Gosling, S., Augustine, A., Vazire, S., Holtzman, N., & Gaddis, S. (2011). Manifestations of personality in online social networks: Self-Reported Facebook-Related Behaviors and Observable Profile Information. *Cyberpsychology, Behavior, and Social Networking, 14*(9), 483-488. <http://dx.doi.org/10.1089/cyber.2010.0087>

**Theoretical Perspective**

*Critique the author’s conceptual framework.*

The authors base their study on two main points. First, online social networking sites (OSNs) have become a central medium for online interactions in recent years. Second, these sites have gained popular use across virtually every demographic segment of the developed world. They then seek to “map out the basic connections between online personality and online behavior” through the examination of two separate descriptive exploratory studies. Study one correlates specific pre-selected personality traits with self-reported Facebook usage. Study two associates these same personality traits with observed Facebook related activities. The authors then compare and contrast the reported data from these two studies to provide empirical evidence regarding: 1) The “rich get richer” hypothesis, which proposes that those with pre-existing social structures and adaptive personalities will transfer this social capital to their OSN usage, and 2) the “social compensation” hypothesis, which suggests that individuals with weaker social connections and interpersonal skills can enhance their social capital through OSN use.

While the core components of the framework seem logical and understandable, a glaring omission is the discussion around what defines and quantifies personality. The authors use the “Big Five personality dimensions” from an existing model, but fail to discuss the model’s theoretical basis or how widely accepted it is as a sole evaluative measure. Being that the entire study revolves around these undefined personality traits, this creates potential inconsistencies in participant responses, observers’ evaluations and data analysis, which undermine the consistency and reliability of the study at a fundamental level. The authors also fail to specify which hypothesis they support, further confusing the intent of the study.

*Comment on the need for this study and its importance.*

The “richer get richer” and “social compensation” models are current predominant theories regarding the transfer of social networking in the physical world to online social sites. Being that these two theories contradict one another, an empirical study is a logical step in evaluating the merits of each in order to determine which is supported by the data collected. In this way, the study addresses a gap in OSN research, which may prove integral to furthering our understanding of how social networking sites either support or help to enrich established personality traits. This addition to the body of research, while potentially valuable, is based on measures that are difficult to quantify. The authors make no claim that the personality characteristics chosen are indicators of relationship building. This seemingly casual selection of personality indicators negatively impacts the value of the study, as its implications in informing the problem are called into question.

*How effectively does the author tie the study to relevant theory and prior research?*

While the manuscript explicitly states the guiding theoretical framework of the study, it falls short in applying this to the methodology used. The study, therefore, seems to apply untested and unsubstantiated measures to the problem of supporting or refuting the theories presented. Further, both studies referenced and analyzed by the authors were conducted prior to this aggregate empirical study and, as such, offer vastly different data sets. This effectively limits the validity of prior research, as there is an unclear connection between what the authors intended to measure and what was actually measured.

While prior research does provide a logical argument for the study itself, it does not justify or support the methodology employed by the authors. This weakens the demonstrated scaffolding from prior research through implications for further study, potentially undermining the intended impact of this specific research focus.

*Evaluate the clarity and appropriateness of the research questions or hypotheses.*

Research questions were not provided. The authors instead provided the general categories they addressed in the questionnaires and the models and scales used to evaluate the resulting data. This omission of pertinent information severely inhibits the readers ability to gauge the clarity or appropriateness of the questions presented to participants. Further, the personality characteristics selected for measurement (extraversion, neuroticism, conscientiousness, agreeableness and openness), although reportedly based on an established model of basic personality dimensions, seem insufficient to quantify a person’s personality. I would argue that a person’s neuroses, for example, may represent a very small factor in their ability to form social relationships. In fact, this particular measurement may border on ableism, seeking to evaluate personality based on a degree of mental or emotional conformity.

The authors clearly and succinctly position the purpose of the study as an evaluation of two prior studies, but then fail to state a hypothesis. As such, the appropriateness of the rationale is impossible to evaluate.

**Research Design and Analysis**

*Critique the appropriateness and adequacy of the study’s design in relation to the research questions or hypotheses.*

The research design is difficult to categorize, since the two studies providing data were completed before this study began. Since the authors’ primary goal was to use this pre-existing data to categorize personality traits and compare them with both self-reporting and OSN residue (traceable indicators of online activity), this study could be described as an observational descriptive study. This design is appropriate for comparing and describing observed behaviors. The authors, however, consistently use the words “associations, correlations, and predictive value” which imply causation that this particular research design does not support.

*Critique the adequacy of the study’s sampling methods (e.g., choice of participants) and their implications for generalizability.*

Sampling methods are noticeably missing from the description of Study 1 and therefore cannot be evaluated. Study 2, on the other hand, utilized voluntary sampling, in groups of 5 “previously acquainted friends” with added cash incentives. This raises concerns such as volunteer bias, drawing a respondent pool that may not be representative of the target population. Also, the cash incentive could potentially draw respondents who are more interested in the money than thoughtfully and intentionally participating in the study itself, creating unintended noise in the resulting data. The authors also noted that responses were “averaged and weighted for equal perspective”, with 80% weighting on the 4 friend responses and 20% weighting on the self-reporter. Being that a prerequisite of participation is “friendship” in this group of 5, and that respondents are provided with “feedback about their personality”, I’m concerned that respondents may have provided information that portrays their peer in a positive manner rather than a true objective evaluation. This represents potential reporting bias. Another concern is that information was provided for “those who indicated their ethnicity” without mention of how many participants declined to respond to this question. Also, gender breakdowns for both studies were skewed with 68% and 61% female respondents, respectively. Further, no age breakdowns were provided. Together, these omissions make generalizability impossible to gauge appropriately.

