No Evidence that Smartphone Notifications Lead to Goal-Neglect

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Abstract

These days, young people report to be in a state of permanent alertness due to their smartphones. This state has been defined as smartphone vigilance, an awareness that one can always get connected to others in combination with a permanent readiness to respond to incoming smartphone notifications. We argue that receiving a notification makes users vigilant and activates goals (e.g., checking the message) that interfere with other goals needed to perform a task. We thus hypothesized that smartphone vigilance impairs maintenance of current task-goals in working memory, resulting in increased goal-neglect. To test this hypothesis, we conducted a preregistered experiment that examined the effect of smartphone vigilance (incoming notifications) on goal-neglect in a modified Stroop task. We found evidence that participants perceived notifications as distracting, but vigilance did not lead to increased goal-neglect. To the contrary, there was tentative evidence that vigilant participants performed better at the task.

Keywords: smartphones, vigilance, goal-neglect, performance, notifications

As smartphone ownership approaches saturation in Western countries, especially younger users find themselves “permanently online” (Klimmt, Hefner, Reinecke, Rieger, & Vorderer, 2018; Vorderer & Kohring, 2013). Interestingly, those users report to be alert at all times, expecting to satisfy informational, social, or hedonic needs through their smartphones (Bayer, Ellison, Schoenebeck, Brady, & Falk, 2018; Kanjo, Kuss, & Ang, 2017; Karapanos, Teixeira, & Gouveia, 2016; Kardos, Unoka, Pléh, & Soltész, 2018). In a previous study, we defined this alertness as smartphone vigilance, “a state of being aware that one can always get connected with others or access information, accompanied by a permanent readiness to respond to incoming smartphone stimuli” (Johannes, Veling, Verwijmeren, & Buijzen, in press). We predicted that constant preoccupation in the form of smartphone vigilance would interfere with the executive function of response inhibition (Diamond, 2014). Surprisingly, although we found a large effect of a smartphone vigilance manipulation on self-reported distraction and vigilance, there was no evidence for a negative effect on response inhibition.

These results suggest that smartphone vigilance does not influence the executive function of inhibition, which is in line with recent research demonstrating that smartphone exposure is not related to neurological change in cognition (Mohan, Khaliq, Panwar, & Vaney, 2016). Thus, inhibition impairment cannot explain the ample evidence that receiving notifications affects cognitive performance and attention (Shelton, Elliott, Eaves, & Exner, 2009; Stothart, Mitchum, & Yehnert, 2015; Thornton, Faires, Robbins, & Rollins, 2014; Ward, Duke, Gneezy, & Bos, 2017). Here, we propose that this negative effect might be explained by smartphone vigilance impairing another executive function, that of working memory. We argue that during vigilance, goals become activated, such as connecting to others (Kardos et al., 2018), that are competing with other goals in working memory, such as reading a paper (Cutino & Nees, 2016). In other words, once the phone vibrates, the notification activates the goal to pursue social or

informational goals (i.e., checking the notification content), interfering with other goals kept in working memory (LaRose, 2015). Hence, we expected smartphone vigilance to interfere with the executive function of working memory capacity, signified by goal-neglect.

In order to assess goal-neglect, we employed the modified Stroop task as used by Kane and Engle (2003). During a Stroop task, participants have the goal to identify the ink color of a word. However, when word and ink are incongruent (e.g., RED in the color blue), the automatic tendency to read out the word presents interfering information, which requires participants to retrieve the goal of identifying the ink in order to resolve the conflict. Importantly, participants completed two versions of the Stroop task. First, during the majority-congruent version, 75% of trials are congruent (e.g., RED in the color red). No goal-maintenance is required during congruent trials. Only on occasional incongruent trials must they retrieve the goal of identifying the ink because of conflicting information. Thus, having that many congruent trials makes it harder to maintain the goal of identifying the ink color, which results in more errors during the occasional incongruent trials. Second, during the no-congruent version, 0% of trials are congruent. In this case, all trials are incongruent and require participants to constantly maintain the goal of identifying the ink color and not read the word. Thus, by repeatedly enacting the goal through their own behavior, goal-maintenance becomes easier for participants.

