A Meta-Analysis of Longitudinal Associations between Substance Use and Interpersonal Attachment Security

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Abstract

Substance use has long been associated with close relationship distress. While the direction of influence for this association has not been established, it has often been assumed that substance use is the causal agent and that close relationship distress is the effect. But research seeking to establish temporal precedence in this link has produced mixed findings. Further, theoretical models of substance use and close relationship processes present the plausibility of the inverse pathway—that insecure close relationships may serve as a vulnerability factor for the development of later substance problems. The current review applies an attachment-theoretical framework to the association between close social bonds and substance use and substance-related problems. Targeting longitudinal studies of attachment and substance use, we examined 665 effect sizes drawn from 34 samples (total N=56,721) spanning time frames ranging from 1 month to 20 years (M=3.8 years). Results revealed a significant prospective correlation between earlier attachment and later substance use (r =-.11, 95%CI=-.14 to -0.08). Further, cross-lagged coefficients were calculated which parsed auto-regressive effects, indicating that lower attachment security temporally preceded increases in substance use (r=-.05, $95\%CI$=-.06 to -.04). Analyses further indicated that the pathway from earlier attachment to later substance use was significantly stronger than that from earlier substance use to later attachment. Results also revealed several moderators of the attachment-substance use link. These findings suggest that insecure attachment may be a vulnerability factor for substance use, and indicate close relationship quality as a promising line of inquiry in research on substance use disorder risk.

Keywords: attachment, substance use, close relationships, meta-analysis, substance use disorder

Public Significance Statement: Results of this meta-analysis indicate a longitudinal association between insecure attachment and substance use, suggesting that insecure attachment temporally precedes increases in substance use and substance problems. These findings suggest that insecure relationships may be a vulnerability factor for the development of addiction, and thus of potential utility for identifying those at risk and for informing prevention measures.

Close social relationships exert a powerful influence on people's emotions, motivations, and behaviors. The initiation of a new close relationship can give rise to soaring emotional highs, separation from close others can yield devastating lows, and, in the effort to maintain close relationships, people can engage in profound cognitive distortions and (seemingly) irrational action patterns (Baumeister & Leary, 1995; Bowlby, 1980; Clark & Lemay, 2010). In light of these observations, researchers have proposed that few other factors exercise as profound and pervasive an effect on human emotions as do close relationships (Bowlby, 1980).

While relationship researchers have commented on the powerful psychological effects of close social bonds, addiction researchers have often observed similarly extraordinary responses to drugs of abuse (Koob & Le Moal, 1997; Volkow & Li, 2004). Indeed, many of these researchers have documented strikingly similar patterns of affective, cognitive, and behavioral responding to attachment figures as to addictive substances (Burkett & Young, 2012; Cooper, Shaver, & Collins, 1998; Kassel, Wardle, & Roberts, 2007; Vungkhanching, Sher, Jackson, & Parra, 2004).[1] Some have proposed that the psychological and neurobiological processes supporting addiction overlap with those involved in the development and maintenance of close relationships (Burkett & Young, 2012; Insel, 2003), concluding that insecure close relationships may serve as a vulnerability factor for addiction (Kassel, Wardle, & Roberts, 2007; Vungkhanching, Sher, Jackson, & Parra, 2004).

But despite theorizing surrounding the interconnectedness of close social bonds and addiction processes, there has yet to be a systematic examination of the association between interpersonal attachment and substance use and substance problems. The current review seeks to

[1] In the current manuscript, the terms "addiction" and "Substance Use Disorder" are used to refer to a condition whereby recurrent use of alcohol and/or drugs causes clinically and functionally significant impairment. We also, consistent with the addiction literature, employ the term "substance problems" to refer to such impairment as conceptualized on a continuum (e.g., W. R. Miller, Tonigan, & Longabaugh, 1995).

fill this gap. We draw upon an attachment framework, which offers a model for understanding both normal and abnormal psychological functioning through an exploration of close relationship processes, to examine the association between close social bonds and patterns of substance use and substance use problems. More specifically, we report the first meta-analysis of longitudinal samples to explore the association between attachment and substance use, examining the extent to which insecure attachment orientations (i.e., individual differences in the extent to which people experience insecurity vs. security in their close relationships) represent a risk factor for increases in substance use and substance use problems.

Substance Use and Close Social Bonds

Researchers have documented robust links between close relationship distress and substance use (Epstein & McCrady, 1998; McCrady, 2008). Across varying age groups and relationship types, people who use substances heavily are more likely to experience problems in their close relationships. Within intimate partnerships, substance abusing couples self-report less relationship satisfaction (Goering, Lin, Campbell, & Boyle, 1996; Marshal, 2003; Whisman, 1999), higher levels of intimate partner violence (Lipsey, Wilson, Cohen, & Derzon, 1997; Testa, 2004), and are up to 7 times more likely to divorce (Paolino, McCrady, & Diamond, 1978). Among older and elderly populations, problem drinkers are more likely to experience loneliness, social isolation, and low social integration and social support (Graham, Carver, & Brett, 1995; Hanson, 1994; Meyers, Hingson, Mucatel, & Goldman, 1982; Schonfeld & Dupree, 1991). Families in which at least one parent has substance problems demonstrate lower levels of cohesion (Bijttebier & Goethals, 2006; Jester, Duck, Sokol, Tuttle, & Jacobson, 2000) and are more likely to display negative behaviors during social interactions (Haber & Jacob, 1997; Jacob, Krahn, & Leonard, 1991; Jacob, Leonard, & Randolph Haber, 2001; Moser & Jacob,

1997). Finally, adolescent substance users are more likely to have problematic relationships with their families (e.g., Cooper, Shaver, & Collins, 1998; McNally, Palfai, Levine, & Moore, 2003), and young adult substance users report fewer close social relationships than their peers (C. Power & Estaugh, 1990; Wilsnack, Klassen, Schur, & Wilsnack, 1991).

Based on these and other studies, it has often been assumed that the association between low quality relationships and substance use is due to the harmful effects of substance use on close social bonds. Conventional wisdom, as reflected within scholarly writing and popular discourse, indicates that substance use exerts a destructive effect on close social relationships (Leonard & Eiden, 2007; Newcomb, 1994; Whisman, 1999). This view has long been reflected within the Diagnostic and Statistical Manual of Mental Disorders, which lists interpersonal problems as one of the core signs that an individual has developed a problematic pattern of alcohol or drug use (e.g., American Psychiatric Association, 2013).

Although substance use is widely believed to cause close relationship distress, evidence for this specific causal pathway is ambiguous (see Leonard & Eiden, 2007; Marshal, 2003). More specifically, longitudinal studies examining whether substance use predicts the later deterioration of close social bonds have produced mixed results (Kearns-Bodkin & Leonard, 2005; Leonard & Roberts, 1998a; Newcomb, 1994). For example, Leonard and Roberts (1998a) found a significant prospective association between husbands' alcohol consumption at the time of marriage and decreases in marital satisfaction over a one year period, but Kearns-Bodkin and Leonard (2005) found no prospective effects of alcohol consumption on marital quality over 3 years. Newcomb (1994) found similarly inconsistent results for his examination of close relationship quality and drug and alcohol use in a sample of young adults, finding no overall effects of earlier drug/alcohol use on general (latent) indexes of close relationship quality.

Looking within the experimental realm, where researchers have examined effects of acute intoxication on relationship factors, a handful of studies have found detrimental effects of substance administration on the quality of social interaction between close relationship partners (Leonard & Roberts, 1998b; Samp & Monahan, 2009). However, the majority of studies have revealed no such detrimental effects (e.g., see Fairbairn, 2017), and many have observed a social-cohesive effect of drugs of abuse during social interaction (del Porto & Masur, 1984; Fairbairn & Testa, 2016). As noted by Leonard and colleagues, "although it seems implausible that there should be no causal influence [of substance use on close relationship functioning], the nature and strength of that causal influence are not clear" (Leonard & Eiden, 2007, p. 294). These inconsistent findings have led some researchers to seek alternative ways to understand the association between close interpersonal bonds and substance use that does not necessarily assume that substance use is the predisposing agent and that poor relationship quality is the effect.

An alternative possibility— one that is consistent with contemporary psychological theories —is that poorly functioning relationships may predispose people to substance use (Fairbairn & Cranford, 2016; Fairbairn & Sayette, 2014). Indeed, both theories of close relationships and addiction point to the possibility that low quality social relationships could serve as a vulnerability factor for the later onset of substance use and substance problems (Epstein & McCrady, 1998; Fairbairn & Cranford, 2016; Fairbairn & Sayette, 2014; Insel, 2003; Leonard & Eiden, 2007; O'Farrell, Hooley, Fals-Stewart, & Cutter, 1998; Van der Vorst, Engels, Meeus, & Deković, 2006). One theoretical perspective in the close relationships literature that may provide particularly fertile ground for understanding this pathway is attachment theory—a

theory that specifically addresses different forms of relationship distress while providing a broader framework for thinking about pathological patterns of behavior.

Attachment Theory and Substance Use

Originally proposed by John Bowlby (1969) based on his research with juvenile delinquents, and later elaborated on by both Bowlby (1969, 1973, 1980) and Mary Ainsworth (1978), attachment theory offers a framework for understanding the ways in which interpersonal relationships can shape both normal and abnormal psychological functioning. More specifically, attachment theory proposes that the development of deviance and other forms of psychopathology in adolescence and young adulthood might be attributable, at least in part, to dysfunction in earlier close relationships (Ainsworth & Bowlby, 1991; Bowlby, 1980). According to Bowlby, the quality of people's close relationships, beginning in early childhood, contributes to the development of mental representations (working models) of the self and others. These representations, in turn, have the potential to shape the way people understand their social worlds and, as such, can serve either as vulnerability factors (in the case of insecure relational histories) or sources of resilience (in the case of secure relational histories).

According to attachment theory, humans are biologically predisposed to form close affective bonds to others who can provide them with support, care, and protection (Bowlby, 1969). The prototypical attachment relationship is that between an infant and his or her primary caregiver. And, not surprisingly, attachment research has focused historically on infant-caregiver attachment: How it develops and its implications for social and emotional functioning in childhood. But a key theme in modern attachment research is that attachment bonds continue to

play a role from the cradle to the grave (Bowlby, 1973).[2] Indeed, as children develop, they often forge new attachment relationships with friends and romantic partners (Hazan & Zeifman, 1994; Nickerson & Nagle, 2005), and most adults report using both parents and romantic partners as attachment figures (e.g., Doherty & Feeney, 2004; Fraley & Davis, 1997; Trinke & Bartholomew, 1997).

Although many individuals report being attached to romantic partners and peers (Fraley & Davis, 1997), the quality of those attachments[3] can vary substantially. Some people, for example, are *securely attached* to their attachment figures: They are comfortable using them as a safe haven during times of distress and as a secure base from which to explore the world. In contrast, other people are *insecurely attached* to their attachment figures. These people may be reluctant to depend on their partners and may harbor reservations about whether their attachment figures will be available when they are most needed. These individual differences (what researchers sometimes call attachment "styles," "patterns," or "orientations") have been the focus of the majority of empirical research on attachment. Attachment styles have often been

[2] Attachment orientations demonstrate a moderate degree of stability across the lifespan (Fraley, 2002). Although an individual with a secure attachment orientation during childhood may also demonstrate a secure orientation during adolescence, such consistency is by no means a foregone conclusion. Research has shown that attachment orientations can change in response to a variety of life circumstances, including when people experience breakups (Ruvolo, Fabin, & Ruvolo, 2001; Sbarra & Hazan, 2008), trauma (Mikulincer, Ein-Dor, Solomon, & Shaver, 2011), threats to the availability of their parents and romantic partners (Holman, Galbraith, Mead Timmons, Steed, & Tobler, 2009), and undergo therapy (Taylor, Rietzschel, Danquah, & Berry, 2015). The test-retest stability of attachment orientations is moderate in magnitude, and is similar to the level of stability observed in other measures of relationship satisfaction (Fraley, Heffernan, Vicary, & Brumbaugh, 2011; Kirkpatrick & Davis, 1994). In sum, attachment, like many things, exhibits some degree of stability, but exposure to a range of contextual factors can lead to change.

