

Negative urgency as a state-level process

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Abstract

Objective: Test whether global self-reports of urgency moderated the within-person associations of affect and impulsive behaviors.

Background: Negative urgency is a personality trait that is a risk factor for a range of psychopathology. Although it is assumed that global self-reports of urgency measure individual tendencies to act more impulsively in the face of negative emotions, evidence from ecological momentary assessment studies is mixed.

Method: In this Registered Report, we used ecological momentary assessment data from a large sample of young adults ($n = 496$, age 18–22, 5 surveys per day for 40 days).

Results: All forms of momentary impulsivity were impaired in moments when people reported more intense negative emotions, but global self-reports of urgency did not explain individual differences in this association. Moreover, averaged affective states, rather than specific dimensions, affective circumplex, or appraisals, best predicted impulsive states.

Conclusions: Results suggest that face-valid interpretations of global self-report of urgency are inaccurate, and it may be important to understand how some people come to understand themselves as high on urgency rather than assuming that people's self-reports of their motivations are accurate. Momentary experiences of emotions globally impact multiple weakly to moderately associated impulsive behaviors, and future research should seek to understand both when and for whom these associations are strongest.

KEYWORDS

ecological momentary assessment, negative moods, negative urgency

1 | INTRODUCTION

Negative urgency, the tendency to act impulsively in the face of negative affect, is one of the strongest personality predictors of both internalizing and externalizing psychopathology (Berg et al., 2015; Coskunpinar et al., 2013; Fischer et al., 2008; Kaiser et al., 2012; Stautz & Cooper, 2013; VanderVeen et al., 2016). Theory suggests that negative urgency increases the risk for

psychopathology via a within-person process, where impulsive behaviors (such as acting without thinking, giving up easily, or acting on impulse) occur in an effort to alleviate negative emotions (Carver et al., 2008; Johnson et al., 2013; Smith & Cyders, 2016; Whiteside et al., 2005). However, very little research has tested this hypothesis, and no research has tested it as a state-level process using measures that mirror the multidimensional structure of impulsivity observed in global self-report measures. The

goal of the proposed study is to provide a large sample test of the theoretical mechanisms that underlie negative urgency.

Negative urgency is thought to characterize individual differences in the strength of impulsive reactions to negative emotions. In the face of strong negative emotions, people high on negative urgency are hypothesized to engage in impulsive behavior that is focused on immediate reinforcement or relief from negative emotions, despite the long-term consequences (Cyders & Smith, 2008; Kaiser et al., 2012; Whiteside & Lynam, 2001). The association between urgency and psychopathology are thought to reflect a *canalization* of impulsive responses over time, where pathological behavior (which varies across individuals, i.e., drinking, gambling, risky sexual behavior), becomes an increasingly automatic response to negative emotions (Smith & Cyders, 2016).

These theoretical frameworks on negative urgency suggest two relatively distinct hypotheses. First, negative urgency reflects dysregulated and general behavioral responses to negative emotions, described as “reflexive responses to emotions” (Johnson et al., 2013). Second, the canalization hypothesis suggests that the reason urgency is associated with psychopathology is due to idiographic reinforcement processes where specific prepotent responses to emotions (such as substance use) are successively reinforced over time. In other words, the specific form of psychopathology among those high on urgency may depend on idiographic processes where specific behaviors are reinforced over time. The proposed study is designed to address both hypotheses. In the current manuscript, we directly focus on the former hypothesis. We aim to measure real-time self-reports of emotions and impulsivity, to test whether the strength of those associations varies as a function of urgency. In future, preregistered analyses of these data, we plan to use our sample of regular alcohol or cannabis users to measure the canalization hypothesis, testing whether urgency specifically predicts real-time alcohol or cannabis use in response to emotions.

Personality traits typically describe between-person differences (person A scores higher on negative urgency compared with person B). However, because negative urgency is a context-dependent trait, it also describes a within-person process (person A acts more impulsively when upset compared with *themselves* when they are not upset). Therefore, definitions of negative urgency describe *between-person differences* in the strength of a *within-person association*. Yet, most research to date has measured urgency using only global self-report measures, despite the fact that trait-level measures are not well suited to measuring within-person processes like those described by construct definitions of negative urgency.

Global self-report measures of urgency may be particularly vulnerable to the limitation of these methods. Retrospective recall is often biased because it is an active reconstruction process that can be influenced in systematic ways by other factors (Shiffman et al., 2008). Participant's responses to global self-report items are influenced, at least in part, by salient memories evoked by the items. Relevant to the study of negative urgency, people with a tendency to experience negative emotions also tend to store and retrieve negative memories from long-term memory more easily (Eysenck & Mogg, 1992; Ruiz-Caballero & Bermúdez, 1995). This means that when asked about acting impulsive when upset, someone high in neuroticism may more easily recall instances that fit this description. Further, the double-barreled nature of nearly all negative urgency items (referencing both negative emotions and impulsive behavior) increases the risk of capturing individual differences in the frequency or salience of *either* negative emotions or impulsive behaviors, rather than their co-occurrence. These methodological issues are especially pertinent in the study of negative urgency because the trait explicitly describes a pattern of within-person, context-specific behaviors. In order to avoid these limitations, it is prudent to measure and examine negative urgency at the level of the process described by the construct: as *within-person association* of negative emotional experiences and impulsive behavior.

One way to examine the within-person processes described by negative urgency is to test whether trait negative urgency predicts stronger effects of emotion manipulations on impulsive behaviors in experimental settings. Three studies reported that this association was stronger for those high on trait-measured negative urgency (Billieux et al., 2010; Chester et al., 2017; Emery et al., 2014). However, it is difficult to generalize these findings to real-world settings. Behavioral paradigms used in experiments have low nomothetic overlap and poor correlations with self-reports of behavior (Cyders & Coskunpinar, 2011; de Ridder et al., 2012; King et al., 2014; Sharma et al., 2014), as well as low reliability (Rouder & Haaf, 2019), making their use in experiments and generalizability to other assessment methods (such as self-report) questionable (Dang et al., 2020). The uncertain external validity of these measures can make it difficult to infer how individual differences within them are likely to translate to real-world outcomes.

1.1 | EMA measurement of emotion-contingent impulsivity

Some EMA research has suggested that people high on negative urgency exhibit stronger associations between

negative emotions and impulsive behaviors. A few studies of negative urgency have examined specific impulsive behavioral outcomes that are related to psychopathology. For example, some previous work suggested that trait negative urgency strengthened the effects of negative emotions (such as anxiety) on alcohol use behaviors (Simons et al., 2005, 2010). However, others did not (Gaher et al., 2014). Several recent studies have suggested that there is some convergence between global self-reports of negative urgency and an emotion–impulsivity association measured via EMA. One study showed that a trait-level measure of impulsivity (the Personality Inventory for DSM-5 Impulsivity scale) predicted the magnitude of a participant's association between daily negative affect and daily impulsivity (Sharpe et al., 2019). Another study showed that global self-reports of negative urgency predicted a stronger association between momentary negative mood and a state-level measure of broad impulsivity (Sperry et al., 2018). Two other studies have reported that trait urgency was not related to the association between negative affect and EMA measures of impulsivity (Sperry et al., 2016, 2021).

The primary focus of the present study is the examination of negative urgency, it has been most consistently identified as a risk factor for psychopathology. However, positive urgency, which is conceptualized as acting rashly in the face of positive affect, has also been tied to problematic behaviors such as substance use and risky sexual behavior (Cyders & Smith, 2008). Negative and positive urgency are conceptually similar and have been shown to be highly correlated, especially in EMA data (Halvorson et al., 2021; Sperry et al., 2018). Since the processes underlying positive urgency should mirror those of negative urgency, with the only difference being the valence of the emotional experience, an examination of both facets will provide further insight into the nature of urgency as a broader construct. Thus, in the current study, we will extend our model of negative urgency to positive urgency, to examine the degree to which they produce consistent or inconsistent effects across constructs.

We have completed two pilot data collections using a version of the EMA procedures proposed here (total $n = 222$) and have two manuscripts under review that support the feasibility of the proposed study. Prior EMA studies relied on unidimensional measures of impulsive behaviors. Because state impulsivity can be delineated into distinct facets, using broadband measures of state impulsivity could confound what potentially distinct behavioral effects of negative affect. In the first manuscript from our pilot data, we validated a multidimensional measure of state impulsiveness in our pilot sample and replicated it in a second sample ($n = 221$) (Halvorson et al., 2021). Using items derived from the UPPS model of global

self-reports of impulsivity (Whiteside & Lynam, 2001), we showed that urgency, planning, and persistence could be measured using EMA, that urgency was relatively distinct from planning and persistence (which themselves were highly correlated).

Our second manuscript provided a preliminary test of a within-person model of negative urgency (Feil et al., 2020). Again, using our pilot data ($n = 222$), we aimed to build on prior research by using multidimensional measures of both negative affect and impulsive behaviors in EMA—disaggregating negative affect into discrete emotions, and impulsive behaviors into facets of acting on impulse (i.e., state urgency), persisting and planning. Results suggested that when participants reported negative affect (averaged across all negative emotions), they also reported modest within-person elevations in acting on impulse, and this association was stronger than the effects of any discrete emotion on any impulsive behaviors. However, global self-reports of negative urgency did not moderate any of these within-person associations. In other words, although we identified a “behavioral signature” that we believe maps onto theories of negative urgency, it was ultimately unclear how this signature related to global self-report of negative urgency. Therefore, the primary aim of the current study is to replicate and expand on our previous findings in order to most closely characterize the within-person process of negative urgency as it manifests in real life. Capturing this “behavioral signature” as thoroughly as possible is a critical step in answering process-oriented questions about how negative urgency confers its impressive risk for psychopathology. To achieve this aim, we have increased the scope of our previous study and added what we believe may be important predictors of the effect with wish to characterize.

1.2 | The current study

The current study improved upon our previous studies in a few general ways. First, we previously sampled three times per day for a total of 10 days, for a total of 30 observations. The current study increased the total number of EMA observations per participant to 160, sampling both more frequently throughout the day (5 times) and for more total days (32). We also more than doubled the size of our pilot sample of participants. We expected that these two changes would provide us with more stable estimates than we previously reported.

