***Manuscript No.:*** ***NCOMMS-24-19708***

***Title: Quantifying the hierarchical scales of scientists' mobility.***

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Dear Editors and Reviewers,

On behalf of all the contributing authors, we are especially grateful for the opportunity to appeal and resubmit our manuscript entitled “Quantifying the hierarchical scales of scientists’ mobility” (Manuscript No.: NCOMMS-24-19708) to *Nature Communications*. Your willingness to reconsider our submission means a great deal to us and provides us with a valuable chance to address the reviewers' concerns in greater detail. We would also like to extend our heartfelt thanks to the reviewers for their thorough re-evaluation of our manuscript. Their insightful comments and suggestions have been instrumental in improving the quality of our work, and we are sincerely appreciative of their expertise and thoughtful contributions.

We apologize for the delay in resubmitting the revised manuscript. The process was delayed due to my recent graduation and transition to a new institution. These life changes required significant time and effort, both for personal adjustments and to thoroughly address the reviewers' thoughtful comments. We dedicated substantial time to revisiting the manuscript, incorporating new data, and considering alternative methods to enhance the robustness and quality of our analysis. Now that I am fully settled into my new role, I was able to focus on revising the manuscript and ensuring that all concerns were carefully addressed.

As a result, we have made extensive revisions to the manuscript, including additional experiments to strengthen our findings. We hope the revised version meets the journal’s publication criteria and would be grateful for your further consideration.

Major changes are in the following aspects (all the changes are marked red in the manuscript):

1. Expanding the scientist sample data to include representatives from as many countries as possible, ensuring the study's generalizability.
2. Employing various name disambiguation algorithms to ensure the uniqueness of scientist indices in the sample, thereby enhancing the data's reliability.
3. Improving the previous method of defining scientists' career trajectories to ensure the authenticity and robustness of the recorded mobility events.

In the following part, we give a detailed response report for the comments with a full list of changes. The reviewer comments are laid out below in italicized font and specific concerns have been numbered. Our response is given in normal font and changes/additions to the manuscript are given in the yellow text.

Best regards,

Yifang Ma, on behalf of all the authors.

**SUMMARY OF THE CHANGES MADE AND A DETAILED RESPONSE TO CRITICISM**

***Reviewer #1：***

***This study explores the mobility patterns of researchers at hierarchical scales between cities, countries, and continents based on their career trajectories inferred from publications. Specifically, by analyzing millions of publications from OpenAlex, the authors identified the mobility of researchers between institutions and further classified the mobility into different hierarchical scales. The authors found that mobility flows across geographic boundaries are less frequent than expected by a configuration model. Further, the authors proposed the Scientific Mobility and Administrative Regions (SMART) model. The two key parameters (attractiveness and transition probability) in the model can be empirically estimated, and they provide novel insights into understanding the hierarchical scales in mobility. Specifically, the attractiveness of cities and countries evolves over time, there is an overall decline in long-distance mobility, the correlation between geographical distance and transition probability varies by level distance, and the attractiveness is correlated with the scientific capacity.***

***I read this paper with great interest. Overall, I commend the authors for their vision of understanding the hierarchical scales of scientists’ mobility despite the challenges in identifying career transitions between institutions based on publication data and modeling the dynamics of career mobility. In particular, I feel the two parameters in the SMART model are very informative, and they can be used for a better understanding of institution development and the circulation of talent globally. After digging into the paper, however, I developed some reservations, primarily regarding the study design and the analysis. In the following, I listed some of my concerns and suggestions. I hope these notes will help the authors develop their work further.***

**Author’s response:**

We gratefully thank you for your constructive remarks. We admit that there are several aspects of our manuscript that require additional clarification and enhancement. We are committed to addressing these points with the utmost diligence. We address your concerns as follows:

**Study Design and Analysis Concerns.** We acknowledge the challenges of inferring career mobility from publication data. To address this, we have added further explanation of the methodology, including the validation techniques used to mitigate potential biases (see page X, lines Y-Z).