[3] Attachment and relationship quality are related constructs, but are typically operationalized in different ways. For example, common self-report measures of relationship quality tend to focus on contextual and evaluative features of the relationship, such as "I feel satisfied with our relationship" from the Investment Model Scale (Rusbult, Martz, & Agnew, 1998). Common attachment measures, in contrast, are designed to assess the personal securities/insecurities that people hold (e.g., "I worry that others won't care about me as much as I care about them", from the Fraley et al. ECR-RS). Although insecurity clearly has implications for relationship quality (i.e., if someone is insecure about opening up to others, it is difficult to build an intimate relationship), security per se is not the same thing as relationship quality. A person can be insecure with respect to attachment without actually being in a close relationship, for example. In short, attachment and relationship quality are not synonymous.

treated along a bipolar continuum, from insecure to secure (e.g., Armsden & Greenberg, 1987), although some conceptualizations have also identified subtypes of insecurity (see below). Through the examination of attachment security, researchers have learned, for example, that people who are relatively secure in their attachments are more likely than those who are not to be better adjusted psychologically; to manage conflict effectively; to provide more constructive support to their children; and to have more satisfying and rewarding relationships with peers, family, and with co-workers (see Gillath, Karantzas, & Fraley, 2016, for a review). In short, the quality of attachment relationships has implications for understanding relationship distress and psychological adjustment.

Not surprisingly, a growing number of scholars have turned to attachment theory as a way to understand the interface of interpersonal relationships and the development of various forms of psychopathology (see Cassidy & Shaver, 2016 and; Gillath et al., 2016 for reviews). For example, attachment theory has been used to explain a variety of psychological disorders, ranging from depression to anorexia (Brennan & Shaver, 1995; Madigan, Brumariu, Villani, Atkinson, & Lyons-Ruth, 2016). Importantly, researchers have suggested that attachment theory may be especially well suited to the study of addiction. Scholars have noted numerous parallels between patterns of responding to attachment figures and to drugs of abuse (Cooper, Shaver, & Collins, 1998; Insel, 2003; Kassel, Wardle, & Roberts, 2007). As noted at the outset of this article, people tend to experience joy and euphoria when reunited with attachment figures, and, in the absence of their loved ones, often experience despair, restlessness, and a preoccupation with them (Bowlby, 1980). Similar emotional responses occur in the context of addiction: The opportunity to use can be met with joy and euphoria and, in the absence of such opportunities, addicts often experience despair, restlessness, and a preoccupation with the substance of interest

(Babor, Berglas, Mendelson, Ellingboe, & Miller, 1983; Robinson & Berridge, 1993; West & Gossop, 1994). Indeed, some theorists have proposed that there may be common psychological and neurobiological processes supporting addiction and attachment (Burkett & Young, 2012; Insel, 2003).

Both attachment relationships and drugs of abuse operate as an external means of regulating emotions, with the exact emotion regulatory functions of these factors varying depending on prior history of exposure (Koob & Le Moal, 1997; Simpson, Collins, Tran, & Haydon, 2007). Substances of abuse and attachment relationships both demonstrate a shift from a positive to a negative affect regulatory function with repeated exposure—exposure to novel attachment relationships and to novel drugs of abuse tends to lift the individual from a neutral to a positive emotional state, while, after repeated exposure, lack of contact to each can impose a negative affective state that is relieved only by exposure (Baumeister & Leary, 1995; Bowlby, 1973; Koob & Le Moal, 1997; Solomon, 1980). Both attachment relationships and substance use disorder (SUD) are believed to involve higher-order cognitive processes centered around the conceptualization of the self, with attachment researchers suggesting that early insecure attachment relationships can lead to psychopathology by inducing a negative self-concept ("working model of the self") (Bowlby, 1973), while addiction researchers theorize that the reinforcement value of certain drugs of abuse are derived from their ability to reduce feelings of negative self-awareness (Fairbairn & Sayette, 2014; Hull, 1981, 1987; Kassel, Stroud, & Paronis, 2003). More recently, neuroscientists have observed marked overlap in the brain systems involved in attachment and those associated with addiction (e.g., dopamine, opioids, and corticotropin-releasing factor), concluding that brain systems that evolved to govern attachments between parents and children and between monogamous partners may be the targets of drugs of

abuse and thus serve as the basis for addiction processes (Burkett & Young, 2012; Insel, 2003; Young, Gobrogge, & Wang, 2011). In line with such theorizing, research has indicated that insecure parental attachment relationships may be a primary mechanism whereby family history of addiction—one of the most robust SUD risk factors—confers vulnerability for problematic substance use (Vungkhanching et al., 2004). In sum, while the precise mechanism of action proposed for explaining the link has often varied, several strands of research converge to suggest that the processes involved in close relationship formation and maintenance overlap with those involved in substance use and addiction, leading some to posit that insecure attachments may represent a risk factor for substance use.

In the current review, we examine longitudinal associations between attachment security and substance use. Reflecting long-standing interest in conceptual connections between attachment and addiction processes, a number of literature reviews have examined this association (e.g., Becoña, del Elena, Amador, & Ramón, 2014; Schindler et al., 2005), but, importantly, these reviews have been qualitative rather than quantitative in nature. One recent quantitative review did examine the association between attachment and the broader construct of "externalizing behavior" in children, a construct that includes substance use (Madigan et al., 2016), but individuals over the age of 18 were excluded from the analysis, the age after which the majority of SUDs develop (Kessler et al., 2005). Thus, no prior review has had the capability to quantify the size and directionality of the association between attachment and substance use. Of note, in the current review, we not only examine attachment as a risk factor for substance use, but also incorporate an examination of temporal precedence in the attachment-substance use link—explicitly exploring the extent to which insecure attachments precede substance use and, inversely, the extent to which substance use precedes insecure attachments. Finally, we also

examine several moderators of the attachment-substance use association to learn more about how this association may vary across types of attachment measures, types of substance use measures, and study populations.

The Measurement of Attachment

Individual differences in attachment have been modeled in a variety of ways over the past few decades. It is not uncommon, for example, for scholars to conceptualize attachment as a bipolar construct ranging from secure to insecure (Armsden & Greenberg, 1987). Some frameworks distinguish among different forms of insecurity. For example, contemporary models in social and personality psychology emphasize the notion that some people are anxious with respect to attachment concerns (e.g., they are worried that others are not invested in them) and others are avoidant (i.e., they have a tendency to withdraw or pull away from close others)(e.g., Brennan et al., 1998). The current review focuses mainly on attachment measured as a bipolar construct—an approach reflected within several commonly-implemented measures of attachment (Armsden & Greenberg, 1987)—although we also examine subtypes of insecurity where measures permit.

There are at least two ways to assess attachment beyond infancy. One approach uses interview-based measures of attachment, such as the Adult Attachment Interview (AAI; George, Kaplan, & Main, 1984). The AAI is a semi-structured interview in which participants are asked to reflect on their childhood attachment experiences with primary caregivers. The interview is transcribed and coded for the quality of discourse, such as whether the individual is able to provide an integrated and believable account of early experiences. A person who is classified as secure or autonomous with respect to attachment is able to coherently describe early experiences and provide a non-defensive interpretation of them. In contrast, people who provide inconsistent

accounts, or who idealize their caregivers, or who insist that they cannot recall details to support the narrative they provide, are classified as preoccupied or dismissing (insecure) with respect to attachment. These classifications are designed to reflect the participant's *current* state of mind with respect to attachment—the way in which the person currently appraises and interprets early experiences.

Another common approach uses self-reports, such as the Inventory of Parent and Peer Attachment (IPPA; Armsden & Greenberg, 1987) and various measures developed by social-personality psychologists [e.g., the Adult Attachment Scale (AAS; Collins & Read, 1990) and the Experiences in Close Relationships Scale (ECR; Brennan, Clark, & Shaver, 1998 and ECR-R; Fraley, Waller, & Brennan, 2000).] Some of these self-report measures focus broadly on close relationships more generally (Fraley, Heffernan, Vicary, & Brumbaugh, 2011), whereas others focus specifically on relationships with peers and/or parents (Armsden & Greenberg, 1987) or on romantic relationships (Brennan et al., 1998; Collins & Read, 1990). Like the AAI, these approaches are designed to assess the person's *current* state of mind with respect to attachment, but with a focus on attitudes, appraisals, beliefs, and feelings rather than the organization of discourse with respect to early attachment experiences. The working assumption is that a person's security in his or her relationships will manifest in the ways in which he or she appraises the relationship and the feelings that are commonly experienced. A prototypical secure person, for example, should report confidence in the attachment figure's availability and responsiveness. Moreover, a prototypically secure person tends not to worry about whether attachment figures will abandon them in times of need; this individual is secure in the knowledge that the attachment figure will be available when required.

Although we include studies using both kinds of measures in our review, we should note upfront that there are many more studies that have used self-reports than have used interview methods. One potential reason for this is that interview-based approaches require substantial labor and training, both for administration and coding. Importantly, although the two kinds of measures do not correlate strongly with one another (e.g., Roisman et al., 2007), they both predict a wide array of outcomes anticipated by attachment theory (see Crowell, Fraley, & Roisman, 2016). Both interview-based and self-report measures have been shown to relate to adjustment, relationship functioning, and other outcomes (see Crowell et al., 2016). Some scholars have argued that both kinds of measures are valid ways of assessing the quality of attachment relationships, but they tap into different components of what it means to be secure or insecure (e.g., Crowell et al., 2016). The AAI, for example, uses indirect methods to examine the coherence of discourse during the interview, whereas self-report measures use direct methods to assess the quality of attachment in parental, romantic, and/or peer relationships. Because the majority of studies reviewed here employed self-report methods, we borrow the concepts and terminology used in the social-personality tradition from which many of these measures derive. We note that recent meta-analyses indicate that questionnaire-based methods, including those that simply index overall level of security without identifying insecure subtypes (e.g., IPPA) can map to relevant clinical outcomes as strongly if not more strongly than interview-based measures (Madigan et al., 2016). We capitalize on the variability in the methods employed in the studies reviewed to examine whether the type of measure used (self-report vs. interview) moderates the association between attachment and substance use. We further examine whether the identity of the attachment figure (e.g., mother, father, close friend/romantic partner) also serves as moderators of the attachment-substance use association. Finally, we examined whether the

specific subtype of insecurity assessed (e.g., “general” insecurity, anxious subtype, avoidant subtype) moderated the attachment-substance use association.

The Measurement of Substance Use

A variety of different measures have been employed to assess the extent of substance use. Most commonly-employed measures rely on the report of the participant for information; the analysis of breath, saliva, or urine samples may give little sense for the quantity and/or pattern of use and, in some cases, may capture use only over a very limited time scale (Day & Robles, 1989). Commonly employed measures of substance engagement include indexes of the frequency of use (# of occasions of use) and also the quantity of use (amount consumed per occasion) (Johnston, O’Malley, Bachman, & Schulenberg, 2000; Saunders, Aasland, Babor, De la Fuente, & Grant, 1993; SAMHSA, 2013). The frequency and quantity of an individual's substance use can have direct health implications and also closely corresponds to that individual’s likelihood of developing SUD (Johnston et al., 2000; Saunders et al., 1993).

To directly index disordered patterns of substance use, researchers often employ indexes of substance use problems. These problem indexes include questions that correspond closely to DSM SUD criteria, but have often been preferred to dichotomous/categorical indexes offered within the DSM in that they reflect a full continuum of disordered substance involvement (e.g., W. R. Miller, Tonigan, & Longabaugh, 1995). In the current review, we examined whether the type of substance use index—frequency, quantity, substance use problems, other—moderated the association between substance use and attachment.