Consequently, in addition to the well-established Stroop effect (i.e., more errors on incongruent compared to neutral trials), the amount of goal-neglect shows in the interaction of Stroop effect and Stroop version. There should be no difference between the Stroop versions on the amount of errors on neutral trials. However, because goal-maintenance is harder on the majority-congruent Stroop version, there should be more errors on incongruent trials on the majority-congruent version compared to the no-congruent version. Crucially, to obtain the goal-neglect effect, Kane and Engle (2003) recommend this exact order of Stroop versions.

We predicted that smartphone vigilance would strengthen this goal-neglect effect in the vigilance condition compared to the control condition. Specifically, for smartphone vigilance to affect goal-neglect, we expected that there would be a larger difference in errors on incongruent trials in the vigilance condition compared to the control condition, as smartphone goals interfere with maintaining the goal to identify the ink.

Method

We preregistered our hypothesis, stopping rule, exclusion criteria, and analysis. The preregistration, data, analyses, and materials are on the Open Science Framework (<https://osf.io/z6aed/>). The study had IRB approval.

Design

The experiment employed a 2 (smartphone vigilance: control, notifications) x 2 (Stroop version: no-congruent, majority-congruent) x 3 (trial type: neutral, incongruent, congruent) factorial design, with the first factor between subjects.

Procedure and Manipulation

The manipulation to induce the smartphone vigilance was identical to our previous study (Johannes et al., in press) and aimed to reproduce the feeling of vigilance that participants encounter in their everyday lives, namely, being in a state of alertness when receiving a notification.

In the notification condition, participants were informed that the experimenter would set their phones to either silent or vibrate, but participants were not allowed to check the setting of their phone. The experimenter then set the participant's phone to vibration mode, but also switched off Wi-Fi and mobile data services. Thus, only SMS could be received, which minimized the chance that participants received other notifications. The phone was then placed on the table, display down, next to the participant's dominant hand. Participants were told that

they were not allowed to touch their phone during the experiment. During the last 20 trials of the last practice block preceding each Stroop version, the program sent three SMS messages, separated by seven seconds as not to be mistaken for a call. Although participants could conclude their phone was set to vibrate, they could not know for certain that the notifications came from the experimenter.

In the control condition, participants were only instructed to set their phone to silent mode and put it in their pockets, as is customary at our lab. In addition, a notebook was placed next to their dominant hand. This way, the difference in performance between the two groups could not be attributed to just having an object next to one's dominant hand.

After the manipulation, participants proceeded to do the task. The task was coded in Python, version 2.7. The notifications in the notification condition were sent using the cloud-based messaging service Twilio (<https://www.twilio.com/>). After the task, participants reported several manipulation checks.

Dependent Measure

Participants first performed a majority-congruent, followed by a no-congruent Stroop task. Each task had 144 randomly presented trials. In order to have an equal amount of trials across Stroop versions and trial types, we followed the procedure of Kane and Engle (2003); the program thus randomly determined 18 critical trials of each trial type before participants did the task. All analyses are conducted on the critical trials. Critical congruent trials paired each of the three color words (RED, BLUE, GREEN) with their respective ink colors six times. Critical incongruent trials paired each color word with the two conflicting colors three times. Critical neutral trials paired each word with the three colors two times. Neutral trials had the same length as color words, but did not share any letters (e.g., VHZT).

Trials appeared in the center of the screen with a black background. Participants categorized the ink color by pressing one of three keys with their index, middle, or ring finger (counterbalanced). Before each task, participants completed three practice blocks (21 neutral trials each) to make them familiar with the task. Participants had to have 15 out of 21 practice trials for each block correct, otherwise the block would repeat. After the third block, participants were warned that they would not receive feedback anymore. Also, they were told they would now also encounter actual words rather than just the neutral trials.

To avoid inflated error rates at the beginning of the experimental blocks, participants first saw ten neutral trials (without feedback), followed by ten words. At the beginning of those 20 filler trials they received the three SMS if they were in the notification condition. This procedure was the same before each Stroop version. Each trial started with a fixation cross, displayed for 200ms. The colored word appeared immediately afterwards, and stayed on the screen for either 5s or until response. Intertrial interval was set to 500ms.

