**Peer Review and Communication History**

**MS Title**: Cyberloafing: Investigating the Importance and Implications of

New and Known Predictors

**Author Names**: Casey Giordano, Brittany K. Mercado

**Submitted:** June 15, 2021

**Editor First Decision**: Revise & Resubmit

Nov 5, 2021

Thank you for your patience on receiving this decision letter, and thank you for your submission to Collabora! Your paper has been reviewed by two reviewers, and I have also read it independently myself.

Overall, your paper has several strengths. I think the study is in general well-conducted and on an important topic. The two reviewers were split in their evaluations of the paper. One reviewer had general positive evaluations with minor suggestions. The other reviewer was more negative and recommended rejection. In my evaluation, I think that the second reviewer’s concerns could be adequately addressed in a revision, so I would like to invite a revision of the paper.

In your revision, please focus on the 3 major concerns raised by Reviewer 2. Most notably, please provide much greater clarity as to how the variables were chosen for the current study and the their relationship to the previous meta-analysis. When strong correlates were excluded, please explain why. Please ensure that the scope of conclusions drawn in the study is appropriate to the scope and range of variables included. In your discussion, please clarify limitations about causal inferences that can be drawn given potential confounding variables that were not included in the study, if appropriate.

To address Reviewer 2’s second major concern, I suggest grouping the sample by occupational types and fitting a mixed effects model to estimate heterogeneity in relationships across groups.

Please also attend to other reviewer comments and requests and provide a point-by-point response letter.

Thank you again for your submission. I look forward to your revision.

Best regards,
Brenton Wiernik

# Reviewer 1

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  | ✔ |  |  |  |

##### Open response questions

### Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

The authors describe a three-sample investigation into the correlates of self-report cyberloafing behavior, with a specific focus on personological and situational variables. The study of cyberloafing behavior is important and I appreciate the focus on person and situation variables, the link to relevant prior work and theory, the open science practices, and the meta-analytic synthesis of the data. Nevertheless, my reading of the manuscript leaves me with three relatively serious concerns that I outline below. I also have some other more minor comments that primarily relate to issues of clarity and that may simply reflect confusion on my part.

Major Points

1. The authors draw on prior empirical work (e.g., the meta-analysis by Mercado et al., 2017) and theoretical work (e.g., the work by Marcus & Schuler, 2004) to identify which variables to examine in their own study. My concern is that the connection between this prior work and the chosen variables appears to be weak. Some of the strongest correlates of cyberloafing identified by Mercado et al. include variables such as organizational norms (ρ= .37), attitudes toward cyberloafing (ρ= .42), ability to hide cyberloafing (ρ= .41), neutralization (ρ= .43), and self-control (ρ= .32), but none of these were included in these studies. Instead, some of the weakest correlates identified by Mercado et al., including organizational justice types (ρ = .00 to ρ = -.12) and job satisfaction (ρ= -.04) were included. This needs to be explained and justified much more carefully. Similarly, the work by Marcus and Schuler would suggest that variables such as the opportunity to engage in cyberloafing or the perceived consequences of cyberloafing, and triggering events (e.g., boredom) and various positively-valenced (e.g., self-control, conscientiousness, integrity) and negatively-valenced (e.g., impulsivity) dispositional variables might be relevant. Again, none of these appear to have been included. If the goal of the paper is to contrast and compare the relative utility of personological and situational variables then surely those variables that had previously been shown to predict cyberloafing should have been included. The opening sentence of the discussion is that “The central goal of this paper was in uniting disparate construct domains to identify the most important constructs for explaining variance in employee cyberloafing”. I don’t think that studies that seem to have excluded the most promising variables can fulfill this goal.
2. On page 12 the authors argue that the “broad sampling of people, jobs, and organizations from employed students and MTurk participants helps maximize the generalizability of our result.” I do not agree that this follows. If mean scores on the predictors and/or the criterion variable vary systematically across jobs (as seems likely given what we know about job differences in cyberloafing relevant variables such as opportunity, company policies, personality traits etc.) then aggregating across the jobs can result in misleading results. That is, correlations can be attenuated in either direction. The authors could speak to this issue by coarsely categorizing the reported jobs in some manner (e.g., white collar versus blue collar) and examining differences in the examined variables across these categories.
3. The supplemental materials (also Table 2) show very different sample sizes across meta-analytic estimates (range N=343 to N=1133). How did this happen? Was there that much missing data or were some scales/inventories only administered to a subset of the samples. Perhaps I missed this in the manuscript but this needs to be discussed explicitly in the methods section.

