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The relationship between online vigilance and affective well-being in everyday life: Combining smartphone logging with experience sampling

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ABSTRACT

Through communication technology, users find themselves constantly connected to others to such an extent that they routinely develop a mind-set of connectedness. This mind-set has been defined as *online vigilance*. Although there is a large body of research on media use and well-being, the question of how online vigilance impacts well-being remains unanswered. In this preregistered study, we combine experience sampling and smartphone logging to address the relation of online vigilance and affective well-being in everyday life. Seventy-five Android users answered eight daily surveys over five days ($N = 1,615$) whilst having their smartphone use logged. Thinking about smartphone-mediated social interactions (i.e., the salience dimension of online vigilance) was negatively related to affective well-being. However, it was far more important whether those thoughts were positive or negative. No other dimension of online vigilance was robustly related to affective well-being. Taken together, our results suggest that online vigilance does not pose a serious threat to affective well-being in everyday life.

Research on the question of how communication technology affects the well-being of users has accumulated rapidly in recent years (Meier, Domahidi, & Günter, *in press*). Results so far illustrate that the use of such technology has small (Heffer, Good, Daly, MacDonell, & Willoughby, 2019; Orben, Dienlin, & Przybylski, 2019; Orben & Przybylski, 2019a, 2019b) and possibly nonlinear effects on well-being (Przybylski & Weinstein, 2017). These findings all refer to communication technology use at the *behavioral level* (e.g., by investigating “screen time”). However, such approaches neglect that people are constantly connected to others *psychologically* through their smartphones. This connection has led smartphone users to develop a mind-set of constant connectivity,

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a phenomenon recently defined as online vigilance (Klimmt, Hefner, Reinecke, Rieger, & Vorderer, 2018; Reinecke et al., 2018).

Importantly, in contrast to other theoretical approaches such as problematic internet use (Kardefelt-Winther et al., 2017), online vigilance refers to a non-pathological form of constant psychological connectedness to online content and communication. People high in online vigilance are perpetually aware of streams of mediated communication in daily life. This awareness has the potential to contribute to users' well-being, for example, when it takes the form of perceived social support (Domahidi, 2018; Reinecke, 2018). Yet many users experience such a mind-set as bothersome and conflicting with personal goals and obligations (Mihailidis, 2014; Näsi & Koivusilta, 2013). Thus, the constant cognitive preoccupation resulting from online vigilance may impair individuals' psychological well-being (Reinecke, 2018). Therefore, in addition to focusing on *usage of technology*, there is a need for research investigating how a *mindset of connectedness due to technology* relates to well-being.

Despite the proliferation of smartphone use and resulting opportunities to develop online vigilance, such research examining the relation between online vigilance and well-being is scarce. Moreover, previous work assessed the relationship between online vigilance and well-being on the basis of cross-sectional self-reports (Johannes et al., 2018). However, cross-sectional approaches cannot analyze situational within-person processes (Hamaker, Kuiper, & Grasman, 2015), while self-reports provide only unreliable accounts of actual behavior (e.g., Scharrow, 2016). As a result, the current literature can only provide a coarse picture, leaving room for many confounding factors and limiting insights into the mechanisms of how online vigilance relates to well-being.

To address these shortcomings, there is a need for research that investigates this relation in the situational contexts of everyday life. Thus, the present study extends prior work by combining experience sampling with behavioral data (i.e., objective smartphone logging). This approach makes several contributions. First, it addresses methodological limitations of prior work. Second, it allows us to get a better understanding of the online vigilance construct on the state level. Third, instead of expecting online vigilance to display a uniform relation with well-being, we relied on a more fine-grained theoretical approach and investigated how individual dimensions of online vigilance relate to well-being. Fourth, rather than focusing on online vigilance concerning online media use in general, we focus on users' mindset toward *smartphone-mediated social interactions*, because social interaction is a core function of smartphone use (e.g., Klimmt et al., 2018). Together, the current study advances our understanding of how constant cognitive connectedness in the form of online vigilance relates to well-being.

Online vigilance and well-being

Via their smartphones, users are now permanently connected to social contacts (e.g., Mihailidis, 2014). This *technological* connectedness can lead users to internalize a *psychological* connectedness, such that users are constantly aware of ongoing streams of mediated communication and interaction. Reinecke et al. (2018) have introduced the concept of online vigilance to describe individual differences in this connectedness mindset. Online vigilance is reflected in three features of users' psychology: "(1) their *cognitive orientation* to permanent, ubiquitous online connectedness; (2) their *chronic attention* to and continuous integration of online-related cues and stimuli into their thinking and feeling; and (3) their *motivational disposition* to prioritize options for online communication over other (offline) behavior" (Reinecke et al., 2018, p. 2).

These features are expressed in three dimensions of online vigilance: salience, reactivity, and monitoring. *Salience* refers to the frequency and intensity of thoughts about online streams of communication and interaction, thus representing the cognitive component of online vigilance. *Reactivity* refers to the motivational component of online vigilance, specifically the sensitivity to smartphone cues and how responsive the user is to them, even when this requires postponing ongoing offline activities. *Monitoring* describes the attentional component of online vigilance, specifically to what extent users observe their online sphere, expressed in how often a user checks their mobile device proactively without being prompted by a notification.

The dimensions of online vigilance can exert different influences on well-being, depending on whether the dimensions manifest themselves in thoughts and behavior that are conducive to the current task or not (Reinecke, 2018). Supporting such a view, the literature on media use and well-being suggests that goal-directed, purposeful social interactions via media can enhance social gratifications (Bayer, Ellison, Schoenebeck, Brady, & Falk, 2018; Burke & Kraut, 2016; Jung & Sundar, 2018) and contribute to well-being (Domahidi, 2018; Trepte, Dienlin, & Reinecke, 2015). In contrast, passive use or technology use as procrastination can have negative consequences for well-being (Meier, Reinecke, & Meltzer, 2016; Reinecke & Hofmann, 2016; Verduyn et al., 2015).