*Critique the adequacy of the study’s procedures and materials (e.g., interventions, interview protocols, data collection procedures).*

For study 1, participants completed self-reporting questionnaires regarding personality and Facebook-related behaviors. These were completed in a computer lab and items from each measure were presented in randomized order. Missing from the authors’ descriptions were the specific questions used, the time allotted for completion, and the series of steps followed by each participant. No training was reportedly provided to respondents regarding how to interpret or accurately respond to questions, nor were definitions provided for personality descriptors or specific Facebook activities.

For study 2, observers were granted access to each participant’s main Facebook page, along with 10 or fewer randomly selected Facebook photos. Nine research assistants then “rated how well each trait describes the profile owner”. Lastly, the profiles were coded based on the frequency of engaging in eight defined categories of Facebook activity. The first issue that arises is that the term “observers” was not explained. Also, no details are provided regarding the information these observers reported as a result of their observations. Were these the same people that served as the research assistants in the second step of the procedure? If not, who were they and what training, if any, did they receive prior to participation? Also, what data did they collect as a result? Another concern is the fact that only 10 photographs (or fewer) were used and these were randomly selected. It seems that a person’s social connectedness, as well as their personality, may be reflected in the number of photos on their profile as well as the types of activities and number friends or relations present in these photos. Finally, no details were provided regarding training or pertinent definitions provided to research assistants. While all raters reported on all profiles (with 2 exceptions due to acquaintance relationships), this may introduce systematic error.

*Critique the appropriateness and quality (e.g., reliability, validity) of the measures used.*

The data gathered from this study is faulted, at best. Failure to define fundamental conceptual terms, as well as an apparent lack of training for participants and research assistants results in variation that could affect data reporting as well as subsequent observer ratings and coding. This directly impacts the validity of the study, since the measures employed may not match the construct the authors intended. The reliability is similarly affected because each respondent (and/or research assistant) is forced to utilize their own interpretations in order to respond to survey questions or categorize personality traits, resulting in inconsistent reporting results.

*Critique the adequacy of the study’s data analyses. For example: Have important statistical assumptions been met? Are the analyses appropriate for the study’s design? Are the analyses appropriate for the data collected?*

The authors make no claim of having met important statistical assumptions. The analysis of study 1 is limited to the alignment of self-reported Facebook behaviors and the “associated” personality characteristics. Similarly, the analysis of study 2 aligns observable Facebook activities with combined self-reported and peer-reported personality traits. While the authors report that these personality dimensions are based on the Ten Item Personality Inventory, no further discussion or rationale is offered to support this measure. Arguably more disconcerting, though, is that no theoretical or logic-based explanation is provided regarding the alignment of these personality factors with specific Facebook activities. The analyses themselves, therefore, inappropriately imply correlation despite a lack of theoretical or research based support.

**Interpretation and Implications of Results**

*Critique the author’s discussion of the methodological and/or conceptual limitations of the results.*

The authors provide little to no discussion of methodological or conceptual limitations. They do mention that self-reports may reflect “self-views rather than actual behavior”, but quickly offer the explanation that not all behaviors leave discernable residue. While this may be true, it fails to comprehensively address any possible false consistencies and/or apparent inconsistencies resulting from this reporting error. Also, the authors claim the ability to assess the degree to which observers correctly interpret behavioral residue when forming personality impressions based on Facebook profiles. This implies that participants were given evaluative criteria or working definitions with which to evaluate correctly. The authors should have included this in their discussion.

*How consistent and comprehensive are the author’s conclusions with the reported results?*

The authors’ conclusions align with the “rich get richer” hypothesis. Specifically, the personality characteristic of extraversion is supported by self-reported OSN frequency. Also, the results showed observers are able to use observable profile information to form accurate impressions of some basic personality traits. However, these conclusions are not directly based on results, but rather on the authors’ rather questionable analysis that correlates collected data with undefined personality traits. The authors do note that they were unable to identify valid and usable cues of “openness”, but attribute this to observers’ inability to pick up on existing cues. This consistent failure to acknowledge the validity of possible conflicting data reveals inappropriate evaluation of reported results. As a whole, the authors seem to intentionally align their conclusions with the “rich get richer” hypothesis, to the exclusion of conflicting data.

*How well did the author relate the results to the study’s theoretical base?*

The results of this study were related to the theoretical base almost exclusively through comparisons of supporting data and the existing “rich get richer” hypothesis. Conflicting and inconclusive data is attributed to participant and/or research assistant error. This, at best, reveals inadequate analysis of results and, at worst, potential researcher bias.

*In your view, what is the significance of the study, and what are its primary implications for theory, future research, and practice?*

The significance of this study is, in my opinion, completely negated by incomplete procedures and methodology as well as the unsubstantiated alignment of personality traits with specific Facebook activities. There is no prior research or relevant theory to support this correlation, and this study fails to provide explanation or theoretical reasoning as justification. This study also fails to provide complete demographic information, making generalizability impossible. As a whole, this study does not contribute meaningfully to the existing body of research on online social networking sites.