Sampling Procedure and Participants

Because previous work on the effect of smartphones on cognitive performance used different measures, we could not directly transfer previous effect sizes to calculate power. Instead, we employed a sequential Bayesian sampling design (Schönbrodt, Wagenmakers, Zehetleitner, & Perugini, 2017), which is efficient while maintaining superior error control compared to classical NHST approaches. We set a minimum sample size of $n=20$ and a maximum of $n=50$ per condition (after exclusions), with a threshold of 3 and $1/3$, respectively, for the Bayes Factor (BF) for BF_{10} . We reached the threshold after the minimum sample size.

Participants ($N = 51$) were students from a university in the Netherlands, who received €5 or course credit. We observed interaction with cameras. Following our preregistration, we excluded six participants because they touched their phones during the experiment. No

participants received any notifications other than the SMS sent by the experimenter. Furthermore, six participants were excluded because their phone did not vibrate, due to notification customizations the experimenter could not access. Nobody fulfilled our exclusion criterion of no more than 50% errors on neutral trials. The final sample consisted of 39 participants ($M_{age} = 22.00$, $SD_{age} = 2.97$, 31 females; $n_{notification} = 21$, $n_{control} = 18$).

Stopping Rule

We calculated the difference between errors on neutral and incongruent trials (Stroop effect) separately for the majority-incongruent and no-congruent tasks. Then we calculated a difference score of these two Stroop effects (goal-neglect effect). To determine our stopping rule, we conducted a Bayesian independent t-test (after exclusions) on goal-neglect between the vigilance and control groups (JASP Team, 2017; the standard JASP prior was used, 0.707). We preregistered to stop collection at $BF_{10} = 3$ or $BF_{01} = 1/3$.

Results

Manipulation Checks

As expected, everyone in the notification condition perceived the vibration of the SMS that they received. Participants in the notification condition also reported much higher ($M=59.57$, $SD=26.14$) distraction on a 100-point VAS compared to the control condition ($M=12.78$, $SD=22.02$; $t(36.99)=-6.07$, $p<.001$, $d=-1.94$). The same pattern emerged when comparing how distracting the notebook in the control condition was ($M=2.61$, $SD=2.78$) to the phone in the notification condition, $t(-20.52)=-9.92$, $p<.001$, $d=-3.07$). Further descriptive measures can be found on the OSF.

Preregistered Analysis

Contrary to our expectation, participants in the notification condition had lower total errors on the goal-neglect measure ($M=.29$, $SD=1.42$) than participants in the control condition

($M=1.28$, $SD=1.64$); consequently, under the contrary assumption that we hypothesized, the data were about eight times more likely under the null hypothesis, $BF_{01}=8.34$, $95\%CI=[-.33, -.002]$, $d=.65$. Table 1 displays the average total errors for each condition by trial type and Stroop version. Apparently, the goal-neglect effect was only present in the notification condition compared to the control condition (see Figure 1), the opposite of what we hypothesized.

Nonpreregistered Analyses

The t-test we conducted for our stopping rule only gave us an overall test of the hypothesized three-way interaction, but was not informative with regard to whether we replicated the common Stroop or the goal-neglect effect. To further examine the exact contributions of each factor we conducted a 2 (vigilance: control, notification) $\times$ 2 (Stroop version: majority-congruent, no-congruent) $\times$ 2 (trial type: neutral, incongruent) Bayesian repeated-measures ANOVA, with the last two factors within, on total errors.

Inspecting the resulting table (Table 2), the second row demonstrates the Stroop effect with strong evidence (Lee & Wagenmakers, 2013) in favor of a difference between neutral and incongruent words compared to the null model ($BF_{10}=29$). In addition, the model with both main effects of trial type and Stroop version as well as their interaction ($BF_{10}=289$) outperformed the model without the interaction ($BF_{10}=289/63=4.59$). This constitutes moderate evidence for the goal-neglect effect, as the Stroop effect is more pronounced in the majority-congruent version compared to the no-congruent version. Thus, the goal-neglect model offers the best explanation for the data as it fits only slightly worse than the best fitting model ($BF_{10}=382/289=1.32$), yet is more parsimonious. Hence, the data replicate the findings of Kane and Engle (2003). Last, the full model with the hypothesized three-way interaction fit better than the null model ($BF_{10}=157$), but did not offer substantial improvement over the goal-neglect model ($BF_{10}=157/289=0.54$).