More Minor Points

1. On page 4, in the section on “Predictor Selection” the authors write that variables should have an “effect” on cyberloafing or CWBs. Presumably this should be “relationship” rather than effect?
2. On page 8 the authors classify organizational justice as a situational stressor. Meta-analytic evidence (e.g., Colquitt et al., 2001) is that justice perceptions correlate very highly with job satisfaction so I am unsure that job satisfaction should be classified as a personological predictor while justice perceptions are considered situational predictors. Both reflect perceptions of the workplace.
3. On page 9 the authors appear to argue that a low workload is likely to result in cyberloafing (the negative correlation reported by Pindek et al.). You may want to clarify this because it seems that the stressor-emotion model would imply a positive correlation (more work ◊ more stress ◊ more cyberloafing).
4. Page 10 – The eligibility criteria suggest that someone who uses technology as a very small part of their job would be eligible to participate in this study. For example, a plumber who uses a tablet device to bill customers or a phone to interact with his dispatcher would qualify but cyberloafing would presumably be very low. Is this a problem, particularly since variables such as the “opportunity to engage in cyberloafing” are unmeasured?
5. Page 13 – Was any incentive offered to perform well on the measure of cognitive ability? Low stakes settings, including lab studies and online studies, can result in low effort that in turn produces scores that reflect motivation more than it does cognitive ability (see Duckworth et al., 2011 PNAS). Without knowing more about the distribution of scores and how outliers (esp. very low scores) were treated I am unsure how to interpret these scores.
6. Can the authors report on the results for all the Big Five individually?
7. I was struck by the decision to measure job satisfaction with only a single-item while other constructs were measured with much greater care (e.g., 20 items for organizational justice). If the goal is to compare and contrast the value offered by different constructs then each should be measured with approximately equal care.
8. Page 15 – Why were artifact distributions used for corrections for unreliability? The editor as an expert in meta-analysis can probably speak to this better but you obviously have local reliability estimates for each sample. Why not use those?
9. On a related note, on page 17 you note the fact that many SDrho estimates were zero. Again, the editor can probably speak to this better but I believe that this is often simply an artifact of a small number of studies and should probably only be interpreted with great caution.
Signed
Marcus Crede

# Reviewer 2

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
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##### Open response questions

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This article builds on previous research on cyberloafing by: using a person-situation framework to compare personological and situational construct domains, expanding the cyberloafing nomological network by including two previously unexplored antecedents, applying a multivariate approach to identifying important cyberloafing predictors. Practical implications for organizations to deal with cyberloafing are discussed. The article was clearly written and easy to follow, with the research questions clearly laid out. I have a few questions, comments, and considerations regarding the manuscript listed below.

Specific comments:
• I would be interested to see the validity ranges reported in this sentence to see what the difference looks like: “Most prominently, personality exhibits drastically weaker validities for predicting cyberloafing (Mercado et al., 2017) than for overall CWB (Berry et al., 2007).”
• The “Situational Constructs” section begins with a paragraph giving an overview of the contextual factors that ends with the clear outline of which situational constructs will be explored: “Therefore, we explore the relative contribution of three commonly studied organizational stressors for predicting cyberloafing: organizational justice, employee workload, and interpersonal conflict.” For clarity, could you add a similar intro paragraph to the beginning of the “Personological Constructs” section? It would also be helpful if the three personological constructs were outlined here.
• The text provides an explanation of why cognitive ability was included as a personological predictor for cyberloafing but the table listing the constructs includes logical reasoning rather than cognitive ability. Could you add a sentence or two in the predictor selection section describing your rationale for including logical reasoning specifically to reflect cognitive ability?

General comments:
• I like the ideas surrounding the narrower focus of cyberloafing behaviors (e.g., gaming versus social media) and investigating how predictors relate to specific outcomes. Great ideas for future research.
• I’m curious what the distribution of responses was for the cyberloafing measure. I would expect that with the student sample in particular (mostly entry-level, part-time employment), the majority of responses would fall on the high end of the scale (daily to several times a day). Even with the MTurk sample, I would expect the majority of responses to fall on the high end due to MTurkers generally reporting greater computer and internet knowledge (Aguinis et al., 2021) and the technologically-mediated component of the cyberloafing definition. Moreover, the MTurkers inherently have a reason to be checking non-work email, sending non-work related messages, and browsing non-work related websites to keep up with MTurk. Consider reporting what the distribution looked like and addressing any associated limitations if the distribution is skewed.
• Based on the supplemental materials, it looks like the items for interpersonal conflict do not specify that the conflict is with coworkers. I would be interested to see if the important of interpersonal conflict predicting cyberloafing depends on whether the conflict is stemming from coworkers or customers. I would expect those who experience coworker conflicts would be more likely to withdraw through cyberloafing while those with customer conflict may emotionally regulate by commiserating with coworkers about the conflict. Moreover, those in customer facing positions may not have the same opportunity to engage in cyberloafing during work. That is, if much of your work time is spent dealing with customers directly, you may have fewer opportunities cyberloaf. Consider addressing the discrepency between the items and the interpersonal conflict definition “generally refers to the frequency and severity of experienced harassment **from colleagues**” and any implications this may have.
• A consideration for future directions for research could be investigating the impact of remote versus in-person work on cyberloafing. Specifically, considering the increase in remote work with the onset of the COVID-19 pandemic, has there been an increase in cyberloafing? It could be that as there are more distractions with remote work (e.g., kids, pets, household tasks), there is less cyberloafing because people need to focus on work when they are on their laptop. If data were collected during the pandemic, consider adding any implications this may have to the discussion.