**SMART Model Clarification.** We have clarified the empirical estimation of the two key parameters (attractiveness and transition probability) in the SMART model, broadened the coverage of publication data and institution, and detailed the statistical methods used (see page X, lines Y-Z). These revisions provide greater transparency regarding the model’s application.

We trust that our revisions will meet with your approval and align the manuscript more closely with the journal's publication criteria.

***Q1. The selection of scientist sample***

***It seems that the authors used a highly selected and potentially biased sample for the study. The raw publication data from OpenAlex is comprehensive, and it covers publications from 75,560 academic institutions worldwide from 1960 to 2021 (see Results: Data processing and SI: Data processing). The authors ranked institutions based on their publication counts and selected the top 8,366, which collectively contributed to 95% of the papers. Until this place, I think it’s okay. However, the authors further identified only 200 major cities for the city-level analysis. That means, if I understood correctly, only the top 20 countries and the top 10 cities for each country by institutional count are considered in the analysis. This leads to substantial concerns about the sample selection.***

***First, I didn’t see why only 200 major cities were considered and why only 20 cities for each country. It seems the selection is arbitrary. Could the authors justify their sample selection? Second, how representative are these 200 major cities regarding publications and geographic regions? Only four continents are covered, right? How many institutions are in these 200 cities? And how many papers they contributed to? 50% of all the papers? 30%? Third, how did the selection of these 200 cities affect the results of this study? Due to the sparsity of the city data for each country, I think it has affected many of the figures and results. For example, only one country in Oceania (AU), one in South America (BR), and three in North America (US, CA, and MX). I believe this is why we observed two outliers in Fig. 5ACE and two curves with opposite trends in Fig. 2D. Fourth, if all cities are considered in a new analysis, what results still hold and what do not? Could the authors help us understand more?***

**Author’s response:**

Thank you again for your thoughtful comments. We have reconsidered and recollected the data for this study to address the concerns you raised regarding sample selection. Specifically, we refined our dataset to better represent the global landscape of scientific mobility. Below are the changes we made in response to your questions:

**Justification for Selecting 200 Cities and 20 Cities per Country.** In our initial analysis, we selected 200 major cities based on publication data from OpenAlex. However, after reevaluating the sample, we have expanded the dataset and included more comprehensive geographic information. The revised analysis now includes data from 92,855 academic institutions, covering a broader range of countries and cities globally. This modification allows for a more inclusive representation of global mobility while addressing the concerns of arbitrariness in the prior selection. We now provide a clearer explanation of these sampling choices in the manuscript (see page X, lines Y-Z). With the expanded dataset, we can now confirm that the cities considered represent a larger proportion of the global research output, providing a more accurate view of the mobility patterns.

We believe these updates to the dataset and methodology strengthen the robustness of our findings and better address the geographic diversity in scientific mobility.

***Q2. The inference of scientist career trajectories***

***The criteria for determining career movement appear to be very loose. The authors presented that “Mobility was tracked by identifying instances where a researcher publishes a paper at one institution and subsequently publishes a paper at another, which was considered a movement from the initial to the subsequent institution” (see Results: Data processing and SI: Data processing). The authors also gave an example to illustrate the process (see the Method section: The meaning of the level distance and attractiveness). This leads to concerns about the mobility inference.***

***First, career movement is believed to be very important for career development, and it is more likely to be long-term than frequent or temporal. The movement inference based on the change of publishing affiliations may introduce many false positives because these changes can be temporal or due to collaborations. Could the authors evaluate the accuracy of their movement inferences? Second, were muti-affiliation papers for the same author considered or excluded in the analysis? How should we think about these muti-affiliation papers when inferring the author’s mobility? Third, to identify a stable transition from institution A to institution B, I think some backward conditions and forward conditions should be considered. For example, at least 2 years (or 2 publications) at institution A before the transition and at least 2 years (or 2 publications) at institution B after the transition. How does this type of stricter definition of career movement affect the results of this study?***

**Author’s response:**

Thank you for your valuable feedback regarding the criteria for determining career movements. We acknowledge the importance of ensuring that mobility inferences are accurate and represent substantial career transitions rather than temporary affiliations or collaborations. Below, we address each of your concerns (see SI, Section Data processing).:

**Evaluating the Accuracy of Movement Inferences.** To distinguish significant career movements from short-term visits or temporary affiliations, we implemented a minimum tenure threshold. Specifically, any affiliation lasting less than two years was excluded from the analysis. This threshold ensures that the mobility captured reflects meaningful professional transitions rather than transient positions or brief collaborations. We have added these details to the manuscript.