Our previous study used a convenience sample of undergraduate students, who are a narrow subset of the young adult population. The current study drew from the general population of young adults who reported alcohol or cannabis use at least weekly, from a major metropolitan

and surrounding areas. We expected that using a targeted, high-risk sample would exhibit more variability and a greater range of urgency, and in turn, the emotion–impulsivity associations that we aimed to measure, but we are cautious to note that it is possible that study results may not be generalizable.

Finally, it may be that variation in discrete emotions fails to capture the emotional circumstances that are most likely to reflect the process of urgency. In this registered report, we considered two alternative conceptualizations. First, some models of emotion hypothesized that emotional experiences are better characterized by differences in valence (pleasant vs. unpleasant) and arousal (or activation, Kuppens et al., 2013; Russell, 1980). This circumplex model of emotions explains neurobiological and psychophysiological findings that appear to be inconsistent with discrete emotion theories (Colibazzi et al., 2010; Gerber et al., 2008). In the proposed study, we collected data on participants' perceptions of their current valence and arousal, hypothesizing that participants high on negative urgency would exhibit stronger associations between highly arousing, negatively valenced emotional states and impulsive behavior. This hypothesis emerges from theories of urgency that strong and *activating* (rather than inhibiting) emotions may be especially likely to produce impulsive behavior for those high on urgency (Johnson et al., 2013). Prior research suggested that high-arousal states interfere more strongly with response inhibition in high-urgency individuals (Pearlstein et al., 2019) and that negatively valenced emotions were more strongly associated with impulsive behaviors in that high in negative urgency (Sharpe et al., 2019; Sperry et al., 2018). To our knowledge, no experimental or EMA study of emotional impulsivity has compared the relative impacts of arousal versus valence in driving these effects.

Second, because negative urgency is thought to reflect a process of emotion regulation, it may be that urgency is better reflected by individuals' *appraisals* of their emotions rather than the intensity of an emotional experience. Thus, we measured participants' perceptions of the controllability of emotions, and their desire to change how they feel. In other words, the notion of affective-driven impulsivity may better reflect individuals who believe that it is important to change how they feel, or that it will be difficult to do so. Some correlational studies have shown that the relationship between negative urgency and impulsive behaviors may depend on higher-order cognitive processes such as beliefs and expectancies about the function of impulsive actions to reduce emotional distress (Adams et al., 2012; Fischer et al., 2004). Results from studies like these indicate that an individual's motivations for engaging in impulsive action are likely an important aspect of how negative urgency creates risk for negative outcomes.

There is a long history of considering how appraisals of situations impact emotions (Kaloerinos et al., 2017; Smith & Lazarus, 1993), and a somewhat newer body of research showing that trait-like beliefs that emotions are unchangeable and harmful/undesirable is related to psychopathology (Ford & Gross, 2019; Tamir et al., 2007), there is very little work examining the impact of secondary appraisals of *current emotion states* on emotion regulation or impulsivity. For the current study, we sought to translate key concepts from these literatures into a concise momentary measure, while focusing on constructs that have been consistently associated with emotion regulation and/or psychopathology at the global self-report level (see Ford & Gross, 2019 for a review). With these goals in mind, we chose to focus on two aspects of momentary emotional appraisal: perceived changeability of current emotional state (which encompasses both changeability beliefs and emotion-regulation self-efficacy) and perceived importance of changing the current emotion (which relates to distress tolerance, perceived desirability of the current emotion, and emotion-regulation motivation).

Finally, we improved the measurement of discrete emotions (i.e., anger, sadness, or anxiety) by broadening our measurement of them. Literature across cultures and languages has suggested that negative affect may be broken into distinct facets, such as fear and distress, or fear, self-disgust, and hostility (Bagozzi, 1993; Drobnjaković et al., 2017; Mehrabian, 1997). Some prior research has suggested that global self-reports of negative urgency were differentially related to these facets in EMA (Sperry et al., 2016, 2018). One recent EMA study suggested that within-person variation in negative emotions reflected multiple underlying dimensions (Jacobson et al., 2020). Our prior studies used a single item per emotion (e.g. *angry*, *unhappy*). In the proposed study, we measured multiple dimensions of negative and positive affect and tested for specificity of the urgency process by each negative affect dimension. Because we did not have strong differential hypotheses for dimensions of positive affect, we only conducted exploratory tests with dimensions of positive affect.

We focused on concurrent, rather than lagged, associations because we believed that the emotion–impulse association arises contemporaneously within the constraints of the time resolution in the proposed study (about an hour). This “concurrent” association could contain several processes: emotions causing impulsive behavior, impulsive behavior causing emotions, or some third variable causing both; all would be reflected in a contemporaneous association. However, sufficiently modeling temporal lags, which might allow us to disentangle directionality, is complicated by several factors. First, it would require a highly intensive EMA schedule, creating a substantial

participant burden, and likely requiring us to dramatically shorten the number of days of assessment. Second, an intensive EMA schedule would run the risk of missing significant instances of both emotions and impulsiveness, which had zero-inflated and skewed distributions in our pilot data. For example, on a 0–100 scale (from “Not at all” to “Very much”), 25% of observations of negative emotions had a value greater than 30, whereas only 10% of observations had a value greater than 40. Thus, our EMA schedule was designed to maximize our ability to observe the co-occurrence of negative emotions and impulsive states at the cost of disentangling their temporal ordering.

Our hypotheses were as follows. First, we aimed to replicate the within-person associations between aggregated negative emotions and acting on impulse, the weak to null associations between negative emotions and persisting and planning, and the relatively weaker associations of discrete dimensions of negative emotions and acting on impulse, persisting, and planning. We tested whether aggregated positive emotions are also associated with acting on impulse, persisting, and planning. Second, we tested whether global self-reports of negative and positive urgency moderate these within-person associations. Third, we tested our exploratory hypotheses that discrete dimensions of positive emotions provide greater specificity of prediction than global positive affect, that the associations between valence/arousal of emotions or emotional appraisals (rather than ratings of discrete emotions), and impulsive behaviors are moderated by global self-report of negative and positive urgency. Finally, because the processes we are testing are thought to be specific to urgency, we hypothesized that we would observe null results for planning, persistence, and sensation seeking as moderators.

2 | METHODS

Participants were recruited for a larger study supported by a grant (R01 DA047247) aimed at understanding the role of EMA-assessed negative urgency in the development of alcohol and cannabis misuse. The larger study recruited regular alcohol or cannabis users and assessed them for 2 years following the initial EMA period. Because the proposed study was focused on the EMA period only, we do not discuss the larger study design further. All measures are included in the supplemental codebook (<https://osf.io/u8zka/>).

2.1 | Sample

We proposed collecting the following sample: Participants will be young adults (age 18–22, $n = 500$,

50% female) who report drinking alcohol or using cannabis regularly, recruited from King County, WA. We proposed collecting a sample comprised of 60% non-Hispanic Caucasian, 18% Asian, 12% African American, and 10% Hispanic/Latino young adults, reflecting the demographics of the sampling region in 2010. The final sample for the EMA portion of the study was comprised of 496 participants (age 18–22, $M_{\text{age}} = 20.3$, $SD = 1.3$, 45% cisgender women, 42% cisgender men, 8.5% nonbinary/genderqueer/gender nonconforming, 4.0% transgender men or women, and 0.2% nongendered). Participants endorsed a variety of races/ethnicities: 54% were solely non-Hispanic White, 28.5% Asian, 6.6% African American, 8.4% Hispanic/Latino, and 22.7% endorsed more than one ethnicity. The majority of participants identified as heterosexual (52%), with the remaining participants either identifying as LGBTQ+ (47.6%) or declining to respond ($n = 2$). Finally, 9.8% of the sample was born outside the U.S. Racial/ethnic proportions broadly reflected 2020 Washington census data from the sampling region.

Variation in race/ethnicity and socioeconomic status (SES) was achieved by sampling from a wide variety of community locations and neighborhoods with differential aggregate SES (as indicated by the 2010 Census). Because we aimed to recruit a representative sample of young adults, we recruited from both college and noncollege sources, aiming for a sample that reflects the broader demographics of King County, WA, where approximately 64% of those aged 18–24 have achieved some college education (U.S. Census Bureau, [n.d.](#)). We capped enrollment at 50% female to ensure equal distribution of gender in the sample.

2.1.1 | Screening procedures

Although our advertising materials targeted regular alcohol and cannabis users, we implemented screening procedures to ensure participant eligibility. We recruited using Internet (Facebook, Instagram, SnapChat, Craigslist, and X/Twitter) and noninternet (newspaper advertisements and flyers) sources. Interested potential participants were directed to a web survey where they completed basic demographic information, contact information, and information about the past month substance use and other health behaviors. To promote honest responding, we included items designed to screen for invalid or careless responding (Kim et al., 2017), disguised the screening criteria by surveying participants about multiple health related behaviors (such as diet, sleep, and exercise behaviors), and allowed the survey to be taken a single time for each IP address within

a reasonable timeframe (i.e., 1–3 months between screener attempts). We proposed including the name of a fake drug to screen for overreporting but did not include this item due to time constraints in the survey (Pape & Storvoll, 2006). As an additional precaution, we compared responses in the baseline data to the screening survey for the first 50 participants to determine the proportion of participants who meet substance use criteria in the screening survey but who deny substance use at baseline (i.e., false-positive rate). If the false-positive rate was greater than 10%, we planned to implement a secondary screening process by phone with a research assistant (RA) prior to the baseline survey. Only 2 (0.004%) participants denied any past 30-day alcohol or cannabis use at the baseline survey. Participants who met study criteria scheduled an appointment using an online reservation system to visit the lab for their baseline visit.