**Author Response**
Apr 6, 2022

**Editor:**

In your revision, please focus on the 3 major concerns raised by Reviewer 1. Most notably, please provide much greater clarity as to how the variables were chosen for the current study and their relationship to the previous meta-analysis. When strong correlates were excluded, please explain why. Please ensure that the scope of conclusions drawn in the study is appropriate to the scope and range of variables included. In your discussion, please clarify limitations about causal inferences that can be drawn given potential confounding variables that were not included in the study, if appropriate.

To address Reviewer 1’s second major concern, I suggest grouping the sample by occupational types and fitting a mixed effects model to estimate heterogeneity in relationships across groups.

* + We thank you for the opportunity to revise the paper for resubmission to *Collabra*. We feel that the paper is sincerely strengthened after incorporating the insightful comments from the editor and peer reviewers. In the remainder of this document, we address the concerns the editor and two reviewers. All editor and reviewer comments are numbered and our responses are provided, in bullet point form, immediately below each numbered comment. When possible, we quoted exact changes and provided page numbers for changes in our manuscript.
	+ Addressing an editorial comment, we have attempted to replace causal language (e.g., predictor, strong driver) with non-causal, correlative language (e.g., correlate, association). Furthermore, we now expressly caution against causal interpretations in our discussion, which states that “our results do not shed light on the underlying causal mechanisms” (p. 23). We agree that causality cannot be inferred from our manuscript, and we appreciate this advice to reduce the potential for misconstruing our paper as causal in nature.

Response to reviewers

**Reviewer #1**

The authors describe a three-sample investigation into the correlates of self-report cyberloafing behavior, with a specific focus on personological and situational variables. The study of cyberloafing behavior is important and I appreciate the focus on person and situation variables, the link to relevant prior work and theory, the open science practices, and the meta-analytic synthesis of the data. Nevertheless, my reading of the manuscript leaves me with three relatively serious concerns that I outline below. I also have some other more minor comments that primarily relate to issues of clarity and that may simply reflect confusion on my part.