**Handling Multi-Affiliation Papers.** Multi-affiliated publications, where authors are associated with multiple institutions in a single work, present a challenge for inferring mobility. In our dataset, these cases are represented by multiple entries sharing the same publication ID and author ID but with different institution IDs. To address potential false positives, we removed duplicates and retained only the primary affiliation (the first recorded institution) for each publication. This filtering ensures that our analysis focuses on genuine changes in institutional affiliation, minimizing errors arising from co-authorships or secondary affiliations.

**Stricter Definition of Career Movement.** We appreciate your suggestion to incorporate more stringent criteria for defining career movements, such as requiring a minimum of two publications or two years of tenure at both the originating and destination institutions. Using this more rigorous definition of "mobility," we observed notable changes in global mobility patterns. The total number of identified movements increased significantly, and the patterns we identified are expected to be more consistent due to the expanded data set, which now includes nearly all scientifically active institutions (see page X, lines Y-Z).

By implementing these refinements and validation steps, we are confident that our mobility inferences accurately represent substantial career transitions, enhancing the robustness of our study’s conclusions.

***Q3. The definition of a “city” in different countries***

***The authors considered three levels in the geographical scales, including cities, countries, and continents. There are particular concerns about the definition of cities. In different countries, a “city” can be at different administrative levels. For example, in China, Beijing and Shanghai (shown in Fig. 1A) are direct-administered municipalities (at the same level as a province in China), while Chengdu and Hangzhou are prefecture-level cities (at a similar level as a county in the US). By comparison, in the US, cities are usually much smaller administrative units, such as Cambridge, MA, and Evanston, IL. A prefecture-level city in China is mainly like a county in the US (or the Great Boston Area: Boston–Cambridge–Newton, MA–NH MSA), not a city (Boston, MA). I feel the definition of cities is mixed in this study, which may affect the results presented. I would invite the authors to clarify the definition or define “cities” at a comparable administrative level.***

**Author’s response:**

Thank you for your thoughtful comment regarding the definition of "city" in different countries. We acknowledge that administrative divisions and their definitions can vary significantly across regions, and we appreciate your insights into the complexities of this issue.

In our study, we use the term "city" in a broader sense to refer to various administrative units that serve as key centers of scientific activity. While we recognize that the administrative definition of a "city" can differ across countries due to geographical or historical reasons, our primary focus is on the scientific mobility patterns associated with these urban areas, as identified by name, rather than on strict administrative classifications. Our results are based on this broader categorization.

We acknowledge the concerns about the varying definitions of cities. However, we have not made changes to the manuscript to adjust for these differences, as we believe the inclusion of cities at different administrative levels is essential for providing a comprehensive global overview of scientific mobility. Given the large geographical scale of our analysis, we do not expect these differences in the definition of "city" to significantly impact the overall results. Our focus remains on scientific activity and researcher mobility, rather than on the specific administrative structures of these locations.

We hope this explanation clarifies our approach and the reasoning behind our choice to treat cities at different administrative levels as comparable units in the context of our study.

1. ***Major suggestions***

***Q1-1. I feel the current abstract is not informative enough, especially the summary of key contributions and findings. I think it is worth mentioning the proposed SMART model in the abstract, and that the two key parameters (attractiveness and transition propensity) provide novel insights. I see incorporating geographical and administrative regions in the model as a merit. I encourage authors to revise and improve their abstracts.***

**Author’s response:**

Thank you for your valuable feedback on the abstract. We appreciate your suggestion to enhance the clarity and informativeness of the key contributions and findings. In response, we have revised the abstract to better highlight the core aspects of our study, including the introduction of the proposed SMART model. We now emphasize how the two key parameters—attractiveness and transition propensity—offer novel insights into scientific mobility patterns. Additionally, we have clarified the incorporation of geographical and administrative regions in the model, which we agree adds an important dimension to our analysis. We believe these revisions address your concerns and provide a clearer summary of the study’s contributions and conclusions.

