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|  | Associate Editor |  |
| No. | Comments | Responses |
|  | Thank you for your continued efforts in revising the manuscript. The reviewers appreciate the significant improvements made, particularly the enhanced clarity in the introduction and theoretical framing, and the comprehensive presentation of the summary statistics. Incorporating the concepts of "social constructs" and "social value" has strengthened the theoretical foundation. That said, several important issues remain to be addressed before the manuscript can proceed to acceptance. The paper is progressing well and instead of repeating all comments made by our expert reviewers, I am providing a summary below. |  |
|  | 1. First, reviewers note that certain control variables (such as Transaction and Awareness) are highly skewed and suggest either rescaling these variables (via standardization or log transformation) or alternatively providing standardized coefficient tables in the appendix. They also recommend that subgroup analyses aimed at testing moderation effects be accompanied by formal statistical tests comparing key coefficients across subgroups. |  |
|  | 2. Second, there is a need for a clearer articulation of the network effect theory, including a precise definition and stronger theoretical linkage to each hypothesis. |  |
|  | 3. Relatedly, reviewers request clarification regarding sample size inconsistencies, a more detailed explanation of the week-by-week matching procedure, and additional information on how variables such as ProfilePictureRatio were computed. |  |
|  | 4. As a minor point, it is suggested that the research model diagram be moved to the end of the theory section to improve the flow. |  |
|  | 5. Overall, the manuscript is viewed as progressing well toward acceptance, pending satisfactory revisions addressing these remaining concerns.  Please turn in a revised document with a comment by comment response. |  |
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|  | Reviewer 1 |  |
|  | The authors have made significant improvements in the second revision. I am pleased to see the enhanced clarity in the introduction and theoretical foundation. The incorporation of "social constructs" and "social value" addresses my earlier concerns and improves the readability of the manuscript. I also appreciate the authors' thoughtful responses to comments 5, 6, 8, and 10, which I now find satisfactory.  I am confident that the manuscript is progressing toward acceptance, contingent upon the resolution of the following remaining issues. |  |
|  | 1. While I agree that network effect theory is a suitable theoretical foundation for this study, the current version still presents a relatively weak articulation and integration of the theory. Specifically:  The theory section lacks a clear and concise definition of the network effect and an explicit rationale for its application in this context. This lack of clarity appears to weaken the argumentation in the hypothesis development. For instance, the justifications for H1a and H1b, and similarly for H2a and H2b, remain highly interchangeable and difficult to distinguish. Statements such as "NFT diffusion leads to collective identity" and "NFT diffusion leads to cultural richness" are not sufficiently differentiated from one another in theoretical or empirical terms.  I encourage the authors to delve deeper into the underlying mechanisms of network effects. Alternatively, consider complementing the framework with additional or alternative theoretical lenses that may better explain the observed dynamics. In either case, a stronger integration of the theory with the hypotheses is necessary. |  |
|  | 2. There appears to be a discrepancy in the reported sample sizes: The initial sample is reported as 4,186. However, the descriptive statistics show an average CC0 value of 0.039, which—when multiplied by the full dataset size of 113,914—yields approximately 4,442 observations. This inconsistency raises concerns regarding the accuracy of data reporting. (I might be wrong, but the number made me confuse). Additionally, please clarify why the number of observations differs across variables. A transparent explanation will enhance the credibility of your dataset and analysis. |  |
|  | 3. The manuscript would benefit from a more detailed description of the matching procedure. Specifically, how does the week-by-week matching generate a collection-level balanced dataset? If the matching is done on a weekly basis, the number of time-variant observations could differ across collectives, potentially introducing imbalance and bias in the panel structure. As this appears to be one of the first studies to use propensity score matching (PSM) in a panel data setting, a deeper discussion of its limitations and potential biases is warranted. |  |
|  | 4. Although this was not raised in earlier rounds, I now suggest that the authors provide more detail on how they collected the data and computed the ProfilePictureRatio. That may be helpful to readers. Since I feel like author seem have to exhaust all the data in X to get it. |  |
|  | 5. Minor Comments  It would be more effective to place the research model at the end of the theory section, after all hypotheses have been introduced. This will provide readers with a clearer, more integrated overview. |  |
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|  | Reviewer 2 |  |
|  | I appreciate the opportunity to review the revised manuscript (INFMAN-D-24-00956R1). This study examines how sharing copyright for NFT-associated artworks affects their success. It finds that copyright sharing enhances both social visibility, measured by increased use as social media profile pictures, and financial value, reflected in higher average sale prices. These positive effects are further amplified when the artworks are highly remixable. While copyright sharing encourages broader engagement and network effects, it may also diminish creators’ exclusivity.  I served as Reviewer 2 in the previous round. I acknowledge the authors’ thorough effort in addressing my earlier comments. Overall, the manuscript has improved significantly. The methodology is more robust, and the narrative is now more cohesive in terms of empirical justification and rationale. While I am satisfied with most of the responses to my previous comments, I have a few remaining concerns that I believe are addressable. Therefore, I recommend a minor revision. |  |
|  | 1.Data and Measurements:  I appreciate the authors for providing comprehensive summary statistics across the variables. Upon reviewing this information, I noticed that some variables, such as Transaction and Awareness, are highly skewed and differ greatly in scale. This may explain why their corresponding regression estimates are on very different scales and difficult to interpret (e.g., in Tables 4 and 5). Although these are control variables and not central to the main analysis, I recommend that the authors consider rescaling them through standardization or log transformation. Alternatively, providing standardized coefficient tables for the main results as supplementary materials in the Appendix would also be helpful. |  |
|  | 2. Empirical Analysis:  I appreciate the clarifications regarding H2a and H2b, as well as the associated empirical examination. I am fine with the use of subgroup analysis over interaction terms to test moderation effects. Given the current model specification and the hypothesized moderation effects across levels of remixing, it is important to demonstrate statistical differences between subgroups. Consequently, I recommend that the authors conduct and report statistical tests to assess whether the differences in key coefficients of interest across subgroups are significant. This practice should be applied to all subgroup analyses aimed at testing moderation effects. |  |