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	* Whereas our study has similarities with prior empirical work (e.g., Mercado et al. and Marcus & Schuler), the present work is distinct in that our emphasis was on actionable variables. This preference for actionable variables was balanced by parsimony. This was seen in both personality and attitudinal domains. For instance, although all personality variables are actionable (e.g., personnel selection), it is impractical to investigate or base an intervention on all personality conceptualizations. Therefore, we chose the Big Five variables because of (a) a bandwidth-fidelity matching between a higher-order factor and the comparably broad range of potential cyberloafing behaviors and (b) higher *k* and lower SDρ values than self-control, for which the Mercado et al. meta-analysis reported a low *k* and a massive SDρ value = (SDρ = .34; 80% CV = -.76, .12). The Big Five were not included as separate entities due to parsimony. Using Stability instead of the three individual variables requires 128 analyses as opposed to 512, and personality generally falls under the same action [personnel selection], so combining them as one entity was attractive from computational, interventionist, and bandwidth-fidelity perspectives. Similarly, parsimony of a domain influenced our selection of attitudinal variables. For example, the Mercado et al meta-analysis found a sizable point estimate for attitudinal engagement (ρ = -.18), yet we opted to include job satisfaction. Although the point estimate for engagement is larger in magnitude than satisfaction, their credibility intervals heavily overlap; there is substantially more interest in examining job satisfaction and cyberloafing (e.g., *k* = 14 and 7 for satisfaction and engagement, respectively); and the “attitude general factor” (e.g., Newman, Joseph, & Hulin, 2010) is sufficiently strong to warrant inclusion of one major attitude.
	* Intentionality (e.g., intention to cyberloaf, ρ = .61) and norm-based variables (e.g., cyberloafing norms, ρ = .37) are questionably “actionable,” because they do not suggest the use of a specific intervention to reduce them. Consider descriptive norms, which reflect one’s understanding of typical behaviors for a referent group; unless the individual is extremely inaccurately observing others, an intervention aimed at reducing those norms would be an intervention aimed at reducing cyberloafing more generally. In contrast, both justice and satisfaction are frequently the center of specific and well-established forms of intervention research (e.g., job crafting to improve one’s satisfaction with their work tasks). Similar concerns of questionably actionable interventions apply to neutralization (metaphor of the ledger); although a potent antecedent, it is not directly actionable. A loose definition for the construct is that people justify their misdeeds by rationalizing their behaviors as being acceptable under some circumstances. For example, “I cyberloafed because my assigned tasks are unreasonable” and “I cyberloafed because I am underpaid” are both instances of neutralization but with distinct antecedents (e.g., workload, pay satisfaction) and therefore distinct practical implications. Therefore, rather than focus on the less actionable outcome of neutralization, we focused on the key driver (e.g., specific stressors, such as workload and justice perceptions).
	* We agree that opportunity to cyberloaf is a necessary consideration in this study; although it does not explain why one might cyberloaf, it facilitates the behavior. For that reason, we attempted to exclude individuals who did not have the opportunity (see our technology screening questions). However, we also recognize that—even among those with access to technology—the opportunity to cyberloaf may differ, and we attempted to include key contextual factors driving such differences that could be directly addressed by job design (e.g., workload). However, some factors that influence opportunity to cyberloaf are less amenable to managerial intervention and therefore were not a focus of our investigation. For example, one’s ability to hide cyberloafing is very difficult to influence. Previous research has demonstrated that simple interventions aimed at reducing ability to hide are ineffective (e.g., electronic monitoring, Mercado et al., ρ = -.06). Without those straightforward interventions, reducing the ability to hide cyberloafing would require meaningful changes in the work itself for many employees (e.g., making digital work analog, more direct supervision) that are inefficient and unlikely to be accepted in a post-pandemic workplace.
	* This review highlighted the importance of clearly explaining not only why we *included* variables but also why some relevant variables were *excluded* (see p. 5). Therefore, we have expanded the explanation of our exclusion criteria in the manuscript, which now includes a summary of the reasons stated above. We also must note the different goal and methods of the present study compared to Mercado et al.’s exhaustive systematic review and Marcus and Schuler’s application of a criminology theory to workplace settings. In both of their cases, over-inclusion of variables was warranted. In contrast, applying this methodological approach to the 39 covariates that Mercado included (which would notably exclude two of our three most important variables) would require almost 550 *billion* analyses (i.e., 2^39). Some concessions were warranted for practical reasons, but we made those concessions based on the criterion of variables with the potential for interventions.
	* Our discussion section now begins with the following revised statement to illustrate the qualifiers to our study: “The central goal of this paper was in uniting disparate construct domains to identify the most important, proximal constructs—emphasizing those with actionable and practical relevance—for explaining variance in employee cyberloafing” (p. 22).
2. On page 12 the authors argue that the “broad sampling of people, jobs, and organizations from employed students and MTurk participants helps maximize the generalizability of our result.” I do not agree that this follows. If mean scores on the predictors and/or the criterion variable vary systematically across jobs (as seems likely given what we know about job differences in cyberloafing relevant variables such as opportunity, company policies, personality traits etc.) then aggregating across the jobs can result in misleading results. That is, correlations can be attenuated in either direction. The authors could speak to this issue by coarsely categorizing the reported jobs in some manner (e.g., white collar versus blue collar) and examining differences in the examined variables across these categories.
	* Thanks for raising this particularly interesting insight. To properly address the core issue raised here (i.e., whether occupational differences obfuscate “true” differences in relationships), we applied an additional set of analyses that are included in the new online supplement. To conduct these analyses, we used the well-established, five-level O\*NET categorization schema to examine our variables across occupational levels. These levels roughly correspond to job complexity and are arranged by the amount of “preparation” needed for each job (1 = lowest, 5 = highest). In our new supplemental materials, we include (a) a table breakdown of each variable, separated by each job category and (b) a series of mean plots with the 80% confidence interval (CI) wherein each plot depicts the mean & CI for each job category for a specific variable—there is one plot per variable. Cyberloafing appears to vary across job categories, with the largest raw mean difference across groups less than 0.50; in contrast, within all job groups, the standard deviation exceeds 1.50. There are clearly much larger differences *within* each job group than there are *between* job groups, suggesting that intergroup differences are not sufficiently pronounced to undermine these findings. The remaining variables are generally flat in their trends across job groups with the unsurprising exception of personality (mostly due to conscientiousness and emotional stability) due to range restriction. Boredom is the other notable exception, which appears to decrease as job group increases. Justice varies somewhat by job group; however, our results demonstrate that it is largely an irrelevant variable. Conflict at work is relatively consistent but notably peaks (with large variability) in the first job group. Again, these tables and plots can be seen in our new version of the supplemental materials.
	* Although substantial intrasample variance on these variables demonstrates the contribution of present findings, we agree that investigations within narrowly defined occupational contexts will yield additional insights. This is highlighted in our discussion (e.g., see p. 27). From our breakdown analyses of each variable across job groups, future research directions emerged that can directly examine which aspects of a specific job group influence variability in each variable. For example, *why* do job groups #2 and #4 differ on cyberloafing? Do certain mechanisms and pathways differ across job groups (e.g., bored employees in lower job groups coping differently than those in higher job groups)? Such questions could not be satisfactorily addressed by our data but future research in this domain can further explore cyberloafing mechanisms and its nomological network across occupational groups.
3. The supplemental materials (also Table 2) show very different sample sizes across meta-analytic estimates (range N=343 to N=1133). How did this happen? Was there that much missing data or were some scales/inventories only administered to a subset of the samples. Perhaps I missed this in the manuscript but this needs to be discussed explicitly in the methods section.
	* Thank you for raising this concern. In a previous version of this manuscript, we had verbiage stating that not all samples received the same battery of assessments. The mechanism explaining the discrepancies in sample sizes is due to the differences in administration rather than non-random missing data. We have reintroduced the following language to clarify that the discrepancies in sample size are not due to worrying levels of missing data: “Note that in our data collection efforts, not all samples were provided every test battery, such that discrepancies in the sample size for each variable interrelation are predominantly due to survey administration rather than incomplete participant responses” (p. 12).