Thus, in accordance with the t-test we conducted for our stopping rule, there was only weak evidence for the inclusion of the three-way interaction across all models ($BF_{\text{Inclusion}}=2.50$).

To better quantify the goal-neglect effect as proposed by Kane and Engle (2003), we conducted a Bayesian repeated-measures ANOVA without condition as a predictor. Indeed, the model with both main effects of trial type and Stroop version and their interaction was the best fitting model ($BF_{10}=287$) and there was moderate evidence for the interaction effect ($BF_{10}=287/65=4.42$) with a considerable effect size ($\eta^2_p=.18$, $\omega^2=.06$).

To further investigate the weak evidence for a three-way interaction and gain more insight into the nature of the goal-neglect effect, we conducted separate Bayesian ANOVAs for each condition. The control condition (Figure 1) clearly showed the expected pattern of goal-neglect, as the model with the interaction displayed superior fit compared to a model with only the main effects ($BF_{10}=215/35=6.14$). In contrast, a separate ANOVA for only the vigilance condition demonstrated weak support for any of the effects, with the best fitting model only containing trial type as predictor (i.e., the Stroop effect, $BF_{10}=2.87$); instead, there was weak evidence for the lack of an interaction effect when comparing the full model to a model with two main effects ($BF_{01}=3.65/1.57=2.32$), which by itself displays weak fit.

Discussion

This experiment demonstrated the common Stroop effect, such that incongruent words produced more errors compared to neutral trials. Furthermore, it is encouraging that we replicated the findings of Kane and Engle (2003) with a preregistered study. The Stroop versions produced a strong interaction effect, with an effect size in the same range as their work. It indeed appears that a majority-congruent Stroop task leads to neglecting the goal of identifying the ink compared to a no-congruent Stroop task. Replicating both the Stroop and the goal-neglect effect lends credibility to our measurement.

With regard to our manipulation, the notifications were perceived as very distracting in comparison to the control group. This finding replicates our previous study (Johannes et al., in press). In addition, although we did not employ a direct vigilance measure here (because the study reported here was conducted earlier than Johannes et al.), this exact manipulation has now been shown to influence vigilance (Johannes et al., in press). In addition, the Bayesian sequential sampling design we employed in general needs smaller samples compared to a classic NHST approach to reach a conclusion about an effect. Thus, it is sensitive to detecting whether the data support or do not support an effect, given a certain prior, even with small samples such as ours, while performing well in terms of long-term error control (Schönbrodt et al., 2017). Taken together, those results imply that our experimental manipulation worked and our task measured what it was intended to measure, while the analysis was sensitive to detect an effect.

It is important to note that Bayesian sequential sampling is not yet commonly used and there are no clear conventions on setting the BF cut-offs. We chose 3, which reflects anecdotal evidence and can thus be considered a relatively low evidential threshold (Lee & Wagenmakers, 2013). However, BFs have the advantage that readers can decide for themselves whether they find cut-off and evidential value convincing. Therefore, we recommend future research choose cut-offs based on the literature and what they themselves consider convincing evidence.

Contrary to our hypothesis, there was moderate evidence that vigilance did not lead to stronger goal-neglect. If anything, our data suggest the opposite. In the control condition, we observed the typical goal-neglect effect. For the notification condition, we expected that this pattern would be more pronounced. However, we found no evidence for the goal-neglect effect; instead, our analysis suggests the lack of an effect. There was also no evidence for the general Stroop effect here. Thus, we can conclude that vigilance did not interfere with goal-neglect; if

anything, it might have even lead to improved performance, reducing the Stroop effect and eliminating the goal-neglect effect.

