HHS Public Access

Author manuscript

Addict Behav. Author manuscript; available in PMC 2019 August 01.

Published in final edited form as:

Addict Behav. 2018 August ; 83: 130–135. doi:10.1016/j.addbeh.2017.11.031.

Momentary Assessment of Impulsive Choice and Impulsive Action: Reliability, Stability, and Correlates

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Abstract

Impulsivity is associated with substance use, including tobacco use. The degree to which impulsivity fluctuates over time within persons, and the degree to which such intra-individual changes can be measured reliably and validly in ambulatory assessments is not known, however. The current study evaluated two novel ambulatory measures of impulsive choice and impulsive action. Impulsive choice was measured with an eight-item delay discounting task designed to estimate the subjective value of delayed monetary rewards. Impulsive action was measured with a two-minute performance test to assess behavioral disinhibition (the inability to inhibit a motor response when signaled that such a response will not be rewarded). Valid data on impulsive choice were collected at 70% of scheduled reports and valid data on impulsive action were collected on 55% of scheduled reports, on average. Impulsive choice and action data were not normally distributed, but models of relations of these measures with within- and between-person covariates were robust across distributional assumptions. Intra-class correlations were substantial for both impulsive choice and action measures. Between persons, random intercepts in impulsive choice

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These findings have not been published and have not been submitted elsewhere. The authors have full control of the content of this study.

Contributors: Authors McCarthy and Chapman designed the study. Authors McCarthy, Minami, Bold, and Yeh wrote the protocol. Author McCarthy conducted the statistical analyses and wrote the first draft of the manuscript and all authors contributed to and have approved the final manuscript

Conflicts of interest: None

and action were significantly related to laboratory levels of their respective facets of impulsivity, but not self-reported or other facets of impulsivity. Validity of the ambulatory measures is supported by associations between abstinence from smoking and increased impulsivity, but challenged by an association between strong temptations to smoke and reduced impulsive choice. Results suggest that meaningful variance in impulsive choice and action can be captured using ambulatory methods, but that additional measure refinement is needed.

Keywords

Impulsive choice; impulsive action; delay discounting; behavioral disinhibition; ecological momentary assessment; smoking cessation

1. Introduction

Impulsivity is a broad construct of relevance to substance use (Gullo & Potenza, 2013). The varied facets of impulsivity are measured in diverse ways (e.g., self-report, performance tests), with weak correlations among facets (de Wit, 2009; McCarthy et al., 2016; Reynolds, Ortengren, Richards, & de Wit, 2006; Stanford et al., 2009). Although broad conceptualizations of impulsivity have not replicated well, two-facet models focused on impulsive choice (strong motivation to approach rewards) and impulsive action (inability to inhibit responding), appear to fit animal and human data well (Gullo, Loxton, & Dawe, 2013).

Impulsive choice (the tendency to make short-sighted, self-defeating decisions) is typically measured by administering a series of intertemporal choices (e.g., “Would you prefer $88 today or $100 in one week?”) in which a respondent is asked to select either a smaller reward available sooner or a larger reward available after a delay (Johnson & Bickel, 2002). Choosing a smaller reward sooner reflects discounting of delayed rewards. Such discounting is associated with substance use, including smoking (Baker, Johnson, & Bickel, 2003; Bickel, Odum & Madden, 1999; Mitchell, 1999; Reynolds, 2004).

Impulsive action is often operationalized as the failure to inhibit a response when this is optimal (Dick et al., 2010; Mitchell, 2004). In validated laboratory paradigms, a participant is instructed to inhibit a motor response when a stop or no-go stimulus is present. Failing to inhibit a motor response (disinhibition) may be punished or unrewarded, making disinhibition maladaptive in this context (much like failing to inhibit smoking is maladaptive when trying to quit). In cross-sectional research, disinhibition is positively associated with smoking (McClernon et al., 2008; Mitchell, 2004).