Analogously, the dimensions of online vigilance likely do not have a general positive or negative relation to well-being. Just as mediated communication that conflicts with other goals presents a challenge for well-being, we expect different mechanisms to connect each dimension of online vigilance to well-being. Specifically, we suggest dimensions of online vigilance to relate negatively to well-being if higher levels of the specific dimension (salience, reactivity, monitoring) are expressed in thoughts and behaviors that represent an *interference* with people's lives or higher-order goals.

If the online vigilance dimensions take the form of interferences in everyday life, they should exert their influence on a situational level. In other

words, it is unlikely that such interferences instantly influence how people generally evaluate their lives. Satisfaction with life, for instance, has shown to be rather stable and only displays gradual changes (Diener, Lucas, & Oishi, 2018). Instead, if dimensions of online vigilance take the form of situational thoughts and behaviors, they should relate to situational *affect*. Therefore, we expect the online vigilance dimensions to relate to affect as a transient well-being construct that is sensitive to small, moment-to-moment changes in individuals' well-being (Wilhelm & Schoebi, 2007). Such affective well-being has been shown to be affected by intraindividual variations in social media behavior (Bayer et al., 2018).

Salience

So far, there is no evidence linking situational online vigilance to affective well-being. However, on the trait level, there is initial evidence that online vigilance indeed can take the form of interference. For example, online vigilance related negatively to well-being through decreased mindfulness. This indirect association appeared to be mostly driven by the *salience* dimension (Johannes et al., 2018). In other words, thoughts about mediated interactions were most detrimental when they distracted from the current moment. Such a mechanism can also explain why online vigilance has been linked to perceived stress (Reinecke et al., 2018): Salience in the form of interfering thoughts could be perceived as stressful.

The prominent role of the salience dimension in relation to well-being is not surprising, given that absentmindedness in the form of mind-wandering has been shown to be negatively related to well-being (e.g., Smallwood, O'Connor, Sudbery, & Obonsawin, 2007). Such task-irrelevant thoughts may distract from the current moment, thereby decreasing well-being (Franklin et al., 2013; Killingsworth & Gilbert, 2010), particularly when pondering about the past (Spronken, Holland, Figner, & Dijksterhuis, 2016). Salience and mind-wandering are conceptually related. Whereas mind-wandering manifests in conscious thoughts, salience encompasses both conscious thoughts and unconscious preoccupation with online communication. When salience takes the form of conscious, task-irrelevant thoughts, we expected such thoughts to interfere with experiencing the current moment. In line with such a prediction, when people use their smartphone in a mindful rather than in a mindless way, these negative effects can reverse (Bauer, Loy, Masur, & Schneider, 2017). Taken together, we expected thoughts about mediated interactions (i.e., situational salience) to distract from the immediate environment. Hence, we predicted that salience is related negatively to affective well-being (H_1).

However, such a view assumes that all experiences of salience represent task-irrelevant thoughts. Yet thoughts can be of different valence. Thinking of desired experiences can alleviate boredom (Eastwood, Frischen, Fenske, & Smilek, 2012) and induce goal-setting for the distant future (Mooneyham &

Schooler, 2013), which is experienced as positive (Spronken et al., 2016). Likewise, social daydreaming, over time, can lead to positive affect and less loneliness (Poerio, Totterdell, Emerson, & Miles, 2016). Thus, the valence of thoughts plays a role in the effect of mind-wandering on well-being, with interesting and positive thoughts relating positively, but other thoughts relating negatively to well-being (Franklin et al., 2013). Such a mechanism likely applies to salience as well, as the online vigilance construct allows for both negative and positive effects on well-being (Reinecke, 2018). Thinking about one's social network and the support it provides, analogous to how interactions with close ties via technology can increase well-being (Burke & Kraut, 2016), may thus alleviate the negative, distracting effect of salience. In other words, thoughts about online streams of communication and interaction may distract from the current environment. If those thoughts are positive, however, they might compensate for the distraction. Thus, we predicted that the valence of thoughts moderates the relationship between salience and affective well-being, such that the negative relationship is stronger with negative valence, but weaker with positive valence (H_4).¹

Furthermore, not only is there no work documenting the frequency of thoughts about mediated interactions; it is also unclear how frequently these thoughts occur compared to thoughts about face-to-face interactions. Similarly, a comparison of the valence of thoughts about mediated interactions versus thoughts about face-to-face interactions would provide valuable insights into the nature of the salience dimension. That is, without such descriptive information, it is difficult to assess whether salience differs from any other sort of mental preoccupation with interpersonal communication. For descriptive information, and to provide an exploratory comparison, we thus investigate the frequency and valence of both thoughts about online as well as face-to-face interactions.

Reactibility

In the case of the reactibility dimension, previous research shows that people appear to be extremely sensitive to smartphone notifications. Many users respond to notifications almost instantly; even if their phone is in silent mode they check notifications within minutes (Pielot, Church, & de Oliveira, 2014). Such responsiveness to smartphone cues may come at the cost of increased stress. Smartphone users high in reactibility who attend instantly to notifications will routinely interrupt other tasks (Mehrotra, Pejovic, Vermeulen, Hendley, & Musolesi, 2016). Smartphone notifications can serve as connection cues that automatically capture attention for users with high reactibility (Bayer, Campbell, & Ling, 2015). This can interrupt current tasks (Stohart, Mitchum, & Yehner, 2015). Crucially, smartphone-induced interruptions can ultimately lead to high communication load and stress (e.g., Reinecke et al., 2017). For instance, increasing smartphone cues led to feelings of social

pressure (Halfmann & Rieger, 2019). Conversely, minimizing notification alerts has shown to decrease inattention, which was responsible for increased well-being (Kushlev, Proulx, & Dunn, 2016). We therefore reasoned that people high in reactivity would be more responsive to notifications, resulting in lower well-being. Hence, we predicted reactivity relates negatively to affective well-being (H_2).

Monitoring

Last, people high in monitoring display more quick checks of their smartphone, unprompted by a notification. Such checks have shown to occur frequently and often manifest themselves in the form of habits (Hintze, Hintze, Findling, & Mayrhofer, 2017; Oulasvirta, Rattenbury, Ma, & Raita, 2012). Whereas monitoring has the potential to remind people of their social network (Domahidi, 2018), unprompted checks often do not serve an explicit goal; instead, monitoring regularly takes the form of non-purposeful checks (Oulasvirta et al., 2012). Monitoring can then be understood as a specific form of mindless media use, which has shown to relate negatively to well-being (Meier et al., 2016; Verduyn et al., 2015). Repeatedly checking in to online streams of communication and interaction without an explicit communication goal represents a distraction from the current moment.

