Investigating the Psychometric Properties of the Suicide Stroop Task

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Behavioral measures are increasingly used to assess suicidal thoughts and behaviors. Some measures, such as the Suicide Stroop Task, have yielded mixed findings in the literature. An understudied feature of these behavioral measures has been their psychometric properties, which may affect the probability of detecting significant effects and reproducibility. In the largest investigation of its kind, we tested the internal consistency and concurrent validity of the Suicide Stroop Task in its current form, drawing from seven separate studies (N = 875 participants, 64% female, aged 12 to 81 years). Results indicated that the most common Suicide Stroop scoring approach, interference scores, yielded unacceptably low internal consistency (rs = −.09–.13) and failed to demonstrate concurrent validity. Internal consistency coefficients for mean reaction times (RTs) to each stimulus type ranged from rs = .93–.94. All scoring approaches for suicide-related interference demonstrated poor classification accuracy (AUCs = .52–.56) indicating that scores performed near chance in their ability to classify suicide attempters from nonattempters. In the case of mean RTs, we did not find evidence for concurrent validity despite our excellent reliability findings, highlighting that reliability does not guarantee a measure is clinically useful. These results are discussed in the context of the wider implications for testing and reporting psychometric properties of behavioral measures in mental health research.
The prediction of suicidal thoughts and behaviors continues to challenge both researchers and clinicians. These life-threatening behaviors, which include suicide ideation, suicide attempts, and suicide deaths, are alarmingly common; 9.3 million adults report suicidal thoughts, 1.3 million adults report suicide attempt, and over 40,000 people die by suicide each year in the U.S. (Centers for Disease Control and Prevention, 2013; Substance Abuse and Mental Health Services Administration, 2014). The addition of more objective behavioral measures to assess suicide risk may aid in the detection and prediction of suicidal thoughts and behaviors, particularly in clinical settings where patients may conceal suicidal intent or lack insight into their own risk states (Busch, Fawcett, & Jacobs, 2003). Behavioral measures also offer the possibility of capturing relevant cognitive processes that are outside of one’s awareness, including transient suicide ideation (Cyders & Coskunpinar, 2011). Despite these strengths, behavioral measures have not been subjected to the same psychometric scrutiny as self-report measures, leaving issues of reliability and validity often underaddressed (Green et al., 2016; L. Sharma, Markon, & Clark, 2014). The current study tackles these issues for the Suicide Stroop Task by testing its psychometric properties.

The Suicide Stroop Task has been tested as a potential suicide risk assessment tool among adults and adolescents (Cha, Najmi, Park, Finn, & Nock, 2010; Stewart et al., 2017; Williams & Broadbent, 1986). It is adapted from the Emotional Stroop Task (EST), which measures the degree to which emotionally valenced stimuli interfere with the effortful process of suppressing prepotent responses (Cisler, Bacon, & Williams, 2009). ESTs have been used to capture psychopathology-specific emotional interference among several clinical populations, including those with depression and anxiety-related disorders (e.g., McNally, Kaspi, Riemann, & Zeitlin, 1990; Mitterschiffthaler et al., 2008; Phaf & Kan, 2007). While the specific cognitive processes assessed by ESTs have been a subject of ongoing debate (e.g., Algol, Chajut, & Lev, 2004; McKenna & Sharma, 2004; Williams, Mathews, & MacLeod, 1996), recent evidence suggests that increased response latency on ESTs among patient populations is driven by deficits in the top-down modulation of attention in the context of salient affective stimuli (Feng et al., 2018; Zimmermann et al., 2017). Within this framework, increased response latencies on the Suicide Stroop Task reflect increased salience of suicide-related information, coupled with impairments in regulating attention away from emotional stimuli and toward the external environment (Kaiser et al., 2015). We refer to this increased response latency as suicide-related attentional bias to maintain continuity with prior theoretical and empirical literature. Suicide-specific attentional bias is conceptually linked with a cognitive model of suicide (Wenzel & Beck, 2008), wherein individuals with an activated suicide schema have difficulty disengaging from suicide-related thoughts due to an attentional fixation on suicide-relevant information. As such processing biases may accelerate suicidal crises (Wenzel & Beck, 2008), the Suicide Stroop Task could be used as a unique source of data that cannot be assessed through subjective ratings, providing an important augmentation to suicide risk assessments and enhancing our understanding of cognitive processes associated with the transition from suicidal thoughts to behavior.