Impulsive choice and impulsive action are typically treated as trait-like. Scores on impulsive choice and action measures tend to exhibit at least moderate test-retest reliability (Ohmura, Takahashi, Kitamura, & Wehr, 2006; Weafer, Baggott, de Wit, 2013). Recent work suggests, however, that stability in impulsivity scores may depend on concordant substance use status across measurement occasions (McCarthy et al., 2016). Other evidence suggests that discounting and disinhibition are influenced by states (e.g., nicotine withdrawal) and contexts (e.g., stressful events), which suggests that impulsive choice and action may be

state-like (Field, Santarcangelo, Summal, Goudie, & Cole, 2006; Jones, Christiansen, Nederkoorn, Houben, & Field, 2013; Schepis, McFetridge, Chaplin, Sinha, & Krishnan-Sarin, 2011; Weafer et al., 2013).

The current research question was whether impulsive choice and action measured with ambulatory assessments (hand-held electronic diaries) fluctuated within smokers attempting to quit, and whether these fluctuations were associated with recent events or internal states. Participants completed validated, laboratory measures of impulsive choice and action (see McCarthy et al., 2016) and carried electronic diaries for one week pre-quit and three weeks post-quit to complete investigator-initiated assessments at four unpredictable times per day. Reports comprised: a novel eight-item ambulatory impulsive choice task adapted from validated laboratory delay discounting tasks (Johnson & Bickel, 2002); a novel, two-minute ambulatory measure of impulsive action adapted from Conner's Continuous Performance Test-II (Conners, 2004); and self-reports of recent events (smoking, temptations, and trigger exposures) and internal states (withdrawal symptoms, and motivation and confidence to quit smoking). The present analyses address the sources of variance (between- versus within-subjects), reliability, and validity of these novel real-time measures of impulsive choice and action. Validity was investigated by examining relations between laboratory and ambulatory measures of impulsivity, and relations between states (e.g., abstinence, affect, motivation) and momentary impulsivity.

2. Method

2.1. Participants

The Rutgers, the State University of New Jersey institutional review board approved the protocol. Participants who volunteered in response to media advertisements provided written informed consent for this study. Inclusion criteria included: being at least 18 years old; smoking at least 10 cigarettes per day for at least 6 months; providing an expired breath sample with at least 8 parts per million carbon monoxide (CO); and being motivated to quit smoking and willing to complete study activities. Exclusion criteria included: contraindications to use of the nicotine lozenge (i.e., recent heart attack or heart surgery, irregular heartbeat, heart disease, or adverse reactions to the nicotine lozenge); history of bipolar disorder, schizophrenia, or psychosis; pregnancy, breastfeeding, or unwillingness to prevent pregnancy during the study; current use of stop-smoking treatments; regular use of marijuana, illegal drugs, or other forms of tobacco; and living with another participant.

2.2. Procedures

Individuals who met eligibility criteria in a telephone screen provided informed consent prior to completing group in-person screening including CO testing and baseline questionnaires. Enrollees were given palmtop computers (EDs, Palm Z22 Palmtop Computer, Palm, Inc., Santa Clara, CA) for ecological momentary assessment (EMA, Stone & Shiffman, 1994). Participants were prompted at four pseudo-random times per day (once per quarter of the waking day with at least 30 minutes between reports) for 31 days (3 practice days, one week pre-quit, and three weeks post-quit). Enrollees completed five weekly office visits and a 15-minute follow-up call 12 weeks after the target quit day. All

participants received evidence-based smoking cessation treatment [four 15-minute counseling sessions based on Clinical Practice Guidelines (Fiore et al., 2008) and nicotine lozenges (4 mg for those who smoked within 30 minutes of waking, 2 mg for all others) for up to 12 weeks post-quit] at no cost. Participants could earn up to $130 for completing visits and calls. To enhance task engagement, participants could earn up to $300 for laboratory impulsive choice tasks, up to $21.60 for laboratory impulsive action tasks, up to $75 for an EMA impulsive choice task that occurred on the 90th of 124 scheduled EMA reports, and up to $148.80 for EMA impulsive action tasks ($0.02 for every correct response, up to $1.20 per report). These bonuses were based on responses (e.g., the participants' choice of $25 in 1 week or $75 in 2 weeks on the 90th EMA report; the number of correct responses on impulsive action tasks). The average bonus earned was $331 (range $27 to $512, mean=$26/hour).