Evidence for such a prediction comes from research showing that checking e-mail less frequently was associated with increased well-being (Kushlev & Dunn, 2015). The distracting mechanism behind the hypothesized effect of monitoring is distinct, though, from that of salience or reactivity. Both salience and monitoring are self-initiated and not necessarily prompted by notifications, yet salience refers to the cognitive components of online vigilance, whereas monitoring is expressed behaviorally. Furthermore, both reactivity and monitoring are expressed behaviorally, but reactivity is exclusively prompted externally, whereas monitoring can be prompted both internally and externally (Klimmt et al., 2018; Reinecke et al., 2018). Thus, monitoring should result in distraction from the current moment, independent of the distraction caused by salience or monitoring. Therefore, we predicted monitoring relates negatively to affective well-being (H_3).

The current study

With the current study, we had two central goals. Theoretically, we investigated the relationship between online vigilance and affective well-being in a new context, namely, within smartphone-mediated social interactions on the state level. In our study, we focused on social interactions for several reasons. First, social media are the most used technological platforms by young adults, who are the focus of our study (Pew Research Center, 2018). Second, this heavy use is reflected in theoretical assumptions about online vigilance. Reinecke et al.

(2018) state that it is the social features of smartphones in particular that afford users a *technological* connectedness to social contacts. This technological connectedness is the primary source of *psychological* connectedness (Klimmt et al., 2018; Reinecke et al., 2018). Therefore, a focus on smartphone-mediated social interactions will capture this crucial aspect of online vigilance.

Methodologically, we aimed at addressing several shortcomings in the literature. Traditionally, the majority of research on smartphone use and well-being has relied on self-reports of usage behavior (for a recent critique, see Ellis, 2019). However, there is increasing evidence that people are poor estimators of their phone use, casting doubt on the validity of self-reported screen time (Ellis, Davidson, Shaw, & Geyer, 2019; Scharkow, 2016; Wilcockson, Ellis, & Shaw, 2018). Moreover, it is questionable whether general trait measures are predictive of behavior in the moment (e.g., Masur, 2018). To address these limitations, in the present study we combined smartphone logs as behavioral indicators of online vigilance with experience sampling of situational self-report data of the three online vigilance dimensions and of affective well-being. To our knowledge, there is only one study that combined objective smartphone use with experience sampling. Katevas, Arapakis, and Pielot (2018) found that phone use at night, not general phone use, negatively predicted well-being. However, the analysis aggregated phone use and well-being per day, rather than predicting each instance of reported well-being with preceding phone use variables. For one, this demonstrates the need for more fine-grained analyses. Second, their analysis was data driven and exploratory, demonstrating the need for confirmatory, hypothesis testing work.

The need for confirmatory research is particularly important given that many findings in the Social Sciences do not replicate (e.g., Camerer et al., 2018), likely due to undisclosed flexibility in data analysis (e.g., Nelson & Simmons, 2018). These criticisms have led to calls for confirmatory research that explicates all hypotheses and analysis steps before the data are collected in so-called *preregistrations* (Nosek, Ebersole, DeHaven, & Mellor, 2018). As a consequence, the effect of media use on well-being might have been overestimated so far (e.g., Orben & Przybylski, 2019a). We thus preregistered this project, thereby restricting flexibility in confirmatory data analysis in an attempt to increase the reliability of our findings.

Method

We preregistered all hypotheses, operationalizations, exclusion criteria, and analysis steps on the Open Science Framework (OSF), where readers can also find all materials, data, and analysis scripts (<https://osf.io/n6d8k/>).

Participants

Due to our analytical approach and a lack of previous research, power calculations were difficult. We followed the pragmatic recommendation to recruit as many participants as we had resources for (Albers & Lakens, 2018). Thus, we preregistered to collect data from 200 participants or to end collection at a preregistered date (i.e., September 1, 2018). Between February and September 2018, we recruited 111 students from a Dutch university. Participants had to be undergraduate students, proficient in English, between 18 and 30 years old, use an Android phone, and had to use at least one of the following social media apps daily: WhatsApp, Facebook, Facebook Messenger, Instagram, Snapchat, Twitter, SMS/Messenger.² Participants could choose between receiving money or credits as reimbursement for their participation. We only recruited Android users because smartphone logging only worked on that operating system. Both forms of reimbursement followed an incentive scheme, such that participants received 5 € for the intake session and an additional 0.50€ for every two surveys they filled out. In total, participants could earn up to 15€ or an equivalent amount of course credit.

We had to exclude a substantial number of participants because of technological issues. Because of these issues, the logging app could neither log phone use nor send surveys to several devices. For a detailed explanation and all decisions made during data collection, see the Online Supplementary Materials (OSM) on the OSF project. We retained the data from 77 participants who had complete logging and survey data. According to our preregistered exclusion criteria, we excluded one additional participant for answering less than eight surveys (i.e., 20% of 40 scheduled surveys). Finally, one participant received 69 non-duplicate surveys due to the technological issues described above. Because we could not be sure whether we could trust the data from this participant, we excluded the data.

In total, we retained data from 75 participants (53 female, 21 male, 1 preferred not to indicate gender) with a typical age range for undergraduate students ($M_{age} = 21.89$, $SD_{age} = 2.48$). All participants indicated that they used at least one of the social apps of interest daily. All participants gave informed consent; the study had approval from the ethics board (ECSW-2C17-059R1).

Procedure

Participants arrived in the lab to participate in a study titled “Smartphone use in everyday life”. They were informed that the goal of the study was to assess how people use their smartphones in daily life. They received further information about the nature of the study, the logging procedure, how many surveys they would receive daily, and the survey questions. Next, participants installed the logging app with the help of the researcher. We used the app

PACO (www.pacoapp.com) for data collection. The app can log phone use and distribute experience sampling surveys, is open source, and free to use, but works only for Android devices, as iOS does not allow phone logging. The app recorded when the screen was unlocked or locked, when and what app was used, and when and from what app notifications arrived.