Interestingly, the lack of effect can be explained by the same account that also explains a lack of effect on response inhibition (Johannes et al., in press). It is possible that a mindset of avoidance was induced because participants were not allowed to touch their phones. This mindset might have raised arousal and recruited additional resources which has shown to improve performance on a Stroop task (Koch, Holland, & van Knippenberg, 2008). Thus, smartphone vigilance might have had both a positive and a negative effect on working memory. On the one hand, the state might have been taxing to executive functioning. On the other hand, the increased arousal might have recruited cognitive resources that helped to keep the goal in working memory, ultimately evening out the negative effect of smartphone vigilance. Yet, it is not clear if and how this avoidance-induced alertness interacts with or is theoretically similar to the alertness of smartphone vigilance.

Taken together, there is increasing evidence that smartphone vigilance is experienced as distracting, but does not impair cognitive control. Consequently, smartphone vigilance impairing working memory does not provide a viable explanation for previous work finding negative effects of smartphone notifications on cognitive performance and attention (Stothart et al., 2015; Thornton et al., 2014; Ward et al., 2017). Furthermore, it appears necessary to distinguish between constant interruptions (in the form of checking behavior) and the mindset of vigilance. Whereas actual behavior, for example multitasking, has shown to impair executive functions (Baumgartner, van der Schuur, Lemmens, & te Poel, 2017), situations where smartphones are considered temptations might not be as detrimental as previously assumed by both scholars and users. Our findings call for more research differentiating smartphone interaction and smartphone vigilance.

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Table 1

Mean and standard deviations for all groups of total errors on critical trials

Trial Type	Stroop Version	Condition	Mean	SD
Neutral	Majority-Congruent	Control	0.667	0.907
		Vigilance	0.571	0.598
	No-Congruent	Control	0.556	0.705
		Vigilance	0.714	0.784
Incongruent	Majority-Congruent	Control	1.944	1.798
		Vigilance	1.095	0.995
	No-Congruent	Control	0.556	0.616
		Vigilance	0.952	0.805

Table 2

*Bayesian mixed ANOVA***Model Comparison**

Models	P(M)	P(M data)	BF_M	BF₁₀	error %
Null model (incl. subject)	0.053	7.781e -4	0.014	1.000	
Trial Type	0.053	0.023	0.418	29.190	1.134
Stroop Version	0.053	0.001	0.026	1.858	1.070
Trial Type + Stroop Version	0.053	0.050	0.942	63.932	1.754
Trial Type + Stroop Version + Trial Type * Stroop Version condition	0.053	0.225	5.236	289.628	2.564
Trial Type + condition	0.053	2.006e -4	0.004	0.258	2.505
Stroop Version + condition	0.053	0.006	0.104	7.373	1.546
Trial Type + Stroop Version + condition	0.053	3.579e -4	0.006	0.460	1.250
Trial Type + Stroop Version + Trial Type * Stroop Version + condition	0.053	0.014	0.251	17.670	4.571
Trial Type + condition + Trial Type * condition	0.053	0.056	1.075	72.430	3.182
Trial Type + Stroop Version + condition + Trial Type * Stroop Version + condition	0.053	0.002	0.034	2.439	4.495
Trial Type + Stroop Version + condition + Trial Type * condition	0.053	0.004	0.076	5.424	5.054
Trial Type + Stroop Version + Trial Type * Stroop Version + condition + Trial Type * condition	0.053	0.020	0.365	25.534	5.704
Stroop Version + condition + Stroop Version * condition	0.053	0.001	0.024	1.738	4.312
Trial Type + Stroop Version + condition + Stroop Version * condition	0.053	0.059	1.130	75.886	2.721
Trial Type + Stroop Version + Trial Type * Stroop Version + condition + Stroop Version * condition	0.053	0.298	7.634	382.735	3.943
Trial Type + Stroop Version + condition + Trial Type * condition + Stroop Version * condition	0.053	0.019	0.346	24.213	2.858
Trial Type + Stroop Version + Trial Type * Stroop Version + condition + Trial Type * condition + Stroop Version * condition	0.053	0.098	1.965	126.515	3.867
Trial Type + Stroop Version + Trial Type * Stroop Version + condition + Trial Type * condition + Stroop Version * condition + Trial Type * Stroop Version * condition	0.053	0.122	2.503	156.919	6.601

Note. All models include subject.

Figure 1. Line graph of the total errors per trial type and Stroop versions, separately for experimental condition. Lines represent 95% credible intervals.