While some indexes of substance engagement employed in research consider all categories of substances together, many involve a specification of the specific drug type. In particular, alcohol, nicotine/tobacco, and marijuana are three of the most frequently used

substances in the U.S., and research has often included measures that index involvement with these specific drugs (SAMHSA, 2013). Researchers exploring vulnerability to, and effects of, substance use have observed both commonalities and differences across discrete drug classes (Kendler, Prescott, Myers, & Neale, 2003; Koob, 2008; Robinson & Berridge, 1993). Different classes of substances can act through different psychological and neurobiological mechanisms and often hold different socio-culturally determined places in society (Blum, 1969; Room & Makela, 2000). At the same time, consistencies have also been observed across substances in the psychological processes supporting SUD (Koob & Le Moal, 1997; Robinson & Berridge, 1993) and also in the vulnerability factors differentiating those at risk (Hawkins, Catalano, & Miller, 1992). Note that many of the theoretical models that most directly inform our hypotheses concerning links between attachment and addiction processes apply across drug types (Burkett & Young, 2012; Insel, 2003; Koob & Le Moal, 1997). Therefore, we hypothesized that links between attachment and addiction would emerge across different classes of substances.

Study Populations

Some substance use research has explicitly targeted clinical samples—individuals who have already developed SUD. Much research tracing substance use over time, however, has targeted samples of younger individuals, and thus has the capability for studying these individuals not only after, but also before, intensive substance involvement has developed. Note that substance related disorders are highly prevalent, with the results of a 2014 national survey indicating that approximately 21.5 million individuals in the U.S. meet past-year criteria for SUD (SAMHSA, 2013). Thus, non-clinical samples have the capability to capture considerable variation in patterns of substance use.

We examine a variety of characteristics of these study populations in this review, including the gender composition of the sample, the racial composition of the sample, and their country of residence. Since prior research did not inform strong predictions concerning a potential role for these factors in moderating the attachment substance use link, we had no firm a priori predictions with respect to these parameters.

One study population characteristic that has received attention in prior work is participants' age. Prior reviews have suggested that the association between attachment and psychopathology may be larger among younger individuals, an effect that researchers suggest may be attributable to the greater salience of attachment relationships among younger children and to the increasingly diffuse nature of social networks as individuals age into adolescence and young adulthood (Madigan et al., 2016). On the basis of this prior work, we predicted that the association between attachment and substance use would be stronger among younger individuals and weaken with age.

Longitudinal Designs, Effect Size Estimates, and Meta-Analytic Methods

One important aim of the current review is to examine the question of temporal precedence in the association between attachment styles and substance use. In order to inform causal inferences, three necessary prerequisites must be met—covariation must be established, temporal precedence of the purported “cause” must be demonstrated, and all possible alternative explanations (e.g., third variables that could account for the effect) must be ruled out. Of note, only experimental designs, involving random assignment to manipulations, can be used to satisfy all three of these conditions and thus directly inform causal inferences. But not all research questions are well suited to experimental paradigms, and so researchers have often needed to rely on survey methods to examine associations. Longitudinal survey methods, which repeatedly

sample constructs over time, can be used to examine not only covariation, but also the key question of temporal precedence. Thus, although longitudinal survey methods cannot be used to establish causality, since third variable explanations cannot be ruled out using non-experimental longitudinal designs, they can inform the question of whether the purported cause comes before the effect, and thus help the researcher move one step closer to an understanding of causality.

In examining longitudinal associations, effect size statistics have been developed that are well suited to addressing issues of temporal precedence. Researchers have sometimes examined longitudinal associations using effects that we will refer to here as *prospective correlations* (Khazanov & Ruscio, 2016; Locascio, 1982; Sowislo & Orth, 2013). These prospective correlations simply quantify the association between two constructs measured at different time points. While prospective correlations offer important information about the extent to which a correlation is sustained over time, they cannot speak specifically to the issue of temporal precedence (the question of "which came first"). Thus, when applied to the current research questions, a significant prospective correlation between earlier attachment and later substance use does not exclusively suggest that attachment precedes substance use. Instead such prospective correlations may capture a significant cross-sectional correlation between attachment and substance use that is sustained via temporal stability (autocorrelation) in the measure of substance use (Locascio, 1982). Importantly, cross-lagged coefficients can be calculated that account for these cross-sectional associations as well as for autocorrelations, and can therefore better address temporal precedence in longitudinal associations. In our review, we examine both prospective correlations, which index the extent to which there is an association between attachment and substance use that is sustained over time, and also cross-lagged coefficients, which more directly address temporal precedence.

An important feature of the statistical methods employed in the current review is our use of meta-analytic models that account for clustered standard errors and thus allow for the estimation of effects on the basis of multiple effects sizes per study (Briley & Tucker-Drob, 2014; Cheung, 2014; Luhmann, Hofmann, Eid, & Lucas, 2012; Roberts et al., 2017). Traditional meta-analytic regression approaches do not permit the simultaneous consideration of both within- and between- study effects (e.g., Khazanov & Ruscio, 2016; Madigan et al., 2016; Sowislo & Orth, 2013). Within traditional meta analytic approaches, researchers are required to estimate only one effect size per study. Therefore, an examination of temporal precedence within such a between-study framework would compare those studies that have predicted later attachment from earlier substance use with those studies that have predicted later substance use from earlier attachment, and, where data allowed for the estimation of effects in both directions within the same study, only one of these could be selected for inclusion in the model. Because many factors vary across studies, not all of which would be feasible to code within the context of a meta-analysis, examinations of moderation effects at only the between-study level must necessarily include additional confounds. In contrast, meta-analytic models that allow for not only between- but also within-study comparisons have the advantage of being able to examine moderators while holding study-level variables constant. In the current analysis, we use a random-effects modeling approach both in the estimation of overall effect sizes and also in meta-regression models examining moderation effects (Luhmann et al., 2012; Roberts et al., 2017).

We should note that substance use is a complex, multi-determined behavior, involving both genetically and environmentally determined elements. As such, effect sizes for even well-established individual psychosocial risk factors may often emerge as small in magnitude (Hawkins et al., 1992). Thus, commensurate with effects observed in prior reviews examining

links between psychosocial constructs and health-related behaviors (Bogg & Roberts, 2004), we anticipated that both prospective correlations and cross-lagged effects examining insecure earlier attachment as a predictor of later substance use would be relatively small in magnitude. In contrast, in light of inconsistent prior findings (e.g., Leonard & Roberts, 1998a), we had no specific predictions concerning the size of the inverse effect—earlier substance use as a predictor of later attachment.

The Current Review

Our review had several aims. First, we aimed to determine whether there exists a significant cross-sectional association between interpersonal attachment and substance use and, further, to estimate its size. Second, we investigated whether there exists a significant prospective correlation between attachment and substance use, such that attachment measured at an earlier point in time predicts substance use measured at a later point in time. We further estimated the size of this prospective correlation relative to the inverse pathway, in which substance use is examined earlier and attachment later. Third, we examined whether there exists a significant cross-lagged association between attachment and substance use—in other words, whether attachment forecasts changes in substance use once autocorrelation has been parsed. We further estimated the size of this effect relative to the inverse cross-lagged association—earlier substance use to later attachment. Finally, we evaluated moderators of the correlation between attachment and substance use, including characteristics of the substance use measure, characteristics of the attachment measure, and also characteristics of the study population.

Methods

We used the following strategies to search for relevant literature: 1) The databases PsycINFO and EBSCO[4] were searched according to the following parameters: ("*attachment*") AND ("*alcohol*" OR "*drinking*" OR "*substance*" OR "*addiction*" OR "*drug*" OR "*marijuana*" OR "*cannabis*" OR "*smoking*" OR "*tobacco*" OR "*nicotine*" OR "*cocaine*" OR "*opiate*" OR "*heroin*" OR "*illicit*." Methodological limits: "*Longitudinal Study"* OR "*Prospective Study."* Search terms were allowed to appear anywhere in the record, including the title, abstract, and keywords. Search limits were defined so as to include unpublished dissertations, a category of unpublished studies suggested to have important advantages for addressing publication bias (Ferguson & Brannick, 2012; McLeod & Weisz, 2004). 2) Once a study was identified as meeting inclusion criteria, all studies that had cited that study since its publication were reviewed for potential inclusion. Furthermore, the reference sections of all studies meeting inclusion criteria were scanned. 3) The reference sections of several recent reviews examining relationship quality/attachment and substance use were scanned (Becoña et al., 2014; Schindler et al., 2005; Visser, de Winter, & Reijneveld, 2012). The search included studies published prior to June 2016. A total of 4,297 abstracts were scanned for potential inclusion in this review (see Figure 1 for PRISMA flow diagram).

The following inclusion criteria were used: 1) The study was required to directly assess substance use or substance problems. Substances could have included any substance of abuse including (but not necessarily limited to) alcohol, marijuana/cannabis, nicotine/tobacco, stimulants (e.g., cocaine/amphetamine/methamphetamine), opioids (e.g., heroin, prescription pain killers), inhalants, "club" drugs and hallucinogens (e.g., ecstasy, LSD), and

[4] EBSCO represents a "host" search platform for scanning multiple online databases simultaneously. We used the "academic search complete" option within EBSCO, which offers the most comprehensive database search available, spanning fields from medicine to the social sciences.

benzodiazepines. A variety of measures were included, ranging from those assessing the frequency and/or quantity of alcohol or drug use to those assessing drug/alcohol problems and counting DSM SUD symptoms. Note that, at many points in this review, we will refer to all of these outcomes collectively as "substance use." Studies were excluded if they assessed broader constructs (e.g., externalizing behavior, delinquency) of which substance problems might be one sub-component, but substance use outcomes were not examined independently. Studies assessing substance use in women during pregnancy, and not during any other period, were excluded. Studies adopting a dichotomous categorization of SUD or no SUD (e.g., according to DSM criteria) were also included, although these studies tended to be relatively scarce. 2) The study was required to employ at least one measure identified as assessing interpersonal attachment. A range of attachment figures were acceptable, including parents and family, romantic figures, and close friends. Measures were not included if they asked participants to report attachment to a diffuse group of targets (e.g., "peers" or "friends") (see Madigan et al., 2016). [5] We only included questionnaire attachment measures in which the participant him/herself reported on the security of the attachment relationship; 3) The study was required to be longitudinal or prospective in nature, and to include measures that would allow for the estimation of at least one longitudinal effect size—either earlier substance use predicting later attachment, or earlier attachment predicting later substance use. No age restrictions were placed on participants. See Figure 1 for specific reasons for study exclusion at each stage of the search process.

Study characteristics were coded by both the first author and a research assistant. The following characteristics were coded: 1) sample size; 2) year of publication; 3) country in which data was collected; 4) age of participants at study initiation; 5) time lag between attachment and

[5] Four studies were excluded for assessing attachment to diffuse groups of targets. See Figure 1.

substance use measures, in months; 6) overall racial composition of participants; 7) specific drug type examined (e.g., alcohol, nicotine, …); 8) pattern of substance use examined (e.g., frequency, quantity, problems/SUD symptoms); 9) specific attachment figure identified; 10) insecure attachment dimension assessed (general insecurity, anxious insecurity, or avoidant insecurity); 11) specific type of attachment measure employed, differentiating interviews from questionnaire, as well as among the different sub-categories of questionnaire measures. Where we had access to N's for each individual effect size (e.g., where we had access to author-supplied correlation matrices or raw data), sample sizes were coded individually for each within-study bivariate association. Interrater agreement was excellent for both continuous and categorical variables, with an average intraclass correlation (ICC) across coders of .996 for variables coded on a continuous scale (range, .98-1.00) and an average Cohen's kappa of 1.00 for variables coded categorically. Disagreements were resolved by discussion. Effect sizes were coded by the first and second authors, and a randomly chosen subset of 182 effect sizes was re-coded by research assistants. Interrater agreement was also excellent for effect sizes (*ICC*=1.00)

In calculating effect sizes, we aimed to ensure that the statistic derived from each study was comparable. Studies often varied dramatically in the nature of the longitudinal analyses conducted, the means by which change over time was assessed and, of importance, the covariates included in longitudinal models. In an effort to ensure that variation in effect sizes across studies reflected interpretable differences and not, for example, different choices of combinations of covariates or in the choice of analytic strategy/trajectory analysis, we calculated effect sizes based on raw bivariate relations/correlations (although see also footnote 6). In the case of 13 articles, raw correlation matrices, or other raw bivariate indexes, were included in the article itself. Where more than one article reported on the same sample of individuals, and/or when

multiple effect sizes could be derived from a single article, all of these effect sizes were examined in analyses (see below for analytic approach, and Table 1 for sample descriptions). Where bivariate indexes were not published, authors were contacted. A total of 15 correlation matrices were obtained from correspondence with authors. In a handful of cases where Structural Equation Modeling was employed and sufficient information was supplied, the model-implied correlation matrix was imputed based on the values of path parameters supplied in the research report (3 studies). Finally, in the case of 3 studies, datasets were publicly available, and correlation matrices were derived directly from these datasets. Effect sizes were estimated as correlation coefficients or *r* values. To ensure comparability of effects across studies, all effects were recorded such that higher levels of attachment security were reflected in higher scores on the attachment scale—when measures indexed attachment insecurity, such that raw scores increased as level of attachment insecurity increased (e.g., anxiety subscale within the AAS; Collins & Read, 1990), effect sizes based on these measures were inverted. Thus, across all studies, effect sizes were coded in a manner such that a negative correlation indicates that substance use increases as attachment security decreases.