More Minor Points

1. On page 4, in the section on “Predictor Selection” the authors write that variables should have an “effect” on cyberloafing or CWBs. Presumably this should be “relationship” rather than effect?
	* Yes, we agree that relationship is a more appropriate term—good catch. We have changed this language as well as similar instances of this language in our paper to be less causal in nature.
2. On page 8 the authors classify organizational justice as a situational stressor. Meta-analytic evidence (e.g., Colquitt et al., 2001) is that justice perceptions correlate very highly with job satisfaction so I am unsure that job satisfaction should be classified as a personological predictor while justice perceptions are considered situational predictors. Both reflect perceptions of the workplace.
	* Justice is indeed a complex variable to classify. On the one hand, as you state, perceptions of justice cannot be separated from a person’s unique vantage point. On the other hand, perceptions of justice are also a function of context. Consider Greenberg’s (1990) successful attempt to manipulate justice in a quasi-experiment. Inadequately justified pay cuts were met with disproportionately larger theft rates than those who received proper justification. Regardless of individual differences, the justice of the situation was manipulated.
	* Moreover, a sizable part of the evaluative (i.e., personological) component to justice is heavily related to personality, notably emotional stability (neuroticism). For example, in Colquitt et al.’s follow-up meta-analysis (2013), justice is highly related to negative affect (neuroticism, ρ ~ -.32). By measuring neuroticism alongside conscientiousness and agreeableness, a large portion of the person-focused evaluative component of justice is included in our models. The remaining variance in the justice composite should then be more objective and less related to the evaluative side. Therefore, justice might make more sense as a situational variable, if only by a small margin.
3. On page 9 the authors appear to argue that a low workload is likely to result in cyberloafing (the negative correlation reported by Pindek et al.). You may want to clarify this because it seems that the stressor-emotion model would imply a positive correlation (more work ◊ more stress ◊ more cyberloafing).
	* In practice, the stressor-emotion model generally relates to negative stressors that produce negative emotions (e.g., anger, frustration), therein yielding negative behaviors. This would result in a positive relationship between a stressor and CWB. However, in their paper introducing the stressor-emotion model, the authors are concerned with any emotional reaction as it pertains to CWBs. This is demonstrated in the authors’ description of the model pertaining to “emotional reactions to [stressors] (usually anger)” (Spector & Fox, 2005, p. 156). When measuring general CWB, anger is a core emotional antecedent. However, Pindek et al established that boredom, as an emotional reaction to understimulation, is more fundamental to cyberloafing. Furthermore, the authors of the stressor-emotion model have elsewhere discussed low workload (a stressor in the technical sense) as a core driver of understimulation that in turn produces CWBs (Spector & Fox, 2010, *Applied Psychology*). From these pieces of information, it is expected that lower workload is associated with higher cyberloafing. Recognizing that this logical sequence was not sufficiently detailed in our manuscript, we have updated our language to better clarify this logic. See page 9 for our revisions.
4. Page 10 – The eligibility criteria suggest that someone who uses technology as a very small part of their job would be eligible to participate in this study. For example, a plumber who uses a tablet device to bill customers or a phone to interact with his dispatcher would qualify but cyberloafing would presumably be very low. Is this a problem, particularly since variables such as the “opportunity to engage in cyberloafing” are unmeasured?
	* The reviewer raises an interesting point. In Lim’s original definition of cyberloafing, it required using company-supplied technology (e.g., company internet, company device). However, we and other contemporary cyberloafing researchers define the construct as any technologically-mediated loafing to specifically address this issue. Having our own internet-connected devices greatly increases the opportunity to engage across many varied situations. There appears to be little to no research on cyberloafing in more traditionally “blue collar” jobs to verify whether cyberloafing is indeed “very low”. However, a study of nurses in an emergency department (Black et al. 2013, *Journal of Medical Internet Research*), a physically demanding job with less significant technology use, found that, on average, 12 minutes per hour (20%) were spent on Facebook alone. This is a very specific form of cyberloafing that was measured conservatively (e.g., usage on the nursing station computer, ignoring all other devices, including personal), suggesting even higher rates of overall cyberloafing. Oddly, Facebook use peaked during the ER’s busiest times, suggesting that even in physically demanding (i.e., less technologically dependent), customer-facing (patient-facing), and time-sensitive jobs, cyberloafing can still be highly problematic. One would imagine that higher influxes of ER patients would decrease the opportunity to engage in cyberloafing, yet the above study demonstrated increased engagement. Therefore, we do not feel as though our manuscript necessarily suffers by not measuring opportunity to engage as a variable.
	* This point is also somewhat related to this reviewer’s previous point about occupations. Here, some occupations might have more opportunity to engage than others (e.g., plumber). We are not aware of an occupational categorization schema that breaks jobs down by their technology access/usage. Otherwise, we would have more formally assessed this occupational opportunity idea during data collection. Nevertheless, reiterating our response to an earlier comment, the difference between the job category with the lowest cyberloafing and the job category with the highest cyberloafing is not appreciable.
	* The specific example of plumbers, an industry in which many tradespeople are self-employed, introduces other concerns regarding appropriateness of the cyberloafing construct. We identify many important distinctions between self-employed individuals and employees of organizations, including their discretion over their own time use and the personal consequences of their slacking. Therefore, we required that all participants work for an organization that they did not own.
5. Page 13 – Was any incentive offered to perform well on the measure of cognitive ability? Low stakes settings, including lab studies and online studies, can result in low effort that in turn produces scores that reflect motivation more than it does cognitive ability (see Duckworth et al., 2011 PNAS). Without knowing more about the distribution of scores and how outliers (esp. very low scores) were treated I am unsure how to interpret these scores.