After the installation, participants took an intake survey that assessed trait measures and demographic information. Afterward, participants were free to choose a time frame of five consecutive working days (e.g., Thursday to Wednesday, excluding Saturday and Sunday). Such a time frame has shown to be representative of people's typical phone use (Wilcockson et al., 2018). On these days, they received eight daily surveys between 09:00 and 21:00. We chose working days because affective well-being is generally different between weekdays and weekends (Helliwell & Wang, 2014). Surveys were sent at semi-random intervals, with at least 45 minutes between surveys. Due to the time frame, some participants received less than the total 40 surveys ($M = 36.33$, $SD = 3.81$), as some of them turned on their phones only later in the day. Surveys contained 18 short questions and took less than a minute; participants had a five-minute time window to respond to surveys. Response rate to surveys was 59.6% ($SD = 17.5\%$). We also collected experiences about the study in an exit survey, the results of which can be found in the OSM on the OSF.

Measures

State salience

We chose self-reports instead of a behavioral measure for salience because self-reports are the most adequate measure of thought frequency (Mooneyham & Schooler, 2013). To assess salience on the state level, participants answered one item ("In the last half an hour, how much were you thinking about mediated interactions (e.g., phone calls, WhatsApp messages, Facebook likes, Instagram posts etc.?)" on a scale from 1 (*not at all*) to 7 (*a lot*), $M_{raw} = 3.56$, $SD_{raw} = 1.93$. During the set-up process, participants were provided with a short explanation of mediated interactions, stating that the term did not refer to face-to-face interactions but to any form of contact participants had with others via their smartphone, tablet, laptop, etc. We informed participants that mediated interactions did not refer only to talking or texting with others, but also more general forms of contact such as receiving or providing a like or a comment. For exploratory purposes, participants also answered the same question about face-to-face interactions ("In the last half an hour, how much were you thinking about face-to-face interactions?", $M_{raw} = 3.84$, $SD_{raw} = 2.02$).

Valence of situational thoughts about social interaction

Participants indicated the pleasantness of thoughts about mediated interactions ($M_{raw} = 4.84$, $SD_{raw} = 1.21$) and face-to-face interactions ($M_{raw} = 5.11$, SD_{raw}

= 1.23) with one item, respectively (“How pleasant were those thoughts about mediated/face-to-face interactions?” on a scale from 1 (*unpleasant*) to 7 (*pleasant*).

State reactivity

Originally, we operationalized behavioral reactivity as the average time participants took to open a notification from a social app (i.e., WhatsApp, Facebook, Facebook Messenger, Instagram, Snapchat, Twitter, SMS/Messenger). However, due to the technical issues described above, only 37.8% of days had at least one notification logged, which would have resulted in an inordinate amount of missing data for the behavioral reactivity measure. To account for possible problems with logging, we preregistered a decision tree, specifying how we would proceed in case of missing data or other problems. Thus, we followed our a priori decision tree and operationalized behavioral reactivity as the time between receiving a survey and opening the survey, $M_{seconds} = 64.50$, $\tilde{x} = 26.00$, $SD = 71.23$. We also assessed self-reported reactivity with one item (“In the last half an hour, when I received an online message, I immediately gave it my full attention.”) developed by Reinecke et al. (2018). Participants indicated their agreement on a scale from 1 (*strongly disagree*) to 7 (*strongly agree*), $M_{raw} = 3.72$, $SD_{raw} = 1.98$.

State monitoring

Originally, we operationalized behavioral monitoring as the amount of checks before the survey was opened, with a check defined as any sequence of unlocking and locking the screen again without having been preceded by a notification from a social app. However, such unlocks barely occurred, most likely because the app did not log if the screen was turned on without being unlocked. Thus, we followed our preregistered decision tree and operationalized behavioral monitoring as the total time social apps were used in the 30 minutes before the survey was opened, $M_{seconds} = 122.43$, $\tilde{x} = 47.00$, $SD = 190.33$. We also assessed self-reported monitoring with one item (“In the last half an hour, I was constantly monitoring what was happening online.”) developed by Reinecke et al. (2018). Participants indicated their agreement on a scale from 1 (*strongly disagree*) to 7 (*strongly agree*), $M_{raw} = 2.92$, $SD_{raw} = 1.83$.

State affective well-being

Participants indicated how they currently felt on a mood scale (Wilhelm & Schoebi, 2007). The items were preceded with “At this moment, I feel”, followed by six mood dichotomies (e.g., “tired-awake”) from the original scale plus an additional dichotomy (“depressed-happy”) to explicitly capture happiness. Items were aggregated per survey to form a mean index, $M_{overall} = 5.02$, $SD_{overall} = 1.03$.

Trait measures

We measured four traits in the intake survey for exploratory analysis. The full details of their measurement can be found in the OSM. To measure online vigilance as an individual difference variable, participants filled out the Online Vigilance Scale (Reinecke et al., 2018), $M = 2.85$, $SD = 0.64$, $\alpha = .86$. To assess the smartphone checking habit of participants, we employed an established 12-item measure of habit strength in the intake survey (Verplanken & Orbell, 2003) and added an item reflecting the lack of intentionality, following suggestions of previous work on smartphone habits (Bayer & Campbell, 2012), $M = 4.85$, $SD = 0.82$, $\alpha = .86$. We assessed the affective component of well-being with the scale of positive and negative experience developed by Diener et al. (2010), $M = 8.73$, $SD = 5.62$. We assessed the evaluative component of well-being with the satisfaction with life scale (Diener, Emmons, Larsen, & Griffin, 1985), $M = 5.02$, $SD = 0.98$, $\alpha = .80$.