Data Analysis

Effect Size Calculations: Primary analyses for this meta-analysis are based on 7 types of effect size estimates: (1) the cross-sectional correlation between attachment and substance use; (2) the prospective correlation between attachment at time 1 and substance use at time 2, (3) the prospective correlation between substance use at time 1 and attachment at time 2, (4) the cross-lagged association between attachment at time 1 and substance use at time 2 (controlled for substance use at time 1), (5) the cross-lagged association between substance use at time 1 and attachment at time 2 (controlled for attachment at time 1), (6) the autocorrelation between

attachment at time 1 and attachment at time 2, and (7) the autocorrelation between substance use at time 1 and substance use a time 2. Cross-lagged associations were estimated as semipartial correlations (see below). Note that, for most samples included in the meta-analysis, sufficient data were not available to calculate all of these statistics. Samples were included as long as sufficient data could be obtained for the calculation of at least one prospective correlation (see also inclusion criteria listed above).

Cross-lagged associations ($r_{C.L.}$) were calculated as semipartial correlations according to the formula presented by Cohen, Cohen, West, and Aiken (2003) (see also Aloe, 2014 for recommendations on application to meta-analysis).

$$r_{C.L.} = \frac{r_{Y1} - r_{Y2} r_{12}}{\sqrt{1 - r_{12}^2}} \quad (1)$$

In Equation 1, $r_{C.L.}$ represents the correlation between X_1 and Y_2, adjusting for Y_1 (e.g., the effect of attachment at Time 1 on substance use at Time 2, adjusting for substance use at Time 1). The prospective correlation between the predictor [X_1] and the outcome variable [Y_2] is r_{Y1} (e.g., the association between attachment at Time 1 and substance use at Time 2). The autocorrelation between the outcome variable and itself over time is represented by r_{Y2} (e.g., the correlation between substance use at Time 1 and substance use at Time 2). The cross-sectional correlation between the two predictors, X_1 and Y_1, is r_{12} (e.g., the correlation of attachment at Time 1 with substance use at Time 1). Note that this same equation was used to calculate the semipartial correlation examining earlier attachment as a predictor of later substance use, controlling for earlier substance use, as well as the inverse semipartial correlation examining earlier substance use as a predictor of later attachment, controlling for earlier attachment. The variance of cross-sectional correlations (including model-implied correlations), prospective correlations, and autocorrelations was calculated based on standard formulas for the variance of correlations as

employed in CMA software, and the variance of cross-lagged associations was calculated based on formulas for the variance of semipartial correlations as provided in Aloe (2014).

***Meta-Regression Models*:** We used the general framework laid out by Cheung (2008) for random-effects meta-analysis using structural equation modeling. Random-effects meta-analysis is generally considered the more robust and conservative meta-analytic approach compared with fixed-effects meta-analysis, which makes the assumption that all effect sizes are drawn from the same population (Hedges & Vevea, 1998). Our meta-analytic search strategy produced a dataset that included non-independent effect sizes. For example, in the event that information on both anxious and also avoidant attachment orientations was available for a single sample of participants, then effect sizes from both of these indexes would be entered into analyses, and our models would incorporate information from both effect sizes in estimating aggregate effects.

We used Mplus statistical software (Muthén & Muthén, 1998) to fit a meta-analytic structural equation model that incorporated clustered standard errors, accounting for the clustering of multiple effect sizes within samples (see supplemental materials for MPlus code used in analysis). Clustered standard errors correct for non-independence of observations (see McNeish et al., 2017 for a general description of the technique, and; Cheung, 2014 for a somewhat similar meta-analytic approach). Effects were weighted by the inverse of the sampling variance multiplied by the inverse of the number of effect sizes derived per sample. In contrast to typical weighting schemes that only incorporate sampling variance, the inclusion of the number of effect sizes derived per sample functions solely to *down*-weight effect sizes from samples that provide many effect sizes. This correction, along with the use of clustered standard errors, ensures that our meta-analytic estimates and confidence intervals are robust and that our results are not simply driven by samples that provide many effect sizes. The benefit of this analytic

approach is that we were able to incorporate all available information from each study and test whether effect sizes differ across a variety of moderators. In contrast to multi-level modeling approaches to correcting for nonindependence (e.g., Cheung, 2014), the use of clustered standard errors does not separate within- and between-sample variance. Therefore, we do not (and cannot based on our analyses) interpret our results as reflecting within-sample differences in effect sizes. Power to detect such effects is likely low in the current context, in part because coverage across moderators was inconsistent in the included samples. Further, we do not quantify the amount of heterogeneity that occurs at the within- or between-levels. Instead, our results represent aggregated effects (Muthen & Satorra, 1995).

We examined variance in effect sizes using τ, representing heterogeneity across effect sizes. In models examining directionality as a moderator of effect size (e.g., attachment predicting later substance use OR substance use predicting later attachment), we included only those samples for which we were able to estimate longitudinal effects in both directions within the same sample. We did this in order to ensure that the effects being compared were comparable along other key characteristics aside from directionality (e.g., age of participants, time lag between assessments, type of attachment measure, sample population). In these models, effects in both directions were included within the same dataset, and then the direction of effects was examined as a moderator of effect size within meta-regression models.

***Cluster Size and Critical Values*:** The utility of corrections for nonindependence depends on the number of clusters (i.e., the number of samples that provide effect sizes) and the number of observations contributed by each cluster (i.e., the number of effect sizes derived from each sample). Corrections are more effective as both values increase. For example, Hedges, Tipton, and Johnson (2010) used simulation to demonstrate the amount of bias in meta-analysis given

differing cluster sizes. When the number of clusters is relatively low (~10) and with relatively few effect sizes per sample, confidence intervals are too narrow. Tanner-Smith and Tipton (2014) suggested adjusting critical p-values to account for this bias. In the worst-case scenario, they suggested that estimated p-values are approximately half as large as they should be. Therefore, we set our critical p-value threshold at $p < .025$ and only interpret effects that meet this level as statistically significant. The current meta-analytic dataset contains many more clusters and effect sizes per cluster (ranging from 2.8 to 6.4 depending on effect size type) than the worst-case scenario, meaning that this correction is conservative.

***Publication Bias*:** Since no single approach can adequately assess publication bias, we addressed this issue using multiple methods (Borenstein, Hedges, Higgins, & Rothstein, 2009; Sutton, 2009). First, since unpublished dissertations were included in our sample (see above), we explicitly examined publication status as a moderator in our meta-regression models to determine whether effect sizes differed across published and unpublished samples (Sowislo & Orth, 2013). Second, we visually inspected funnel plots of the data. Funnel plots depict the effect size for each sample against its standard error. When publication bias is not present, such plots approximate the shape of a funnel, with larger samples clustered around the average effect size at the top of the graph, and smaller samples more spread out along the bottom of the graph. In the presence of publication bias, the bottom of the plot appears asymmetrical (Sutton, 2009). Finally, we extended our random-effects meta-analysis to include the squared standard error (the PEESE model following Stanley & Doucouliagos, 2014). Here, our approach involved two steps. First, we entered the square of the standard error as a predictor in an otherwise empty meta-regression model, an approach that allows for an examination of associations between sample size and effect size. Second, we entered the square of the standard error into all moderator models, and

examined the extent to which moderation effects changed after variance associated with standard errors were accounted for (Stanley & Doucouliagos, 2014).

Results

Description of Sample: Our search of the literature produced 34 independent samples meeting inclusion criteria, including a total of 56,721 individual participants. A total of 665 effect sizes were derived from these samples—65 cross-sectional correlations, 182 prospective correlations with T1 attachment predicting T2 substance use, 115 prospective correlations with T1 substance use predicting T2 attachment, 105 cross-lagged associations with T1 attachment predicting T2 substance use, 70 cross-lagged associations with T1 substance use predicting T2 attachment, 49 attachment autocorrelations, and 79 substance use autocorrelations. Of our 34 samples, 76% were collected in the USA, 3% in Canada, 9% in Europe, 6% in New Zealand, and 6% in Australia. Within published samples, publication dates ranged from 1986-2015. Nine percent of samples were unpublished. The average age of participants at sampling initiation was 15.4, with a range from 7.5 to 30.8 years. The average length of longitudinal follow-up was 3.8 years, but ranged from 1 month to 20 years. We were able to calculate at least one prospective correlation for all samples (see inclusion criteria). With respect to 62% of samples (k=21, total N=42,178) sufficient information was provided such that we were able to calculate at least one cross-lagged effect, and, with respect to 50% of samples we were able to calculate prospective correlations in both directions within the same sample of individuals—i.e., earlier attachment as a predictor of later substance use, as well as earlier substance use as a predictor of later attachment. Table 1 contains descriptive information about samples and sample characteristics, and a complete table of the 665 effect sizes can be found in supplemental materials.

Initial Analyses: Initial analyses revealed that there existed significant heterogeneity in the size of effects across all 7 effect types (all *p*'s <.025, see Table 2). With respect to the examination of publication bias, our dataset contained 86 effect sizes derived from 3 unpublished samples and 579 effect sizes derived from 31 published samples (see also descriptives provided above and Table 1 for N's subdivided by effect size type). Visual inspection of funnel plots indicated that effects were reasonably evenly distributed around the mean (Figure S1). For the majority of effect types, there was no evidence that either publication status (published vs. not) or that the standard error of effect sizes (PEESE models) significantly moderated the magnitude of effects (see Table 2). Models examining earlier substance use as a predictor of later attachment (both prospective correlations and cross-lagged effects) indicated that unpublished samples and samples with smaller standard errors actually had *larger* (more negative) effects than published samples and samples with larger standard errors. One model examining earlier attachment as a predictor of later substance use (cross-lagged effect model) indicated that unpublished samples tended to have *smaller* effects than published samples. Although evidence of publication bias in these initial analyses tended to be modest, we re-ran all of the models below incorporating standard error as a covariate (Stanley & Doucouliagos, 2014). We also include discussion of how publication bias might have impacted our primary models below.

Cross-Sectional Effects: Our first aim was to examine whether a significant cross-sectional correlation existed between interpersonal attachment and substance use. Cross sectional correlations were estimated based on a total of 65 effect sizes derived from 23 independent samples (total N=44,149). Findings revealed a significant cross-sectional correlation, r_{12}=-.16, 95% *CI*=-0.19 to -0.12 (See Figure 2). This finding indicates that, when substance use and

attachment were measured at the same point in time, people with less secure attachment relationships are more likely to engage in substance use.

Temporal Stability: Next we examined the temporal stability (assessed using the autocorrelation) of measures of attachment and measures of substance use. Substance use autocorrelations were estimated based on 79 effect sizes drawn from 21 independent samples (total N=41,691), and attachment autocorrelations were estimated based on 49 effect sizes drawn from 15 independent samples (total N=29,144). With an average time lag between assessments of 3.8 years (see descriptive statistics above), the autocorrelation of both substance use and also interpersonal attachment measures was moderate in magnitude. The autocorrelation for measures of interpersonal attachment, r_{Y2}= 0.53, 95% *CI*=.46 to .60, was larger than the autocorrelation for substance use measures, r_{Y2}= 0.39, 95% *CI*=.31 to .47. The difference between the autocorrelation for these two measure types was significant, *b*=-0.12[6], *p*=.003.

