N_T SD_{Tpre}^2 + N_C SD_{Cpre}^2)/(N_T + N_C)}\,]}$$

when means and standard deviations were not reported, we calculated effect sizes based on available test statistics (e.g., t-values) following Schmidt and Hunter's (2015) procedures.

2.4. Meta-analytical procedure

The analysis was completed using the Hunter-Schmidt meta-analysis program (Schmidt & Le, 2014) using a random effects model. We corrected for sampling error by weighting each individual effect size by its sample size. Additionally, we corrected unreliability in the EI measure). Reliability estimates of the EI measure, either at the sample- or population-level, were available for 48 out of the 56 total pre-post samples, and for 24 out of the 26 treatment-control samples. We imputed the average reliability score for studies not reporting reliability.

3. Results

Descriptive statistics for the dependent variable (emotional intelligence score) for both meta-analyses (pre-post and treatment-control) are presented in Table 2. A positive and significant effect size of the mean true score across each meta-analysis would indicate the positive effect of training on EI scores. As shown in Table 2, the mean true score effect size across all pre-post samples was 0.61. In other words, EI scores were on average slightly over one half a standard deviation higher on post-test than pre-test. For the treatment-control meta-analysis, the mean true score effect size was 0.45, indicating a moderately positive effect of training on emotional intelligence, albeit not quite as strong as an effect as we found for the pre-post meta-analysis. For both meta-analyses, the 95% confidence intervals did not include 0, thus supporting the statistical significance of the effects. We can therefore conclude that emotional intelligence is a trainable construct, thus providing support for Hypothesis 1. In addition, while the confidence intervals for the two designs overlapped, we note that treatment-control designs are generally more rigorous than pre-post designs; thus the 0.45 estimate for the former design is likely a better estimate of the population effect for training EI.

The obtained effect sizes across data sets were moderate in size (Cohen, 1988). A *d* value of 0.45 would mean that the average participant in the training group would be at the 67th percentile of the control group; $d = 0.61$ or that the average score on post-test

Table 2
Meta-analytic effect of training on emotional intelligence.

							95% C.I.	
Meta-analysis	k	N	d	SD_d	d_c	SD_{dc}	LL	UL
Pre-post	56	2136	0.574	0.54	0.605	0.475	0.449	0.761
Treatment-control	28	2174	0.434	0.438	0.453	0.413	0.274	0.632

k = number of samples; N = total sample size; d = average observed effect size.
SD_d = standard deviation of average observed effect size.
d_c = mean true score correlation (Delta).
SD_{dc} = observed standard deviation of corrected effect size.
The 95% confidence intervals (C.I.) were set around the mean true score correlation (LL = lower level; UL = upper level).

Table 3
Summary of main effect analyses for emotional intelligence for pre-post samples.

								95% C.I.	
Variable	Level	N	k	d	SD_d	d_c	SD_{dc}	LL	UL
Publication status									
	Peer-reviewed	1431	33	0.681	0.602	0.705	0.673	0.475	1.934
	Dissertation	478	16	0.299	0.228	0.312	0.245	0.192	1.432
	Report/manuscript	227	7	0.598	0.357	0.604	0.375	0.326	1.882
EI measure									
	Ability-model	1213	26	0.517	0.218	0.542	0.233	0.452	1.632
	Mixed-model	923	30	0.679	0.774	0.691	0.871	0.380	1.003

k = number of samples; N = total sample size; d = average observed effect size.
SD_d = standard deviation of average observed effect size.
SD_d = standard deviation of average observed effect size. d_c = mean true score correlation (Delta).
SD_{dc} = observed standard deviation of corrected effect size.
The 95% confidence intervals (C.I.) were set around the mean true score correlation (LL = lower level; UL = upper level).

would be equivalent to the 73rd percentile score on pre-test.

We used the 75% rule to assess the degree to which variation in deltas is due to statistical artifacts not corrected for (Schmidt & Hunter, 2015). In other words, we assessed the presence of moderators based on the percentage of variance explained in the corrected average effect sizes. The ratio of observed variance in corrected d-values that could be explained by variance due to the artifacts of sampling error and test reliability was 36.9% for the pre-post meta-analysis and 27.0% for the treatment-control meta-analysis. Therefore, we were statistically justified to probe for moderators, and thus, our proposed moderator variables were further investigated.