Williams and Broadbent (1986) provided initial evidence for suicide-related attentional bias using a manual version of the Suicide Stroop Task. They found that psychiatric inpatients who had recently attempted suicide had greater response latencies for suicide-related words versus neutral words compared to psychiatric nonattempters and healthy controls—a finding replicated by Becker and colleagues (Becker, Strohbach, & Rinck, 1999). This was eventually adapted to a computerized format and administered to recent suicide attempters in a psychiatric emergency department (Cha et al., 2010). Findings revealed that suicide attempters displayed greater suicide-related attentional biases compared to nonattempter psychiatric controls at baseline, and that biased attention predicted suicide attempts six months later. Since then, the computerized Suicide Stroop Task has been used in a number of studies (e.g., Chung & Jeglic, 2016, 2017; Stewart et al., 2017), but to date, findings are mixed. Richard-Devantoy and colleagues’ (Richard-Devantoy, Ding, Turecki, & Jollant, 2016) meta-analysis on computerized Suicide Stroop Task studies concluded that there is evidence for suicide-related attentional bias among suicide attempters, but the effect size was small (Hedge’s $g = .22$); only one study (Cha et al., 2010) included in the meta-analysis found significantly larger suicide-related interference scores among suicide attempters compared to patient controls, suggesting this study alone accounted for significant overall effects.

There are several potential explanations for mixed results across Suicide Stroop Task studies. First, the Suicide Stroop Task may genuinely be unreliable. Testing its reliability via internal consistency estimates could help determine the degree to which task performance scores are affected by measurement error, which is necessary to accurately interpret interindividual differences. Since prior studies have reported poor internal consistency for other adaptations of the EST (e.g., emotion faces EST, Brown et al., 2014; substance-related ESTs, Ataya et al., 2012), here we test the internal consistency of the Suicide Stroop Task across and within multiple samples.
Second, Suicide Stroop Task studies vary substantially in their methodology. As a consequence, no matter how reliable the Suicide Stroop Task may be, variable findings across studies would be expected. For example, studies have used different scoring approaches for the Suicide Stroop Task. ESTs are typically scored based on the difference in RT for emotional words relative to neutral words across trials (i.e., interference score; Cha et al., 2010). But other studies have used scoring approaches such as extracting RT to the word “suicide” only (disregarding RTs to other words in the “suicide” category) to calculate an interference score (Chung & Jeglic, 2016, 2017), dividing the interference score by neutral word RT (Stewart et al., 2017), or using untransformed mean RTs (Becker et al., 1999). As different scoring approaches may artificially increase the proportion of error variance in reliability calculations due to methodological artifacts (e.g., due to correlation between the two component variables in difference scores), testing reliability across these approaches could explain the emergence of inconsistent findings.

Finally, Suicide Stroop studies have compared distinct clinical groups. Although most studies have compared suicide attempters to psychiatric patients with no suicide attempt history (i.e., regardless of suicide ideation status; Becker et al., 1999; Cha et al., 2010; Richard-Devantoy et al., 2016; Williams & Broadbent, 1986), other studies compared a broad suicidal group (i.e., including history of suicide ideation, plan, or attempt) to a nonsuicidal group (Chung & Jeglic, 2016, 2017), or suicide attempters to suicide ideators (Stewart et al., 2017).

In the present study, we pursue two aims pertaining to reliability and validity of the Suicide Stroop Task. First, we aim to test internal consistency of the Suicide Stroop Task. Second, we aim to test concurrent validity of this task. Specifically, we test whether individuals with a history of suicide attempts (“attempters”) show suicide-related attentional bias (beyond interference for other emotion conditions), compared to participants without a history of suicide attempts and, in a second analysis, to those with and without a history of suicide ideation (i.e., “ideators” and “non-suicidal controls”). Groups are defined by distinct suicide-related behaviors in order to clarify how suicide-related attentional bias may characterize different stages on a pathway from suicidal thoughts to behavior. To further explore the concurrent validity of the Suicide Stroop Task, we also estimate the sensitivity and specificity of task scores using receiver operating characteristic (ROC) analysis. Given the aforementioned methodological inconsistencies across studies, we examine reliability and concurrent validity for three different scoring approaches. We also examine these outcomes across different clinical presentations of study samples (psychiatric vs. community-based) and among adults and adolescents.

Method

Sample

The sample was drawn from seven separate studies (n = 875) that administered the Suicide Stroop Task, the Self-Injurious Thoughts and Behaviors Interview (SITBI; Nock, Holmberg, Photos, & Michel, 2007), and the Beck Scale for Suicidal Ideation (SSI; Beck, Kovacs, & Weissman, 1979) at the same time point. Study participants included adults and adolescents (M = 27.17 years, SD = 13.47), of whom 63.9% were female and 63.0% White, 6.6% Hispanic, 6.3% Asian, 6.2% Black/African American, and 17.7% Other. They were recruited from surrounding communities (i.e., community sample), and psychiatric emergency department (ED) or inpatient settings (i.e., psychiatric sample). Eligible participants were fluent in English. Exclusion criteria included inability to provide informed consent or assent and presence of gross cognitive impairment. Sample characteristics for each study are summarized in Table 1.