Prospective Correlations: Next, we examined whether attachment security measured at an earlier point in time was associated with substance use measured at a later point in time, estimates that were based on a total of 182 effect sizes derived from 33 independent samples (total N= 56,542). We found a significant prospective correlation between earlier attachment and later substance use, r_{Y1}=-.11, 95% *CI*=-.14 to -0.08. These analyses suggested that individuals with lower levels of attachment security are more likely to later use substances.[7] The strength of

[6] The specific effect size estimates for the individual group analyses may differ very slightly from estimates derived from models where groups are compared due to small differences in the weighting variable EffN (See attached code).

[7] The substance use of close others has been shown to correlate with an individual's own substance use. While a variety of mechanisms have been posited to explain such links—ranging from modelling to, in the case of family relationships, biological mechanisms—the security of attachment relationships has emerged as one factor that might explain the association (Vungkhanching, Sher, Jackson, & Parra, 2004). For example, children of alcoholic parents may themselves be vulnerable to drinking, at least in part, because of stress associated with low quality parental relationships (Leonard & Eiden, 2007). While we were not in a position to address questions of mechanism directly within this meta-analysis, we were in a position to examine (within a subset of samples) the extent to which

the cross-sectional correlation between attachment security and substance use was larger than the prospective correlation, *b*=-0.08, *p*<0.001. Within longitudinal samples, however, the magnitude of the time lag between assessments (indexed in months) was not significantly related to the size of prospective correlations, *b*=.006, *p*=.634.

Next, we examined the inverted prospective correlation, exploring whether substance use measured at an earlier point in time significantly predicted attachment measured at a later point in time. Estimates here were based on a total of 115 effect sizes derived from 18 independent samples (total N= 31,219), and indicated a significant prospective correlation between earlier substance use and later attachment, r_{Y1}=-.08, 95% *CI*=-.11 to -0.05.

Next, we directly compared the magnitude of the prospective correlation examining earlier attachment as a predictor of later substance use to the magnitude of the inverted correlation, in which earlier substance use predicts later attachment. We compared the size of these correlations using meta-regression models in which the direction of effects was entered as a moderator. We focused on those samples for which is it was possible to estimate effects in both directions within the same sample (see data analysis plan), a dataset that included a total of 224 effect sizes estimated based on 17 samples (total N=31,040, see also descriptive statistics above). Importantly, given the stronger temporal stability of measures of attachment vs. measures of

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attachment predicted substance use above and beyond the effects of the attachment figure's own substance use: a) One of the samples included in our meta-analysis examined attachment and substance use in a sample of sons of alcoholics (Zhai, 2015). Even within this sample of participants—where the substance use of attachment figures was, in relative terms, held constant—a significant relationship between attachment and participants' own substance use still emerged, *r*=.19, *p*<.01; b) Of importance, with respect to 5 of our samples (total N=17,195 participants), we had access to correlation matrices that included measures of the attachment figures' substance use/SUD status. On the basis of these matrices, we were able to calculate semipartial correlations (see Equation 1) between attachment and substance use that removed variance associated with the substance use of the attachment figures. We then conducted a meta-analysis of these semipartial correlations. The prospective correlation between attachment and substance use remained significant after accounting for variance associated with the attachment figures' use and, in fact changed very little compared with the raw prospective correlations (*r*=-.115, *95%CI* -0.141 to -0.089 for semipartial prospective correlations parsing the attachment figure's use, *r*=-.123, *95%CI* -0.151 to -.095 for raw prospective correlations in this same subsample).

substance use, we might have anticipated that prospective correlations between earlier substance use and later attachment would have been the larger correlation, if only as an artifact of the stronger temporal stability for attachment measures. In other words, in light of the significant cross-sectional association between attachment and substance use and the fact that attachment patterns tend to remain more stable over time than do patterns of substance use (see above), we would have predicted a larger longitudinal correlation for substance use predicting later attachment. The data, however, reveal the opposite pattern of findings than might be expected based solely on these autocorrelations. Earlier attachment predicted later substance use, r_{Y1} = -.14, 95% *CI*=-.17 to -.11, to a significantly greater extent than earlier substance use predicted later attachment, r_{Y1}= -.08, 95% *CI*=-.12 to -.04 (See Figure 2). The difference between these two effects was significant, *b* =.06, *p*=.004. Note that this estimate may represent a conservative estimate of the true difference in light of the specific patterns of publication bias observed in our data—published samples examining earlier substance use as a predictor of later attachment appeared to represent an overestimation of true effect sizes, whereas published samples examining earlier attachment as a predictor of later substance use appeared to represent an underrepresentation of effect sizes. Thus, despite stronger temporal stability in attachment measures, and despite some publication patterns that might tend to diminish the effect, the prospective correlation between earlier attachment and later substance use emerged as significantly stronger than the prospective correlation between earlier substance use and later attachment.

Cross-Lagged Associations: To further examine attachment as a predictor of later substance use, we next addressed temporal precedence. As detailed previously, we calculated semipartial correlations aimed at examining whether insecure attachment tended to temporally

precede later increases in substance use or, alternatively, whether prospective correlations might be best accounted for by cross-sectional associations that are sustained through temporal stability. We calculated cross-lagged associations, accounting for autocorrelations and cross-sectional effects (see Equation 1) and then entered all calculated effects into a random effects model. Importantly, based on 105 effect sizes derived from 20 samples (total N=41,512), results revealed a significant cross-lagged association between earlier attachment and later substance use, $r_{C.L.}$= -.05, 95% *CI*=-.06 to -.04. This finding suggests that insecure attachments may temporally precede increases in substance use, even after accounting for significant cross-sectional correlations and autocorrelations in measures of substance use. This association is similar in magnitude to cross-lagged associations observed in prior meta-analyses examining risk factors for psychological disorders (Khazanov & Ruscio, 2016; Sowislo & Orth, 2013).

Based on 70 effect sizes drawn from 15 independent samples (total N=29,144), we found no evidence of a cross-lagged association between earlier substance use and later attachment, $r_{C.L.}$= -.01, 95% *CI*=-.04 to .01. Results of analyses examining publication bias (Table 2, see also above) indicated that, if anything, this effect was likely to be an overestimation of the true effect.

Next, examining samples among which we were able to estimate effects in both directions within the same sample, we compared the sizes of the semipartial correlations of earlier attachment on later substance use and earlier substance use on later attachment. Analyses were based on 152 effect sizes derived from 15 independent samples (total N=29,144). As with prospective correlations, we observed that cross-lagged effects in which earlier attachment predicted later substance use, $r_{C.L.}$ = -.05, 95% *CI*=-.06 to -.05, were larger than cross-lagged effects in which earlier substance use predicted later attachment, $r_{C.L.}$ =-.02, 95% *CI*=-.04 to .01 (See Figure 2). The difference between these two effects was statistically significant, *b*=-.04,

p=.006. The cross-lagged effect of earlier substance use on later attachment did not reach statistical significance, *p*=.178. In sum, these analyses indicated that insecure attachments at earlier points in time predict increases in substance use at later points in time. Moreover, these results reveal that the cross-lagged association between earlier attachment and later substance use was significantly larger than the cross-lagged association between earlier substance use and later attachment.

Moderators of Attachment Substance Use Correlations: Next, we examined, using meta-analytic regression analyses, the extent to which the association between attachment and substance use varies according to characteristics of the substance use measure, the attachment measure, and characteristics of the sample population. Moderators were first examined in separate models (see Table 3 and Table S1). Where moderation effects reached significance in these initial models, and where other factors covaried with these moderators (e.g., average age of participants and type of attachment measure administered to them) then we examined whether effects would replicate when both/all moderators were included in the same model together. Consistent with prior attachment meta-analyses (Madigan et al., 2016), we focused on raw correlations. We chose to conduct moderation analyses using raw correlations because only a subset of samples (see above) provided sufficient information for the calculation of cross-lagged effects, and therefore, with respect to several of our categorical moderators, we lacked sufficient power to conduct meaningful moderation analyses of cross-lagged associations. Further, estimates of temporal precedence (cross-lagged effects) are necessarily highly dependent on the time period over which outcomes are allowed to change, and time lags were not held constant across levels of our moderators, as they were in our previous examination of temporal precedence. Thus, moderation analyses of cross-lagged effects were less readily interpretable.

Again, consistent with prior reviews (Madigan et al., 2016), we examine moderators in a single dataset that combines cross-sectional and prospective correlations, including 247 effect sizes estimated based on all 34 samples. Separate analyses were also conducted examining cross-sectional and prospective correlations individually, and any deviations are noted below and also reported in Table S1.

We present results in two complementary ways. First, Table 3 presents effect size estimates based on meta-regression models. These effect sizes are derived from the model-implied meta-regression equation, and the reported confidence intervals are useful for determining whether an effect size is different from zero. Second, a supplemental table reports the regression parameters from our models (Table S1). Because we use effects coding for all (non-continuous) analyses, the regression parameters represent deviations from the midpoint. Conceptually, these parameters indicate whether effect sizes within a specific moderator class (e.g., Drug Type) differ from the expected average of general effect sizes within that moderator. In this context, the reported confidence intervals are useful for determining whether a specific moderator class differs from the expected average, rather than zero.

Participant Age. Analyses revealed a significant moderating influence of participant age on the association between attachment and substance use, b=.009, p=.001, with the association between substance use and attachment weakening as the average age of samples increased. Simple contrasts in the continuous age variable were examined by centering this variable at one standard deviation above and below the mean. For samples assessed beginning at the age of 11, the association between attachment and substance use was estimated as r = -0.16, 95% *CI*= -.20 to -0.13 whereas, for samples assessed beginning at the age of 20, the association between attachment and substance use was estimated as r = -0.08, 95% *CI*= -0.12 to -0.05. The

association between attachment and substance use weakened as the average age of samples increased. Effects emerged in the same direction across both longitudinal and cross-sectional correlations, although the effect did not reach significance when cross-sectional correlations were examined alone (see Table S1). Participant age did tend to be related to the attachment figure(s) identified by researchers in measures of attachment (e.g., parents vs. romantic partners), but the effect of age remained significant even after attachment figure was controlled, b=.008, p=.001.

Drug Type. We next examined drug type as a moderator of the association between attachment and substance use. We examined whether a significant association between attachment and substance use emerged across all classes of drugs, including alcohol, nicotine, marijuana, and other substances. Importantly, interpersonal attachment and substance use were significantly related across all drug types examined here (see Table 3). Effects coding indicated that the association between attachment and substance use tended to be particularly strong with respect to nicotine/tobacco use, b =-.03, p=.004—the correlation between attachment and substance use was stronger for nicotine than for other drug classes, an effect that was largely consistent across cross-sectional and longitudinal samples (see Table S1).

Substance Use Pattern. Indexes of substance use employed in our samples assessed a range of different substance use patterns, including substance use frequency, substance use quantity, and SUD symptoms/substance-related problems. Again, across all of these divergent measures, we observed consistent and significant associations between interpersonal attachment and substance use (see Table 3), with attachment emerging as a significant correlate of substance use frequency, substance use quantity, and also of substance-related problems. Moderation

analyses indicated that none of these effects differed significantly from others according to our corrected *p*-value (see Table S1 and data analysis plan).

Attachment Figure. Attachment security emerged as a significant predictor of substance use regardless of the attachment figure identified. Attachment to mother, father, family/parents (general), as well as attachment to romantic figures and close friends emerged as significant predictors of substance use (see Table 3). Moderation analyses indicated that none of these effects differed significantly from others (see Table S1).

Attachment Measures. Analyses comparing types of attachment measures revealed a significant association between attachment and substance use regardless of the type of attachment measure used (see Table 3). Again, moderation analyses indicated that none of these effects differed significantly from others according to our corrected *p*-value.

Insecure Subtype. Results indicated that indexes of the anxious subtype of insecurity tended to correlate with substance use to a lesser extent than indexes of avoidant insecurity or of general insecurity, b =.04, p= .001. Note, however, that attachment subtype was associated with the type of attachment measure used, and the effect of anxious insecurity did not remain significant after attachment measure type was controlled.