3.1. Analysis of moderators

Tables 3 and 4 display the main effect analyses of the categorical moderator variables (publication status and EI measure), including the number of effect sizes examined (K), the mean weighted effect size (delta or d_c) and its 95% confidence interval (CI). Addressing Research Question one, in both data sets, effect sizes were larger for mixed-model than for ability measures ($\overline{d}_{mixed-model}$ = 0.69, $\overline{d}_{ability}$ = 0.54 for pre-post designs, and $\overline{d}_{mixed-model}$ = 0.51, $\overline{d}_{ability}$ = 0.35 for treatment-control designs). However, the confidence intervals for mixed-model and ability effects overlapped, indicating that there were no significant differences in population estimates for the two measure types.

A significant difference was found for the effect sizes for the published variable[2] in both the pre-post ($\overline{d}_{peer-reviewed}$ = 0.71, $\overline{d}_{dissertation}$ = 0.31) and the treatment-control ($\overline{d}_{peer-reviewed}$ = 0.59, $\overline{d}_{dissertation}$ = 0.12) data sets. In both cases, the average effect size of peer-reviewed studies was significantly greater than the average effect size of the dissertation studies, as evidenced by a non-overlapping confidence interval between the two sub-groups. Therefore, Hypothesis 2 was supported.

We conducted one additional analyses to look at the effects of study quality on treatment effects. For studies with both a treatment and control group, we calculated effect sizes for those using random assignment of participants to conditions (K = 12, N = 398) and those without random assignment (K = 13, N = 525). (For three additional studies, there was insufficient information to determine whether random assignment was used.) The mean effect size for studies with random assignment was greater than for that for studies with non-random assignment (d_c = 0.72, v. 0.26, although the confidence intervals overlapped). Consistent with the results based on publication type, larger effect sizes for training EI were found for better quality studies.

To assess Hypothesis 3, we correlated the percentage of males with the effect size of EI training for each study per the

Table 4
Summary of Main Effect Analyses for Emotional Intelligence for Treatment-Control Samples.

								95%	C.I.
Variable	Level	N	k	d	SD_d	d_c	SD_{dc}	LL	UL
Publication status									
	Peer-reviewed	755	19	0.577	0.393	0.589	0.529	0.351	0.827
	Dissertation	300	8	0.115	0.159	0.123	0.168	0.007	0.239
EI Measure									
	Ability-model	582	14	0.314	0.289	0.349	0.306	0.189	0.510
	Mixed-model	510	14	0.558	0.508	0.508	0.631	0.244	0.905

k = number of samples; N = total sample size; d_c = mean true score correlation (Delta).
SD_d = standard deviation of average observed effect size.
SD_{dc} = observed standard deviation of corrected effect size.
The 95% confidence intervals (C.I.) were set around the mean true score correlation (LL = lower level; UL = upper level).

Table 5
Regression analysis of training properties on effect size of treatment on EI for pre-post samples (n = 55).

	Model 1				Model 2			
Predictor	*b*	*se*	*LL*	*UL*	*b*	*se*	*LL*	*UL*
(Constant)	−0.45	0.29	−0.13	1.04	−0.50	0.22	−0.05	−0.95
Lecture	−0.35	0.21	−0.78	1.08	−0.33	0.19	−0.71	−0.06
Modeling	−0.29	0.31	−0.91	- 0.34				
Exploration	−0.26	0.24	−0.75	−0.22				
Discussion	−0.60	0.29	−0.02	1.19	−0.40	0.21	−0.01	−0.78
Feedback	0.02	0.22	−0.42	0.46				
Practice	−0.28	0.20	−0.13	0.69				
Coaching	−0.22	0.24	−0.28	0.71				
Assessment	−0.36	0.25	−0.86	−0.13				
Homework	−0.02	0.20	−0.38	−0.43				
Technology	−0.22	0.21	−0.65	−0.20				
Adjusted R^2 =	0.07				0.11			
ΔR^2 =					+0.04			

Note. *b* = unstandardized coefficients; *se* = standard error of *b* (LL = lower level; UL = upper level).