2.1.2 | Inclusion and exclusion criteria

As planned, participants were required to be between the ages of 18 and 22, own a smartphone, can travel to the PI's university for the baseline screening, and report drinking *or* using cannabis "about once per week" over the past 3 months. With the onset of the COVID-19 pandemic (3 months prior to the planned start of data collection), we moved all study procedures online. The specific screening item for alcohol was "In the past 3 months, how often did you drink alcohol (i.e. beer, wine, liquor, wine coolers or energy drinks with alcohol)?" The specific screening item for cannabis was "In the past 3 months, how often have you used cannabis or cannabis infused products (cannabis, pot, weed, joints, blunts, dabs, edibles, vaped) or hashish (hash, hash oil)?" For both items, responses ranged from "Not at all" to "Nearly every day."

Because the parent study was focused on understanding the emergence of problem alcohol and cannabis use, which is often strongly culturally influenced, participants were excluded if they were not fluent in English or if they moved to the United States after age 12 (to screen out participants who are not acculturated to United States norms of substance use). We aimed to cap the participation of women at 50%, so participants were excluded if the cap for sex was met.

A total of 12,812 participants completed the screening protocol, and 10,521 completed the eligibility survey. In this study, 1884 participants were eligible (18% of those completing the eligibility survey), and 982 (52%) of eligible participants scheduled a baseline interview. Five hundred and twenty-nine participants ultimately completed a

baseline orientation, and 25 of those participants were excluded either because they were suspected scammers (e.g., could not provide a photo ID, or IP addresses indicated they lived outside the target region or the United States, or gave inconsistent demographic information) or withdrew from the study. Five hundred and four participants initiated the EMA period, and eight participants withdrew from the study during the EMA. Thus, the final sample was 496. Figure 1 provides a diagram of participant flow from recruitment to EMA completion.

2.1.3 | Baseline assessment

Participants completed the baseline assessment over Zoom, where RAs verified their identity with a photo ID before completing consent. Participants then completed a computer-administered survey battery and received \$50 for this visit.

2.1.4 | EMA orientation/training

As part of the consent process, RAs described the study, emphasizing the intensive nature of the sampling procedures, the importance of high compliance (and bonuses for compliance), and the scientific significance of the data collected. The goals of these procedures were

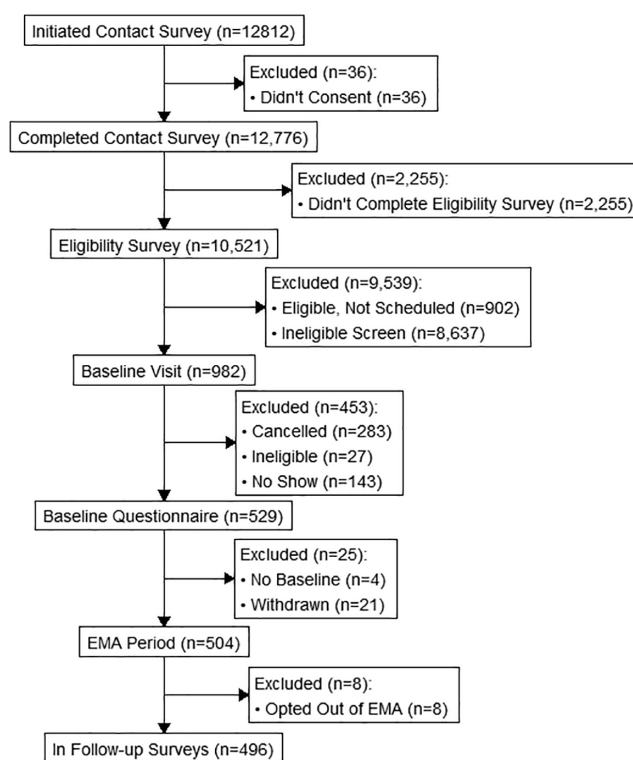


FIGURE 1 CONSORT diagram of participant recruitment.

to ensure that participants understood the rigor of the study before agreeing to participate, to build rapport between the participant and study staff, and to highlight the importance of the EMA survey data. RAs presented participants with a calendar of the dates for which they would be receiving EMAs (i.e., Thursday–Sunday for the next 8 weeks, between 9 am and 11 pm) and asked participants to identify times or days they may not be able to complete EMAs (e.g., long work shifts or trips out of the country). If participants indicated at this time that they anticipate being unable to respond to 50% or more of the prompts, their participation in the study was discontinued. Research assistants also emphasized that they were the participant's personal contact for the extent of the EMA period and reminded participants that if they miss more than 1 day of data collection, the RA will call them personally to check in and troubleshoot to support study participation. Research assistants then sent a sample survey to the participant's smartphone and walked through the steps to completing the survey. Research assistants defined key measures and provided examples as needed. We trained RAs to answer questions and build rapport and motivation for completion of the EMA surveys. We successfully implemented these EMA procedures in our pilot studies (Feil et al., 2020; Halvorson et al., 2021) with participants of similar ages and achieved high rates of compliance (>88%).

2.1.5 | EMA data collection

For the next 8 weekends (Thursday–Sunday), 5 times per day, participants received texts with a link to a brief web EMA survey. They received an additional text on Monday mornings in order to capture behavior from Sunday nights. Texts were sent using an EMA platform with automated survey prompts, reminder texts, real-time response tracking, and randomization of EMAs within blocks of time (5 three hour blocks between 9 a.m. and 11 p.m., with at least 1 hr between surveys). The EMAs were collected using secure, encrypted data connections. Pilot subjects completed around 10 items per minute.

Past research has suggested several methods to improve participant compliance with daily diary assessments (Shiffman et al., 2008). We had phone check-ins after the first day of data collection to troubleshoot, and as needed to boost compliance. We sent email and text reminders the day before each weekend of data collection. Survey links were active for an hour after the initial text was received to allow some flexibility in survey completion, with a reminder text sent to participants who have not completed the survey within the first 30 min. We changed the reminder schedule to 20, 40, and 55 min to increase response rates after the first

2 months of data collection to improve participant compliance. Prior research has suggested that participants can exhibit high compliance with daily data collection when electronic reminders are used (Hufford & Shiffman, 2002; Shiffman et al., 2008). We additionally increased compliance by offering “bonuses.” Participants were paid \$1 per EMA, with a \$5 bonus for completing 80% (i.e., 17/21) of EMAs for a given weekend (\$208 total possible).

2.2 | Measures

These measures came from a more extensive battery. Below we report the battery relevant to the proposed study.

2.2.1 | Baseline assessment measures

Negative and positive urgency and other impulsive traits were assessed with the *UPPS-P* (Whiteside & Lynam, 2001). The 59 items on this scale are used to assess five impulsive traits: negative and positive urgency, along with planning, persistence, and sensation seeking, all $\alpha > 0.80$ (Smith et al., 2007). An extensive literature supports the psychometric and predictive validity of the *UPPS-P* (Coskunpinar et al., 2013; Smith et al., 2007). Negative urgency is one of two impulsive traits of interest for the present study, and this subscale consists of 12 items such as “When I am upset, I often act without thinking” and “I have trouble controlling my impulses.” Positive urgency consists of 14 items such as “I am surprised at the things I do while in a great mood” and “I tend to act without thinking when I am really excited.” Analyses used a mean value of responses to these items, which are rated on a 4-point Likert scale ranging from “strongly agree” (1) to “strongly disagree” (4). Both negative and positive urgency subscales demonstrated good reliability in our pilot data ($\alpha = 0.86–0.89$).

2.2.2 | Ecological momentary assessment measures

We used a core battery of 35 items for each assessment. For all items relevant to the current study, we used visual analog slider bars rather than Likert responses, to increase variability in responding and avoid anchoring effects (Palmblad & Tiplady, 2004). In lieu of multiple response anchors, we included only end-point labels and a single central anchor representing neutral or 50%. For all EMA slider-bar measures, participant responses were coded between 0 and 100 based on the final placement of the slider—though this number was not visible to the respondent.

TABLE 1 Negative and positive affect dimensions and items.

Negative affect				Positive affect		
Anger	Sadness	Anxiety	General negative affect	Joviality	Attentiveness	Serenity
Angry	Unhappy	Anxious	Upset	Happy	Alert	Calm
Irritated	Sad	Afraid	Distressed	Cheerful	Attentive	Relaxed
Hostile	Blue	Nervous	Guilty	Delighted	Determined	At ease
Annoyed	Alone	Jittery	Ashamed	Joyful	Concentrating	
Scornful	Lonely	Shaky		Enthusiastic		
Disgusted	Downhearted	Frightened		Energetic		
Loathing		Scared		Lively		
				Excited		

Impulsive behaviors (planning, persistence, and acting on impulse) were measured with items adapted from the UPPS (Whiteside & Lynam, 2001), with item stems changed to reflect behavior “in the past hour” (Halvorson et al., 2021). Acting on impulse items were adapted from UPPS urgency items to be free of emotional content (i.e., “I had trouble controlling my impulses” or “I lost control”) such that they only reflect the acting on impulse aspect of urgency. Planning items were adapted from UPPS global self-report of planning (premeditation), such as “I thought carefully before doing anything” or “I followed a rational, ‘sensible’ approach to things”. Persistence items were adapted from UPPS global self-report of persistence (perseverance), such as “I saw things through to the end” and “I gave up easily.” Participants rated their experiences in the past hour on a visual analog scale ranging from 0 to 100 (although the specific numeric choice is not shown) with anchors “strongly disagree,” “strongly agree,” and a central anchor of “neither agree nor disagree.” Item means were computed for each impulsive trait within each observation. At each assessment, three items from each subscale were randomly administered to subjects to reduce response burden. We successfully used this missing completely at random (MCAR) method of item presentation in our pilot studies (see above). Reliability in pilot data was high ($\alpha = 0.71\text{--}0.84$), items loaded strongly on their respective factors at the between- and within-person level, with no evidence of cross-loading, and strong evidence of validity in both the pilot samples, and replicated in an independent sample of adults (Halvorson et al., 2021). Supplementary Table S1 (available at <https://osf.io/u8zka/>) presents the original UPPS item along with their EMA adaptation for the present study.

Discrete emotions were measured by participants rating how much they felt (Russell, 1980) specific negative and positive emotions in the past hour with anchors “not at all,” “very much,” and a central anchor of “somewhat.” We selected emotion words to reflect multiple dimensions of negative and positive, based on the PANAS-X and other prior work (Larson & Lampman-Petratis, 1989; Silk et al., 2003).