Results

Main effects model

Following our preregistration, we computed two models. The main effects model, testing the situational relationships specified in $H_1 - H_3$, predicted affective well-being from self-reported salience, behavioral reactivity, and behavioral monitoring and was run on the full data. The moderator model, testing H_4 , predicted well-being from the same variables as the main effects model, but also included valence of thoughts related to mediated social interactions as well as an interaction term between salience and valence of thoughts. The interaction model was run on a subset of the data, because the moderator, thought valence, occurred only when people indicated non-zero levels of salience. Because both models were based on the same data, we adjusted for multiple testing by applying a Bonferroni correction, such that we regarded parameters to be significant if they were below $\alpha = .025$.

There was substantial variation of affective well-being between and within participants, $ICC = .38$, demonstrating that multilevel modeling was appropriate. We conducted all analyses in *R* (version 3.5.2, R Core Team, 2018); data wrangling and visualization were done with *tidyverse* (version 1.2.1, Wickham, 2017). To account for the nested nature of our data, we ran mixed-effects models with the *lme4* package (version 1.1–21, Bates, Maechler, Bolker, & Walker, 2015). We followed recommendations to control Type I errors by employing a maximal random effects structure (Barr, Levy, Scheepers, & Tily, 2013). That is, in the original preregistered models we specified random intercepts and random slopes for each predictor for participants on level 3 and days on level 2. In addition, we preregistered to person mean-center all continuous predictors. To obtain p -values, we

computed bootstrapped Likelihood Ratio Tests using the *mixed* function (*afex* package, version 0.20–2; Singmann, Bolker, Westfall, & Aust, 2018).

The model did not immediately converge, most likely due to an overly complex model structure. We followed current best practices and simplified the model (Barr et al., 2013). Removing random slopes generally leads to inflated Type I errors. Instead, we removed day as a grouping factor within participants, because there was little variation of all variables across weekdays. We also standardized the predictors. The model still yielded a warning, but at an acceptable tolerance level. For full details on the modeling procedure, see the OSM. Model diagnostics, such as residuals and checks for heteroscedasticity, displayed acceptable fit. Of the three predictors, only behavioral monitoring negatively predicted well-being, $PBtest = 7.02$, $p = .008$. To obtain an indication of the standardized effect size, we also standardized the outcome variable (not preregistered). The standardized effect size was small ($\beta = -.06$); see Table 1 for all parameter estimates. We called the *r.squaredGLMM* function to obtain *Pseudo R*² for mixed-models (*MuMIn* package, version 1.40.4, Barton, 2018), displaying $R^2 = .004$ for the variance explained by fixed factors, and $R^2 = .38$ for the variance explained by both fixed and random factors. We conducted several robustness checks (not preregistered, see OSM). Excluding an outlier yielded a nonsignificant (at $\alpha = .025$) estimate of the effect of behavioral monitoring, $PBtest = 6.63$, $p = .035$. Because of the convergence issues, we also estimated a Bayesian mixed-effects model with the *brms* package (version 2.2.0, Bürkner, 2017), which estimated parameters very close to those of the frequentist model, increasing our trust in the estimates. The Bayesian model also supported our suspicion that the behavioral monitoring effect is not robust.

Interaction model

Confirming that our model was too complex, the original model with an added interaction term yielded a convergence error, as the number of observations was smaller than the number of random effects. We followed the same steps as with the main effects model. In addition to removing the weekday grouping and standardizing the predictors, we also had to remove all correlations between random effects (Barr et al., 2013); the resulting singularity warning was within an acceptable level of tolerance. Model diagnostics displayed good fit.

Supporting our conclusion that the effect of behavioral monitoring was not robust, the effect was not significant (at $\alpha = .025$) and small, $PBtest = 5.89$, $\beta = -.056$, $p = .042$, when also accounting for the valence of salience and its interaction with salience. In this model, salience was significantly and negatively related to well-being at average valence, $PBtest = 8.62$, $\beta = -.11$, $p = .006$. However, valence was positively related

Table 1. Parameters and estimates of the two confirmatory mixed-effects models.

Predictors	<i>B</i>	<i>SE</i>	<i>t</i>	β	<i>p</i>	R^2_F	R^2_R
Main effects model						.004	.38
Salience	-0.004	0.03	-0.14	-.01	.97		
Reactibility	0.023	0.02	0.95	.03	.48		
Monitoring	-0.06	0.02	-2.67	-.06	.008		
Interaction model						.05	.44
Salience	-0.089	0.03	-3.00	-.11	.006		
Valence	0.169	0.03	5.13	.20	.001		
Reactibility	0.034	0.02	1.47	.04	.171		
Monitoring	-0.052	0.02	-2.43	-.06	.042		
Salience x Valence	0.054	0.02	2.23	.06	.086		

Beta coefficients are based on a model with standardized predictors and outcomes. The other estimates are based on the preregistered models with standardized predictors but non-standardized outcomes. Hence, beta coefficients should be seen as an indicator of the effect size, not a direct translation. R^2_F denotes the variance explained by fixed factors. R^2_R denotes the variance explained by both fixed and random factors.

to well-being at average salience with a larger effect size, $PBtest = 22.83$, $\beta = .21$, $p = .001$. Although in the expected direction, their interaction failed to reach significance, $PBtest = 4.85$, $\beta = .06$, $p = .086$. Thus, salience and the valence of salience were independently related to well-being. Those effects were evident in a larger $R^2 = .05$ for the variance explained by fixed factors, and $R^2 = .44$ for the variance explained by both fixed and random factors. Exploratory robustness checks and Bayesian models demonstrated that the interaction model was robust and displayed better fit than the main effects model.

Exploratory analyses

Relation between state and trait measures

Following our preregistration, we also explored correlations between state measures (aggregated per person) and trait measures. As those correlations were entirely exploratory, p -values are not meaningful (Gelman & Loken, 2013), which is why we only present and interpret effect sizes, see Figure 1. Correlations between the behavioral indicators (behavioral monitoring and behavioral reactibility) and the online vigilance trait were relatively low ($r < .21$), compared to moderate correlations between self-reported state-level vigilance dimensions (self-reported salience, self-reported monitoring, and self-reported reactibility) and the online vigilance trait ($r > .34$). Trait online vigilance was only weakly correlated to the trait well-being measures ($r > -.10$), but was strongly related to smartphone habits ($r = .64$).