Table 6
Regression analysis of training properties on effect size of treatment on ei for treatment-control samples (n = 28).

	Model 1				Model 2			
Predictor	*b*	*se*	*LL*	*UL*	*b*	*se*	*LL*	*UL*
(Constant)	0.28	0.33	−0.42	−0.99	−0.30	0.16	−0.02	0.62
Lecture	−0.06	0.26	−0.49	−0.61				
Modeling	−0.08	0.34	−0.80	−0.64				
Exploration	−0.15	0.29	−0.77	−0.47				
Discussion	−0.03	0.31	−0.64	−0.69				
Feedback	0.51	0.33	−0.19	1.21	−0.46	0.26	−0.07	0.99
Practice	−0.79	0.27	−0.23	1.35	−0.75	0.21	−0.32	1.18
Coaching	−0.62	0.36	−1.38	−0.14	−0.65	0.26	−1.19	−0.12
Assessment	−0.12	0.30	−0.75	−0.51				
Homework	−0.40	0.24	−0.91	−0.11	−0.44	0.20	−0.84	−0.03
Technology	−0.08	0.22	−0.38	−0.54				
Adjusted R^2 =	0.10				0.29			
ΔR^2 =					+0.19			

Note. *b* = unstandardized coefficients; *se* = standard error of *b* (LL = lower level; UL = upper level).

recommendation of Schmidt and Hunter (2015) for assessing continuous variables as moderators. For the pre-post sample, we found a moderate and positive correlation ($r = 0.24$, $p = .102$) between the percentage of male participants and effect size. We found a similar correlation for the treatment-control sample as well ($r = 0.31$, $p = .113$). While the correlations are in the hypothesized direction, the effects are small and nonsignificant. Accordingly, Hypothesis 3 was not supported.

Hypothesis 4 stated that training properties as a set would predict a significant amount of variance in effect sizes across studies. To test this hypothesis, we regressed the study effect size on the nine dummy-coded training property variables. For the pre-post sample, the nine training properties did not explain a significant amount of variance in effect sizes ($R^2 = 0.22$, Adjusted $R^2 = 0.04$, $F_{(10,44)} = 1.21$, $p = .315$). Similar results were found for the treatment-control sample ($R^2 = 0.43$, Adjusted $R^2 = 0.10$, $F_{(10,17)} = 1.29$, $p = .311$). Therefore, Hypothesis 4 was not supported. Tables 5 and 6 show individual regression coefficients for each training property.

4. Discussion

The purpose of this study was to answer the question of whether emotional intelligence can be trained. We addressed this by conducting meta-analyses of both pre-post and treatment-control studies. Both meta-analyses found that the effect of training on is both moderate and positive, thus confirming that scores on EI measures can be increased via training.

4.1. Primary findings

Across both data sets, we found a moderate positive effect for training on emotional intelligence scores: $d_c = 0.45$ for treatment-

[2] The third level of this moderator variable—report/manuscript—was not found to be significantly different from the other two levels of the publishing variable since its confidence intervals overlapped with the peer-reviewed and dissertation sub-samples.

control designs and 0.61 for pre-post designs. It is instructive to compare it to average effect sizes from other training meta-analyses. Salas et al. (2012) reported average effect sizes for multiple training meta-analyses. For purposes of comparison with their results – and because most of our studies used self-report EI measures – we compare our findings to meta-analytic effects on what Salas et al. referred to as learning criteria. Arthur et al. (2003) conducted a meta-analysis of *all* known training interventions and reported an overall *d* of 0.63 for learning criteria. Burke and Day (1986) aggregated the effects of 21 treatment-control studies of managerial training and reported a corrected *d* of 0.34. Powell and Yalcin (2010) conducted an updated meta-analysis of managerial training programs and reported mean *r*'s of 0.48 ($K = 7$, $N = 381$) for posttest only with a control group designs and 0.55 ($K = 3$, $N = 270$) pretest-post-test designs. Finally, Keith and Frese (2008) examined the effectiveness of 24 error management training in 24 studies ($N = 2183$) and reported a mean *d* of 0.44. Thus, our meta-analytic results were within range of effects typically reported in training meta-analyses.