One recent EMA study suggested that affect can be reliably measured and exhibits strong evidence for multidimensionality within-person (Jacobson et al., 2020).

As with impulsive behaviors, we administered two words each from seven sets of items reflecting four negative affect dimensions (anger, sadness, anxiety, and general negative affect), and three positive affect dimensions (joviality, attentiveness, and serenity). Table 1 presents the dimensions and item list. Each dimension of affect (except general negative affect) was scored as the mean of items within that dimension. General negative affect was scored as the mean of all negative affect items across all four dimensions, and positive affect was scored as the mean of all positive affect items. This strategy increased the reliability of these within-person measures of emotions, broadened the construct representation, and balanced survey length against participant burden. In our pilot study, negative/positive affect had high reliability within-person, $\alpha = 0.77\text{--}0.88$.

Emotional valence and activation, as described by the circumplex model of emotions (Russell, 1980), were measured by asking participants to rate how pleasant they felt on a scale from “extremely pleasant” to “extremely unpleasant,” and how energetic they felt with anchors “extremely low-energy” and “extremely high-energy,” with a central anchor of “neutral.”

Emotional appraisals were measured by asking participants to rate how well they think they could control, fix, or change their current mood on a scale from “not at all” to “completely,” and how important they believe it is to do so with anchors “not at all important” and “extremely important,” with a central anchor of “somewhat.”

2.3 | Data analyses and power

The original proposed methods can be found in online supplementary materials (<https://osf.io/u8zka/>). Below we summarize the analytic approach and report any deviations from the preregistered data analytic protocol.

2.3.1 | General analytic approach

Hypotheses were tested with generalized linear mixed models (GLMM). GLMMs are a flexible class of analyses that allows for the analysis of nonindependent data and can appropriately specify multiple distributions of dependent variables (e.g., continuous, categorical, binary and count data). GLMMs allow researchers to account for nesting within different data structures and at the same time to explicitly model effects across that nesting. Data processing and analysis were conducted in R 4.2 (R Development Core Team, 2016), a powerful and flexible open-source data analytic software. For data processing, we relied on the *tidyverse* and *dplyr* packages (Wickham et al., 2019, 2023). For visualization, we used *marginaleffects* (Arel-Bundock, 2024) and *ggplot2* (Wickham, 2016). For analyses, we relied on *lme4* and *nlme* (Bates et al., 2015; Pinheiro et al., 2015).

2.3.2 | Covariates

For all models, we included a core set of covariates. At the between-person level, we will control for age, gender, college/noncollege status, the ratio of reactive to effortful emotion regulation strategies, and other impulsive traits (planning, persistence, and sensation seeking) as each may influence either emotions or momentary impulsive behavior. Within-person, we controlled for time effects (i.e., week of study, day of the week, observation number, and time of day). *Post hoc deviation*: Many models failed to converge due to very large eigenvalues, which was a result of variables being on very different scales. To obtain convergence, we rescaled all predictors to be in a similar range by either standardizing or dividing by the scale maximum. All within-person predictors were person-mean centered, while continuous between-person predictors were grand mean centered. Focal predictors (momentary affect, appraisals, and trait impulsivity) were standardized by dividing the observation by the sample *SD* for that variable. Rescaling in this way only affects the interpretation of coefficients and not their significance.

2.3.3 | Treatment of missing data

To reduce participant burden, participants were presented with a randomly selected subset of items within each subscale at each assessment. *Post hoc deviation*: In the Stage 1 version of this manuscript, we stated that we would only present a random subset of impulsive behavior items only. However, as we (correctly) described in the measures section of that manuscript, we presented random subsets of *both* impulsive

behaviors and affect items at all EMAs. Because items are by definition MCAR, mean scores may be computed without bias (Schafer & Graham, 2002), which we have done in our pilot studies (Feil et al., 2020; Halvorson et al., 2021). GLMMs are robust to missingness at the level of the EMA if the data are missing at random because they use full information maximum likelihood estimation (Enders, 2010).

2.3.4 | Hypothesis testing

Table 2 summarizes all statistical tests we intend to report in the proposed study. In short, we proposed 45 confirmatory main effect hypothesis tests (15 main effects, 15 interactions with global self-report of negative urgency, and 15 with positive urgency), and many more exploratory hypothesis tests. Because we increased the sample size and representativeness of the sample relative to our pilot studies, we were able to estimate effect size estimates with more precision. On the other hand, we wished to avoid overinterpreting very small or nonmeaningful effects given the relatively high power of our study.

Thus, hypotheses testing followed a minimum-effect significance testing (MEST) framework (Smiley et al., 2020). In short, used the “greatest effect of no interest” (GENI) for each confirmatory hypothesis, and we considered that effect significant if the null hypothesis that the population value is as big as GENI could be rejected. The MEST approach avoids both of these problems because it states a hypothesis in terms of the population value and sets a false positive rate (i.e. $p < 0.01$) relative to that value. We a priori took a relatively conservative approach to the GENI, which does run the risk of missing small but stable effects. We focused on two estimates of GENI for all confirmatory hypotheses, because all confirmatory hypotheses represented minor variations of two central hypotheses. We used a p -value of 0.01 because of the number of hypotheses tested. We specified a GENI of 0.05 for the within-person association of emotions and impulsive behaviors, such that a 1 *SD* within-person change in emotions would predict a 0.05 *SD* within-person change in impulsive behaviors. We chose a relatively small effect because even small effects, when repeated over many occasions, could be meaningful in aggregation. Our prior study estimated an effect of $\beta = 0.16$ (unstandardized $b = 0.20$) for the within-person association of negative emotions and acting on impulse (Feil et al., 2020).

The second GENI was the moderating effect of negative or positive urgency on the within-person association between emotions and impulsive behaviors. Our prior work found an (nonsignificant) interaction effect of $\beta = 0.11$, meaning that for a 1 *SD* difference in global self-report of urgency, we would expect a 0.11 change in the slope of the

TABLE 2 Proposed hypotheses.

	Predictor	Outcome	Type	Within person	Moderation by urgency	Moderation by PPS
<i>Confirmatory hypothesis</i>						
1. Replicate within-person associations between aggregated negative emotions and acting on impulse	Negative affect	Acting on impulse	Confirmatory	0.05	0.06	Null
2. Test within-person associations between aggregated positive emotions and acting on impulse	Positive affect	Acting on impulse	Confirmatory	0.05	0.06	Null
3. Replicate weak to null associations between negative emotions and persisting and planning	Negative affect	Persisting	Confirmatory	−0.05	Null	Null
	Negative affect	Planning	Confirmatory	Null	Null	Null
4. Test within-person associations between positive emotions and persisting and planning	Positive affect	Persisting	Confirmatory	0.05	Null	Null
	Positive affect	Planning	Confirmatory	Null	Null	Null
5. Test for relatively weaker associations of specific dimensions of negative emotions and acting on impulse, persisting, and planning	Anger	Acting on impulse	Confirmatory	0.05	0.06	Null
	Sadness	Acting on impulse	Confirmatory	0.05	0.06	Null
	Anxiety	Acting on impulse	Confirmatory	0.05	0.06	Null
	Anger	Persisting	Confirmatory	−0.05	Null	Null
	Sadness	Persisting	Confirmatory	−0.05	Null	Null
	Anxiety	Persisting	Confirmatory	Null	Null	Null
	Anger	Planning	Confirmatory	Null	Null	Null
	Sadness	Planning	Confirmatory	Null	Null	Null
<i>Exploratory hypotheses</i>	Anxiety	Planning	Confirmatory	Null	Null	Null
	Joviality	Persisting	Exploratory	−	−	−
	Attentiveness	Persisting	Exploratory	−	−	−
	Serenity	Persisting	Exploratory	−	−	−
	Joviality	Planning	Exploratory	−	−	−
	Attentiveness	Planning	Exploratory	−	−	−
	Serenity	Planning	Exploratory	−	−	−
	Joviality	Acting on Impulse	Exploratory	−	−	−
	Attentiveness	Acting on Impulse	Exploratory	−	−	−
	Serenity	Acting on Impulse	Exploratory	−	−	−
	Joviality	Acting on Impulse	Exploratory	−	−	−
	Attentiveness	Acting on Impulse	Exploratory	−	−	−
	Serenity	Acting on Impulse	Exploratory	−	−	−

TABLE 2 (Continued)

	Predictor	Outcome	Type	Within person	Moderation by urgency	Moderation by PPS
7. Explore associations between facets of negative emotion and acting on impulse, persisting, and planning	Valence	Acting on Impulse	Exploratory	—	—	—
	Arousal	Acting on Impulse	Exploratory	—	—	—
	Valence × Arousal	Acting on Impulse	Exploratory	—	—	—
	Confidence in changing emotion	Acting on Impulse	Exploratory	—	—	—
8. Explore associations between valence/arousal of emotions or emotional appraisals and impulsive behaviors	Importance of changing emotion	Acting on Impulse	Exploratory	—	—	—
	Confidence × Important	Acting on Impulse	Exploratory	—	—	—
	Valence	Persisting	Exploratory	—	—	—
	Arousal	Persisting	Exploratory	—	—	—
	Valence × Arousal	Persisting	Exploratory	—	—	—
	Confidence in changing emotion	Persisting	Exploratory	—	—	—
	Importance of changing emotion	Persisting	Exploratory	—	—	—
	Confidence × Important	Persisting	Exploratory	—	—	—
	Valence	Planning	Exploratory	—	—	—
	Arousal	Planning	Exploratory	—	—	—
	Valence × Arousal	Planning	Exploratory	—	—	—
	Confidence in changing emotion	Planning	Exploratory	—	—	—
	Importance of changing emotion	Planning	Exploratory	—	—	—
	Confidence × Important	Planning	Exploratory	—	—	—

Note: All analyses included testing of the proposed main effect and their interaction with negative urgency. We did not have hypotheses about the magnitude of exploratory analyses. Effects reported reflect GENI. The moderation hypothesis refers to moderation by global self-report of negative or positive urgency; we expected them to have similar if not identical effects due to their strong overlap. GENI, greatest effect of no interest; PPS, global self-reports of planning, persistence, and sensation seeking.

relation between momentary negative affect and acting on impulse on the 0–100 response scale, which represents a 50% increase in the magnitude of the effect. Thus, we chose a smaller GENI threshold of $\beta = 0.06$, which would represent a 0.06 change in the momentary association between negative affect and acting on impulse for a 1 *SD* difference in urgency.

