



**Open Peer-Review for
“Personality Predictors of Emergency Department Post-Discharge Outcomes”
(Atherton et al.)**

Note: 3 reviewers were invited in Round 1, of which 2 accepted and provided reviews. In that round, 2 reviewers wished to have their reviews openly published but not with their names attached. The authors of the article agreed to have any available pre-publication peer-reviews published. There was only one round of reviews.

Reviewer	Round 1	
	Name	Open Review
A	-	-
B	Anonymous #1	Available
C	Anonymous #2	Available

Anonymous #1:

The current study investigated the associations between personality traits (and their individual items) and several emergency department post-discharge outcomes. The authors did not find significant results for the broader Big Five traits, however, investigation of the specific items revealed several associations with the three outcomes of interest (filling prescriptions, following up with primary care physician, and making an unscheduled return to the emergency department).

This study is novel in its approach, collecting personality data in an emergency hospital setting, and explores a novel application of personality and health research. The longitudinal design is a commendable aspect of the present study. While I agree with the authors, this work is encouraging for the study of personality in emergency medicine settings, there are some methodological and theoretical concerns that should be considered and appropriately addressed.

First, I would encourage the authors to address in the introduction the filtering effect of personality and dispositional tendencies prior to emergency room admittance. The authors note previous research that indicates, for example, that extraversion predicts both likelihood of being hospitalized (Hajek et al., 2017) and chance of returning to the emergency room. Personality indicators of ED admission should be discussed in terms of how they then implicate the interpretation of the present outcomes. Relatedly, the influence of demographic characteristics of the sample related to where the data collection took place, should be addressed in the introduction as a relevant factor impacting the current findings. Central to this comment, is the acknowledgement for the role of socio-economic status in increasing the likelihood of emergency department admittance (e.g., Stern, Weissman, Epstein, 1991) and how it relates to the three outcomes. The authors address some of these factors in the discussion (page 18), however, discussing the interactions of these predispositions in the interpretation of the current study seems would bolster the information value that can be taken from this work. In some ways, the null findings for global traits are not terribly surprising, given the multifarious predictors of seeking out ED care, which include a variety of demographic factors, perceived access, symptom recognition and sensitivity, pain-seeking behaviors, loneliness/social isolation, etc. What likely results is a complex confluence of traits and such factors (e.g., neuroticism X pain sensitivity X isolation). Moreover, there could be some range restriction of global traits for ED populations that results in the attenuation of effects for ED outcomes. Although a number of these possibilities cannot be directly addressed, the authors should acknowledge some of these patterns/possibilities in the interpretations of the findings.

Relatedly, one broad concern with this work is the overarching exploratory nature of all the analyses. I appreciate the authors acknowledging this in the manuscript, but it raises concerns about the decisions post-hoc to assess the individual personality items after the broader Big Five traits did not meet significance requirements. Any justification that could be made to include individual items should be clearly presented in the introduction.

Minor Comments:

Considering the journal to which this manuscript was submitted, I would suggest the authors consider the necessity of defining the personality traits broadly and instead focus more directly on the evidence for associations among the traits in hospital/ED contexts.

On page 10, the authors indicate that there was precedence for including additional items for Conscientiousness (“Conscientiousness had the most theoretical relevance for predicting post-discharge outcomes”). This further emphasizes the need to explain how this trait specifically has been shown to be associated with emergency department discharge outcomes earlier on in the manuscript.

Regarding the methods, I am concerned that the dichotomous nature of the discharge outcomes limits the opportunity for finding significant results. The lack of variance may be one reason that the authors did not find associations and the potential of this should be addressed.

While this limitation is acknowledged in the discussion, the relatively large percentage of the sample who were not recommended to follow up with their physician and who were not given prescriptions by the emergency department is a significant concern for sample size. Considering this in correspondence with the variety of types and severity of the emergency visits, presents a challenge in parsing out personality traits’ unique influence on the outcomes. The context of the visit is vital in understanding patients’ perceptions of discharge instructions, their intentions, and subsequent behavior. The situation and probability of reoccurrence, for example, in patients with urinary infections/kidney stones (versus someone seeking medical attention after a motor accident) likely will influence perceptions of follow-up behavior (likely above and beyond personality traits alone).

When the authors discuss the results of the individual item associations with the outcomes, there were several contradictory findings that lack explanation. For example, the results for being “moody” and “broad-minded” and their associations with filling prescriptions are not well explained. Similarly, the authors describe that being “more nervous” (item taken from the Neuroticism scale) was associated with being more likely to follow-up with primary care physician, but in the methods section, it is described that more neurotic individuals were less likely to participate in the follow-up wave of assessment. The authors do discuss how these item-level analyses revealed opposite effects within personality domains but there is a general lack of theoretical explanation for why these more specific tendencies would drive behaviors in opposite directions if they are presumably contributing to the same personality domain.