2.3. Measures

At enrollment, demographics, nicotine dependence (Heatherton, Kozlowski, Frecker, & Fagerström, 1991), and smoking history were assessed (Table 1).

2.3.1. Impulsive choice: Delay discounting task—Participants completed an intertemporal choice task modeled after Johnson and Bickel (2002) at the baseline laboratory visit and at every EMA report. This task identifies indifference points, or the amount of money subjectively equivalent to a larger reward available later. In the laboratory version programmed in DMDX software (Forster & Forster, 2003), indifference points identified by repeated choices between smaller, immediate rewards and rewards of $20, $50, $100, delayed by 1, 7, or 30 days (fully crossed) were used to compute an average daily discounting rate: $k = 1/D(V_d/V_p - 1)$, where D is the delay in days, V_d is the value of the delayed reward in dollars, and V_p is the value of the present (or smaller, sooner) reward in dollars, or indifference point (Johnson & Bickel, 2002).

In the EMA task programmed in Pendragon Forms 4.0 (Chicago, IL), average daily k in the baseline laboratory session was used to tailor the temporal choice items using the formula $V_p = V_d/(1+ kD)$ to determine the initial smaller reward value. In each report, the delayed amount was fixed to $20, $50, or $100 and the delay was fixed to 1, 7, or 30 days. Amounts and delays were fully crossed across reports and presented in randomized order. Across the eight items in each report, only the magnitude of the smaller, sooner reward varied. Choices on each item reset upper and lower limits for the indifference point. The initial lower limit was set at 25% of the V_p and the initial upper limit was set at 75% of the V_d to facilitate convergence on an indifference point in only eight items. Choosing the smaller reward on an item reset the upper limit to the current V_p and did not change the lower limit. Choosing the larger reward reset the lower limit to the current V_p and did not change the upper limit. The next V_p was set to the midpoint between the upper and lower limits. The V_p after the eighth item was used to calculate mobile discounting rate k as above. If delayed rewards were chosen on all eight items, estimated k was set at 75% below baseline (i.e., the full range was truncated because the true indifference point was not detected).

2.3.2. Impulsive action: Behavioral disinhibition—A 360-trial modified Continuous Performance Test-II (CPT-II; Conners, 2004) was administered at the baseline laboratory visit (using DMDX software, Forster & Forster, 2003) and a 60–66-trial (mean 60.47, *SD*=. 78 trials) version of the task was presented at every EMA report (using MiniCog software, Cambridge, MA; Shephard, Kho, Chen, & Kosslyn, 2006). Respondents were instructed to press a key whenever a letter appeared, except when the letter was an X; 90% of trials were non-X trials in which the correct response was a key press, 10% of trials were X "no-go" trials in which the correct response was to inhibit responding. The same letter stimuli, stimulus duration (250 ms), inter-trial intervals (1, 2, or 4 s), randomized item presentation order, and instructions were used in the laboratory and EMA tasks. Feedback regarding correct responses and money earned was available in both versions of the task, although viewing EMA results required entering a passcode. In both laboratory and EMA tasks, we computed summary omission error rates (proportion of non-X trials on which no response occurred) indicative of inattention; mean response latencies on non-X "go" trials (in ms); and commission error rates (proportion of X-trials on which a response occurred) indicative of disinhibition.

2.3.3. Covariates—EMA reports assessed subjective experiences ["willing to work hard at quitting" and "confident I can quit smoking for good", rated from 1=not at all to 7=extremely; agitation (mean of "impatient," "tense or anxious," and "restless"); distress (mean of "sad or depressed," "upset," and "distressed"); cigarette cravings (mean of "bothered by the desire to smoke" and "trouble getting cigarettes off my mind") all rated from 1=not at all to 5=extremely] using items derived from validated scales (Watson, Clark, & Tellegen, 1988; Welsch et al., 1999) and previous EMA studies (McCarthy et al., 2010; McCarthy, Minami, Yeh, & Bold, 2015), with promising validity (Bold, McCarthy, et al., 2016; Minami et al., 2014). Questions regarding recent events (temptations in the last 30 minutes, stressful events, exposure to smokers and smoking opportunities in the last 15 minutes, all measured as yes=1, no=0), and behaviors (alcohol or caffeine consumption, smoking, coded as yes=1, no=0) were also assessed. Baseline self-reported attentional, motor, and non-planning impulsivity assessed with the Barratt Impulsiveness Scale (BIS-11; Patton, Stanford, & Barratt, 1995) were screened for association with ambulatory impulsive choice and action.