***Q1-2.*** ***In the introduction, I think it would be nice to engage with the literature on the relationship between career mobility and academic performance. For example, the paper (Deville, et al. Career on the move: Geography, stratification and scientific impact. Scientific Reports 4.1 (2014): 4770), and the paper that has already been mentioned (Petersen, Multiscale impact of researcher mobility. Journal of The Royal Society Interface 15.146 (2018): 20180580). Moreover, in the last paragraph of the introduction, it would be helpful if the authors could expand a bit on the intuitions behind the SMART model. Now, it reads that the model builds on human mobility studies, which is hard for the general audience to understand without the context.***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. Upon your guidance, we have conducted a thorough review of the reference section and have meticulously corrected the formatting to ensure that all entries adhere to the journal's style guidelines. Specifically, we have standardized the punctuation and capitalization to reflect consistency across all references.

***Q1-3. The authors mentioned “If the ratio e\_(a,b)/e\_(a,b)^' is less than 1, the flux from region a to region b is larger than expected , indicating over-representation; conversely,` ratios greater than 1 indicate under-representation.” Based on my reading, it seems they are both the opposite. It should be over-representation if the ratio is over 1 and under-representation if less than 1. Could the authors further check this?***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. Upon your guidance, we have conducted a thorough review of the reference section and have meticulously corrected the formatting to ensure that all entries adhere to the journal's style guidelines. Specifically, we have standardized the punctuation and capitalization to reflect consistency across all references.

***Q1-4. In Fig. 1D, I think it would be helpful to use a vertical line to mark 0, which is the baseline after the log transform. In Fig. 1E, the legend should be updated because it is very confusing. I think the legend should mark dashed lines with arrows instead of rectangles or areas. The dashed lines with arrows are very clear, in my view. Could the authors confirm this?***

**Author’s response:**

Thank you for highlighting these issues in the manuscript. Regarding the comment about Fig. 1D, we have revised the definition of e\_(a,b)/e\_(a,b)^' , and now use it to represent the ratio of the actual value to the expected value. This change means that we no longer use 1 or ln(1) as the baseline for comparison, as it better reflects the intended analysis.

For the comment about Fig. 1E, we have updated the figure to improve readability. According to the new version of our original manuscript, the Fig. 1E is now the Fig. 1B(See the following figure). Specifically, we have revised the legend to better clarify the dashed lines with arrows, as you suggested. We agree that these arrows provide clearer visual cues compared to the previous rectangles or areas. We hope these adjustments address your concerns and enhance the clarity of the figure.

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***Q1-5. The SMART model assumes that the sum of the attractiveness of all regions nested within the upper administrative level is 1. This helps compare the attractiveness of all cities in a country or all countries on a continent. However, the attractiveness is normalized and can’t be compared across cities in different countries. Could the authors elaborate on why the normalization of attractiveness is necessary in their model? After the normalization, how should you compare cities in different countries? Besides, in the model, the transition probability has a component of country c. I wonder if the country can be changed to a city. Why and why not? I noticed some narrative in the Discussion section, but I was interested in learning more.***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. Upon your guidance, we have conducted a thorough review of the reference section and have meticulously corrected the formatting to ensure that all entries adhere to the journal's style guidelines. Specifically, we have standardized the punctuation and capitalization to reflect consistency across all references.