Conclusion:

Overall, this manuscript provides insight to the study of personality in a context where it not often assessed. I would encourage the authors to consider taking a different angle in discussing and interpreting the study and its implications, focusing less on the individual item-outcome results, and instead emphasizing how this study can inform future work in similar settings. It is evident that this work demonstrates the feasibility of collecting personality data in an ED setting. However, it importantly illuminates the challenges of research in this context. I would recommend that the authors consider major revisions to provide more theoretical support for the study of these traits in an ED context and to clarify what specific considerations are necessary in similar research.

Anonymous #2:

The present manuscript reports the results of a study of patients discharged from an emergency department (ED) to examine whether personality traits are associated with post-discharge outcomes, specifically follow-up with a physician, filling a prescription, and readmittance to the ED, over 1-2 weeks after initial discharge. There were no associations between the broad traits and the three outcomes, but there were some item-level associations. Overall, I greatly appreciate the focus of this research and the effort to recruit and collect data on patients discharged from the ED. Such data collection is difficult and quite commendable. I do, however, have some concerns about the potential contribution of this work.

First, it seems that important information was not collected or at least not included as a covariate. In particular, it would seem really important to know whether the patients had insurance or not. Whether someone has insurance would likely take up a large chunk of the variance as to whether someone made a follow-up appointment with their physician or filled a prescription. This issue is important because the ED can be a primary source of medical care for patients without insurance. The authors mentioned that the ED where they collected data served low-income populations, as well as more affluent populations. There is thus likely to be a mix of participants with and without insurance.

Second, the analyses are underpowered. For example, the analysis of filling a prescription is based on about 91 participants (160 participants who completed the follow-up questionnaire x 57% who were given a prescription and thus could be included in the analysis). Again, I really appreciate the lengths needed to collect these data and expect the sample size to be more modest than research on the general population. Yet, regardless of the difficulty of data collection, analyses still need to be adequately powered to be interpretable.

Third, there is no justification for why some variables were included as covariates (gender, age, income) but not others (education, race/ethnicity, reason for ED visit, chronic conditions, etc.). It is not necessary to throw everything in as a covariate, but more care and rationale should be given for why certain variables were chosen over others.

Fourth, the authors bring up the potential issue of selection bias for conscientiousness in the Discussion. One way to evaluate selection bias is to examine the standard deviation of the trait – If there is a selection toward individuals lower in conscientiousness using the ED, there should be less variability in the trait. And, indeed, the standard deviation is smaller for conscientiousness compared to the other traits. Is this due to selection bias or does the SD tend to be lower for conscientiousness compared to the other traits? Although conscientiousness did have the smallest SD, it did have a fairly high mean – 3.24 on a scale from 1 to 4. This relatively high mean seems to speak against the selection bias argument.

Fifth, there is another selection bias issue. From the description of sample recruitment, potential participants were first filtered through ED staff (i.e., “nurses on staff provided guidance to the interviewers about which patients would be best to approach” [p. 8]). There is certainly utility in this approach, but it is also likely to select for patients with a more positive personality profile (e.g., more agreeable) and better prognosis (e.g., patients who face greater recovery/obstacles to recovery may be less likely to be judged as potential participants because of the burdens they already face at discharge without adding participating in a research study), among others. This potential source of bias also needs to be addressed.

Sixth, I have concerns about the item-level analyses, particularly that the results do not make that much sense and are primarily reported (again) but not interpreted in the Discussion. One reason that we aggregate items into a scale is to create more reliable measurements and reduce random fluctuations. How confident are the authors that the item-level findings are reliable? On the other hand, there is growing interest in the “nuances” or personality. To that end, to what extent are the item-level

analyses similar or different to the work of René Möttus and colleagues (both in terms of how the data were analyzed and in any patterns in health-related associations)?

Some minor issues:

- It would be helpful to have the full bivariate correlation matrix that includes the correlations between the traits and the three outcomes.
- On page 12, it would be helpful to report the number of participants in addition to percentage (as the authors did for ED returns).
- The sample size for each analysis should be reported in the tables because it varies across analyses.
- The abstract is a little misleading. Although 200 participants completed the initial assessment, the follow-up analysis is based on 160 participants who had both high-quality data and completed the 1-2 follow-up, and none of the analysis is based on the full 200-participant sample size.