2.4. Analysis Plan

Report-level descriptive analyses of EMA impulsive choice and action, inter-item correlations, and reliability were conducted in SPSS 22.0 (IBM Corporation, Armonk, NY). Multilevel models were conducted in HLM 6.04 (Scientific Software International, Inc., Skokie, IL) to partition within- and between-subject variance and examine concurrent validity by estimating relations between laboratory and EMA measures of impulsivity. Models used full maximum likelihood estimation and permitted missing data at the report level. Listwise deletion was used at the person level. Coefficients were set as random if there was significant variance in the coefficient and if this decreased model deviance. In all models, the significance and variance of recording epoch (pre- vs. post-target quit day), any smoking since the last report (1=yes, 0=no), and their interaction were tested and only

significant terms were retained. All report-level covariates were entered simultaneously and then removed from the model one-by-one if not significant.

3. Results

3.1. Response rates

Participants completed an average of 69.6% of EMA impulsive choice reports. Report durations (2.32 minutes, SD=1.12 minutes) and response patterns to reverse-coded items suggested that participants read the items. Participants completed an average of 61.9% of EMA impulsive action tasks, which required proactive navigation following the self-report measure. When EMA reports in which the omission error rate (failure to hit a key when a non-X letter appeared) exceeded 5% were excluded (12.4% of reports), the mean response rate fell to 55.0, uncorrected for attrition. Data were not missing completely at random (Little's *Chi square*=1827.703, *df*=105, $p<.001$).

3.2. Variance and Reliability

There was limited variance in the EMA discounting measure and this was non-normal (*skewness*=7.98, *SE*=.03; *kurtosis*=95.95, *SE*=.06). Participants elected the larger, later reward in all eight trials in 52.2% of reports. The smaller, sooner reward was selected across all eight trials in only 1% of reports. Given the constraints imposed on smaller reward magnitudes in the task, it is reasonable to conceptualize the eight trials as redundant items. Internal consistency of response across all eight trials was high (*Cronbach's alpha*=.82) and Guttman split-half reliability was adequate (.74). To address range restriction, impulsive choice was coded as binary (where 1=increased discounting from baseline (19.7% of reports) and 0=no change or a decrease in discounting) for sensitivity analyses.

Ambulatory impulsive action (commission error rate) data were also non-normal (*skewness*=3.02, *SE*=.03; *kurtosis*=13.34, *SE*=.003). Square root transformation reduced this to acceptable levels (*skewness*=1.75, *SE*=.04; *kurtosis*=1.65, *SE*=.07). The EMA measure with six no-go trials was much less reliable (*Guttman split-half reliability*=.32) than the full-length laboratory version (*Guttman split-half reliability*=.88; McCarthy et al., 2016). Guttman split-half reliability is higher for EMA omission error rates (.83) and reaction time on non-X trials (.83, *Mean*=469.54 ms, *SD*=5.89 ms). Poisson models treating commission errors as a count variable were conducted for sensitivity analyses.

3.3. Intra-class correlations

The intra-class correlation for EMA discounting rate was .58 (i.e., 58% of the variance in momentary discounting rates was at the person level, and 42% at the report level). For the binary indicator of increases in discounting from baseline, 83% of variance was at the person level. For EMA disinhibition, only 14% of the variance in square-root transformed commission error rates was at the person-level. When treating commission error rates as a count variable, the intra-class correlation was .41.

