

Interactive comment on “Transnational mobility and the spaces of knowledge production: a comparison of different academic fields” by H. Jöns

H. Jöns

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First, I would like to express my gratitude to both referees for engaging with the paper and making some very helpful suggestions for clarification and improvement of the argument. In the following I will respond to the referees' comments point by point.

Referee #2 (RC = referee comment, AR = author's response)

a. RC “Firstly, the aim of the paper is depicted in a rather general way: “I explore the complex relationships between knowledge, mobility and space” (p. 81). It would be desirable to be more precise, which conceptual gaps exist in the discourse and which specific contribution the paper wants to make in order to fill these gaps. It is difficult for the reader to estimate, whether or not it is appropriate to address this issue by

“looking at the ways in which geographical patterns, motivation for and outcomes of transnational mobility vary among different academic fields” (p. 81). This decision, though not implausible, appears a bit arbitrary.”

AR I would be glad to elaborate on this in the revisions. An important starting point for my argument is the question of how geography matters in the pursuit of science (Jöns 2003, 473). This question was discussed at the Hettner Lecture 2001 with David Livingstone in Heidelberg (Livingstone 2002). During these discussions it was particular Richard Powell’s interest to go beyond the mere statement that geography or location matters for scientific practice and instead take up the question of how this is the case (see Powell 2007, 321). Based on 61 semi-structured interviews I had conducted with US senior scientists working in different scientific fields in 1999, I began to explore this question from the perspective of the travelling scientists. First, I asked myself: How do the scientists’ needs ‘and possibilities to reach out from a place of knowledge production in order to communicate, to interact and to mobilise new resources’ (Jöns 2003, 473) vary when comparing different scientific practices? In the SGD paper, I explore this question by examining geographical patterns of academic mobility to Germany and the motivations for these. I would argue that one needs to address the geographical patterns before studying the motivations, and thus these two aspects are central to the paper. Second, I asked myself: “How did the different geographical context at the host compared to the home institution matter in the researchers’ work and interaction during their visits?” (Jöns 2003, 471). There are at least two possibilities to address this question when comparing different scientific practices. First, one could look at the type of interaction between the visiting researchers and their hosts at the guest institution. Second, one could look at the collaborative cultures expressed in resulting publications. In this paper, I chose to compare ‘the visiting researchers’ collaborations in Germany before and after their Humboldt stay’ (p. 93). Therefore, the third empirical section looks at these particular outcomes of academic mobility.

b. RC “Secondly, the paper oscillates between an exploration of the field and hypoth-

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esis testing. The former impression results from the relatively open research question and the conceptual contribution at the end of the paper. The latter impression stems from the quantitative approach, which applies predefined categories (for instance the rather traditional distinction between natural sciences and arts and humanities). As it is, the paper is neither purely explorative nor does it test hypotheses in an appropriate way, rather, both intentions seem to be contradictive.”

AR The paper builds upon an actor-network based approach to scientific practice and interaction. This has at least two important methodological consequences. First, empirical research should be unbiased in regard to predefined categories. Second, this means that one tries to account for as many influencing factors as possible without stating a priori which realms are more important than others (e.g., human/nonhuman, science/social context). While the referee identified an apparent contradiction between using actor-network theory and a quantitative approach, I would like to outline how I solved this problem in previous research. The quantitative approach enables to identify typical characteristics of actor-networks and to quantify their meaning and their long-term effects. This requires the definition of pre-defined categories. However, as I wrote in my paper, the categories applied in the survey were ‘constructed out of the rich qualitative data’ (p. 82) and thus can be regarded as the result of following network building processes without many presumptions. In my understanding, actor-network theory does not reject predefined categories per se but tries to provide a framework for understanding how these categories are constructed. Being aware of this heuristic quality of categories and by constructing categories out of individual experiences, I think it is possible to combine a quantitative analysis with an actor-network based approach in order to learn something about common and different features of a great variety of actor-networks and practices (see Jöns 2003, 69-75). On the one hand, I indeed use the rather traditional distinction between natural sciences and arts and humanities, but the argument of the paper is that this distinction is less helpful to understand the geographies of different scientific practices than the differentiation between different methods (p. 93, figure 5). This is why figures 5 and 7 display the data on empirical,

experimental, theoretical and argumentative-interpretative work (p. 118). Thus, I would argue that comparing the meaning of different quantitative categories subscribes to an explorative quantitative approach, and I agree that it would be important to strengthen this argument in the revisions.