Materials and Procedure

Each study included distinct aims and procedures, with some studies focusing on assessment (Studies 1, 3, 5, 6, and 7) and others on interventions (Studies 2 and 4). Intervention study data were used from the baseline assessment (i.e., preintervention). Measures were administered in several different settings, ranging from a university laboratory (Studies 3–7), to an interview room located within a psychiatric inpatient unit (Study 2), to a psychiatric ED (Study 1). Please see individual studies for information on study-specific procedures (Cha et al., 2010, 2017, 2018; Glenn, Lanzillo, et al., 2017; Stewart et al., 2017). The studies received research ethics committee approval.

Suicide Stroop Task. In the Suicide Stroop Task, participants were instructed to name the font color of suicide-related, emotionally valenced (i.e., positive, negative), and neutral words as quickly and as accurately as they could. Stimuli for the task were presented and RTs recorded using Empirisoft DirectRT v2004 software (Jarvis, 2004) or SuperLab 4.5.1 (Cedrus Corp, 2011). Instructions were presented on the screen at the beginning of the task. Each trial started with a blank white screen for 4 seconds followed by the presentation of a centered “+” for 1 second, another blank screen for 1 second, and then the word presented in red or blue font. The word remained on the screen until a response was recorded. Participants indicated the color of the word by pressing the red or blue key on the computer keyboard. Participants completed eight practice trials featuring neutral words, followed by 48 critical trials including suicide-related words (e.g., suicide, dead, funeral), negatively valenced words (e.g., alone, rejected, stupid), positively valenced words (e.g., happy, success, pleasure) and neutral words (e.g., paper, engine, museum).

Participants’ RTs to identify the color of each word were recorded on each trial. Raw data from each study was obtained and analyzed together. Consistent with prior scoring procedures (e.g., Cha et al., 2010; Chung & Jeglic, 2016, 2017; Stewart et al., 2017), only trials with correct responses were included in the analysis, and participants with an error rate greater than 2 standard deviations above the mean error rate for the entire sample were excluded from analysis. Individual trials with RTs ± 2 standard deviations

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1 Demographic survey for Study 6 allowed participants the option of identifying by nationality, which we opted to classify as “Other” when no other race or ethnicity was reported.

2 Community-based sample had an average age of M = 28.39 years, SD = 14.93, and were 61.6% female and 57.2% White, 6.9% Black/African-American, 8.4% Hispanic, 5.4% Asian, and 21.8% Biracial, Other, or identified by nationality.

3 Psychiatric sample had an average age of M = 20.16 years, SD = 11.72, were 69.2% female and 77.7% White, 4.3% Black/African-American, 1.9% Hispanic, 8.5% Asian, and 7.6% Biracial or Other.
from each participant’s mean RT were eliminated from analyses, and participants for whom the mean RT across all trials was ± 2 standard deviations from the mean RT for the sample were also excluded.

Three scoring approaches using these RTs were employed: Mean RTs, Interference Scores, and Ratio Scores. Mean RTs represent the averaged RTs untransformed for each valence type. Averaging each participant’s raw RTs yielded Mean RTs for suicide-related words (Mean RT\textsc{sui}), negative words (Mean RT\textsc{neg}), positive words (Mean RT\textsc{pos}), and neutral words (Mean RT\textsc{neu}). Interference Scores were calculated by subtracting each participant’s mean RT for neutral words from their mean RT for suicide-related words (Interference\textsc{sui}), negative words (Interference\textsc{neg}), or positive words (Interference\textsc{pos}). We also computed an Interference Score that consisted of RTs only from the presentation of the words “dead” and “funeral” in that category (Interference\textsc{suiWord}). Reliability was assessed with Spearman-Brown correction. Individual trial RTs for the task were divided into odd–even trials, and two Stroop scores were created and correlated to calculate reliability coefficients for each valence (i.e., suicide, negative, positive, and neutral words) and scoring type (i.e., Mean RTs, Interference, Ratio). Reliability