We examined the effects of several study properties on overall effect sizes. For both data sets, we found that studies published in peer-reviewed journals tend reported higher effect sizes on averaged than those reported in dissertations, thus inflating the expected effect of training on EI. This effect occurs in many meta-analyses (e.g., de Smidt & Gorey, 1997; Lipsey & Wilson, 1993). Since the goal of a meta-analysis is to estimate accurately the population effect size for a treatment, it can be argued that while it is important to know that effect sizes differ between publication types, the overall effect size across published and "grey literature" sources represents the best estimate of the population value (Hopewell, Clarke, & Mallett, 2005).

The next study characteristic we investigated was the percentage of males in the training groups. Contrary to our third hypothesis, we found no significant relationship between the density of males in training and the effect size. Our results replicate other meta-analyses in which there is no moderating effect for gender on training results (e.g., Baenninger & Newcombe, 1995; Uttal et al., 2013). While women score higher than men on both ability-based (Brackett & Mayer, 2003; Brody & Hall, 1993, 2000) and mixed-model EI measures (Goldenberg et al., 2006; Schutte et al., 1998; Van Rooy, Alonso, & Viswesvaran, 2005), the differences are likely not great enough to cause a ceiling effect. Both males and females are expected to equally benefit from EI training. That said, we recommend that in future training studies, researchers report separate test scores for males and females to more closely study possible gender differences in the trainability of EI.

Additionally, we found no support that training properties as a set had a significant influence on study effect sizes, even though the R^2 values were in fact non-trivial (0.24 for the pre-post design, 0.43 for treatment-control). A problem with this analysis was the accuracy of the coding. As discussed below, most studies provided insufficient detail on what was trained and how it was trained. Given that these were primarily studies of whether EI could be trained, this absence of detail is puzzling. That aside, the problem here is that when we coded training properties, a "1" meant that the property was mentioned. A "0" meant either that it was not mentioned, or the researchers specifically mentioned that the property was absent. Thus, it is not clear whether our null findings would have held had there been better reporting in the primary studies and a binary distinction clearly meant the presence or absence of the training property.

Although not reported in our results, we conducted an exploratory backwards stepwise regression analyses to see if we could identify a subset of training properties that seem to predict EI training effectiveness. We *cautiously* interpret these results here, noting both the small *K* in both data sets and the susceptibility of stepwise regression to capitalization on chance (Tabachnick & Fidell, 2007). Although the predictors that emerged across the two data sets were independent, we see overlap in the meaning of the results. For the pre-post studies, effect sizes are larger when discussion is included and lecture is not. Holding the other variables constant, including discussion or not lecturing results in an effect size about 0.40 units higher, nearly doubling the effect. These results are consistent with a case for training being active and personal. Our results suggest that trainees should acquire more emotional intelligence when they can discuss the meaning of the construct and how it applies to them, and they will learn less if they sit and listen. For the treatment-control designs, effect sizes were larger when there was practice and feedback, and no coaching and homework. The regression coefficient for practice was particularly high; holding other variables constant, providing practice opportunities results in an effect size 0.75 units higher. Again, the results suggest that training must be active (practice) and personal (feedback). EI scores increase when participants can practice diagnosing situations and responding accordingly, and receive feedback to how well they do. While coaching and homework would seem to be positive training properties, they are both used when training is spread out over multiple sessions and trainees are expected to do something (e.g., analyze a case or meet with a coach) between sessions. Again, we note that these interpretations should be considered with caution, but we encourage future researchers to systematically vary these properties to see their effect on training effectiveness.

4.2. Ability v. Mixed-model measures

We also investigated the research question of *whether effect sizes differ significantly depending on whether ability or mixed-model EI measures are used.* No significant differences on training effectiveness were found between ability-model and mixed-model EI measures for either research design. The mean effect sizes *appear* different between designs, but the confidence intervals overlapped. In the studies we reviewed, researchers typically noted the two approaches to emotional intelligence, but tended to endorse one over the other. Not surprisingly, researchers designed the training and chose the measures consistent with the approach they endorsed. Thus, the absence of differences in effect sizes between measure types could merely indicate that researchers selected training criterion measures consistent with their specific learning objectives and instructional methods (Kraiger et al., 1993). A more interesting analysis could be whether training effects generalize across both types of measures, but there not enough studies to allow us to investigate that question.