The explorative approach applied in the paper is in fact based on the idea of grounded theories as outlined by Glaser and Strauss (1967). Therefore, the aim is rather to start with open research questions (pp. 82-85 and response a. above) and generate hypothesis out of the empirical material rather than to test hypothesis as one would do in a rigid quantitative research paradigm. While grounded theorising is often related to a qualitative approach, Glaser and Strauss argued that it can also involve quantitative methods, which resolves the contradiction identified by the referee between open research questions and generation of theory on the one hand and the application of quantitative analysis on the other:

[T]here is no fundamental clash between the purposes and capacities of qualitative and quantitative methods or data. What clash there is concerns the primacy of emphasis on verification or generation of theory - to which heated discussions on qualitative versus quantitative data have been linked historically. [...] Although the emphases on qualitative data is strong in our book, most chapters also can be used by those who wish to generate theory with quantitative data, since the process of generating theory is independent of the kind of data used" (Glaser and Strauss 1967, 17ff.).

c. RC "For a hypothesis testing approach, the paper does not give sufficient account on the hypotheses, which have guided the quantitative survey. Why did the author investigate the issues presented in sections 3, 4 and 5? What were the expected results (in relation to hypotheses that circulate in the scientific discourse, or in relation to own hypotheses developed elsewhere) and in how far do the empirical findings deviate from these expectations? How can these deviations be interpreted? Why are the interrogated categories adequate indicators? And what exactly do they indicate?"

AR In my responses to previous points, I outlined that I am following an explorative approach that is inspired by grounded theorising and actor-network theory (see b.). I also explained why I investigated the issues presented in sections 3, 4 and 5 (see a.)

d. RC “For a primarily explorative paper, however, it remains unclear, how it will be possible to raise relevant information with a quantitative empirical approach and a standardised questionnaire that allows to extend one’s conceptual ideas about “scientific cultures” and “scientific practice”. Furthermore, would it not be more appropriate for an explorative approach to inductively create a typology of different practices (such as: strongly vs. weakly contextualised practices; collaborative work practices vs. individualistic scholarship) that crosses the familiar categories, instead of collecting data on predefined categories? For instance it might be possible, that work practices in some humanities and some natural sciences are surprisingly alike, e.g. anthropologists, geographers and some biologists are extremely field dependent. Other (sub-)disciplines, which at the first glance appear to be closely related, might differ considerably concerning their practices, e.g. theoretical vs. experimental physics.”

AR I hope my approach became more transparent in my responses to points a. and b. However, I agree with the referee that the explorative approach would need elaboration in the revisions. I also agree that typical variations of spatial relations cross familiar categories, and this is what I in fact wanted to point out (p. 93, figure 5, see point b). In figure 5, for example, one sees that theoretical approaches in all three disciplines have something in common with each other (e.g., they are the least place dependent), but they also have a lot in common with other types of work within the classical disciplinary boundaries. I agree that the whole story is much more complicated than I can discuss in this paper. For example, the questionnaire did not differentiate between just three realms of natural sciences, engineering and arts and humanities but listed 14 fields for which I could analyse the spatial relations of experimental, theoretical, empirical and argumentative-interpretative work. However, in this paper I am aiming at the big picture, and I think that the quantitative data nicely highlights some differences and

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typical features regarding varying spatial relations that are worth to be presented in this fashion. When doing the data analysis, I was in fact surprised and very delighted that the empirical results of this survey fitted in so nicely with the two-dimensional matrix proposed on the basis of the 61 semi-structured interviews and the related thick description of research practices on sabbatical in the previous project (Jöns 2003, 420-428).

e. RC “These two general problems lead to a range of related more specific problems. In the present form the empirical sections and the conceptual consequences remain more or less detached from one another. The conceptual ideas are not “conclusions drawn from empirical findings” (p. 95). The first (degree of materiality) and third dimension (degree of abstraction) of the matrix of spatialities of scientific practice already existed before (p. 103/lines 4f.), so, quite obviously, they do not depend on the empirical survey presented above.”