	* Thank you for raising this potential issue to our attention. We closely reviewed responses to the cognitive ability measure and found a very Gaussian distribution of test scores from each sample—if anything, there is a fatter tail at the positive end of the distribution. There were a few individuals that completed the assessment suspiciously fast, indicating that they were not properly motivated to complete the assessment to the best of their ability. However, this number of respondents was a small minority of participants (ranging from a couple to less than a couple dozen, depending on where one places the cutoff). These few outliers only serve to attenuate the relationship between each variable and intelligence. Discarding them would, if anything, trivially increase the effect size obtained for one of our most dominant variables. That potential outcome, the very few cases of poor responses, and the subjective manner of determining which cases to disregard, led us to continue with our conservative estimate of effects.
6. Can the authors report on the results for all the Big Five individually?
	* We were unsure if the reviewer was requesting the meta-analytic correlations of each of the Big Five with cyberloafing or the inclusion of the Big Five dimensions individually in the dominance analysis. Therefore, we will respond to each. First, we have added a new table in the supplemental materials that includes the meta-analytic relationships between cyberloafing and each of the Big Five. Due to our goals in predictor selection of investigating actionable predictors and maintaining parsimony, we maintain that the most appropriate method of including personality in the dominance analysis was to use the composite of conscientiousness, agreeableness, and emotional stability described in the original manuscript. This approach holds the greatest potential to guide practice, because personality variables are typically assessed together and involved in the same intervention.
7. I was struck by the decision to measure job satisfaction with only a single-item while other constructs were measured with much greater care (e.g., 20 items for organizational justice). If the goal is to compare and contrast the value offered by different constructs then each should be measured with approximately equal care.
	* We appreciate you raising this concern. Here, the phrase “with approximately equal care” can take several meanings. If we consider “approximately equal care” to be operationalized by reliability (i.e., an approximately equal proportion of true-score to error-score variance), then we feel confident that satisfaction can be appreciably measured via a single item. According to Wanous, Reichers, & Hudy’s (1997) meta-analysis, single-item measures of satisfaction correlate about .63 with job satisfaction batteries. As a comparison, the average correlation between two personality *scales* is around .50. In other words, a single item satisfaction measure is closer to a larger job satisfaction test battery than two personality tests are with each other. As internal consistency reliability relates to both the number of items and the average correlation between items, we cannot simply discuss the number of items without also considering the average intercorrelation among the items. Therefore, comparing the number of test items for each construct, on its own, would be an unfair comparison. Wanous et al and others have concluded that from a reliability standpoint, a single item measure of job satisfaction is sufficient. Moreover, considering we correct for unreliability using our sample-based estimates, differential reliabilities of the construct measures in our study are psychometrically controlled for.
	* If the statement “approximately equal care” is meant to convey detail (i.e., content validity), it is important to consider the difference in the meaning of job satisfaction inventories. Detailed inventories (e.g., Minnesota Satisfaction Questionnaire) better address employees’ feelings towards specific facets of the job whereas wholistic, single-item measures better capture a general sense of (dis)satisfaction (Scarpello & Campbell, 1983). This again relates to a bandwidth-fidelity argument wherein a general appraisal is more appropriately compared to a more general class of cyberloafing behaviors. Motivated by the premise that feelings towards any element of the job might be leading to cyberloafing, we deemed a general inventory most appropriate.
	* Moreover, single-item satisfaction measures are deemed useful when one or both of the following conditions are met (cf Wanous et al., 1997): (a) the main objective is to examine overall satisfaction as opposed to its elements (e.g., satisfaction with pay vs. coworkers vs. the work itself) and/or (b) when practical constraints limit lengthy questionnaires, such as when a large battery of assessments are provided. Both of those conditions are met in our analysis.
8. Page 15 – Why were artifact distributions used for corrections for unreliability? The editor as an expert in meta-analysis can probably speak to this better but you obviously have local reliability estimates for each sample. Why not use those?
	* Artifact distributions were used because they are *slightly* better. Reliability values are a function of the sample (not the test) and are therefore subject to sampling error as well. Much like we aggregate correlation values to produce a more precise estimate of the “true” correlation, we herein aggregated reliability values to produce a more precise estimate of the “true” reliability. This is useful because an under (over)-estimated reliability value will over (under)-correct the observed correlation. With a more accurate estimate of the reliability value, we theoretically obtain a more accurate correction factor. However, the practical difference between an artifact distribution and individual corrections is typically of negligible size.
9. On a related note, on page 17 you note the fact that many SDrho estimates were zero. Again, the editor can probably speak to this better but I believe that this is often simply an artifact of a small number of studies and should probably only be interpreted with great caution.
	* This is an excellent point, and one we certainly agree. Just as including more effect sizes produces a better estimate of the “true” mean, including more effect sizes produces a better estimate of the “true” standard deviation. Particularly considering the standard deviation is a second moment of a distribution. We do not pretend that our SDρ estimates are comparable in precision to an SDρ value obtained by a true quantitative review (we merely use meta-analysis as a statistical technique to combine samples). Therefore, as a technique to aggregate samples, SDρ was only discussed as an imperfect metric to gauge whether there were discrepancies between samples. If, for instance, a student and worker sample produced a large SDρ, that would be a signal that combining these two samples may have been inappropriate. A median SDρ of zero gives *some* analytic support that combining samples did not muddle effects.
	* We have added a short caveat to the part of our results section describing these SDρ values to guard against an overinterpretation of these unstable and imprecise measures of variability. See footnote 4 on page 19 that clarifies the inherent instability/inaccuracy of our SDrho values and the care in which they should be interpreted.