**Table 1**  
**Sample Characteristics by Study**

<table>
<thead>
<tr>
<th>Study</th>
<th>Sample size</th>
<th>Age group</th>
<th>Age (years) M (SD)</th>
<th>Gender (% female)</th>
<th>Race/Ethnicity (% white)*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Psychiatric sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>126</td>
<td>Adult</td>
<td>33.75 (11.44)</td>
<td>42.9</td>
<td>81.0</td>
</tr>
<tr>
<td>2</td>
<td>37</td>
<td>Adult</td>
<td>41.70 (14.57)</td>
<td>48.7</td>
<td>81.1</td>
</tr>
<tr>
<td>3</td>
<td>177</td>
<td>Adolescent</td>
<td>15.60 (1.34)</td>
<td>73.1</td>
<td>77.0</td>
</tr>
<tr>
<td>Community sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>117</td>
<td>Adult</td>
<td>32.41 (13.60)</td>
<td>51.3</td>
<td>59.0</td>
</tr>
<tr>
<td>5</td>
<td>144</td>
<td>Adult</td>
<td>31.74 (13.47)</td>
<td>56.3</td>
<td>72.9</td>
</tr>
<tr>
<td>6</td>
<td>128</td>
<td>Adult</td>
<td>33.55 (13.46)</td>
<td>56.3</td>
<td>30.2*</td>
</tr>
<tr>
<td>7</td>
<td>146</td>
<td>Adolescent</td>
<td>17.37 (1.80)</td>
<td>81.6</td>
<td>63.7</td>
</tr>
</tbody>
</table>

Note. Study 1 = Cha et al., 2010; Study 2 = Cha et al., 2017; Study 3 = Stewart et al., 2017; Study 4 = Cha et al., 2017; Studies 5–6 = Cha et al., 2018; Study 7 = Glenn, Lanzillo, et al., 2017.

* For all studies except Study 6, non-White race and ethnicity categories included Black/African American, Hispanic, Asian, and Other or Bicultural. b Demographic survey for Study 6 allowed participants the option of identifying by nationality, which we opted to classify as “Other” when no other race or ethnicity was reported.

**Data Analysis**

In pursuit of our first aim, we calculated split-half reliability with Spearman-Brown correction. Individual trial RTs for the task were divided into odd–even trials, and two Stroop scores were created and correlated to calculate reliability coefficients for each valence (i.e., suicide, negative, positive, and neutral words) and scoring type (i.e., Mean RTs, Interference, Ratio). Reliability

**Table 2**  
**Suicide Stroop Task Scoring Approaches**

<table>
<thead>
<tr>
<th>Scoring type</th>
<th>Calculation method</th>
<th>Name</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Mean RT</td>
<td>Mean RT for Suicide, Negative, Positive, and Neutral words</td>
<td>Mean RT\textsc{sui}, Mean RT\textsc{neu}</td>
</tr>
<tr>
<td>2. Interference</td>
<td>Suicide word RT - Neutral word RT Negative word RT - Neutral word RT Positive word RT - Neutral word RT “Suicide” word only RT - Neutral word RT</td>
<td>Interference\textsc{sui}, Interference\textsc{neg}, Interference\textsc{pos}, Interference\textsc{neu}</td>
</tr>
<tr>
<td>3. Ratio score</td>
<td>(Suicide word RT - Neutral word RT)/Neutral RT (Negative word RT - Neutral word RT)/Neutral RT (Positive word RT - Neutral word RT)/Neutral RT</td>
<td>Ratio\textsc{sui}, Ratio\textsc{neg}, Ratio\textsc{pos}</td>
</tr>
</tbody>
</table>

Note. RT = Reaction time.
coefficients were calculated across participants in the full sample 
\((n = 875)\), as well as within samples recruited from the community 
\((n = 340)\) or psychiatric treatment settings \((n = 535)\), and across 
adults \((n = 552)\) and adolescents \((n = 323)\).

Aligned with our second aim, we performed mixed design 
analyses of variance (ANOVAs). We conducted multiple Group \(\times\) 
Valence ANOVAs, but each factor varied depending on the 
selected experimental groups and scoring approach. For the between-
subjects factor, Group comparisons included suicide attempters 
versus nonattempters or suicide attempters versus suicide ideators 
versus nonsuicidal controls. For the within-subjects factor, Va-
lence had four levels in Mean RT analyses \(\text{(i.e., suicide-related,}\) 
negative, positive, neutral), and when testing Interference and 
Ratio Scores, there were three levels \(\text{(neutral mean RT was omit-
ted because it was subtracted from the mean RT of the other valences).}\) 
For these ANOVAs, we estimated a Group \(\times\) Valence 
interaction to determine whether suicide attempters and ideators 
would show significantly larger RT interference, or ratio scores for 
suicide-related words relative to the other valences categories \(\text{(i.e.,}\) 
to test whether participants with suicidal thoughts or behaviors 
demonstrated attentional bias for suicide-related stimuli above and 
beyond deficits for other emotion words), compared to participants 
with no suicidal thoughts or behaviors or ideation only. We con-
ducted post hoc tests with Holm-Bonferroni corrections \(\text{(Holm,}\) 
1979). Analyses with the different groups and scoring approaches 
were explored within the full sample, within community-based and 
psychiatric subsamples, and among adults and adolescent sub-
samples. To test classification accuracy, Suicide Stroop interfer-
ence scores were converted to a categorical variable representing 
positive scores \(\text{(i.e., longer latencies for suicide-related words}\) 
relative to neutral words) and negative scores \(\text{(i.e., longer latencies\) for neural relative to suicide-related words).} \) 
ROC curve analysis was used to estimate the classification accuracy for both 
the continuous Suicide Stroop interference scores and the categorical 
Stroop score variables in differentiating suicide attempters from 
nonattempters. For a detailed account of these results as well as 
those based on analyses within subsamples, alternative analytic 
approaches, and alternative Suicide Stroop scoring approaches, see 
the online supplemental materials.