We also proposed multiple null hypotheses. For these, we used equivalence testing (Lakens et al., 2018) to test whether an observed association was effectively zero.

Because we did not have effect size expectations for exploratory hypotheses, they were considered significant with a sign consistent with hypotheses and $p < 0.001$ to avoid capitalizing on chance and multiple testing.

2.3.5 | Statistical power

We had power to detect within-person associations between negative affect and impulsive behaviors as small as a 1 *SD* change in affect predicting a 0.013 *SD* change in impulsive behaviors. We had the power to detect a cross-level interaction effect as small as a 1 *SD* change in global self-report of negative urgency predicting a 0.03 change in a within-person association. Full details of the power analysis and assumptions are reported in the Stage 1 manuscript (<https://osf.io/u8zka/>).

2.3.6 | Effect sizes

There is little prior research to guide effect size estimation. In Table 2, we also report our expected effect size estimates. There, we defined moderate-sized within-person effects as $\beta > 0.15$, small within-person effects as $0.05 \leq \beta \leq 0.15$, and null or “essentially null” hypotheses to be $\beta < 0.05$. We defined moderate-sized moderation effects as $b > 0.075$, small within-person effects as $0.02 \leq b \leq 0.075$, and null or “essentially null” hypotheses to be $b < 0.02$. These effect sizes were necessarily smaller than the within-person effect sizes because they refer to predicted changes in a slope. In other words, we defined a moderate moderation finding as a 1-unit change in global self-report of urgency predicting a 0.075 or greater increase in the within-person association between emotions and impulsive behaviors.

3 | RESULTS

3.1 | Missing data

We obtained data from 496 eligible and valid participants, who provided 51,471 EMAs (61.2% of 83,160 possible). The median response rate was 118 EMAs (70.2% of

168 possible). The number of missing observations was weakly associated with several variables in the models ($|r| = 0.08$ – 0.22). Participants who reported lower acting on impulse, lower negative or positive urgency, less negative emotions, and lower sensation seeking, and those who reported more planning, persistence (both momentary and trait), more positive emotions, and who were older completed somewhat more EMAs across the study.

3.2 | Descriptive statistics

As in prior studies, participants reported relatively low levels of acting on impulse across EMAs ($M = 11.84$, $SD = 19.20$) and low levels of negative emotions ($M = 10.20$, $SD = 14.69$), with relatively high levels of planning ($M = 58.32$, $SD = 23.11$), persisting ($M = 58.38$, $SD = 25.03$), and positive

TABLE 3 Descriptive statistics.

	<i>M</i>	<i>SD</i>
Total observations	103.98	45.94
<i>Momentary variables</i>		
Acting on impulse	11.84	19.20
Planning	58.32	23.11
Persisting	58.38	25.03
Negative affect	10.20	14.69
Positive affect	46.04	22.68
Anger	8.64	16.30
Sadness	12.04	19.88
Anxiety	10.22	18.10
Joviality	41.62	27.61
Attentiveness	44.07	28.77
Serenity	52.04	27.55
(Un)pleasantness	34.75	23.40
Energetic	49.31	23.10
Confidence appraisals	48.43	31.35
Importance appraisals	18.78	26.18
<i>Between-person variables</i>		
UPPS negative urgency	3.31	0.61
UPPS positive urgency	2.88	0.61
UPPS premeditation	4.01	0.51
UPPS perseverance	3.66	0.57
UPPS sensation seeking	3.84	0.60
Maladaptive emotion regulation ratio	0.41	0.07
Age	20.27	1.33
Currently not in college	23%	
Female gender	47%	
Male gender	44%	
Gender expansive	13%	

emotions over time ($M=46.04$, $SD=22.68$). Table 3 provides descriptive statistics for all study variables, while Supplementary Table S2 (available at <https://osf.io/u8zka/>) provides the correlation matrix for all study variables.

3.3 | Modeling results

Including random slopes for affect or appraisals improved model fit in all models. Including an auto-regressive (AR-1) residual structure made model fit worse in all models. We compared the main confirmatory models with and without autoregressive effects. The final coefficients and standard errors were nearly identical, and the ultimate inferences drawn from the models were identical. Thus, we present the final models without accounting for residual auto-correlation. Although some covariates were significantly associated with the outcomes, the focal parameter estimates and their standard errors were nearly identical across the models with and without covariates. Thus, as proposed, we present and discuss the covariate-adjusted results below. Supplementary tables (available at <https://osf.io/u8zka/>) present the unadjusted results.

To test confirmatory hypotheses against our GENI, we transformed these coefficients after analyses to reflect the scale of effect sizes as proposed. For the main effects (of emotions and appraisals), we divided the model coefficient by the SD of the outcome, so we could interpret them as a β SD change in the outcome for a 1 SD change in the predictor (matching the standardized effects reported in Feil et al., 2020). For the interaction of trait impulsivity by momentary affect, we divided by the SD of the affective predictor, so we could interpret these effects as the change in the unstandardized relation of momentary affect and the outcome (e.g., on the 0–100 scale) for a 1 SD change in trait impulsivity (as specified in the preregistration and Stage 1 proposal).

For equivalence testing, we followed the TOST procedure (Lakens et al., 2018), where we determined that an effect was effectively zero if 2 one-sided tests of the effect,

tested against the positive and negative values of the GENI threshold (e.g., 0.05 for main effects, 0.06 for interactions) were both significant. A summary of all hypothesis tests (confirmatory, exploratory, and null) can be found in the supplemental materials.

As would be expected, the effects of momentary affect and appraisals were consistent across models with different trait moderators, so we report coefficients from models with trait urgency as the moderator in the text. All specific effects from all models can be found in the supplementary tables.

3.4 | Main effects of negative affect on acting on impulse

First, we investigated the within-person association between negative affect and state acting on impulse across five models (controlling for each facet of the UPPS-P in separate models). Table 4 presents these findings with negative urgency as the moderator, supplementary tables (available at <https://osf.io/u8zka/>) present the full results of all models. There was a positive and moderate association between negative affect and acting on impulse. For example, when a person reported a 1 SD higher in their negative affect, they also reported a 0.19 SD change in state acting on impulse ($p<0.001$), accounting for the covariates and the main effect of trait negative urgency. Moreover, there was substantial variation in this association ($SD=2.37$), such that the association ranged from 0.06 to 0.31 for 68% of participants. In other words, for some participants, there was a weak within-person association of negative affect and momentary acting on impulse, while for others there was a moderate to strong association. Figure 2 illustrates this effect.

There was no evidence of moderation by negative or positive urgency larger than our prespecified GENI ($b=0.06$). The largest moderation effect suggested that a 1 SD increase in trait negative urgency was related to a 0.046 increase in the unstandardized slope of the association

TABLE 4 Within-person effects of negative emotions on acting on impulse.

	<i>b</i>	<i>SE</i>	<i>t</i> -value	<i>p</i> -value	<i>B</i>
Intercept	10.654	0.875	12.183	<0.001	–
Negative affect	3.629	0.134	27.131	<0.001	0.189
Negative urgency	3.82	0.743	5.138	<0.001	0.199
Negative affect × Negative urgency	0.498	0.135	3.685	0.001	0.046
<i>SD</i> of intercept	13.064	–	–	–	–
<i>SD</i> of affect slope	2.366	–	–	–	–
Slope–intercept correlation	0.098	–	–	–	–

Note: controls for age, college status, gender, time of day, day of week, observation number, emotion regulation, premeditation, perseverance, and sensation seeking.

between momentary negative affect and acting on impulse. In other words, a person at the mean of trait urgency was expected to show a 0.33 point change (on a 100 point scale) in acting on impulse for every 1 point change

in negative affect, while a person 1 *SD* higher than the mean on trait urgency would be expected to show a 0.376 point difference for 1 point change in negative affect. We deemed this magnitude of an effect, a priori, to be too small to be of importance, illustrated in Figure 3.

Next, we replicated these models testing for moderation by trait planning, persistence, or sensation seeking. Consistent with hypothesis, the 95% confidence interval for moderation by planning ($p=0.121$), persistence ($p=0.332$), and sensation seeking ($p=0.498$) were equivalent to the null hypothesis per the TOST procedure.

3.4.1 | Effects of specific negative emotions

The effects of specific negative emotions were weaker but nearly identical across anger, sadness, and anxiety ($\beta=0.12$ – 0.14 , all $p<0.001$). The moderating effects of negative or positive urgency were also weaker across specific emotions, and none were larger than our GENI, although all were significant by conventional standards ($p<0.05$). Moderation effects by planning, persistence, or sensation seeking were all equivalent to the null (see tables or supplements). Figure 4 illustrates how these effects varied across specific emotions, full modeling results can be found on OSF (<https://osf.io/u8zka/>).

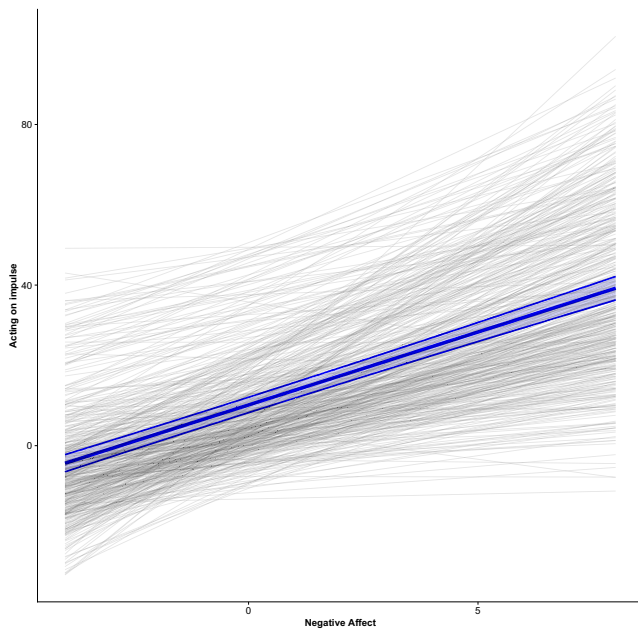


FIGURE 2 Within-person association of negative affect and acting on impulse.