3.4. Covariates

3.4.1. Impulsive Choice—Covariate relations (see Table 2) were generally consistent across dependent variables. In the final model of EMA impulsive choice, there was no significant difference between pre- and post-target quit day levels, and this relation did not vary significantly across participants or interact with recent smoking. Separate analyses of the first four days before the quit day indicated that baseline impulsive choice was strongly and positively related to both continuous and binary measures of EMA impulsive choice (not shown). Throughout the EMA period, impulsive choice was not significantly related to recent smoking at the average level of laboratory discounting. Impulsive choice increased following abstinence significantly more among those with higher laboratory discounting rates, however. Unexpectedly, impulsive choice was lower when a recent temptation was reported than when not, particularly among higher in laboratory discounting. In sensitivity analyses, results for the binary coding of impulsive choice were similar, except that binary increases in impulsive choice were significantly less likely post-quit than pre-quit (*Estimate*=−.69, *SE*=.21, *p*=.002), regardless of laboratory discounting (*Estimate*=−.38, *SE*=.24, *p*=.12).

3.4.2. Impulsive Action—The final model of EMA impulsive action did not include a post-quit indicator, as there was no significant difference in error rates pre- versus post-quit, this did not differ across persons, and this did not interact with recent smoking. Both in models of the full EMA epoch and in analysis of the first four days of EMA pre-quit, concurrent validity was supported by significant report-level positive associations between omission error rates and transformed commission error rates and by significant person-level positive associations between baseline commission and omission error rates. Commission error rates were negatively associated with baseline reaction times on “go” trials (a cross-level speed-accuracy trade-off, Heitz, 2014), but this effect was non-significant when report-level reaction times and omission error rates were added, or when commission errors were modeled as a count variable. Smoking since the last report was the only covariate significantly related to impulsive action; commission errors were significantly lower after smoking than abstinence, to a degree that varied across persons. Poisson models that treated the number of commission errors made as a count (0–6) yielded similar results (not shown).

3.4.3. Relations Across Facets—At the report level, higher omission error rates (inattention) were associated with lower discounting rates (*Estimate*=−.48, *SE*=.21, *p*=.03). This was not robust (it was not significant in binary models of within-person impulsive choice increases). No other associations between impulsive choice and action were significant. Scores on BIS-11 second-order factors were not significantly related to EMA impulsive choice or action.

4. Discussion

This study evaluated novel ambulatory measures of impulsive choice and action. Analyses focused on the variance, reliability, and concurrent validity of an eight-item measure of delay discounting, a form of impulsive choice, and a two-minute measure of behavioral disinhibition, a form of impulsive action. Results indicated that, despite limited variance in

impulsivity, within-person variance in impulsivity across reports was associated with validated laboratory measures of the same facet of impulsivity. Results also showed that abstinence since the last report is associated with increased impulsivity, consistent with prior literature (Field et al., 2006; Jones et al., 2013). These findings were consistent for both impulsive choice and action. Results also highlighted important challenges in assessing impulsivity using mobile technology, such as missing data, unreliability, and distraction.

Developing reliable and valid EMA measures that are sufficiently brief to facilitate high response rates is a challenge. Missing data and narrow response distributions affected both the impulsive choice and impulsive action measures. Reliability of the impulsive action measure was low. The extent to which the substantial within-person variance in this measure reflects impulsivity or measurement error is not clear. Despite these challenges, significant positive relations between laboratory and EMA-measured impulsivity support EMA measure validity. Both impulsive choice and action were also positively associated with states thought to affect impulsivity (abstinence, e.g., Giordano et al., 2002; Jones et al., 2013), such that recent abstinence was associated with greater impulsivity, compared within-subjects to recent smoking.

Two findings challenge the concurrent validity of the impulsivity measures. First, recent temptation events were associated with less impulsive choice, particularly among those with above-average laboratory discounting impulsivity. This robust relation occurred both with and without a recent smoking covariate. This seems to contradict research showing increased discounting in deprivation states (Field et al., 2006). Ratings of urge intensity were not associated with impulsivity, so this association may reflect a process linking less impulsive choice with the decision to complete an EMA report and/or endorse a strong temptation (i.e., greater adherence to reporting instructions), rather than with the experience of strong urges to smoke. As such, EMA measures of impulsivity are related to recent smoking in ways that support the validity of these measures, but are related to temptations in ways that challenge existing notions and merit further investigation.