### Results

#### Reliability

Across all studies \((n = 875)\), Mean RTs for each word valence 
demonstrated excellent reliability \((\text{range } 0.93–0.94)\). For the 
Interference and Ratio scores, reliability was unacceptably low and 
near zero \((\text{range } -0.09–0.13)\). This pattern, wherein Mean RT scores 
demonstrated good reliability, and difference score-
based \(\text{(i.e., Interference and Ratio)}\) reliability was poor re-
mained the same when testing reliability across distinct samples 
\(\text{(community vs. psychiatric)}\) and age groups \(\text{(adults vs. adoles-
cents)}\) \(\text{(see Table 3).}\)

#### Concurrent Validity

To examine concurrent validity, we compared suicide-related 
attentional bias between suicide attempters and nonattempters, as 
well as between suicide attempters and suicide ideators and con-
trols \(\text{(Table 4).}\) \(\text{Group } \times\) Valence interactions were not significant 
when testing Mean RT, Interference, Ratio, or scoring for the 
two-group \(\text{(attempters vs. nonattempters)}\) or three-group compar-
isons \(\text{(attempters vs. ideators vs. controls),} ps = 0.56–0.88, \eta^2_p <\) 
0.01. Results were similar within the aforementioned subsamples 
\(\text{(community vs. psychiatric; adults vs. adolescents), with suicide}\) 
attempters failing to demonstrate a significant suicide-related 
interference effect. For Mean RTs, there was a main effect of Group 
for two-group, \(F(1, 873) = 9.96, p < .01, \eta^2_p = 0.01,\) and 
three-group comparisons, \(F(2, 872) = 5.72, p < .01, \eta^2_p = 0.01.\) 
Post hoc Holm–Bonferroni tests across the two separate analyses 
revealed that suicide attempters showed slower overall RTs com-
pared with nonattempters \((p < .01, d = 0.21),\) as well as with 
suicide ideators \((p < .01, d = 0.25)\) and controls \((p = 0.02, d = 0.15);\) 
however, ideators and controls did not differ in overall RT 
\((p = .18, d = 0.11).\) When comparing groups within the psychiatric 
\or adolescent subsamples \(\text{(Table 5),} \) attempters continued to show 
significantly slower RT compared to nonattempters \((ps < 0.01,\) 
d\(s = .30),\) however, this was not the case within the community or 
adolescent subsamples \(\text{(ps = 0.46–0.80, ds < .05).}\) For three-
group comparisons within Mean RT, Interference, and Ratio scor-
ing approaches, there were main effects of Valence, \(F_S = 2.86–\) 
4.60, \(ps = < 0.01–0.03, \eta^2_p = 0.003–0.01,\) but post hoc tests 
revealed no significant differences across valences via each scor-
ing approach after correcting for multiple comparisons. The ex-
ception was for Mean RT among community samples, where 
participants responded more slowly to neutral words compared to 
suicide words \((p < .01, d = 0.06).\)

We conducted additional analyses that \(\text{1} \) compared Suicide 
Stroop scores among participants with active suicide ideation in 
the past week \(\text{(those who endorsed item 4 of the SSI, “current}\) 
\\text{ideators})\) to scores among those with no current or past history 
of suicide ideation; and \(\text{2} \) tested for the potential association be-
tween Suicide Stroop scores and severity of suicide ideation 
among current ideators \(\text{(i.e., SSI total score).} \) Results from 
ANOVA comparing Suicide Stroop scores between current ideators 
\or nonideators \text{mirrored results from ANOVA comparing} 
nonsuicidal control participants to suicide attempters and lifetime 
ideators, in that no score yielded a significant Group \(\times\) Valence 
interaction, and only Mean RTs showed a main effect of Group. 
Correlations between the Beck SSI total scores and Suicide Stroop 
\text{scores were near zero \((rs = -0.09–0.00, ps = 0.18–0.98).}\)

Suicide Stroop scores failed to accurately differentiate suicide 
\text{attempters from nonattempters. Area under the ROC curve and} 
\text{95% confidence intervals for Suicide Stroop scoring approaches} \(\text{were as follows: interference score AUC = 0.53 (0.49–0.57), ratio}\) 
\text{score AUC = 0.52 (0.49–0.57), and suicide Mean RT AUC} = 
0.56 (0.52–0.60), indicating that Suicide Stroop Task scores per-
formed no better than chance in classifying suicide attempters.