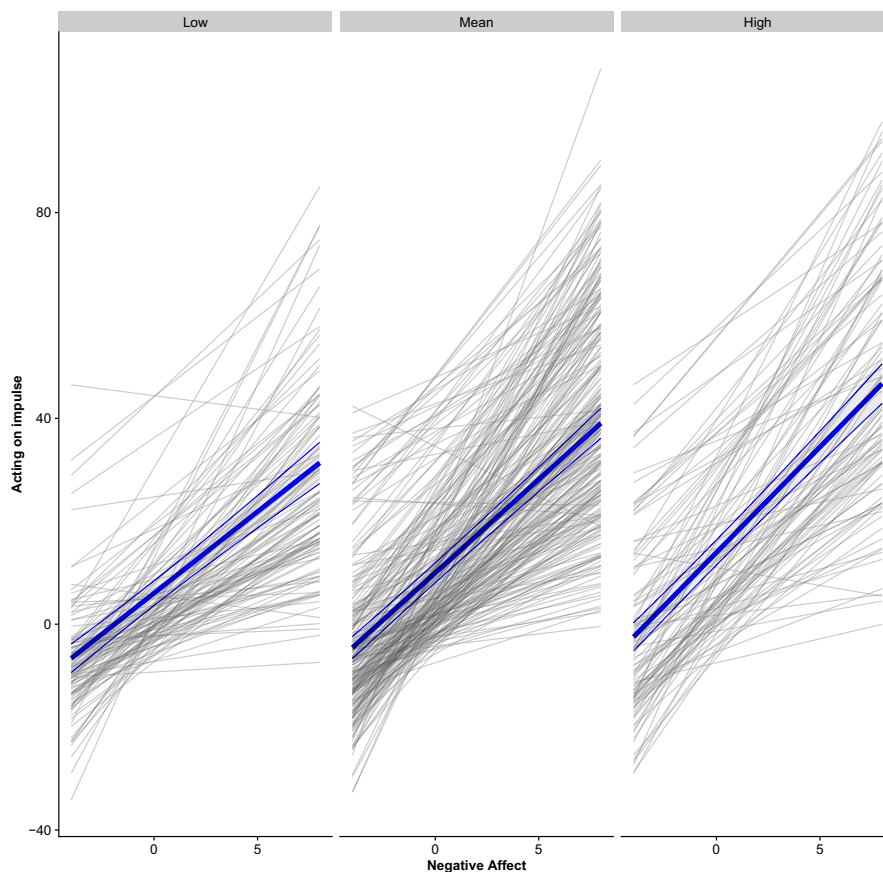


FIGURE 3 Trait urgency moderation of the within-person association of negative affect and momentary urgency.

3.5 | Main effects of positive affect on acting on impulse

Next, we investigated the within-person association between positive affect and state acting on impulse across five models (controlling for each facet of the UPPS-P in separate models). Table 5 presents these findings for the negative urgency moderation model.

Across all models, there was a negative association between positive affect and acting on impulse, which was smaller and of the opposite sign than our GENI ($b=0.05$). For example, when a person reported a 1 *SD* higher in their positive affect, they also reported a -0.06 *SD* change in state acting on impulse ($p < 0.001$),

accounting for the covariates and the main effect of trait negative urgency. As with negative affect, there was variation in this association ($SD=2.57$), such that the association ranged from -0.19 to 0.07 for 68% of participants. In other words, for a subset of participants, the effect was positive and exceeded our GENI, but on average, the effect was small and opposite of what was predicted. Figure 5 illustrates the association between positive affect and acting on impulse.

There was no evidence of moderation by negative or positive urgency larger than our GENI. As in the negative affect models, moderation by trait planning, persistence, or sensation seeking was equivalent to the null hypothesis.

FIGURE 4 Main effects of negative emotions on acting on impulse and moderation by trait impulsivity.

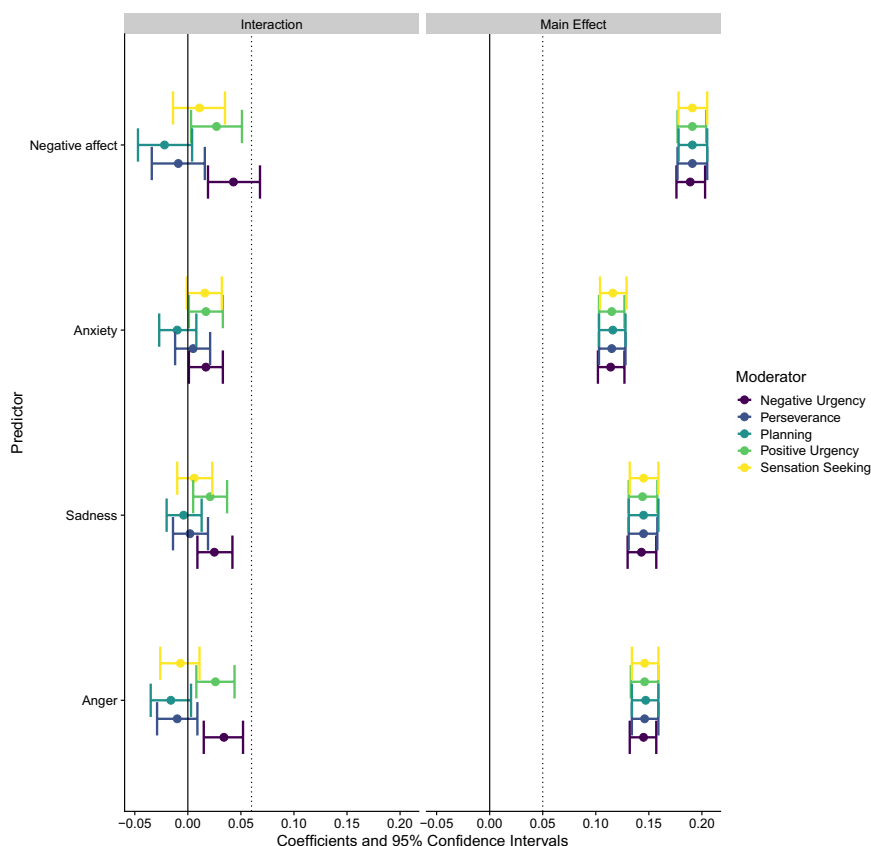


TABLE 5 Within-person effects of positive emotions on acting on impulse.

V1	<i>B</i>	<i>SE</i>	<i>t</i> -value	<i>p</i> -value	<i>B</i>
Intercept	10.23	0.878	11.646	<0.001	–
Positive affect	–1.232	0.134	–9.217	<0.001	–0.064
Negative urgency	3.83	0.744	5.149	<0.001	0.199
Positive affect × Negative urgency	–0.469	0.135	–3.477	0.001	–0.028
<i>SD</i> of intercept	13.059	–	–	–	–
<i>SD</i> of affect slope	2.474	–	–	–	–
Slope–intercept correlation	0.082	–	–	–	–

Note: controls for age, college status, gender, time of day, day of week, observation number, emotion regulation, premeditation, perseverance, and sensation seeking.

3.5.1 | Effects of specific positive emotions

The effects of specific positive emotions were weaker but nearly identical across joviality, attentiveness, and serenity

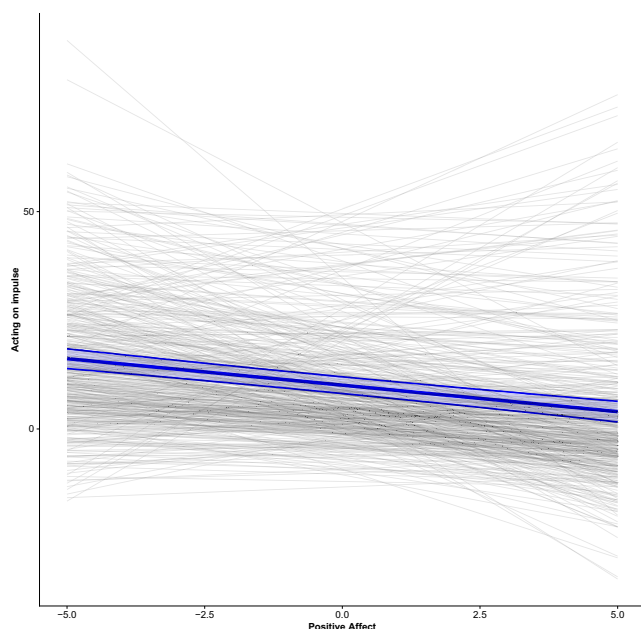


FIGURE 5 Association between positive affect and acting on impulse.

($b = -0.03$ to -0.06 , all $p < 0.001$), and all were the opposite sign of what we predicted. There was no evidence of any trait moderating the effects of positive emotions larger than our GENI. Moderation by planning, persistence, or sensation seeking was equivalent to the null hypothesis. Figure 6 illustrates these results.

3.6 | Confirmatory hypothesis 3A, 3B—Main effects of negative affect on state persistence and planning

3.6.1 | Persistence

Across all models, there was a negative association between negative affect and persistence that was larger than our GENI (-0.05). For example, when a person reported a 1 *SD* higher in their negative affect, they also reported a -0.09 *SD* change in state persistence ($p < 0.001$), accounting for the covariates and the main effect of trait negative urgency, although the random slope for this effect also indicated variation across participants in the magnitude of the effect. Moderation was equivalent to the null across all traits.