Other Moderators. In addition to the above moderators, we also examined the gender composition of the sample (%Female), racial composition, geographic region, and publication year as moderators of the attachment substance use association (See Table 3). Among these moderators, the only one that emerged as significant was publication year, b=.005, p=.016 (see Table 3). Consistent with other reviews, we found that the strength of the correlation between attachment and substance use was somewhat attenuated in reports that were published more recently (see Madigan et al., 2016 for similar results in examining attachment and externalizing

behaviors). For reports published in 2000, the association between attachment and substance use was estimated as r = -0.16, 95% *CI*= -.19 to -0.12 whereas, for reports published in 2012, the association between attachment and substance use was estimated as r = -0.10, 95% *CI*= -0.13 to -0.07.

Summary of PEESE Models

Note that all models indicated above were replicated within the PEESE framework, incorporating the square of the standard error as a covariate in analyses. Results of all models, including examinations of temporal precedence and also of moderation effects, remained unchanged within these PEESE models. This suggests that publication bias is unlikely to have had a major impact on the findings presented here.

Discussion

The purpose of this review was to synthesize the results of prospective studies on attachment and substance use. To do so, we used meta-regression to analyze 665 effect sizes derived from 34 longitudinal samples (a total of 56,721 individuals). Consistent with our hypotheses, we found a small but significant cross-sectional association between attachment and substance use, r=-.16, indicating that insecurely attached individuals engage in more substance use than securely attached individuals. An analysis of prospective correlations, focusing on earlier attachment as a predictor of later substance use, further indicated that the association between attachment and substance use endures over time, r=-.11. Finally, and importantly, we found evidence for temporal precedence in the attachment-substance use link, with significant cross-lagged effects, $r_{C.L.}$ =-.05, indicating that insecure attachments precede later increases in substance use.

Results of this meta-analysis have important implications for understanding links between substance use and relationship distress. Researchers have long-documented a robust association between substance use and relationship problems (Epstein & McCrady, 1998; Leonard & Eiden, 2007), and such correlations have often been attributed to the detrimental effects of substance use on close social bonds. But a number of researchers have noted that there is in fact scant empirical evidence for a causal effect of substance use in bringing about relationship distress (Leonard & Eiden, 2007; Marshal, 2003). Applying an attachment framework to the understanding of close relationships, the current review investigated the temporal ordering of substance use and close relationship functioning (i.e., the security of close relationships) across multiple longitudinal samples. Results revealed a significant cross-lagged effect of earlier attachment in predicting later changes in substance use. Analyses of these cross-lagged effects further revealed that the effect of earlier attachment in predicting later changes in substance use was significantly larger than the effect of earlier substance use in predicting later changes in attachment. Precisely the same pattern of effects revealed itself in an analysis of prospective correlations—with earlier attachment predicting later substance use to a greater extent than earlier substance use predicted later attachment. This finding is notable because it emerged despite the greater temporal stability of attachment measures vs. substance use measures. Although at odds with some conventional wisdom, the finding that earlier attachment predicts later substance use is consistent with theories that posit connections between systems governing attachment and those involved in substance use, and thus implicate insecure attachments as a possible predisposing factor for addiction (Burkett & Young, 2012; Insel, 2003).

These results suggest that interpersonal attachments could serve as a useful early indicator of risk for substance use and substance use problems. A variety of other factors have been identified as denoting vulnerability to problematic substance use. Many of these factors—e.g., anxiety disorders, substance using peers, risk taking/disinhibition—may not manifest, and/or are difficult to reliably measure, until adolescence or young adulthood, by which time heavy substance use itself has often already been initiated (Kessler et al., 2005). In contrast, attachment orientations become evident well before substance use begins, and the kinds of measures surveyed here can be used with children as well as adults. Thus, although insecure attachments represent only one among many potential risk factors for SUD, insecure attachments might nonetheless have utility for identifying those at risk at an early age and thus potentially for informing secondary prevention measures (e.g., Kivlahan, Marlatt, Fromme, Coppel, & Williams, 1990).

Of note, results of our review did not provide evidence that earlier substance use predicts later changes in attachment. While prospective correlations did reveal a significant longitudinal association between earlier substance use and later attachment—albeit one that was smaller in magnitude than the inverse pathway—we found that the cross-lagged coefficient examining this effect did not reach significance. In light of the positive effects revealed when we examined attachment as a predictor of later changes in substance use, this null effect offered an intriguing point of comparison, and it certainly seems to oppose traditional thinking that has sometimes treated the direction of causality in the attachment-substance use link as a foregone conclusion. We wish to emphasize, however, that the non-significant pathway from earlier substance use to later changes in attachment should not necessarily be accepted as evidence of the negative—i.e., as evidence that substance use does not exert a detrimental effect on close social bonds. Factors

worth noting here are the overall characteristics of our study populations and of the attachment relationships assessed in our review. The current review did include studies that spanned a variety of age groups, and also assessed various attachment relationships. Nonetheless, it should be noted that the bulk of our longitudinal studies assessed parental relationships and began in adolescence, a time of particular interest for understanding changes in substance use patterns. One possibility is that substance use might have a more detrimental effect on the close relationships of older individuals—where, for example, responsibilities tend to be distributed more evenly—than it has on bonds between young people and parents.

Moderators of the Attachment Substance Use Association

Our review also involved examination of a number of moderators of the association between attachment and substance use, including characteristics of the attachment measure, characteristics of the substance use measure, and characteristics of the study population. With respect to the substance use measure, we found evidence of a significant link between insecure attachment orientation and substance use across several drug classes, including alcohol, nicotine, and marijuana. We had not hypothesized one finding that emerged in these drug type analyses—the association between insecure attachment and substance use was especially strong for nicotine/tobacco as compared with other drug classes. Although the current review cannot speak to the reasons for this effect, researchers have sometimes argued that nicotine, as compared with other substances, has particularly potent and targeted effects on negative emotions, such as stress and anxiety (Brandon, 1994; Kassel et al., 2003). One of the primary functions of attachment relationships is the regulation of negative affect (Cooper, Shaver, & Collins, 1998). Individuals whose affective regulatory needs are not fulfilled within the context of attachment relationships

may find relief for their negative feelings through nicotine use. However, such questions of mechanism are left for future research to examine.

We further examined a range of characteristics of the attachment measures as potential moderators of effects. Here, we tended to observe more commonalities than differences. We found a consistent association between insecure attachment and substance use regardless of the attachment figure (i.e., a mother, a father, the family/parental unit as a whole or a close friend or romantic partner) identified in attachment measures. Further, we found no significant effect of the type of attachment measure—e.g., AAI, IPPA, AAS—on the association between attachment and substance use. Finally, while we mainly focused on the general index of attachment security/insecurity, rather than specific insecure subtypes, we did examine whether authors' choice to examine general insecurity vs. anxious vs. avoidant subtypes moderated effects. Here, again, we found no significant differences across insecure subtype once potential confounds (e.g., measure type) were accounted for. With respect to some of these non-significant moderators, the number of samples offering information on specific subgroups or measures (e.g., the AAI, anxious and avoidant insecure attachment subtypes, attachment to close friend or romantic partners) tended to be quite low, and so power for the current analysis was limited. Future studies should examine these factors further.

With respect to the study population, consistent with prior reviews of attachment and psychopathology (Madigan et al., 2016), we found that participant age moderated the size of the association between attachment and substance use. The association between insecure attachment bonds and substance use weakened as participants aged. One possible explanation for this finding is the increasing size and diffuse nature of social networks as individuals age into adulthood, potentially decreasing reliance on individual attachment relationships (see Madigan et

al., 2016). We also examined a range of other characteristics of study populations, including the gender composition of the sample and also its racial makeup. Note that, similar to many other reviews, we found non-significant effects here. Our analysis was limited by the extent to which these factors varied at the level of the study. Given that the overall composition of study populations was often reasonably similar across these dimensions (e.g., many studies included ~50% women) our power to detect these population-level differences was often limited. Examination of these factors may thus be best suited to empirical studies or to reviews that specifically examine the strength of these moderation effects.

Limitations and Future Directions

This review represents the first quantitative examination of the association between attachment orientations and substance use, and, although it points to a significant prospective association, such a result does not necessarily indicate a causal relationship. As noted earlier, only experimental designs may be used to infer causality. The results of this meta-analysis of longitudinal studies do indicate that insecure attachment relationships may temporally precede substance use, but they cannot rule out the possibility that a third variable associated with both attachment and substance use explains this link.

The current review is also not capable of addressing the mechanism that explains the association between attachment orientation and substance use. It may be that, as suggested by some researchers, insecure attachment relationships lead to negative views of the self, and individuals with these negative self-views disproportionately turn to substances, some of which have the ability to impair negative self-related cognitions (Hull, 1987; Kassel et al., 2003). In contrast, results may be explained by affective mechanisms: In the absence of a secure attachment relationship, insecure individuals may look disproportionately to substances to

alleviate anxiety. It's also possible that, in light of the social-cohesive functions of drugs of abuse, people may turn to substances specifically to fulfill social needs and form social relationships when these needs have been left unmet within existing attachment relationships (e.g., Fairbairn & Cranford, 2016; Fairbairn & Testa, 2016). It also remains possible, as has been suggested by some researchers, that strong attachment relationships protect against deviant behaviors simply by promoting identification with conventional values (Hirschi, 1969). [Note, however, that results presented in footnote 6, indicating that associations remain significant even after the substance using behavior of the attachment figure is controlled, are somewhat at odds with this final framework.] Through establishing a prospective link between attachment and substance use, this meta-analysis indicates a variety of potentially promising lines of research into possible mechanisms underlying this link.

Another potential limitation of our meta-analysis is that we focused mainly on self-report measures of attachment. As noted previously, questionnaire based measures are widely-implemented and have demonstrated clinical utility (Madigan et al., 2016). Moreover, self-report measures of attachment have been shown to predict a wide array of outcomes, both self-reported and not (e.g., behavioral, cognitive, physiological) in social, personality, developmental, and clinical psychology (Gillath et al., 2016). Although we did not have a theoretical reason for focusing on self-report measures in particular, longitudinal studies using self-reported measures of attachment are much more common than other types. [Note that behavioral measures, which are only administered during early childhood and not at ages during which substance use might have begun, were also not well suited to addressing questions of temporal precedence in the attachment substance use link.] While several of our studies did employ interview-based measures, we found that such measures were relatively rare in longitudinal studies of substance

use and attachment. None of our studies employed behavioral measures of attachment. So this review is best suited to answer the question of whether substance use relates to self-report measures of attachment, and a firm answer regarding the relation between interview measures of attachment is left for future research to examine.

Another factor worth addressing is our decision to focus on measures of attachment, vs. other measures of relationship quality, as a means of understanding close social relationships. We chose to focus on attachment for a variety of reasons. Most importantly, theories of attachment have often been the subject of particular interest in addiction research, given common underlying mechanisms proposed for attachment and SUD (e.g., self-concepts, emotion regulation) (Burkett & Young, 2012; Insel, 2003; Kassel et al., 2007; Solomon, 1980). Moreover, attachment patterns are thought to reflect selection and socialization processes. As such, they have the potential to be relatively stable across time given that insecurity tends to be self-sustaining, but they are also open to revision in light of changes in people's interpersonal experiences (Fraley, 2002). Finally, studies of attachment are particularly well suited for capturing the initial onset of substance use, since attachment measures have been validated for children and adolescents and have thus been widely administered to younger populations of individuals (Ainsworth, Blehar, Waters, & Wall, 1978; Armsden & Greenberg, 1987). In contrast, measures of relationship quality per se tend to be administered in adulthood, and can be quite diffuse in the relationship-related constructs they encompass, with some solely examining subjective feelings of relationship satisfaction while others include indexes of objective relationship characteristics (Fincham & Rogge, 2010; see also discussion of this point in footnote 3). Thus, in this initial quantitative analysis of relationship factors and substance use, we chose to focus on attachment in particular. Nonetheless, future research might do well to expand from this

focus on attachment and to examine substance use in relation to a variety of different indexes of close relationship quality and functioning, such as relationship commitment, satisfaction, and conflict.