\(4 \) ANCOVA analyses were also conducted to account for potential 
age effects, as RT follows a strong developmental trajectory \(\text{(Der\) 
\& Deary, 2006).} \) Controlling for the effects of age had no effects on the statistical 
significance of tests for any between-group comparisons. We also tested 
for possible gender differences in task performance using ANCOVA 
\text{and testing ANOVA within male and female participants, which yielded no}\) 
significant interactions. 

\(5 \) AUC was also estimated for categorical Suicide Stroop score variables 
\text{(described under Data Analysis), with similar results to those reported for}\) 
\text{continuous Suicide Stroop scores.}
from nonattempters. Categorical variables derived from suicide-related interference scores showed an estimated sensitivity of 54.8% and a specificity of 48.8%.

**Post Hoc Analyses**

We conducted post hoc analyses to address two factors affecting concurrent validity. We first ran analyses to rule out habituation or practice effects, found for other ESTs (e.g., Ashley, Honzel, Larsen, Justus, & Swick, 2013). To address this possibility, we conducted analyses focused on Mean RT for the first presentation of each stimulus only. Results were consistent with Mean RT findings, showing main effects of Group and Valence with no significant Group × Valence interaction. Second, we explored the possibility that limitations of interference or difference-based scoring approaches (present in both Interference and Ratio scores), specifically the compounding of the measurement errors of individual scores, would reduce the likelihood of detecting statistically significant group differences in performance (Overall & Woodward, 1975). To address this limitation, we computed standardized residuals by separately regressing RTs for suicide-related, negative, and positive word trials onto RTs for neutral word trials, using the residuals from the predicted value to represent task performance. Tests of scores based on standardized residuals also yielded main effects of Group and Valence with no significant Group × Valence interaction.

**Discussion**

The goal of this study was to test the reliability and concurrent validity of the Suicide Stroop Task in its current form. First, we determined that one of the most common scoring approaches for ESTs, based on the calculation of difference scores, has poor internal consistency and lacks concurrent validity. Mean RTs for all stimuli, however, demonstrated good internal consistency. Second, we found that the Suicide Stroop Task did not reveal suicide-related attentional biases among suicide attempters or suicide ideators across scoring approaches (even those with excellent reliability), nor were scores able to accurately classify suicidal participants based on ROC curve analysis, indicating a lack of concurrent validity for the task. Finally, results were generally consistent across community versus psychiatric subsamples, and

**Table 3**

<table>
<thead>
<tr>
<th>Score</th>
<th>All subjects (n = 875)</th>
<th>Community (n = 340)</th>
<th>Psychiatric (n = 355)</th>
<th>Adults (n = 552)</th>
<th>Adolescents (n = 323)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean RT&lt;sub&gt;Su&lt;/sub&gt;</td>
<td>.94</td>
<td>.94</td>
<td>.94</td>
<td>.94</td>
<td>.95</td>
</tr>
<tr>
<td>Mean RT&lt;sub&gt;Neg&lt;/sub&gt;</td>
<td>.93</td>
<td>.94</td>
<td>.94</td>
<td>.93</td>
<td>.94</td>
</tr>
<tr>
<td>Mean RT&lt;sub&gt;Pos&lt;/sub&gt;</td>
<td>.93</td>
<td>.94</td>
<td>.94</td>
<td>.93</td>
<td>.94</td>
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<tr>
<td>Mean RT&lt;sub&gt;Neg&lt;/sub&gt;</td>
<td>.93</td>
<td>.93</td>
<td>.93</td>
<td>.93</td>
<td>.93</td>
</tr>
<tr>
<td>Interference&lt;sub&gt;Su&lt;/sub&gt;</td>
<td>.13</td>
<td>.23</td>
<td>.02</td>
<td>.02</td>
<td>.32</td>
</tr>
<tr>
<td>Interference&lt;sub&gt;Neg&lt;/sub&gt;</td>
<td>-.09</td>
<td>-.24</td>
<td>.06</td>
<td>-.05</td>
<td>-.18</td>
</tr>
<tr>
<td>Interference&lt;sub&gt;Pos&lt;/sub&gt;</td>
<td>.02</td>
<td>-.06</td>
<td>.11</td>
<td>-.03</td>
<td>.13</td>
</tr>
<tr>
<td>Interference&lt;sub&gt;Su&lt;/sub&gt;Word</td>
<td>-.02</td>
<td>.09</td>
<td>-.19</td>
<td>.00</td>
<td>-.09</td>
</tr>
<tr>
<td>Ratio&lt;sub&gt;Su&lt;/sub&gt;</td>
<td>.04</td>
<td>.10</td>
<td>-.05</td>
<td>-.01</td>
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<td>Ratio&lt;sub&gt;Pos&lt;/sub&gt;</td>
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<td>-.02</td>
<td>-.07</td>
<td>-.07</td>
<td>-.01</td>
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</table>