There was variation across specific negative emotions, with only sadness ($\beta = -0.105$) and anger ($\beta = -0.053$), but not anxiety ($\beta = -0.025$) exceeding our GENI. Moderation

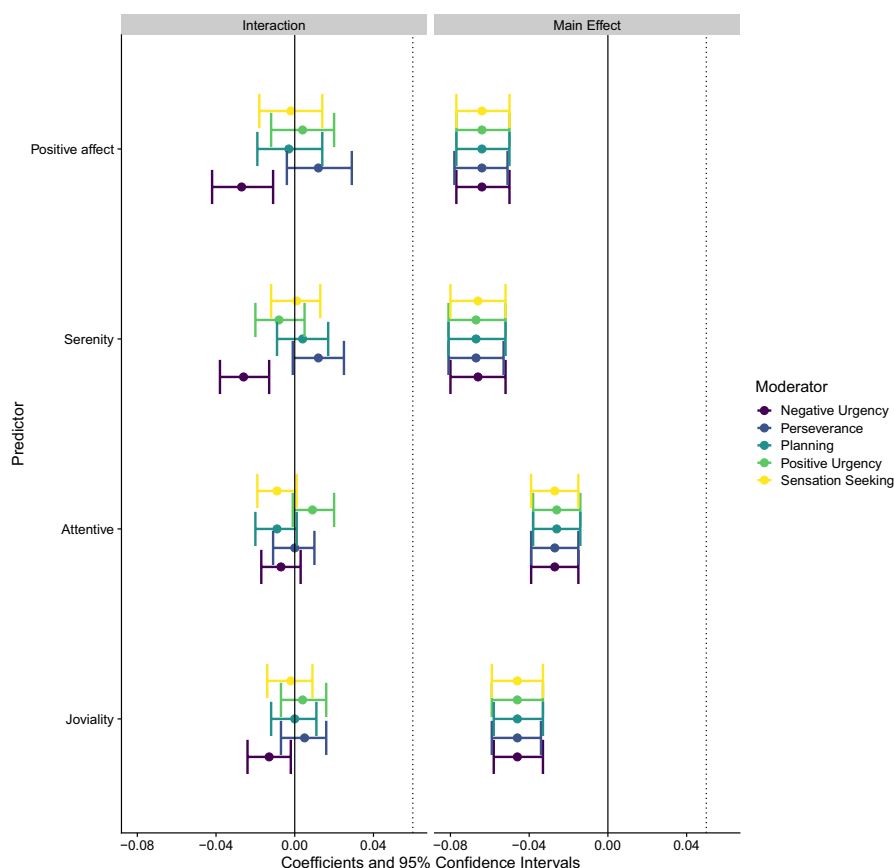


FIGURE 6 Main effects of positive emotions on acting on impulse and moderation by trait impulsivity.

was equivalent to the null across all negative emotions and traits.

3.6.2 | Planning

Across all models, there was a negative association between negative affect and planning larger than our GENI (-0.05). For example, when a person reported a 1 *SD* higher in their negative affect, they also reported a -0.06 *SD* change in state planning ($p < 0.001$), accounting for the covariates and the main effect of trait negative urgency, although the random slope for this effect also indicated variation across participants in the magnitude of the effect. Moderation by negative or positive urgency, or by trait planning, persistence, or sensation seeking was all equivalent to the null.

As with persistence, there was variation across specific negative emotions, where only anger and sadness had effects exceeding our GENI, while anxiety was equivalent to the null hypothesis. Moderation was equivalent to the null across all negative emotion models.

3.7 | Confirmatory hypothesis 4A, B—Main effects of positive affect on state persistence and planning

3.7.1 | Persistence

Across all models, there was a positive association between positive affect and persistence that was larger than our GENI (-0.05). For example, when a person reported a 1 *SD* higher in their positive affect, they also reported a 0.28 *SD* increase in state persistence ($p < 0.001$), accounting for the covariates and the main effect of trait negative urgency, although the random slope for this effect also indicated variation across participants in the magnitude of the effect. Moderation was equivalent to the null across all traits. There was variation across specific positive emotions, but all specific positive emotions were associated with higher levels of state persistence with effects larger than our GENI. Moderation was equivalent to the null across all specific positive emotions.

3.7.2 | Planning

Across all models, there was a positive association between positive affect and planning larger than our GENI (-0.05). For example, when a person reported that they were 1 *SD* higher than usual in their positive affect, they also reported a 0.16 *SD* increase in state planning ($p < 0.001$), accounting for the covariates and the main effect of trait negative

urgency. There was also variation in the magnitude of this effect across participants. Moderation by all traits was consistent with the null hypothesis. As with persistence, there was variation across specific positive emotions, but all specific positive emotions were associated with higher levels of state planning with effects larger than our GENI. Moderation by all traits was consistent with the null hypothesis.

3.8 | Exploratory analyses

3.8.1 | Affective circumplex: Main effects

Valence of emotions

Across all models, there was a positive association between how unpleasant a person reported their mood to be and acting on impulse that was both significant by our exploratory hypothesis *alpha* ($p < 0.001$), and larger than our GENI for confirmatory models. For example, when a person reported that their mood was 1 *SD* less pleasant than usual, they also reported a 0.08 *SD* change in state acting on impulse, accounting for the covariates, although the random slope for this effect also indicated variation across participants in the magnitude of the effect.

Arousal of emotions

Across all models, there was a negative association between how energetic they felt and acting on impulse that was significant by our exploratory hypothesis *alpha* ($p < 0.001$), but smaller than our GENI for confirmatory models. For example, when a person reported that their mood was 1 *SD* more energetic than usual, they also reported a -0.03 *SD* change in state acting on impulse. The random slope for this effect also indicated variation across participants in the magnitude of the effect.

Valence \times Arousal

Across all models, the interaction of valence and arousal was significant. In short, the association between valence and acting on impulse became weaker as arousal was heightened ($b = -0.28$, $p < 0.001$), the opposite of our hypothesis that highly arousing and negatively valenced states would produce the highest ratings of acting on impulse. Figure 7 illustrates this result.

Moderation by urgency or other traits of this two-way interaction was not significant.

3.8.2 | Affective circumplex: Moderation by traits

There was some weak evidence of moderation of the main effects of valence and arousal on acting on impulse by

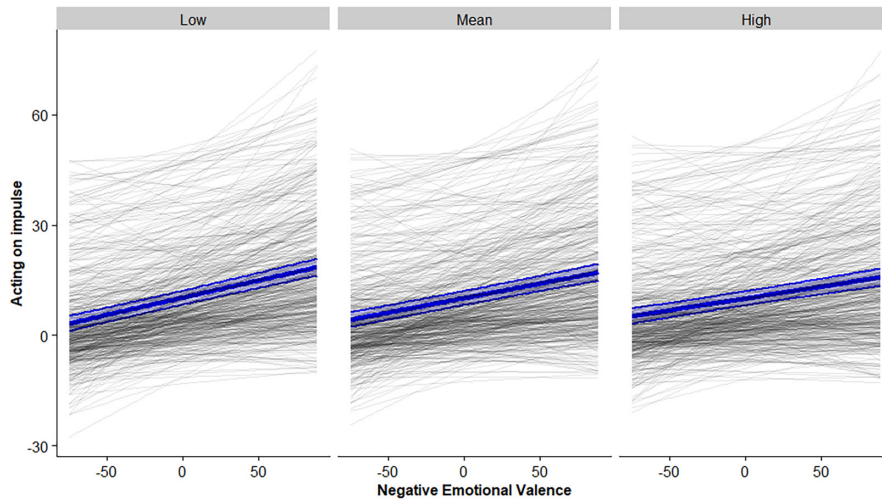


FIGURE 7 Concurrent within-person association of emotional valence and acting on impulse across levels of arousal.

negative urgency, such that the effect was significant by our exploratory hypothesis threshold ($p < 0.001$), but not larger than our GENI. Moderation by other impulsive traits was not significant.

3.8.3 | Appraisals: Main effects

Importance appraisals

Across all models, there was a positive association between how important a person reported that it was to change how they feel and acting on impulse that was both significant by our exploratory hypothesis α ($p < 0.001$), and larger than our GENI for confirmatory models. For example, when a person reported that their mood was 1 *SD* less pleasant than usual, they also reported a 0.15 *SD* change in state acting on impulse, accounting for the covariates, although the random slope for this effect also indicated variation across participants in the magnitude of the effect.

Confidence appraisals

Across all models, there was no evidence that how confident a person reported they were that they could change how they feel was associated with acting on impulse that was significant by our exploratory α ($p < 0.001$), and all effect sizes were small (< 0.02). Moderation by traits was not significant. Random slopes for this effect did indicate variation across participants in the magnitude of the effect.

Importance \times Confidence

Across all models, the interaction of importance and confidence was significant. In short, the association between the importance of changing one's emotions and acting on impulse became weaker as confidence was heightened ($b = -0.34$, $p < 0.001$), in line with our hypothesis that when people were not confident that they

could change how they felt, wanting to change how they felt would be more strongly associated with acting on impulse. Contrary to expectations, this effect was weaker at higher levels of negative ($b = -0.15$, $p = 0.0017$) and positive urgency ($b = -0.12$, $p = 0.0086$), although neither reached the significance threshold ($p < 0.001$) we set for exploratory analyses. Moderation by other traits was not significant.

Finally, we replicate these models predicting planning and persisting. Results mirrored findings for urgency. In short, when people viewed their emotions as more positively valenced, more arousing, and were more confident in their ability to change how they felt, people also reported feeling more planful and persistent ($\beta = 0.05\text{--}0.15$). When people reported that it was more important to change how they felt, they also reported feeling less persistent. We observed no significant evidence of interactions among these variables or moderation by traits.

4 | DISCUSSION

In this registered report, we aimed to replicate and extend prior research on global self-reports of trait negative urgency, which is described as individual differences in how impulsive people are when they experience negative affect. Our findings suggest that although acting on impulse in any given moment is associated with negative emotional states, as well as people's appraisals of those states, global self-reports of urgency do not reliably capture individual differences in these emotions. Multiple prior studies conducted in people's daily lives had shown that in moments when people reported experiencing negative emotions, they also reported higher than usual acting on impulse, but evidence was mixed as to whether this within-person association was or was not explained by global self-reports

of negative urgency (Feil et al., 2020; Sharpe et al., 2021; Sperry et al., 2016, 2018).