AR Well, I would be happy to better connect the empirical and conceptual sections in the revisions. However, this very connection is made in figure 7, isn't it? Although a previous matrix generated from other empirical findings was existing, I did not manipulate the quantitative data to fit in, it just did so. In other words, one could argue that the quantitative data validates the two-dimensional matrix suggested on the basis of the 61 qualitative interviews. This is because the relevant categories constructed out of the experiences of 61 senior scientists were integrated into a new questionnaire and sent out to other scientists world-wide that had spent a sabbatical in Germany. The fact that the responses of almost 1,900 scientists fit into the previously constructed categories and enable the construction of a third dimension, which helps to better understand varying spatial relations and collaborative cultures in different scientific practices, does, at least in my view, create no problem, but rather provides some kind of validation of these ideas in a different contexts of late 20th century cross-boundary science. Is this not the kind of validation that complementary methods may achieve?

f. RC “The second dimension, which is the main new contribution of this paper, how-

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ever, is merely completely deduced from the literature. Only incidentally (on p. 98 there is a reference to figures 3 and 5) it is also illustrated with the empirical data presented above.”

AR I do not quite agree with this observation. First, figure 7 provides a compelling link between empirical findings and conceptual dimensions. Second, the meaning of the first and second dimensions is discussed in regard to the place-dependency of scientific work and additionally validated by describing their meaning for collaborative cultures (pp. 99-100).

g. RC “The empirical sections, rather than selectively exploiting the data in favour of a range of research questions, summarize the data in an own logic. As a result, it is hard to keep all the facts presented in mind, as one does not know, which of them will be relevant for what purposes and what they do indicate. Consequently, the interpretations provided in these sections are not always convincing, a high diversity of topics is addressed, often factors from outside the survey explain characteristics in the data (e.g. the influence of the collapse of the former Soviet Union on scientific mobility from Eastern Europe). Some facts are commented others are only depicted. In short words, the facts are presented comprehensively as if they were interesting in themselves, however, they remain too little integrated into the conceptual endeavour of the paper.”

AR This might be the result of the fact that I discussed several influencing factors on the motivations and outcomes of academic mobility in the first part of the paper. As I elaborated in my response to referee #1 (‘Final author comments, part one of two’ , Heike Jöns, 06.08.2007, 18:47, b.), the second part of my paper focuses on a particular meaningful influence on the geographies of science, namely the nature of their work. This does not imply, however, that other aspects were not meaningful, but it turned out that the spatial ontology of the constitutive element of scientific work shaped the place-dependency of the research project and the collaborative patterns of the visiting researchers more than other factors. It might be worth trying to elaborate on this in the

introduction without changing the structure of the paper too much.

h. RC “I recommend, the author should exploit her quantitative data more systematically and more selectively by testing the validity of her model. The three dimensions are already convincingly deduced from the literature (which is a conceptual merit in itself), however, the causalities suggested by her model (e.g. on page 102: “the more immaterial and standardized the research practice, the lower is the spatial embeddedness”) still need to be tested empirically. This presupposes an operationalisation; to develop a number of verifiable hypotheses, to link the conceptual cornerstones (e.g.: degree of materiality; degree of standardisation; degree of abstraction; degree of spatial embeddedness) of the matrix with applicable indicators, to correlate these indicators with one another in order to test the assumed causalities.”

AR As I followed an explorative approach in this paper (see point b.), I also do not agree with the referee that the three dimensions have to be tested empirically more thoroughly. First, I provided evidence by discussing the influence of country of origin, broader field and type of work on place-dependency and collaborative cultures. Second, the quoted causality can be clearly identified in figure 7. However, what I have done and not integrated in the paper yet are statistical tests. It would be no problem to integrate these findings into the paper. The tests show that the observed differences are all statistically significant.

i. RC “Furthermore, the developed matrix is very instructive for future research. However, compared to the former version (elaborated in Jöns 2003), the three dimensional matrix is not only more complex, but unfortunately also considerably more complicated. The more important it becomes, to keep the argument as simple as possible.