Another surprising finding was that impulsive action was associated with longer reaction times on the EMA impulsive action test, in contrast to the typical trade-off observed between speed and accuracy (Heitz, 2014). Ambulatory commission error rates were significantly associated with slower reactions on “go” trials, despite excluding reports in which inattention may have suppressed commission errors. Commission error rate were positively related to inattention as indexed by omission error rates (in both EMA and the laboratory). It may be that inattention drives both slow responding and low rates of key-presses on the non-X trials. Rewarding participants for faster response times may be a helpful refinement of this task that might increase variance in commission errors and sensitivity of the task.

There are important limitations to note in this study. Missing data were not missing completely at random in which participants carried a dedicated digital device (now made obsolete by smartphones) for a full month. In addition, there was no true baseline in this study (recording started one week pre-quit, a period in which anticipatory reactions are known to occur, McCarthy et al., 2006), and a non-smoking control group was not included, so we do not know how well the laboratory and ambulatory measures of impulsivity relate in

the general population or outside the context of smoking cessation. Response rates were low for the impulsive action task that required effortful initiation, despite incentives for completing this task. Redesigning assessments so they are easier to complete (e.g., on personal smartphones) and require less time may improve response rates and support stronger inferences. In addition, additional "no-go" trials may be needed to achieve acceptable reliability in the measure of impulsive action. Tailoring to baseline discounting rate and truncating the range of rewards displayed reduced meaningful variance in EMA discounting rates. Using an alternative approach such as the five-trial adjusting delay task developed by Koffarnus and Bickel (2014) may be a better way to efficiently measure impulsive choice in future research.

5. Conclusions

Measuring within-person fluctuations in impulsive choice and action is feasible. Results generally support the reliability and concurrent validity of ambulatory measures of impulsive choice and action, but also point to the need to refine the measurements to: expand the range of momentary discounting rates, enhance the reliability of disinhibition measures, and better account for inattention. Although impulsivity is somewhat trait-like, impulsive choice and action increase with abstinence and vary with temptation, in a state-like manner.

Acknowledgments

Role of Funding Sources: The project described was supported by Award Number RC1DA028129 from the National Institute on Drug Abuse. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institute on Drug Abuse or the National Institutes of Health, which had no role in the study design, collection, analysis or interpretation of the data, writing the manuscript, or the decision to submit the paper for publication.

The authors wish to thank Andrew J. Waters for his contributions to the design of the study and the staff of the Smoking Cessation Laboratory and the Institute for Health, Health Care Policy and Aging Research at Rutgers, the State University of New Jersey for their support of this project.

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Highlights

- Ambulatory measures of impulsive choice and impulsive action are feasible
- Ambulatory impulsiveness measures are related to validated laboratory impulsiveness
- Impulsive choice and action vary both between and within persons

Table 1

Demographic and smoking history characteristics of the final sample (N=105).

Variable	n (%)
Female	50 (47.6%)
Member of racial or ethnic minority group	33 (31.7%)
Married or living with a partner	48 (45.7%)
Completed college	32 (30.5%)
Smoke within 30 minutes of waking	86 (81.9%)
	M (SD)
Age	44.75 (12.17)
Years smoked	25.69 (12.02)
Cigarettes smoked per day	18.70 (6.80)
Previous quit attempts	4.34 (9.67)
Baseline Fagerström Test of Nicotine Dependence Score	5.37 (1.90)

Table 2

Multilevel models of EMA delay discounting and behavioral disinhibition error rates.

Report-Level Coefficient	Person-Level Coefficient	Estimate	Robust Standard Error	p	Reliability of random coefficient
Outcome: Final discounting rate (Tau=.026, Sigma squared=.021)					
Random intercept		.085	.017	<.001	.975
	Baseline discounting rate	.162	.070	.024	
Temptation event		−.018	.005	.001	
	Baseline discounting rate	−.051	.020	.010	
Smoked since last report		−.005	.005	.279	
	Baseline discounting rate	−.056	.010	<.001	
Outcome: Square root of commission error rate (Tau=.003, Sigma squared=.026)					
Random intercept		.099	.008	<.001	.732
	Baseline commission error rate	.171	.042	<.001	
	Baseline omission error rate	1.871	.683	.008	
	Baseline mean reaction time	−.000	.000	.10	
EMA omission rate (<5%)		1.249	.275	<.001	
EMA mean reaction time		.0003	.0001	.003	
Smoked since last report		−.014	.007	.042	