4.1 | Within-person associations of emotions and impulsive behaviors

Impulsive behaviors can stem from a failure to plan, a failure to persist toward an important goal, or a failure to control one's impulses (Whiteside & Lynam, 2001). In line with prior studies (Feil et al., 2020; Sharpe et al., 2021; Sperry et al., 2016, 2018), there was strong evidence that when people experienced higher than usual negative emotions, they also reported more acting on impulse. Our findings of substantial random effects in these associations also confirm a core contention of urgency theory: some people are especially prone toward impulsiveness while experiencing negative emotions. These findings generalize across forms of momentary impulsivity: in moments of higher negative emotion, people also reported less persisting toward their goals and moderately less planning (Feil et al., 2020). Given that acting on impulse was very weakly associated with planning or persisting in the moment ($r = -0.07$ – 0.08), and momentary planning and persisting were only modestly correlated ($r = 0.47$), this indicates a *generalized* increase in different forms of momentary impulsive behaviors under conditions of negative affect. This is consistent with laboratory experiments which suggested that negative emotions increased preference for immediate gratification, decreased response inhibition, and decreased people's abilities to persist toward difficult goals (Baumeister et al., 2007; Leith & Baumeister, 1996; Tice et al., 2001), although there are also substantial critiques and high profile failures to replicate this experimental work (Hagger & Chatzisarantis, 2016). Similar to research on impulsiveness at the trait level (Whiteside & Lynam, 2001), this implies that it is important to measure multiple forms of impulsive behaviors at the momentary level (see also Seldin et al., 2024) when seeking to understand how and when people might be prone to different forms of impulsive behavior. Future research should seek to understand *when* negative emotions are especially disruptive, such as in the presence of peers or certain kinds of social interactions (such as conflict), or when people use certain kinds of emotion regulation strategies (such as suppression or rumination).

Conversely, when people reporting feeling more positive emotions, they reported less acting on impulse (but only weakly), more planning, and even more persisting toward their goals (despite a modest momentary correlation between positive and negative emotions, $r = -0.28$). This presents a challenge to common models of emotion and behavior dysregulation, which often posit that *both*

positive *and* negative emotions can produce dysregulated behavior. For example, theories of psychopathology hypothesize that difficulties regulating in the face of positive emotions (e.g., positive urgency, positive emotion dysregulation) make people especially prone to psychopathology (Cyders & Smith, 2008; Elliott et al., 2023; Weiss et al., 2015), and experiencing positive emotions has been consistently shown to precede risky behavior such as alcohol or cannabis use in daily life studies (Dora et al., 2022, 2023). It is possible that we did not find these hypothesized effects because they only occur at the extremes of positive emotion (such as in manic states), or only for some people in certain situations (such as when they are around peers, but not when they are alone). It is also possible that theories of positive emotion dysregulation, which are largely based on people's retrospective attributions of the causes of their behavior, do not accurately describe people's momentary experiences. Below we discuss alternative explanations of why people might retrospectively attribute positive emotions with dysregulated behavior.

Some theories hypothesized that traits emerge as situation-specific contingencies between psychologically relevant stimuli (such as specific emotions) and behavior (Fleeson, 2007; Shoda et al., 2002). Along this line, different emotions (such as anger, anxiety, and sadness) were thought to differ in the degree to which they produce approach versus avoidance-motivated behaviors, and impulsive behaviors were hypothesized to be produced by activating emotional states (such as anger or excitement), rather than those that were not (such as sadness or serenity; Carver & Harmon-Jones, 2009). In the present study, we found compelling evidence against these hypotheses. Neither examining the experience of specific emotions, nor using an affective circumplex framework, produced compelling evidence that there was specificity in the kinds of emotional states that were more or less likely to produce impulsive behavior. Although future research should seek to determine whether and when specific emotional states differentially predict behavior, our findings indicate that what (mostly) matters in terms of influencing impulsive behavior is the intensity and negativity of emotions, rather than its level of arousal or the specific negative emotion word used to label it.

In terms of appraisals, we had hypothesized that people's appraisals of their emotional states might better identify situations when people are more prone to act impulsively, and some work had suggested that people's beliefs and expectancies about the function of impulsive actions to reduce emotional distress might influence impulsive behaviors (Adams et al., 2012; Fischer et al., 2004). We found evidence that people's appraisals

of their emotions were associated with acting on impulse, such that when people reported that it was important to change how they felt, they were less likely to report acting on impulse if they also felt confident they could change how they felt. However, we also found evidence that this effect was *dampened*, not strengthened, by trait urgency, which undermines the hypothesis that trait urgency reflects momentary appraisal processes rather than people's emotional experiences. Future research should seek to measure a broader range of momentary appraisals, such as distress tolerance (Veilleux, 2023), to better understand the interplay of cognitive and emotional factors that may be associated with momentary impulsive behaviors.

4.2 | Moderation of the momentary emotion–impulsivity link

Prior work had reported mixed evidence as to whether global self-reports of urgency were reflected in a strong within-person link between negative emotions and momentary impulsivity (Feil et al., 2020; Sharpe et al., 2021; Sperry et al., 2018, 2021), and no work had tested whether this generalized, as hypothesized, to positive urgency. We found no evidence that people's global self-reports of acting on impulse in the face of negative or positive emotions reflected how they experience these processes, on average, in their daily lives. In line with other prominent failures to align people's attributions of the motivations for their behavior (e.g., Dora et al., 2022; Nisbett, 1977), we must first consider that global self-reports of urgency cannot be taken at face value to reflect individual differences in how much people act on impulse when experiences negative emotions. In other words, studies that infer anything about the causes of or motivations for impulsive behavior in people's daily lives based on global self-reports of urgency, are going far beyond the available evidence. People high on positive and negative urgency did, however, report higher average levels of *both* acting on impulse and negative emotions in their daily lives ($r = 0.25\text{--}0.34$). Thus, these traits seem to capture the *frequent* experience of both emotions and impulsiveness, but not necessarily the “cause” of the impulsiveness. There are parallels in other research: people high on coping motives for alcohol use experience, both more negative emotions and drink more in their daily lives, but do not show stronger within-person associations of emotions and drinking (Dora et al., 2022). It is possible that people may develop incorrect attributions about the associations between emotions and behaviors when they frequently experience both in their daily lives. It may also be that the small interactions we did observe are meaningful, but it is critical for future researchers to move beyond statistical significance and

focus on identification of meaningful effect sizes that can articulate how small interactions may yet be influential. It may also be that there are alternative traits closely related to urgency (such as maladaptive patterns of emotion regulation; King et al., 2018) that actually explain individual differences in the momentary process of urgency. It is also possible that global reports of urgency reflect highly salient but rare instances of acting on impulse in the face of strong negative emotions, which are not reliably observed during the relatively short and intense period of EMA that was deployed in the present study. Finally, it may be that it is not the experience of negative emotions and impulsive behavior that informs people's global self-reports of urgency, but rather their later appraisals of the outcomes of their behaviors. Indeed, many urgency items are characterized by such evaluations (e.g., “Something I later regret”), and it may be that people who (or when people) regret behaviors also report higher levels of urgency. To date, the majority of research has focused on what happens prior to or at the same time as impulsiveness, but global self-reports are by definition retrospective. It may be important to study how, when, and which impulsive moments are later evaluated as being caused by negative affect, and how those evaluations shape people's global self-reports of urgency. Future research should focus on how people come to understand themselves as being high on urgency.

4.3 | Limitations and constraints on generality

This study was well powered, with a larger and broader sample studied for a longer period of time than prior studies. We also measured a broader range of momentary impulsive behaviors and specific emotional states, while also comprehensively testing moderation by multiple impulsive traits. At the same time, the present study has several limitations and constraints on generality. First, because the larger study was focused on alcohol and cannabis use, we only sampled participants during the social weekend (Thursday–Monday morning) and focused on a relatively narrow age range in young adulthood (18–22). Given that impulsivity is thought to decline with age (King et al., 2014; Steinberg et al., 2008), it is possible that these findings may not generalize to older adults who generally have lower levels of impulsivity. Moreover, because impulsivity is itself a risk factor for substance use (Berg et al., 2015), these findings may not generalize to people who are not regular cannabis or alcohol users. Our results also rely on people's self-report of their perceptions of momentary impulsiveness and emotions, and may not generalize to other types of measures (e.g., self-reports of specific impulsive actions

or task measures of impulsive behaviors), especially to the degree that different measures tap different response processes or heuristics among those who answer. We also took a relatively conservative approach to the GENI by relying on estimates reported in our prior studies rather than relying only on significance testing. This approach runs the risk of excluding small but stable effects that could potentially accumulate over time. On the other hand, because of the very high within-person power of the present study, we avoided presenting results that were significant but not meaningful in our opinion. Finally, our study was also conducted during the COVID-19 pandemic, and it is possible that differences in patterns of work, school, and social behaviors changed the relationship between people's emotions and impulsive behaviors.

AUTHOR CONTRIBUTIONS

Kevin King: Conceptualization, methodology, software, validation, formal analysis, investigation, resources, data curation, writing—original draft, writing—review and editing, visualization, supervision, project administration, and funding acquisition. Madison Feil: Conceptualization, data curation, writing—original draft, and writing—review and editing. Nancy Gomez Juarez: Software, validation, formal analysis, writing—original draft, writing—review and editing, and visualization. Diego Moss: Methodology, software, investigation, data curation, writing—original draft, writing—review and editing, and supervision. Max Halvorson: Writing—original draft and writing—review and editing. Jonas Dora: Software, validation, data curation, writing—original draft, and writing—review and editing. Natalie F. Upton and Morgan A. Bryson: Methodology, investigation, writing—review and editing, and supervision. Katherine Seldin: Data curation, writing—original draft, and writing—review and editing. Yuichi Shoda, Christine M. Lee, and Gregory T. Smith: Writing—review and editing, and funding acquisition.

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The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

ETHICS STATEMENT

We conducted all studies in accordance with the Ethical Standards of the American Psychological Association. This study was approved by the University of Washington IRB (#00006464, "Project SMS").

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