On the state-level, the self-reported vigilance dimensions correlated highly with each other ($r > .68$). Behavioral monitoring was moderately related to both self-reported reactibility and self-reported monitoring ($r > .37$), whereas behavioral reactibility was not correlated to any other variable, except trait

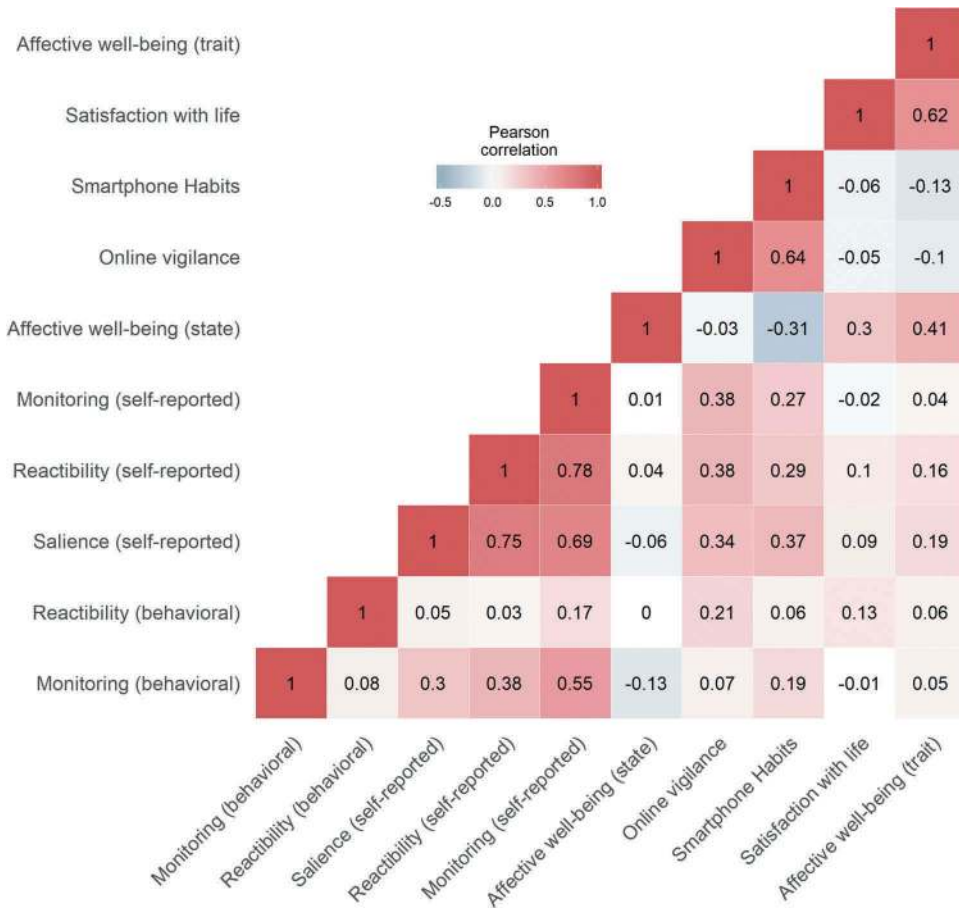


Figure 1. Heat map of correlations between person-aggregated state-level measures and trait measures. The numbers in each field represent Pearson's correlation coefficient. The strength and direction are visualized with colors.

online vigilance. These relationships must be interpreted with caution, as aggregating the state-level variables can only provide coarse estimates.

Comparing behavioral and self-reported vigilance dimensions

We also preregistered to explore the relation between self-reported and behavioral measures of salience and monitoring in a more fine-grained manner. We thus ran two maximal mixed-effects models, one for monitoring and one for reactivity (without the weekday grouping), predicting behavioral indicators with self-reported indicators. In line with the overall correlation, self-reported reactivity was a poor predictor of behavioral reactivity, $t(1) = -5.53$, $SE = 3.18$, $\beta = -.14$, $95\%CI = [-.19, -.09]$. That means that a higher self-reported attention to notifications was weakly related to responding faster to surveys. Self-reported monitoring was a stronger predictor of behavioral monitoring, $t(1) = 8.27$, $SE = 6.17$, $\beta = .31$, $95\%CI = [.26, .37]$.

Predicting well-being with self-reported vigilance dimensions

We were also interested in whether self-reported indicators of reactivity and monitoring would predict well-being better than behavioral indicators of reactivity and monitoring (not preregistered). To that end, we predicted standardized well-being with self-reported standardized salience, reactivity, and monitoring in a maximal mixed-effects model. All estimates were extremely close to zero (all $\beta < |.04|$), yielding no evidence of an effect of any predictor. See the OSF page for the full details of this analysis.

Mediated vs. face-to-face interactions

Inspecting the data showed that there were differences in the frequency and valence between thoughts about mediated interactions and face-to-face interactions. Thoughts about mediated interactions occurred less frequently ($M = 3.52$, $SD = 0.96$) than thoughts about face-to-face interactions ($M = 3.83$, $SD = 1.06$), $t(74) = -2.74$, $d_z = -0.32$. Similarly, valence of thoughts about mediated interactions was more negative ($M = 4.21$, $SD = 0.80$) than that of thoughts about face-to-face interactions ($M = 5.08$, $SD = 0.72$), $t(74) = -7.49$, $d_z = -0.86$.

Consequently, we were interested in whether thoughts about and valence of face-to-face interactions were a stronger predictor of well-being than thoughts about and valence of mediated interactions (not preregistered). A maximal mixed-effects model (no day grouping, no random correlations) showed an interesting pattern of results. Corroborating our previous analyses, the frequency of thoughts about mediated interactions (i.e., salience) was negatively related to affective well-being, $t(76.82) = -4.18$, $SE = 0.04$, $\beta = -.15$, 95% CI = $[-.21, -.08]$, whereas the valence of those thoughts displayed a positive relation, $t(72.28) = -5.84$, $SE = 0.03$, $\beta = .19$, 95% CI = $[.13, .26]$. Surprisingly, the frequency of thoughts about face-to-face interactions was not meaningfully related to affective well-being, $t(77.28) = 1.03$, $SE = 0.03$, $\beta = .03$, 95% CI = $[-.04, .10]$; however, their valence exhibited a positive relation to affective well-being, $t(1041.43) = 6.14$, $SE = 0.03$, $\beta = .20$, 95% CI = $[.13, .27]$. Hence, it appears that the frequency of thoughts about mediated interaction is problematic compared to the frequency of thoughts about face-to-face interactions. However, the relation of the valence of both types of thoughts with affective well-being appears more important and of equal size for mediated and non-mediated interactions.