**Note.** RT = Reaction Time. Community subsample includes participants from studies 4–7; Psychiatric subsample includes participants from studies 1–3; Adult subsample includes participants from studies 1, 2, 4, 5, and 6; Adolescent subsample includes participants from studies 3 and 7.
Theroux-Fichera, Zielinski, & Heilbronner, 1995). Ultimately, the
been shown to be useful for other measures (e.g., Wechsler Adult
2003). Despite these limitations, difference scores have at times

<table>
<thead>
<tr>
<th>Score</th>
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<th>Adolescents</th>
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<td>η²</td>
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<td>.00</td>
<td>5.90**</td>
<td>.03</td>
</tr>
<tr>
<td>Mean RT_Neg</td>
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<td>.00</td>
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<td>Mean RT_Pos</td>
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Note. Community subsample includes participants from studies 4–7; Psychiatric subsample includes participants from studies 1–3; Adult subsample includes participants from studies 1, 2, 4, 5, and 6; Adolescent subsample includes participants from studies 3 and 7.

** p < .01. *** p < .001.

across adult and adolescent subsamples. These findings demon-
strate that commonly used Suicide Stroop scoring approaches have
poor reliability and are unable to differentiate participants based
on history of suicide attempt or ideation, indicating the Suicide Stroop
Task may not be useful in predicting suicide. These findings and
their larger implication are discussed in further detail.

The present study underscores the need for testing and reporting
of psychometric properties of behavioral measures. Reliance on
the face validity of tasks aimed at capturing cognitive processes
has limited our understanding of whether these tasks are actually
measuring the psychological construct(s) of interest and how well
they do so. Measures of RT-based performance such as the Suicide
Stroop Task are sensitive to individual, contextual, and procedural
factors, which may introduce random measurement error6 (Ataya
et al., 2012; LeBel & Paunonen, 2011). Despite this, reliability
estimates for behavioral measures are infrequently reported. A recent
review reported that fewer than 6%–10% of studies report
reliability coefficients for behavioral measures (Green et al.,
2016). Given that unidentified sources of measurement error in
these tasks (and their resultant outcome scores) negatively impact
effect size, power of hypothesis tests, and replicability of results
across studies, tests of reliability are crucial (Green et al., 2016;
LeBel et al., 2013).

Our reliability findings are largely consistent with other studies
testing internal consistency of adapted ESTs, showing poor reliabil-
ity for interference or difference scores and acceptable reliability
for Mean RTs (e.g., emotion faces EST; Brown et al., 2014;
panic attack EST, Dresler et al., 2012). The use of difference
scores remains a topic of debate within the field (e.g., Cronbach,
1958; Overall & Woodward, 1975). Within classical test theory,
the reliability of a difference score is often low when component
measures are highly correlated and have similar variances, which
is typically the case for difference scores calculated from experi-
mental task RTs (Hedge, Powell, & Summer, 2018; May & Hittner,
2003). Despite these limitations, difference scores have at times
been shown to be useful for other measures (e.g., Wechsler Adult
Intelligence Scale subtests; Erdodi et al., 2017; Mittenberg,
Theroux-Fichera, Zielinski, & Heilbronner, 1995). Ultimately, the
benefits or potential detriments of using difference scores may
depend on the measure, study design, subject matter, and whether
corresponding analyses are likely to have appropriate power
(Thomas & Zumbo, 2012).