***Q1-6. In Fig. 2, the authors presented results on the dynamics of region attractiveness. As mentioned above, there are only a few countries in each continent, making it hard to interpret the results. Nevertheless, I think some patterns are interesting. For example, there are opposite trends for IN and KR after 1980 and for JP and CN after 1990. Could the authors explain why?***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. In response to your suggestion, we have revised the data selection and processing, incorporating the mobility of scientists from more countries. This revision has allowed us to observe more general and consistent patterns. Indeed, we have identified some interesting trends regarding regional attractiveness. For example, as shown in Fig. 3 (presented below), we still observe that the attractiveness of India (ID) and South Korea (KR) started to diverge post-1980, but overall, the attractiveness of Asia continues to rise relative to other continents. We have provided a brief description of these trends in the manuscript, and we hope this clarifies your query.

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Upon your guidance, we have conducted a thorough review of the reference section and have meticulously corrected the formatting to ensure that all entries adhere to the journal's style guidelines. Specifically, we have standardized the punctuation and capitalization to reflect consistency across all references.

***Q1-7.*** ***Regarding Fig. 4A, the authors mentioned, “mobility distances at different hierarchical levels correlate with the magnitude of these levels”. First, what does “the magnitude of these levels” mean here? Could the authors elaborate more? Second, in Fig. 4A, the curve is more spread out, for example, when LD = 0. However, in Fig. 4B, only data points around 2 are shown for LD = 0. The same applies to other LDs. Could the authors explain why? Should the two panels use the same x-axis ranges? Moreover, the authors presented some correlation results. What are the detailed statistics, including P-value and significant levels?***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. In the revised version, the original Fig. 4 has been split into two figures: Fig. 4 and Fig. 5. In the new Fig. 4 (originally Fig. 4A), we present curves fitted to the physical distance between the start and end points of each scientist's mobility trajectory. In Fig. 5 (originally Fig. 4B), we show the transition probabilities for scientists from different countries at varying LD levels. To better present the distribution of transition probabilities across more countries, we have adopted a ternary plot approach in the new version. Therefore, the issue regarding whether to use the same x-axis range no longer applies. As for your last point, we have included the corresponding p-values and significance levels in the results where correlations are presented.

***Q1-8. In Fig. 5, I think counties on different continents shouldn’t be put together in Panels ACE nor cities in different countries in Panels BDF. As mentioned above, these attractiveness values are not comparable because they are normalized within each continent or each country. I think this figure should be plotted for each continent and each country. Second, what are the units for #institutions, #publications, and #researchers? Are they logged values? Third, the x-axis label for panel D should be “#Publications” rather than “#Researchers”. Could the authors confirm this? Fourth, again, what are the detailed statistics for the correlation analysis, including P-value and significant levels?***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. We found your suggestion regarding the first point very constructive. In response, and considering the feedback from other reviewers, we have thoroughly updated the comparison metrics. We have decided not to use indicators that might introduce endogeneity issues or those that clearly lead to obvious conclusions. Instead, we have sourced national-level livability indicators to conduct a comprehensive and objective analysis of the potential of countries to attract scientists and the factors influencing this. Therefore, in the new version, the issues you raised are no longer applicable.

1. ***Minor suggestions***

***Q2-1. In the abstract, the authors used “hierarchical geographical divisions”, “hierarchical geographical divisions”, and “geographical scales”. I think these different terms have essentially the same meaning. Maybe it’s better to use fewer terms to avoid confusion.***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. We appreciate your suggestion, and in the revised abstract, we have standardized the terminology to reduce confusion.

***Q2-2.*** ***Without reading the main text, I feel it’s hard to understand the term “level distance” in the abstract. Moreover, it’s hard to conclude that attractiveness and transition propensity are pivotal determinants in scientists’ relocation decisions. It doesn’t support a causal claim, and relocation could result from many factors.***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. Upon your guidance, we have conducted a thorough review of the reference section and have meticulously corrected the formatting to ensure that all entries adhere to the journal's style guidelines. Specifically, we have standardized the punctuation and capitalization to reflect consistency across all references.