Discussion

With smartphones becoming a central part of people's lives, users report to be in a state of constant alertness (Mihailidis, 2014). This alertness has been defined as online vigilance (Klimmt et al., 2018; Reinecke et al., 2018). To date, it is largely unclear whether this constant orientation toward mediated

communication has consequences for well-being. In this study, we address this question. Specifically, we asked whether online vigilance is related to well-being in people's everyday lives. To that end, we combined behavioral logging data with momentary self-reports, going beyond the deterministic behavioral view of connectedness as merely expressed in screen time (e.g., Orben & Przybylski, 2019b). Instead, our study focused on the psychological internalization of connectedness, measured with rarely used real-time logging (Ellis, 2019). Consequently, we provide a thorough test of so far unstudied episodic fluctuations in online vigilance and their relation to situational affective well-being.

Overall, our results suggest small to negligible situational relations between the three dimensions of online vigilance (both behavioral and self-reported) and well-being. These small effect sizes are in line with recent work on the relation between screen time and well-being (Heffer et al., 2019; Orben et al., 2019; Orben & Przybylski, 2019a, 2019b; Przybylski & Weinstein, 2017) and with small effect sizes in the field in general (Rains, Levine, & Weber, 2018). Valence of thoughts about mediated communication was by far the strongest predictor of well-being. The more positively participants thought about mediated interactions in the past half an hour, the better they felt in the current moment. This effect was rather large in comparison to the typically small effect sizes in media effects research (Rains et al., 2018). This finding presents a preregistered replication of previous research showing that thoughts about one's social network and positive, interesting thoughts in general can increase positive affect (Franklin et al., 2013; Poerio et al., 2016). More importantly, we extend previous research by providing evidence that the effect of valence is not limited to thoughts about offline interactions, but also applies to thoughts about mediated interactions. This corroborates accounts arguing for smartphones as an ever-present reminder of one's social network (Carolus et al., 2018).

The role of valence becomes particularly important in light of the negative relation of salience to well-being. In line with our predictions, more thoughts about mediated interactions were associated with slightly decreased well-being. Similar to previous work on the trait level, where salience was the most important predictor (Johannes et al., 2018), salience on the state level was the only dimension of online vigilance robustly predicting well-being. This is in line with previous work showing mind-wandering on the state level to be negatively related to well-being (Franklin et al., 2013; Killingsworth & Gilbert, 2010). Hence, this presents evidence that high levels of salience can present an interference and thus distract from the current moment or task.

Surprisingly, the effect of the positivity of thoughts (i.e., valence) about mediated interactions was independent of their frequency (i.e., salience). We expected the effect of these thoughts to become weaker the more positive they were. Our results suggest that the frequency of thoughts about mediated

interactions is negatively related to well-being. Yet when thoughts are positive, this positivity may more than compensate the possible negative effect of frequency, yielding an overall positive effect of thoughts with positive valence. In other words, the frequency of thoughts may not play a role, as long as they are of positive valence. This has important implications for the online vigilance construct. Specifically, the pattern of effects provide first evidence for the theoretical proposition that online vigilance is not uniformly detrimental to well-being (Reinecke, 2018).

Interestingly, this conclusion does not hold up for thoughts about face-to-face interactions. Although they were of higher frequency than thoughts about mediated interactions, they were virtually unrelated to affective well-being. Like thoughts about mediated interactions, however, the valence of thoughts about face-to-face interactions was a strong positive predictor of well-being. The more positive thoughts were about interactions that people had face-to-face in the last half an hour, the better they felt at the moment. Importantly, this positive relation was about as strong as that of the valence of thoughts about mediated interactions with well-being. These findings have several implications. First, thoughts about the online sphere might be more bothersome than thoughts about the offline sphere, possibly because they increase the salience of social pressure for constant connectedness (Halfmann & Rieger, 2019; Reinecke et al., 2017). Second, the valence of thoughts about mediated interactions can be just as beneficial for well-being as the valence of thoughts about face-to-face interactions. However, the analysis of thoughts about face-to-face interactions was purely exploratory and needs to be independently replicated before we can draw strong conclusions.

Behavioral monitoring was negatively related to well-being. The more time people spent on social apps in the half hour before they answered a survey, the worse they felt at that moment. However, this effect was small and not robust. The robustness of the effect depended on analytical choices (e.g., outlier exclusion), similar to studies showing that the relation between screen time and media use can depend on analysis choices (e.g., Orben et al., 2019). Even if an effect exists, it is doubtful whether it is large enough to have practical consequences. As such, this component of online vigilance does not appear to be negatively related to well-being.

A similar argument applies to behavioral reactivity. Contrary to our prediction, reactivity was not significantly related to well-being and displayed a small effect. In other words, how quickly participants responded to surveys was not related to their well-being. This lack of a meaningful relation could have several reasons. First, it might indicate that the reactivity dimension is not associated with task interruptions. Alternatively, it might be associated with task interruptions, but these interruptions may not be severe enough to lead to interference or higher communication load and subsequently impair well-being. Both accounts would be in contrast to

previous literature showing that such interruptions can result in increased stress and pressure (Halfmann & Rieger, 2019; Reinecke et al., 2017).

Self-reported indicators of state online vigilance correlated moderately with the online vigilance trait, in line with previous work (Reinecke et al., 2018). The self-reported indicators also displayed high correlations among each other. In contrast, the correlations between the behavioral state indicators and the online vigilance trait were much lower. The lack of a relation between trait and situational behavior supports the view that trait measures might sometimes not be predictive of actual behavior (Masur, 2018). Similarly, people who report high levels of general awareness of online streams of communication might not express behavior in line with that self-assessment. This possibility does not invalidate online vigilance as a construct, but rather speaks to the larger issue of how predictive such person-level media-related variables are of actual behavior on the situation level (Ellis et al., 2019; Scharrow, 2016). Even when we assume that the self-reported items are a better indicator of online vigilance, our exploratory analysis showed that these dimensions were not related to well-being. Hence, regardless of whether predictors were behavioral or self-reported, monitoring and reactivity were not meaningfully related to well-being.