Signed
Marcus Crede

**Reviewer #2**

This article builds on previous research on cyberloafing by: using a person-situation framework to compare personological and situational construct domains, expanding the cyberloafing nomological network by including two previously unexplored antecedents, applying a multivariate approach to identifying important cyberloafing predictors. Practical implications for organizations to deal with cyberloafing are discussed. The article was clearly written and easy to follow, with the research questions clearly laid out. I have a few questions, comments, and considerations regarding the manuscript listed below.

Specific comments:

1. I would be interested to see the validity ranges reported in this sentence to see what the difference looks like: “Most prominently, personality exhibits drastically weaker validities for predicting cyberloafing (Mercado et al., 2017) than for overall CWB (Berry et al., 2007).”
	* Thank you for highlighting this opportunity to integrate additional evidence into the manuscript. We have revised this sentence to state the following: “Most prominently, the Big Five personality dimensions exhibit drastically weaker validities for predicting cyberloafing (ρs ranged from .00 to -.16; Mercado et al., 2017) than for overall CWB (ρs ranged from .02 to -.46; Berry et al., 2007)” (p. 4).
	* Both Mercado et al and Berry et al report relationships for each of the Big Five. In our manuscript, we rely on conscientiousness, agreeableness, and emotional stability so the following composites relate only to those three variables (the other two personality traits are relatively weakly related to CWB and cyberloafing). Regarding cyberloafing, the meta-analytic composite correlation with Stability is ρ = -.1833. Regarding counterproductivity writ large, the meta-analytic composite correlation with Stability is ρ = .5090. The intercorrelations of the personality variables used to compute those composites were drawn from Park et al. (2020) in *JAP*.
2. The “Situational Constructs” section begins with a paragraph giving an overview of the contextual factors that ends with the clear outline of which situational constructs will be explored: “Therefore, we explore the relative contribution of three commonly studied organizational stressors for predicting cyberloafing: organizational justice, employee workload, and interpersonal conflict.” For clarity, could you add a similar intro paragraph to the beginning of the “Personological Constructs” section? It would also be helpful if the three personological constructs were outlined here.
	* Thank you for this suggestion. We have added some language to this section to better introduce the personological section as well as improve the parallelization across sections. See the heading Personological Constructs on pages 5-6.
3. The text provides an explanation of why cognitive ability was included as a personological predictor for cyberloafing but the table listing the constructs includes logical reasoning rather than cognitive ability. Could you add a sentence or two in the predictor selection section describing your rationale for including logical reasoning specifically to reflect cognitive ability?
	* Thank you for the suggestion. We have added some text in the manuscript about why logical reasoning is relevant to the discussion of cognitive ability.
	* “Several lines of evidence suggest that logical reasoning is a useful proxy for cognitive ability. In their taxonometric work, Stanek and Ones (2018) and McGrew (e.g., McGrew, 2009) report that general sequential reasoning is central to fluid ability. Kyllonen and Christal (1990) among others have demonstrated that nonsense syllogisms are core to fluid ability, which is itself isomorphic with general cognitive ability (Gustafsson, 1988). Although nonsense syllogisms are not a perfect proxy for cognitive ability, this assessment serves as a good and well-established representation of the broader domain.” (p. 15)