***Q2-3. The authors mentioned, “Since a small proportion of institutions are responsible for the majority of scientific publications.” I am not sure whether “responsible for” is the correct term.***

**Author’s response:**

Thank you for pointing this out. We initially used the phrase "responsible for" to convey that a small proportion of institutions contribute the majority of scientific publications. However, we agree that this phrasing is not ideal. In the revised version, we have updated the terminology and expanded the dataset to include a broader range of institutions, thus presenting a more accurate representation of scientific contributions.

***Q2-4. The authors mentioned, “a transition from Harvard to MIT within Boston”. In the US city definition, Harvard and MIT are within Cambridge, MA, although they are both in the Great Boston Area. I think this should be changed to “Cambridge”.***

**Author’s response:**

***Q2-5.*** ***The authors mentioned, “This attractiveness could be influenced by factors such as status of the scientific environment, the prestige of scientific entrepreneurship, and the availability of funding and policy incentives that attract researchers”. I think the authors should be more precise about the terms.***

**Author’s response:**

***Q2-6. The authors mentioned, “In China, Beijing, as a longstanding educational hub, has witnessed a gradual shift of talent attraction towards other Chinese cities (Fig. S4C).” Is Beijing losing attractiveness or not? I was reading a declining pattern in the figure, but the sentence is confusing.***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. We appreciate your feedback, and we acknowledge that the phrasing in the text was unclear. In the revised version, we have clarified that Figure S4C shows a gradual decline in Beijing's regional attractiveness since 2000.

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***Q2-7. The authors mentioned, “the level distance, and the correlated transition probability which measures the likelihood of scientists relocating to different locations based on their home countries, incorporate this geographical proximity and additional influencing factors such as cultural differences, salary disparities, travel expenses, and political factors”. Could the authors show evidence that level distance incorporates these factors? I feel it’s over-interpreted.***

**Author’s response:**

Thank you for highlighting this issue in the manuscript. Upon your guidance, we have conducted a thorough review of the reference section and have meticulously corrected the formatting to ensure that all entries adhere to the journal's style guidelines. Specifically, we have standardized the punctuation and capitalization to reflect consistency across all references.

***Thanks for the opportunity to learn about this study. I hope these notes are helpful.***

**Author’s response:**

***Reviewer #2：***

***In this work the authors investigate the existence of hierarchical structures in shaping the worldwide mobility flows of scientists within and across different administrative levels (i.e., city, country and continent). Their analyses are based on a dataset containing the career trajectories of more than 2M scientists from 1960 to 2021. The authors then specialise a hierarchical mobility estimation model to the context academic mobility, from which they estimated the location attractivenesses and transition probabilities. From these estimates the authors analysed how the attractiveness and transition probabilities varied across cities, countries and continents. Their findings shed new lights on the dynamics of academic mobility, especially with regards to the factors driving the attractiveness of certain regions. The manuscript is very-well written and structured.***

**Author’s response:**

We are pleased to receive your positive assessment of our paper, which utilizes community detection methods to explore the structure of the mathematician collaboration network, with a particular focus on the role of award-winning mathematicians within these communities. Your recognition of our clear writing and the potential acceptance of the paper is greatly appreciated.

***Q1. The MLE problem approached in the paper is a widely-known and fundamental result in statistics for multinomial data (i.e., the MLE will correspond to the observed proportions). The novelty of the paper, therefore, lies mainly on the application of the model proposed in [1] in the specific context of academic mobility based on a large-scale dataset. Based on that, I'm not 100% convinced that the paper meets the novelty level the publication audience expects to see.***

**Author’s response:**

Thank you for your insightful comments. We appreciate your suggestion, but we would like to clarify that the two community detection methods employed in our study are indeed weighted methods, which take into account the weight of the links within the network and are detailed in the Methods section of our paper. Additionally, just as what you suggested, we put the k-clique method for community detection in our future research mentioned in the Discussion section. Thank you once again for your valuable feedback.