In summary, results of both pre-post and treatment-control group designs show a moderate, positive effect of training on the emotional intelligence scores of participants. This effect was stronger in published data than in dissertations and was robust across participant gender and type of EI measure.

4.3. Substantive and methodological limitations

We note that we were only able to estimate the effect of training on EI scores on measures theoretically related to the models that guided the development of training. Further, most studies used self-report EI measures. Accordingly, our meta-analysis is best interpreted as showing that training improves EI scores, rather than emotional intelligence itself. That is, the demonstrated practical utility of training would have been greater had there been more evidence that either trainees behaved in ways consistent with higher EI (e.g., greater self-awareness and self-management, empathy, and improved interpersonal communication) or that EI training affected other outcomes such as individual job performance or subordinate job satisfaction.

As is evident in Table 1, there was considerable variability in measures used to assess EI. The convergent validity of different EI measures, both within and between EI models, has long been recognized as a concern in the EI literature (e.g., Brackett & Mayer, 2003; Conte, 2005; Joseph et al., 2015; Webb et al., 2013). While meta-analysis methodology often includes combining different operationalizations of the same variable (Schmidt & Hunter, 2015), the general lack of convergence among EI measures suggests a cautionary interpretation of our findings – training positively changes EI scores, but it is not clear what aspects of emotional intelligence are particularly affected.

Further, there were no studies that used multiple EI measures that would have allowed us, for example, to determine of an ability-based EI intervention affected scores on a mixed-model EI measure. It is important to recognize that ability and mixed-model measures of EI tend to only be moderately correlated and may in fact represent different underlying constructs (Joseph & Newman, 2010). Further, studies that have used multiple measures of EI often find low correlations between different measures (e.g., Choi & Kluemper, 2012; Joseph & Newman, 2010; Zeidner, Shani-Zinovich, Matthews, & Roberts, 2005). To the extent that construct/measurement clusters of ability models represent something distinct than those of mixed-models, one could argue that argue that our meta-analytic investigation of training used one measure type v. the other is truly an "apples and oranges" comparison. Even if that is true, it is instructive to note that training to enhance EI by either criterion tends to be equally effective, and practitioners are encouraged to seek out instructional methods that are optimized for their intended training outcomes (Kraiger & Jung, 1997).

While we made every effort to locate all published and unpublished studies on the trainability of emotional intelligence, both the total number of studies and total sample size were relatively small for a meta-analysis. Thus, by only including studies we found within databases and through email list-serves requests of unpublished papers, we may have contributed to the "file-drawer problem" of not including null, yet valid, findings (Rosenthal, 1979). In other words, the effect sizes in additional unpublished works investigating the effect of training on emotional intelligence may differ significantly from effect sizes reported in peer-reviewed journals and dissertations/theses. Rosenthal recommended calculating a fail-safe *N* to estimate the number of unpublished null results needed to determine if the overall calculated effect size is due to sampling error. If the fail-safe *N* exceeds ten plus five times the number of effect sizes, it would be unlikely that a null effect would be found. The fail-safe *N* for the pre-post and treatment-control meta-analyses are 290 and 150, respectively. Therefore, it is possible, though unlikely, that enough unpublished studies exist to result in a null effect of training on EI.

On the other hand, it is also possible that our meta-analysis underestimates the population effect of training on emotional intelligence. We say this for several reasons. First, while some of the studies we aggregated were in top-tier journals, the majority were not. We did not code or control for methodological weaknesses in studies, as primary sources on meta-analysis recommend against this practice (Schmidt & Hunter, 2015). Intuitively, if, as we found, most studies were published in lower-tiered journals, those studies probably contain an atypical number of methodological flaws. Often, these shortcomings have the effect of increasing within cell variability, which in turn would decrease study-level effect sizes. While we realize this is post-hoc speculation, we do suspect that several well-designed, large sample studies could have a great impact on estimates of the population effect.