Drawing from the broader EST literature, there are several
design features of the Suicide Stroop Task that warrant consider-
ation and possible modification. The first design feature pertains to
the presentation of stimuli in random order across trials. Studies
indicate that the presentation of emotional stimuli prior to that of
neutral stimuli disrupts performance on the latter, suggesting the
involvement of a “slow” emotional intrusion effect that is sus-
tained across proximal subsequent trials (McKenna, 1986; McK-
enna & Sharma, 2004; D. Sharma & McKenna, 2001). The use of
a blocked design format may allow for more precise observation
and interpretation of the impact of word category on task perfor-
ance (Ben-Haim et al., 2016). The second design feature pertains to
the limited number of words represented within each valence.
The Suicide Stroop Task presented 4 words for each valence
category (i.e., 16 distinct words total) across 48 trials, so word
repetition was high, considering researchers recommend 20–50
words per category (Ben-Haim et al., 2016). Few stimuli that are
repeated numerous times invite possible habituation effects, in
which emotion-related interference decreases due to faster re-
sponding to emotion words and slower responding to neutral
words (Ashley et al., 2013). Presenting a greater number of stimuli
per valence with fewer repetitions may help buffer these effects.
Although our analyses targeting first presentation of stimuli did
not yield results that differed from analyses of Mean RT, this may
also reflect other study design features such as mixed presentation
of stimuli. The third design feature pertains to the overall number
of trials. Increasing the number of trials administered to each
participant may contribute to more adequate reliability estimates,

Of note, many RT-based tasks demonstrate excellent reliability and
validity (e.g., original Stroop (Erdodi et al., 2018), and the excellent
reliability estimates for mean RTs in our study provide circumstantial
evidence of overall credible responding among participants.
as reliability coefficients are a function of the length of a measure (Hedge et al., 2018). Conners Continuous Performance Task II (CPT-II) is an example of a RT-based measure that implements these design features, demonstrates good reliability and construct validity, and is sensitive to a wide range of neuropsychiatric conditions (Raz, Bar-Haim, Sadeh, & Dan, 2014).

There are a number of additional factors that may contribute to the variability in RT-based scores and thus are important to consider in the context of the Suicide Stroop Task. First, general RT speed is influenced by age-related differences in cognitive ability (Der & Deary, 2006; Kiselev, Espy, & Sheffield, 2009), with some evidence for gender-related differences as well (Dane & Erzurumlu, 2003; Der & Deary, 2006). Second, multiple cognitive factors including lexical processing, language ability, and English language proficiency, as well as individual differences in inattentiveness, motivation, and effort, may account for some of the variance in task performance. However, the lack of objective measures of performance validity in the current study should be considered a limitation, particularly given that the original Stroop task has been shown to be impacted by noncredible responding (Erdodi et al., 2018; Guise, Thompson, Greve, Bianchini, & West, 2014). The present use of data cleaning procedures, which eliminated invalid trials and response sets, may exclude meaningful data and decrease the probability of detecting significant effects and is thus another study limitation.

The present study has a few additional limitations to note. First, we did not systematically sample for specific nonsuicidal control groups (e.g., nonsuicidal depressed individuals). Thus, we are unable to estimate the degree to which psychiatric symptoms may confound results. Second, we were not able to examine additional suicide attempt characteristics that could be meaningfully related to task performance. Recency of suicide attempt was examined inconsistently across studies and thereby difficult to compare across the overall sample. Additionally, we used a categorical measure of suicide attempt status and did not capture features relating to severity such as number of suicide attempts and attempt lethality.

While we did not find evidence to support the reliability or validity of frequently used Suicide Stroop Task scores, there have been more promising results regarding other behavioral measures of suicide and self-harm risk. Implicit association tests (IATs), for instance, measure the strength of association a person endorses between self and self-harm related constructs (i.e., death/suicide and self-injury). IATs have demonstrated acceptable to excellent internal consistency, strong evidence for concurrent validity, and predictive accuracy (Glenn, Wernitz, et al., 2017; Millner, Copper-Smith, Teachman, & Nock, 2018; Nock et al., 2007). One potential explanation for the higher reliability found in studies of the IAT is the blocked design of the task, which has been shown to produce much larger effect sizes than mixed task design in ESTs (Phaf & Kan, 2007). The IATs examined by many of these studies had a greater number of trials than the Suicide Stroop Task (a minimum of 80–120 compared to 48 based on the standard IAT procedure; Greenwald, Nosek, & Banaji, 2003), which would reduce associated measurement error and increase reliability. These IATs have also featured many more practice trials, which would improve participants’ retention of task instructions and accuracy of performance (Schmidt & Bjork, 1992). Future Suicide Stroop research may adopt several of these task design features to determine if they may help improve its reliability and validity, or whether other factors, such as the relevance of the construct to suicidal thoughts and behaviors, explain this difference.

The present study offers important clarifications regarding the interpretation and overall use of the Suicide Stroop Task. While the Suicide Stroop Task in its current form is not a reliable or valid measure of suicide risk, there remain concrete ways to modify and improve upon its current design. On a broader scale, these findings call attention to the larger issue of testing and reporting on psychometric properties for behavioral measures. The present findings also offer a critical reminder that reliability does not ensure measure validity or usefulness, thereby underscoring the impact of psychometric properties on our ability to draw clinically significant inferences.

References


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