AR Yes, this is why I used broader categories than the data would have allowed for (see point d.).

h. RC “One problem is that the three dimensions seem to be not independent from one another. For instance, standardisation and materiality on the one hand represent the

first and second dimension of spatial embeddedness of knowledge. On the other hand they also partly contribute to the grade of abstraction, which is the third dimension: “researchers perform consequential mediations from matter to form involving a trade-off between multiplicity, materiality and locality and the gain of standardisation, immateriality and universality” (p. 100, own emphasis). This would mean, that an increase in the grade of abstraction is closely associated with a decrease of materiality (and an increase of standardisation). I propose to pronounce stronger what characteristics are distinctive for each of the elaborated dimensions. Further, to enhance readability, I recommend to use less synonyms (e.g. on page 97 im/materiality is used synonymous with more or less symbolic content).

AR Yes, thank you for this suggestion. I would be glad to clarify the meaning of the three dimensions “materiality”, “standardisation” and “abstraction” in the revisions. I think it is indeed correct, as Latour (1999) argued, that different stages of research work imply different degrees of abstraction and thus an alteration in the spatial relations of the constitutive elements that can, among other things, be described in a change of materiality and standardisation. This strong interrelation can be explained by the simple fact that several scientific practices built upon results made in other fields or times. Thus, scientists often use the ‘abstracted’ results of their colleagues in other fields or other times for constructing their own arguments. In this construction process, however, they also start with a lot of heterogeneous resources that show a higher degree of materiality and standardisation than the ‘abstracted’ end products in form of new facts and artefacts.

j. RC “Finally, “context dependency” is here understood as the dependency of a scientific practice on the German national context. However, in my estimation, the idea of context in scientific practice is much richer and also more specific than that (the author refers to the German language and to culture in general). For instance the material and infrastructural context of scientific practices may vary in several respects: every researcher needs a computer with an internet connection and a library, only few need

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a laboratory with expensive devices. Some scientific practices need the stable context of a laboratory, while others work directly in the field and so on.”

AR Yes, I would be happy to clarify my use of the term ‘context-dependency’ in the revisions. However, in this particular question that I am dealing with, it was asked: “Could you have carried out your research project in any other country than Germany?”. This already specifies the nature of the context in question, namely everything that is relevant for a particular scientific practice (in comparison, for example, to a context responsible for the personal well-being of the researcher such as the presence of relatives and friends, which might be important for the reason why a researcher spends a sabbatical in Germany but not important for conducting a particular research project). The differences the referee describes in regard to different material and infrastructural contexts show why the spatial relations of research projects and the collaborative cultures of visiting researchers vary in different fields and types of work: for example, while German culture and language may be important for the philosopher, it may not be important for the physicist studying the collisions of heavy ions, which might be done at DESY in Hamburg, CERN in Geneva or RHIC in Brookhaven (if a particular experiments could be run at all three facilities).

k. RC “Minor remarks: - The term of “field/field-specific” (pp. 79, 81, 92, 102, 103) does not become clear. - The issue of face-to-face vs. online interaction raised at the beginning of the conclusions has not been elaborated in the paper. It does not summarize the argument. - You hint at “important conclusions for science policy” (p. 103), which can be drawn from your results, however, you do not give any examples. - The methodological section (pp. 81f.) does not discuss any weaknesses of your approach. This could eventually be supplemented. - Sentences like: “The geographies of academic mobility “had been structured by political, socio-economic, cultural and intellectual relations” (pp. 101f.; also p. 79/lines 9-12; p. 88/lines 17-20) are so general, that they are always true. In my estimation they are dispensable. - Technical corrections: trans-national instead of transnational

AR I would be happy to correct these deficiencies in the revisions. However, I think that the openness of the actor-network based approach sometimes requires general statements such as the one quoted. This is because there are several influencing factors on academic mobility that need to be considered and gain a different meaning for different people and practices. In this paper, I discussed a great variety of these influences but concentrated on particular meaningful influences in the latter part of the paper that resulted from the nature of different scientific practices.

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