Second, our analyses found that training properties had, both individually and collectively, influences on study-level effect sizes. However, many studies did not provide adequate (or any!) documentation of these training properties. Had all studies clearly described their training practices, we would have not only been able to more accurately model the effects of these practices on study effect sizes, but we could have obtained estimates of the effects of *well-designed* training programs. If for example, we identified main effects for practice, feedback, and (absence of) lectures. We could have then identified only those training programs characterized by those properties and, in turn, provided an upper level estimate of the impact of training on EI scores.

More generally, a primary challenge we faced when conducting this meta-analysis was the lack of available details within the coded studies. Authors commonly failed to include detailed information about their training program. This made coding training properties difficult, as explicit information about the content of the training program was not readily available. It is also possible that by treating the absence of a mention of property as the absence of a property may have mispresented true proportions of properties in the studies we reviewed. For example, a method section may have said: "The training program was characterized by lecture, discussion, and practice." Because there was no mention of homework, we assumed that (and coded as if) no homework assignments were given. To the extent that these assumptions were erroneous, our findings on the joint effects of training properties may be misleading.

Even more surprising was how often very basic statistics were left out of results sections, making our calculations of effect sizes either challenging or difficult to accomplish. When authors were emailed to request missing data, they often stated that they no longer had the data, even though the request fell well within the seven-year APA guideline for data storage (American Psychological Association, 2007). We therefore strongly advise authors of future training evaluation papers to, please, be thorough in your reporting of relevant data (e.g., means, standard deviations, sample sizes for all sub-groups) to allow the possibility of meta-analytic procedures to be conducted using your data.

4.4. Future research

Given our evidence that training positively affects EI scores, it would be valuable for EI researchers to move beyond straightforward studies that examine *if* training affects EI to more complex studies as to *how* and *for whom*. We noted a trend in the studies reviewed that suggested training is more effective when lectures are avoided, and coaching, practice, and feedback are included. This pattern suggests the need to individualize EI training and provide accurate feedback on participants' skills. Moreover, this preliminary finding has important implications for practitioners insofar that EI is better trained using an active/experiential approach versus passive. Researchers should further test specific interventions rooted in the EI literature.

Further, while we found no moderating effect for gender, other individual difference variables should be investigated as potential moderators. For example, is training more effective for individuals high in self-monitoring (John, Cheek, & Klohnen, 1996) or regulatory flexibility (sensitivity to context, availability of a diverse repertoire of regulatory strategies, and responsiveness to feedback; see Bonanno & Burton, 2013)? By analogy, we note how the Keith and Frese's (2008) meta-analysis on error management training gradually led to fewer main effect studies and more studies on underlying mechanisms (e.g., Baglin & Costa, 2012; Keith, Richter, & Naumann, 2010) and individual difference moderators (Cullen, Muros, Rasch, & Sackett, 2013; Loh, Andrews, Hesketh, & Griffin, 2013).

Additionally, we noted that we could not assess training impact on any criteria other than EI scores. As noted above, EI has been related empirically to many work-related criteria included job performance, work stress, and health (Ciarrochi, Deane, & Anderson, 2002; Harms & Credé, 2010; Joseph et al., 2015; Joseph & Newman, 2010; Martins et al., 2010 and O'Boyle et al., 2011). Further, it is known that not all learning in training transfers to the job (Salas et al., 2012); how much is transferred depends on many personal, training, and organizational factors (Blume, Ford, Baldwin, & Huang, 2010). Accordingly, it would be useful in primary studies to consider the effects of EI training on other outcomes. If EI increases from pre-test to post-test, do workers feel less stress or greater job satisfaction? Does their performance increase? What is the size of the effect on these outcomes relative to the observed effect on EI scores? While these questions cannot be answered given the current state of the EI training literature, given that our study shows that training influences EI *scores*, in future investigations, researchers are encouraged to move beyond this question and examine the broader impacts of training.

5. Summary and implications

In conclusion, the results of this meta-analysis make both theoretical and practical contributions to the EI literature by identifying the extent to which EI can be trained. The moderate and positive effect of training on EI supports the malleability of this construct, allowing us to infer that EI is trainable. These findings should be beneficial to practitioners curious to know whether implementing workplace EI training programs is indeed a good investment. Based on the findings of this meta-analysis of the effect of training on emotional intelligence, we can conclude: Yes, you can train that.

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