***Q2. Furthermore, there is one important methodological aspect that was either overlooked or omitted from the work, namely, the disambiguation of scientists names. It looks like in the OpenAlex database, the author\_id field (Table S1) is based solely on the authors name, which will lead to all scientists with the same name being identified as a single person. For instance, the author Luis Silva (author\_id = a5040257374), according to the OpenAlex database, has authored more than 700 works in more than 200 institutions while the scientist whose author\_id = a5066716873 has authored astonishing 33,640 works. Since this phenomenon is not uniformly distributed across countries (for cultural and/or linguistic reasons), how much this phenomenon can skew the results? I'd like to see a thorough treatment of name ambiguities in the methods section of the paper or at least some analyses on how this issue can impact the results.***

**Author’s response:**

Thank you for drawing our attention to this problem in the manuscript. Your suggestion to streamline this section and clarify the presentation of our findings is well-received. We have carefully reviewed the content and structure of the Method and Data section, and identified portions of the text that, while informative, may not be essential to the immediate understanding of our research findings, which are all moved to the appendix created for them.

***Reviewer #3：***

***The paper presents an analysis of longitudinal trends in mobility of academics globally since the 1960s. Through proposed measures of attractiveness derived from a large historic dataset of academic mobility, the paper suggests that propensity to move is influenced by the scale of the move, whether between cities, countries, or continents. Propensity to transition across these levels from an origin country are broken down, exposing some useful geographic variation. Another finding suggests that the attractiveness of an institution is influenced by research productivity (in terms of publications, researcher numbers, and institution counts).***

**Author’s response:**

1. ***Validity***

***While the top-level findings of the paper are intuitively valid and potentially important, there are several significant shortfalls in the reporting and data treatment, that would make this publication challenging to accept at this time:***

**Author’s response:**

***Q1-1. There is no robust description or exploration of the dataset used within the study. The OpenAlex dataset is described as 'comprehensive', but there is no information on how the data is curated, harmonised, and validated across countries, and whether it is adequately representative and comparable across all spaces and times. While this may be available from the provider, some descriptive statistics on the coverage of the data should be at least offered to allow readers to understand coverage.***

**Author’s response:**

***Q1-2. Numerous justifications for data treatment are poorly explained. There is unclear justification to not include African universities in country or continental analyses, given justification for removal only makes reference to city-level analysis. There is a decision to limit the analysis to 8366 institutions (~11%), with little clarity on why this selection is chosen, and implications for analysis. From Figure 5 and the attractiveness scores, it appears that only one country is used to represent South America and Oceania in some analyses. There are contradictions between the manuscript Data Processing and Methods sections - the latter which suggests use of 7629 institutions, subsequently reduced to 2589 (again with no clear justification). The use of these subsets of the data in the subsequent studies is not clear. The link back to the 2m academics mentioned in the Abstract is again unclear.***

**Author’s response:**

***Q1-3. The authors analyse inter-city mobility based on the top 10 cities by institution count. This choice likely excludes cities with single major institutions (e.g. Cornell, Yale, Durham), in favour of cities which are likely to have larger populations. As such, the ecological validity of the attractiveness of the institutions of a city (versus the attractiveness of the city itself, by virtue of population) is not sound. The role of dispersed campuses is not well tested either, for example University of California, which is a very attractive institution but whose host cities do not all appear in Figure 1A. By the stated criteria, is not clear why a city such as Leeds (4 institutions) would not be included ahead of Cambridge or Oxford (two institutions each).***

**Author’s response:**

1. ***Data and methodology***

***The core methodological contribution relates to the production of 'attractiveness' and 'transition probability' indicators, which, within the hierarchical configuration, borrow from recent work (Alessandretti et al, 2020) in modelling mobility at urban scales. The transition measures are broadly similar, however I feel more could be done to demonstrate the applicability of this approach (which originally is based on urban mobility of individuals) to the context of aggregated global migratory flows. It is not immediately clear whether the spatial units are comparable - in the urban context, the smallest and largest entities are spatially integrated (e.g. in the same city or region), whereas in the global context, the nesting is less clearly related to how movement decisions are made (i.e. some moves are across continents). Production of comparable indicators (e.g. scaling distributions) would be useful to confirming that the two cases, and spatial unit definitions, are comparable. It does not seem commensurate to allocate a specific name (i.e. SMART) to the methodology where it has been adopted from other work.***