Limitations

Our study comes with several limitations. First, the lack of an interaction effect between salience and valence of mediated thoughts could reflect a lack of power. Because participants did not report salience to occur in every episode, we had to rely on a subset of the data, which greatly reduced observations per participant. However, inspecting the credible interval of the interaction effect indicates that the effect likely is small, possibly too small to have practical relevance.

Second, it is unclear whether time spent with social apps is truly reflecting the monitoring dimension of online vigilance. We originally operationalized monitoring as phone checks unprompted by notifications. We assumed such phone checks were mostly non-purposeful because they did not serve an explicit goal (Hintze et al., 2017; Oulasvirta et al., 2012), thereby representing a manifestation of monitoring. Because the logging app did not consistently record notifications, we followed our preregistered decision tree and relied on time spent on social apps as a measure. Whereas someone high in monitoring would certainly spend more time on social apps, such a measure subsumes both use triggered by a notification and checks unprompted by notifications. That being said, there was a moderate correlation between self-reported monitoring and behavioral monitoring. Consequently, we believe time spent with social apps presents a suitable, but not ideal measure of the monitoring dimension.

Third, it is likely that operationalizing reactivity as how quickly participants responded to surveys captures other processes in addition to reactivity. For example, such response times might reflect compliance to a considerable degree. Alternatively, participants received a higher compensation the more surveys they answered, possibly motivating them to respond faster. At the same time, someone high in reactivity will likely open notifications faster than someone low in reactivity, including surveys from an experience sampling app. However, there was only a low correlation between self-reported and behavioral reactivity. On the one hand, this might indicate that our behavioral reactivity measure was not adequate to capture this dimension of online vigilance. On the other hand, this mismatch might merely reflect a disconnect between people's reported and their actual behavior (e.g., Ellis et al., 2019). It thus remains unclear whether the measure captures the whole spectrum of the reactivity dimension. Furthermore, it could be argued that our operationalization of both reactivity and monitoring was limited compared to our measure of salience because we did not assess the perceived situational valence of these two vigilance dimensions. We argue, however, that for the salience dimension, valence represents an experiential quality of this online vigilance dimension *itself*, whereas for reactivity and monitoring, valence should be more strongly affected by the situational *consequences* of the respective usage behavior (e.g., goal-conflict; Reinecke & Hofmann, 2016). Thus, to avoid confounding valence with situational outcomes of reactivity and monitoring, we only measured the valence of salience. Nonetheless, future research may benefit from comparing the valence of all three vigilance dimensions.

Fourth, the operationalizations for monitoring and reactivity were a result of our preregistered decision tree. Such a decision tree cannot foresee all aspects that researchers should account for after having inspected the data (Szollosi et al., 2019). Logging data are complex and require many decisions about how to process and analyze them. We chose a decision tree because it can reduce such researcher degrees of freedom as much as possible. At the same time, we documented deviations that were necessary based on the data (Frankenhuis & Nettle, 2018). We believe that such an approach prevents arbitrary analytic decisions, ultimately reducing our risk of false-positives, whilst maintaining flexibility.

Fifth, although self-reports are the most appropriate method to measure salience, they necessarily aggregate events that participants must recall. Participants had to differentiate between thoughts about mediated and face-to-face interactions, which can introduce recall bias. These reports also did not differentiate between different types of social interactions (e.g., interpersonal, masspersonal, or mass communication).

Sixth, we focused on smartphone-mediated social interactions because these interactions are the main contributor to online vigilance (Klimmt

et al., 2018; Reinecke et al., 2018). However, other smartphone features could also contribute to online vigilance (e.g., work-related e-mail, news, gaming). Although we believe that a focus on social interactions captures the online vigilance concept well, we ask future research to consider that nonsocial media uses and devices beyond the smartphone may have unique implications for online vigilance.

Seventh, our study focused on university students because young adults use smartphones the most (CBS, 2018; Pew Research Center, 2017). Our findings should not be generalized to older populations. At the same time, younger people use smartphones more than older people, thereby having more opportunities to develop online vigilance. If we cannot find negative effects of online vigilance for such heavy users, it is likely that effects of online vigilance on well-being are small for other populations as well.

Last, online vigilance on the trait level was only weakly related to trait well-being, in line with previous work demonstrating that direct relations were masked by mediators (Johannes et al., 2018). At least on the trait level, online vigilance may thus not directly relate to well-being, but rather through absentmindedness or stress (Reinecke et al., 2018). It is possible such mediating mechanisms must be taken into account when investigating online vigilance and well-being on the state level.

Conclusion

Overall, our study shows that effects of online vigilance on well-being are most likely small, possibly too small to be of practical significance. We take into account that psychological connectedness can be expressed both cognitively and behaviorally. As such, we go beyond the simplistic view of screen time as the most important indicator of connectedness. Furthermore, we extend previous research by testing the expression of online vigilance and its relation to well-being on the situational level. Methodologically, we preregistered the entire project, contributing to a more reliable knowledge base on (social) media effects. Most important, our findings on salience and its valence demonstrate the need for nuance when studying such effects. Specifically, the results warrant caution not to treat cognitive preoccupation with mediated interactions as uniformly negative.

Notes

1. We label hypotheses from H_1 to H_4 to stay consistent with the labeling used in our preregistration; see Method.
2. SMS/Messenger refers to built-in Android apps that send and receive SMS (e.g., “Messages”).

Author note

NJ, AM, LR, SE, DNS, and HV developed the study concept and design. SE and DNS collected the data with help from NJ. NW processed the raw data. NJ analyzed the data with help from SE, DNS, NW, and TD. NJ drafted the manuscript. AM, LR, TD, and HV provided critical revisions. MB and HV supervised the study. All authors approved the final version of the manuscript for submission.

Disclosure statement

The authors report no potential conflict of interest.

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