General comments:

1. I like the ideas surrounding the narrower focus of cyberloafing behaviors (e.g., gaming versus social media) and investigating how predictors relate to specific outcomes. Great ideas for future research.
	* Thank you for this comment.
2. I’m curious what the distribution of responses was for the cyberloafing measure. I would expect that with the student sample in particular (mostly entry-level, part-time employment), the majority of responses would fall on the high end of the scale (daily to several times a day). Even with the MTurk sample, I would expect the majority of responses to fall on the high end due to MTurkers generally reporting greater computer and internet knowledge (Aguinis et al., 2021) and the technologically-mediated component of the cyberloafing definition. Moreover, the MTurkers inherently have a reason to be checking non-work email, sending non-work related messages, and browsing non-work related websites to keep up with MTurk. Consider reporting what the distribution looked like and addressing any associated limitations if the distribution is skewed.
	* This is an excellent comment. In response to the reviewer above, we have added additional descriptive statistics about the distributions of cyberloafing (overall and in specific job groups). From parsing the data, cyberloafing appears to be normally distributed with little skew and no ceiling effects. This is consistent with research more broadly in cyberloafing. Alternatively, most manifestations of counterproductive work behaviors are indeed skewed and often very skewed, reflecting *low* base rates. Most people engage in few to no misbehaviors and some engage in the lion’s share of CWBs. Therefore, in cyberloafing, we would not expect to see effect size attenuation due to deviations from normality like we would with the broader counterproductivity domain.
	* Somewhat related to examining cyberloafing across job groups above, we do not suspect that organizational level (e.g., entry-level vs. other workers) operates as a moderator in our analyses. Evidence from Mercado et al about the lack of a prominent correlation with organizational level provides further support that cyberloafing occurs at all levels with comparable frequencies.
3. Based on the supplemental materials, it looks like the items for interpersonal conflict do not specify that the conflict is with coworkers. I would be interested to see if the important of interpersonal conflict predicting cyberloafing depends on whether the conflict is stemming from coworkers or customers. I would expect those who experience coworker conflicts would be more likely to withdraw through cyberloafing while those with customer conflict may emotionally regulate by commiserating with coworkers about the conflict. Moreover, those in customer facing positions may not have the same opportunity to engage in cyberloafing during work. That is, if much of your work time is spent dealing with customers directly, you may have fewer opportunities cyberloaf. Consider addressing the discrepency between the items and the interpersonal conflict definition “generally refers to the frequency and severity of experienced harassment **from colleagues**” and any implications this may have.
* Thank you for this comment, it helped us think through potential effects of different types of experienced conflict. Our definition was drawn from Spector’s introduction to his scale, but we revised our definition to better align with our operationalization (see page 10). Part of what is addressed in this reviewer’s comment is the potential issue of “opportunity to engage,” which we have partially addressed above. Nevertheless, it would be an interesting follow-up study to examine potential aspects of work that correspond to differences in cyberloafing. For example, maybethe small mean difference in cyberloafing across job groups 1 & 2 compared to group 4 (see our additions to the online supplement and our response to reviewer #1 above) is due to customer-facing versus isolated knowledge worker differences in the job context. However, lower job groups also include many isolated manual labor jobs (e.g., Amazon warehouse workers), and higher job groups can also include customer-facing jobs (e.g., an account manager or an investment advisor). It would be an interesting and worthwhile future direction to explore what job-, organizational-, industry-, or context-specific aspects of work relate to manifestations of cyberloafing.
1. A consideration for future directions for research could be investigating the impact of remote versus in-person work on cyberloafing. Specifically, considering the increase in remote work with the onset of the COVID-19 pandemic, has there been an increase in cyberloafing? It could be that as there are more distractions with remote work (e.g., kids, pets, household tasks), there is less cyberloafing because people need to focus on work when they are on their laptop. If data were collected during the pandemic, consider adding any implications this may have to the discussion.
	* Unfortunately, data were collected prior to the pandemic. Cyberloafing viz-a-viz COVID lockdowns and remote work would be quite a different discussion than our primary goals in this manuscript. That being said, remote work facilitates discussions of situational strength whereby lower monitoring of employees allows employees to better express their general tendencies (e.g., some with less conscientiousness may cyberloaf more but high integrity employees may cyberloaf less). It would also open the door to other forms of coping mechanisms in lieu of cyberloafing (e.g., taking long breaks to exercise or nap rather than hiding their loafing behind a computer).

**Editor Final Decision:** Accept

Sep 14, 2022

Dear Dr. Giordano and colleagues,

I am writing regarding your submission to Collabora. Please let me apologize for the long delay in submitting this decision. After reviewing your resubmission, I believe that you have greatly improved the clarity of your paper and justification for your decisions. I believe that the revisions have addressed my and the reviewers’ major concerns, so I am happy to accept the paper for publication.

Thank you for your submission. Please consider Collabora again for your future work.

Best,
Brenton