Finally, a few statistical limitations are worth mentioning. For three studies in the current meta-analysis we constructed model-implied correlations, as we did not have direct access to raw correlations. In calculating the variance of these model-implied correlations, we used the same formulas as for raw correlations, but the sampling properties of these model-implied correlations are not the same as for raw correlations. For some analyses, we had relatively little data coverage resulting in two important limitations. First, power to detect some moderator effects was likely low. Second, corrections for nonindependence rely on large numbers of clusters to perform successfully. The best remedy for these limitations is continued work in this area so that future meta-analyses can further investigate these topics with greater accuracy.

Summary

Novelists, poets, and song-writers have long remarked upon subjective similarities between close relationship processes and addictions. Researchers have added the weight of scientific theory to these observations, proposing models that indicate overlap between systems underlying interpersonal attachment and those involved in substance use. Here, for the first time, we quantify the association between attachment and substance use, and further trace its emergence over time, indicating attachment orientation as a risk factor for substance use and offering promising directions for research into mechanisms and moderators in this association.

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Table 1. Longitudinal studies examining the relationship between attachment security and substance use

Citation(s)	**Dataset Name/Description**	**Sample Size**	**N Effect Sizes**	**Age**	**Time Span**	**Substance Use Measure**	**Attachment Measure**
Dornbusch, Erickson, Laird, & Wong, 2001[ς]	National Longitudinal Study of Adolescent Health	13,568	49	14.3	12	ALC, MJ, NIC (Freq, Prob)	FAM (Other)
Allen, Porter, McFarland, Marsh, & McElhaney, 2005	Longitudinal investigation of adolescent social development	179	3	13.4	12	SUB (Mix)	FAM (AAI)
Branstetter, Furman, & Cottrell, 2009	Longitudinal study of romantic relationship development	199	7	15.3	24	SUB (Mix)	FAM (AAI)
Brook, Lee, Finch, & Brown, 2012	East Harlem longitudinal study	390	26	16.7	132	MJ, NIC (Freq, Mix)	FAM, DAD, MOM (Other)
Cerdá et al., 2014; Rosario et al., 2014	Growing Up Today Study	7,746	11	20.8	24	ALC, MJ, NIC, SUB (Freq, Mix)	MOM (Other)
Chan et al., 2013	Longitudinal school survey in Australia	808	25	11.0	60	ALC (Freq)	FAM (Other)
Danielsson, Romelsjö, & Tengström, 2011	Longitudinal school survey in Stockholm	1,222	13	13.0	24	ALC (Mix)	FAM (IPPA)
De La Rosa et al., 2015	Longitudinal study of adult Latina mother-daughter dyads	266	3	27.0	72	ALC (Mix)	MOM (IPPA)
Erickson, Crosnoe, & Dornbusch, 2000	Longitudinal Wisconsin-California high school study	2,000	7	14.9	12	SUB (Freq)	FAM, OTH (Other)
Fallu et al., 2010	Longitudinal study of low SES boys in Quebec	764	19	12.0	36	SUB (Mix)	FAM (Other)
Fleming, Kim, Harachi, & Catalano, 2002	Raising Healthy Children Project	810	1	7.5	48	NIC (Freq)	FAM (Other)
Foshee & Bauman, 1994	Longitudinal study of metropolitan adolescents	675	1	13.0	24	NIC (Mix)	FAM (Other)

Table 1. Longitudinal studies examining the relationship between attachment security and substance use

Citation(s)	**Dataset Name/Description**	**Sample Size**	**N Effect Sizes**	**Age**	**Time Span**	**Substance Use Measure**	**Attachment Measure**
Golder, Gillmore, Spieker, & Morrison, 2005	Longitudinal study of pregnant and parenting teenagers.	232	4	21.5	6	ALC, SUB (Freq)	FAM (AAS)
(Harakeh, Scholte, Vermulst, de Vries, & Engels, 2004; Kuntsche, van der Vorst, & Engels, 2009; Van der Vorst, Engels, Meeus, & Deković, 2006)	Longitudinal school survey in the Netherlands	1,070	52	12.5	24	ALC, NIC (Freq, Qant, Mix, Prob)	FAM, DAD, MOM (IPPA)
Henry, 2008; Henry, Oetting, & Slater, 2009	Longitudinal prevention study in U.S. middle schoolers	1,065	22	12.1	24	ALC, MJ, NIC (Mix)	FAM (Other)
Hoffmann, Cerbone, & Su, 2000	Family Health Study	651	19	12.5	36	SUB (Freq)	FAM (Other)
(Kassel et al., 2007)	University of Illinois college student survey	212	9	20.3	2	ALC, MJ, NIC (Freq)	OTH (AAS)
Krishnan, 2007	Suburban high school survey in northeastern U.S.	185	64	15.0	36	ALC, MJ (Freq)	DAD, MOM (IPPA)
Lac, Crano, Berger, & Alvaro, 2013	Longitudinal college student survey	351	6	18.7	1	ALC (Freq)	MOM (IPPA)
Longshore, Chang, Hsieh, & Messina, 2004	Treatment Alternatives to Street Crime (TASC) Study	1,036	3	30.8	48	SUB (Freq)	FAM (Other)
Mason & Windle, 2002	Longitudinal study of adolescents in New York	696	13	15.9	12	ALC (Mix)	OTH (Other)
Massey & Krohn, 1986	Longitudinal study of Midwestern high schoolers	1,065	8	13.5	36	NIC (Freq, Quant)	DAD, MOM (Other)
McCann, Perra, McLaughlin, McCartan, & Higgins, 2016	Belfast Youth Development Study	4,937	19	11.0	48	ALC (Freq)	FAM (IPPA)

Table 1. Longitudinal studies examining the relationship between attachment security and substance use

Citation(s)	**Dataset Name/Description**	**Sample Size**	**N Effect Sizes**	**Age**	**Time Span**	**Substance Use Measure**	**Attachment Measure**
Droomers, Schrijvers, Casswell, & Mackenbach, 2003; McGee, Williams, Poulton, & Moffitt, 2000; Stanton, Flay, Colder, & Mehta, 2004	Dunedin Multidisciplinary Health and Development Study	929	19	15.0	36	ALC, MJ, NIC (Freq)	FAM (IPPA)
Olsson et al., 2005, 2013	Victorian Adolescent Health Cohort Study	1,030	54	15.5	240	ALC, MJ, NIC (Mix)	OTH (AAS)
Crawford & Novak, 2002, 2008	National Education Longitudinal Survey	8,866	8	14.0	24	ALC (Freq, Mix)	FAM (Other)
Ford, 2005; Maume, Ousey, & Beaver, 2005; T. Q. Miller & Volk, 2002	National Youth Survey	1,485	31	17.1	48.0	MJ, SUB (Freq)	FAM, OTH (Other)
T. G. Power, Stewart, Hughes, & Arbona, 2005	Longitudinal study of metropolitan high schoolers	882	67	16.0	18.0	ALC (Mix)	DAD, MOM (Other)
Raudino, Fergusson, & Horwood, 2013; Wells, Horwood, & Fergusson, 2004	Christchurch Health and Development Study	924	35	15.0	180.0	ALC, MJ, NIC, SUB (Freq, Quant, Prob)	FAM (IPPA)
Seffrin, 2009	Toledo Adolescent Relationships Study	1,066	19	15.0	60.0	SUB (Freq)	FAM (Other)
Skinner, Haggerty, & Catalano, 2009	Parents Who Care Longitudinal Study	292	37	13.7	76.0	ALC, MJ, NIC (Freq, Mix)	FAM (IPPA)
Tyler, Stone, & Bersani, 2007	National Longitudinal Survey of Youth	244	2	15.0	48.0	ALC (Mix)	MOM (Other)
Williams, Ayers, Abbott, Hawkins, & Catalano, 1999	Seattle Social Development Project	567	6	12.5	24.0	ALC, MJ, SUB (Freq, Prob)	FAM (Other)

Table 1. Longitudinal studies examining the relationship between attachment security and substance use

Citation(s)	Dataset Name/Description	Sample Size	N Effect Sizes	Age	Time Span	Substance Use Measure	Attachment Measure
Zhai, 2015	Longitudinal study of sons of fathers with SUD	309	3	11.0	132.0	SUB (Prob)	FAM, DAD, MOM (Other)

The information listed in this table does not necessarily reflect all of the information available in the identified dataset, but rather the data that we had access to for the purposes of analysis.

"Sample size" is a conservative estimate, and reflects the smallest sample size represented in bivariate longitudinal relationships used in analysis. "N Effect Sizes" reflects the total number of effect sizes we were able to derive and/or calculate for each dataset. "Age" reflects the youngest age at which we had a measure of either attachment or substance use. "Time Span" reflects the longest span of time, in months, between measurements within the data we had access to for analysis.

"Substance Use Measure": Variables outside of parentheses represent the drug types captured by measures used in analyses (ALC=alcohol, MJ=Marijuana, NIC=nicotine/tobacco, SUB=All other drugs/combined measure), whereas variables inside of parentheses represent the use pattern captured by measures (Freq=frequency of substance use episodes, Quant=quantity of substance consumed per episode, Prob=substance related problems/SUD symptoms; Mix=Other measure).

"Attachment Measure": Variables outside of parentheses represent the attachment figure identified in measures (MOM=mother; DAD=father; FAM=combined parents/family; OTH=romantic partner/close friend/other), whereas variables inside of parentheses represent the type of attachment measure (AAI=Adult Attachment Interview; AAS=Adult Attachment Scale; IPPA=Inventory of Parent and Peer Attachment).

[ς] Relevant citations for the National Longitudinal Study of Adolescent Health are entered only in abbreviated form in the above table due to space constraints. Full list of relevant citations is as follows (Crosnoe, 2006; Dornbusch, Erickson, Laird, & Wong, 2001; Hahm, Lahiff, & Guterman, 2003; Koeppel, Bouffard, & Koeppel-Ullrich, 2015; Kopak, Chen, Haas, & Gillmore, 2012; McCarthy & Casey, 2008; McNulty & Bellair, 2003; Ragan & Beaver, 2009; Su, 2015; Turanovic & Pratt, 2015)

Table 2. Average effect size and publication bias analyses subdivided by correlation type

	Cross-sectional (N=65, k=23)		**Pros. Attach. -> Sub. Use** (N=182, k=33)		**Pros. Sub. Use -> Attach.** (N=115, k=18)		**CL. Attach. -> Sub. Use** (N=105, k=20)		**CL. Sub. Use -> Attach.** (N=70, k=15)		**Autocorr. Attach.** (N=49, k=15)		**Autocorr. Sub. Use** (N=79, k=21)	
Intercept Only Model														
	Coeff (SE)	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*
τ	.063 (.012)	<.001	.045 (.008)	<.001	.036 (.008)	<.001	.009 (.004)	.012	.029 (.009)	.002	.124 (.016)	<.001	.202 (.022)	<.001
Intercept	-.155 (.016)	<.001	-.109 (.014)	<.001	-.078 (.015)	<.001	.048 (.005)	<.001	-.014 (.012)	.241	.530 (.034)	<.001	.388 (.042)	<.001
Pub. Status Moderator Model														
	Coeff (SE)	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*
τ	.063 (.012)	<.001	.045 (.008)	<.001	.034 (.009)	<.001	.000 (.000)	.680	.023 (.010)	.016	.121 (.012)	<.001	.201 (.021)	<.001
Intercept	-.186 (.017)	<.001	-.147 (.021)	<.001	-.028 (.006)	<.001	-.067 (.000)	<.001	.069 (.009)	<.001	.599 (.110)	<.001	.453 (.079)	<.001
Pub Stat	.032 (.024)	.185	.040 (.026)	.116	-.056 (.017)	.001	.019 (.005)	<.001	-.091 (.014)	<.001	-.083 (.115)	.469	-.073 (.091)	.419
PEESE Model														
	Coeff (SE)	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*	*Coeff (SE)*	*p*
τ	.059(.012)	<.001	.045 (.008)	<.001	.029 (.007)	<.001	.000 (.000)	.474	.024 (.008)	.003	.119 (.019)	<.001	.187 (.023)	<.001
Intercept	-.172(.021)	<.001	-.113 (.023)	<.001	-.103 (.021)	<.001	-.046 (.006)	<.001	-.035 (.014)	.010	.547 (.033)	<.001	.439 (.039)	<.001
SE^2	17.02 (12.17)	.162	4.27 (9.51)	.654	20.03 (8.83)	.023	-5.46 (4.00)	.173	30.11 (8.25)	<.001	-24.04 (16.19)	.138	-50.67 (19.00)	.008

Effects are represented as *r*'s. A negative correlation implies that substance use increases as attachment security decreases. "Coeff"=coefficient. "SE"=standard error. "Pub Stat"= publication status (1=published; 0=unpublished). "Pros."=prospective association; "Attach."=attachment; "Sub. Use"=substance use and substance use problems; "CL"=cross lagged association. "Autocorr."=autocorrelation. Arrows reflect the direction of the longitudinal association, with the variable listed before the arrow measured earlier and the variable listed after measured later.