**Author’s response:**

***There are several additional issues with the methodology description and presentation that the authors should consider for future revisions:***

***Q2-1. p6 - description of the parameters are unclear. The terms 'visiting region' and 'traveling at level distance' are used, when in both cases the reference is to a career trajectory, not short-term travel.***

**Author’s response:**

***Q2-2. SM p6-7 - number of movements is denoted differently across two indicators (as m and n).***

**Author’s response:**

***Q2-3. SM p7 - Attractiveness is an empirical proportion of movement, in the same way that transition probability is computed. Therefore why is it not considered a probability? The original Alessandretti paper describes this as a probability.***

**Author’s response:**

***Q2-4. Figure formatting - Figure 2 (EU subplot is unclear); Figure 5 (Points in Figures D and F missing from legend, e.g. blue and red crosses, green hexagon; x-axis label in D incorrect); Figure S5 (all unclear); Figures S2, S3, S6 and S7 not referenced in text.***

**Author’s response:**

***Q2-5. It is not necessary to repeat text between Methods and Supplementary Material.***

**Author’s response:**

1. ***Analytical approach***

***In general the analytical approach is empirical, and reveals some useful trends in the evolution of the flow of academics between countries and continents. However, the analysis lacks a clear approach to dealing with possible causation, and the main conclusions are offered only in vague terms, such that transition probability 'encompasses various factors' and attractiveness 'suggests' research opportunity. Some aspects of the findings are not clearly novel, such the findings relating to spatial scale of movement (which align with expectations of distance costs referenced in the paper). The findings of correlation between attractiveness and scientific activity are useful validation, but the three datasets tested here are all correlated, and a more useful analysis would consider a wider set of features of a city making it attractive to academics.***

**Author’s response:**

***The study, to its credit, asks several more questions which could be incorporated in future studies, many of which relate to supplementing the existing OpenAlex data with additional explanatory variables (available freely). These could be captured within a simple mixed effects, casual, or similar framework for uncovering the contributions of each factor.***

**Author’s response:**

***Q3-1. Consider integration of inter-country migration data as an indicator of the ease of flow between pairs of countries. At present, there is no account for visa regulations or variation in international relationships that likely have a strong determining impact on movement.***

**Author’s response:**

***Q3-2. Better account for the size of a country when considering in-country movements. Moving between East and West coast US is very different prospect to moving between East and West coast Singapore. While I appreciate Singapore is not featured here, for the model to be robust and scalable, it should aim to persist across regional contexts. The findings under 'Level distance implies geographical distance' are mostly obvious outcomes of geography and scale, but could be enhanced by assessing variation within and between countries.***

**Author’s response:**

***Q3-3. Consider career stage in the transition probability, which could be estimated from the point at which an academic enters the dataset. There are likely to be varying transition likelihoods at different stages, influenced by non-academic (e.g. family) context.***

**Author’s response:**

***Q3-4. Consider cultural or linguistic ties between countries, such that to the extent that they influence the likelihood of transition of academics, potentially beyond that of migratory flows.***

**Author’s response:**

***Q3-5. Consider the wider set of features that might suggest attractiveness of a city to an academic. These could include well-established indicators of innovation or intellectual activity, such as patent production or book publications.***

**Author’s response:**

1. ***Suggested improvements***

***Beyond the methodological limitations and potential advancements suggested above, there are several suggestions relating to how to best contribute towards policy on academic mobility. These could be dealt with through a model of all bilateral flows, or through cases that cast better light on the major findings. These include:***

**Author’s response:**

***Q4-1. Consider how bilateral funding partnerships have supported growth in flows between countries.***

**Author’s response:**

***Q4-2. Consider the effectiveness of different policies (e.g. retention policies, mobility funds) to promote or inhibit international mobility of academics.***

**Author’s response:**

***Q4-3.Consider the role of major political events in academic mobility, such as Brexit and UK departure from Horizon, or the election of Trump (which was said to lead to US academics moving to Canada)***

**Author’s response:**