Prospective correlations reflect the absolute magnitude of longitudinal correlations, whereas cross-lagged effects parse variance attributable to autocorrelation and cross-sectional correlations.

The effect size N's for published vs. unpublished studies subdivided by the 7 effect size types are as follows: Cross-Sectional (N=5 unpublished, N=60 published); Pros. Attach -> Sub. Use (N=18 unpublished, N=164 published); Pros. Sub. Use -> Attach. (N=15 unpublished, N=100 published); CL. Attach -> Sub. Use (N=15 unpublished, N=90 published); CL. Sub. Use -> Attach (N=15 unpublished, N=55 published); Autocorr. Attach (N=9 unpublished, N=40 published); Autocorr Sub. Use (N=9 unpublished, N=70 published)

Table 3. Categorical and continuous moderators of the correlations between interpersonal attachment and substance use

Categorical Moderators	N Effect Sizes	*r*	*95% CI*	*τ*
Drug Type				.049
Alcohol	118	-0.115	-0.146, -0.085	
Marijuana	44	-0.131	-0.181, -0.080	
Nicotine	45	-0.156	-0.198, -0.114	
Mixed/Other	40	-0.109	-0.156, -0.062	
Use Pattern				.050
Frequency	139	-0.132	-0.174, -0.091	
Quantity	17	-0.109	-0.174, -0.044	
Problems	19	-0.145	-0.166, -0.124	
Mixed/Other	72	-0.097	-0.125, -0.069	
Attachment Figure				.048
Mother	46	-0.087	-0.131, -0.042	
Father	29	-0.117	-0.171, -0.063	
Family/Parents	137	-0.135	-0.167, -0.104	
Peer/Partner/Other	35	-0.097	-0.160, -0.034	
Attachment Measure				.049
IPPA	88	-0.143	-0.179, -0.107	
AAS	31	-0.059	-0.073, -0.045	
AAI	5	-0.084	-0.222, 0.055	
Other	123	-0.124	-0.164, -0.084	
Attachment Insecurity Subtype				.050
Anxious	22	-0.057	-0.074, -0.041	
Avoidant	37	-0.095	-0.145, -0.045	
General Insecurity	188	-0.128	-0.161, -0.096	
Geographic Region				.050
North America	173	-0.125	-0.162, -0.087	
Europe	28	-0.137	-0.174, -0.100	
Australia/NZ	46	-0.106	-0.180, -0.032	

Continuous Moderators	Slope	*SE*	*τ*	*p*-value
Age	0.009	0.003	.035	0.001
% Female	0.046	0.083	.051	0.581
Publication Year	0.005	0.002	.042	0.016
Racial Composition				
%White	0.015	0.067	.054	0.821
%Black	0.049	0.081	.050	0.544

Results reported above represent those examining both cross-sectional and prospective correlations, in line with moderation analyses reported in the text. The first portion of the table

presents overall *r*-values within each sub-category of non-continuous moderators. Supplemental materials report effects codes that explicitly compare the size of these r-values across sub-categories, and further subdivide results according to cross-sectional and prospective correlations. “N Effect Sizes”=the number of correlations within each sub-category; CI=confidence interval; τ =unexplained variance; SE=standard error. AAI=Adult Attachment Interview; AAS=Adult Attachment Scale; IPPA=Inventory of Parent and Peer Attachment.

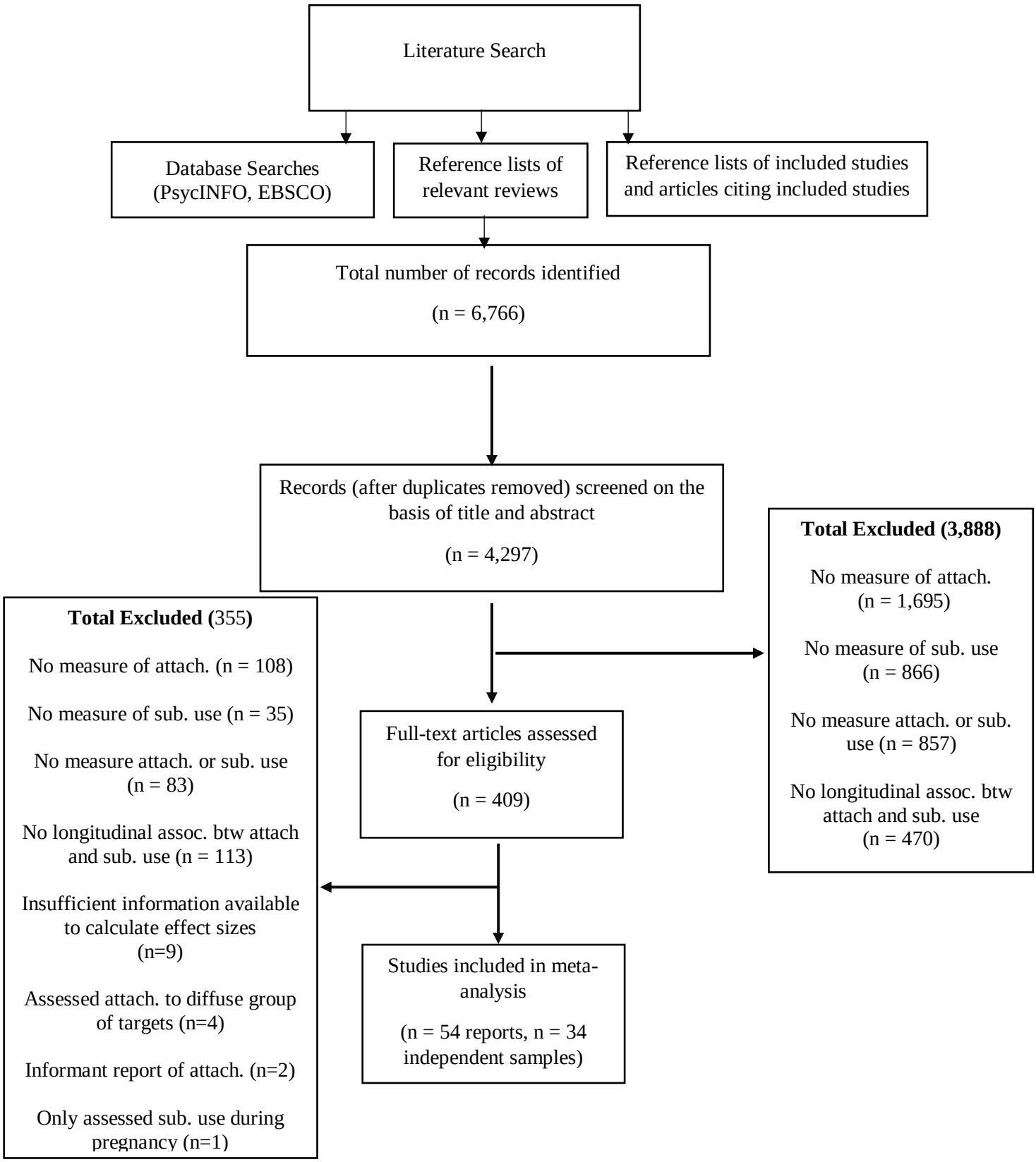

Figure 1. PRISMA flow diagram illustrating the process of identifying eligible studies
"Attach."=attachment; "Sub. Use"=substance use and substance use problems.

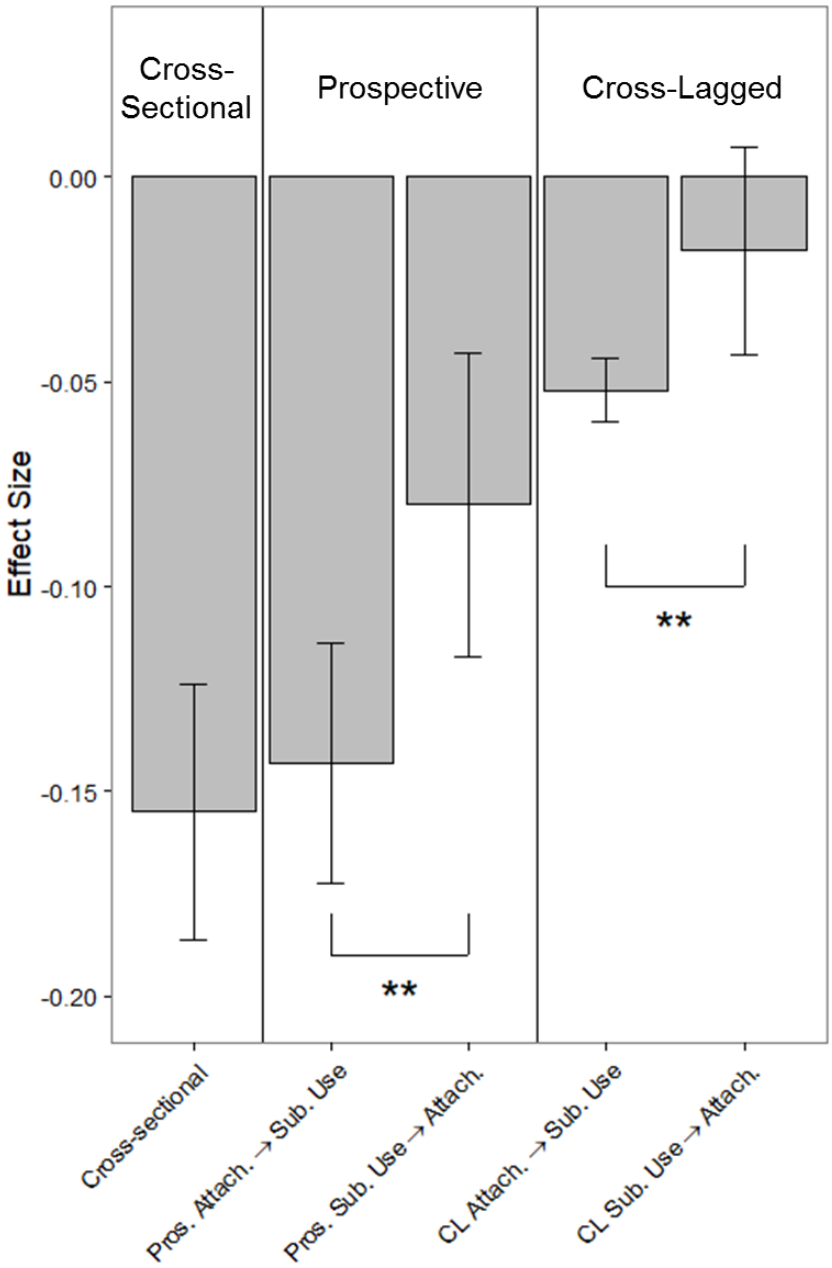

Figure 2. The magnitude of the association between attachment security and substance use as a function of type and direction of effect.

Effects are represented as *r*'s. Bars represent 95% confidence intervals. A negative correlation implies that substance use increases as attachment security decreases. "Pros."=prospective association; "Attach."=attachment; "Sub. Use"=substance use and substance use problems; "CL"=cross lagged association. Arrows reflect the direction of the longitudinal association, with the variable listed before the arrow measured earlier and the variable listed after measured later.

Prospective correlations reflect the absolute magnitude of longitudinal correlations, whereas cross-lagged effects parse variance attributable to autocorrelation and cross-sectional correlations. Effects for prospective and cross-lagged associations represented above reflect only those studies for which we were able to estimate longitudinal effects in both directions within the same sample (see also text of the results section).

**p*<.025, ***p*<.01