Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

The paper is an important contribution to the study of Knowledge Transfer but will also be of general interest to social scientists and historians interested in the growth and development of scientific fields and epistemological communities. It really is a very interesting paper and I enjoyed reading. In it, the authors provide a strong theoretical and empirical account of the emergence of networks of research and researchers organized around questions of knowledge transfer. Inevitably, the paper must deal with the diffusion and institutionalization of EM Rogers' Diffusion of Innovations theory and it does so as a paradigmatic theory of knowledge transfer, evidencing this by means of bibliometric analysis. They also show how the field of diffusion studies has broken down into very specific fields of practice, and argue that these now include EBM. I'm not so sure about that and I will expand on this concern below.

There is no doubt that this paper should be published, and my criticisms are minimal in scope but are important nonetheless. There are three of them:

1. I do not believe that DoI qualifies as a paradigmatic theory in the Kuhnian sense that the authors claim. One of the things that distinguishes this paper from some other (recent) contributions to the latter field is the author's evenhanded treatment of different theoretical perspectives. In particular, the paper demonstrates the continuing significance of RK Merton's social theory and empirical research. But Merton's contribution to the field leads me to my first criticism of the paper which is the claim of Kuhnian paradigmatic unity for the field. I think when Kuhn was making the claim that the social sciences lacked paradigmatic unity he was doing so at a much higher level of abstraction than Rogers' theory operates at. It seems to me that Rogers Diffusion of Innovations theory is a strong contender for being identified as what Merton called a middle range theory. Accounts of the prehistory of DoI seem to reflect this see Elihu Katz's papers in Annals of the American Academy of Political and Social Science (599 November 1999 and 608 November 2006). There is no doubt that DoI has become the paradigmatic way to describe some social processes and also, and most unusually, has become a normative model for them. I think the evidence for its predictive power is much less limited since
as Rogers noted in his 2004 paper (ref 175) it is not often used prospectively. So this claim needs to be toned down a little. I think the authors need to make clear the limits on a claim of (i) paradigmatic unity, and (ii) on predictive power.

This does not undermine the authors' analysis of the importance of Rogers' work but in part the success of his theoretical model is due to (i) its assumptions being implicitly rather than explicitly formed with reference to higher level of social theory (structural functionalist analysis of social roles, action and institutions) and (ii) its limited focus on a single type of social process (social influence). Its implicit foundation in higher level sociological theory and its limited focus enabled it to survive the collapse of structural functionalist social theory in the 60s and 70s and begin to attach itself to constructionist theory in the 1980s, in time for the final 1995 edition. In other words Rogers was able to minimise the effects of changes in theoretical fashion over a period of 40 years by avoiding claims of paradigmatic status and general laws. Staying in the middle-range made his model highly transportable. As I say, this has important implications for claims about its paradigmatic status.

2. EBM is different in an important way from other domains observed in bibliometric analysis. There seems to me to be a difference between the diffusion of innovations (however this is framed) and EBM. Proponents of EBM often deploy DoI in the normative way that I have noted above, as a way of planning and organizing the implementation of an intervention. Trisha Greenhalgh's paper in Milbank Q two years ago is a good example of this. In this context I think that the term Knowledge Transfer describes what is happening in EBM but paradoxically weakens analysis of it. Theories like DoI seek to understand how social change takes place within specific networks and organizational contexts. But EBM seems to me to be about social control and constraint in other words it may be about *preventing* the diffusion of innovations by diffusing social controls. The connection between KT and EBM in this context does seem to me to have an important normative component, just not the one that is often proposed for it. Once again, this criticism does not undermine the authors' analysis, but it does need to be made clear that domains of activity actually relate to different kinds of political context.

3. it is hard to present bibliometric analyses of citations in ways that distinguish between different kinds of contributors to the literature. This is the final point which I wish to make, and it is about citations as an obligatory point of passage. They are certainly a resource, and an acknowledgement, and all of the things that this paper argues that they say they are. But a problem with quantitative analyses of this kind is that they do not always compare like with like. So, although Coleman is cited (and though he later became a major general theorist) it is because of a single diffusion study (which is now disputed - see Van den Bulte, C., and G. L. Lilien. "Medical Innovation Revisited: Social Contagion Versus Marketing Effort." American Journal of Sociology 2001;106, 5: 1409-35.), while Rogers is cited as an Authority. Thus Coleman is a general theorist, Rogers, is a specific theorist, but others perform other functions. Elihu Katz has been a great educator and promoter of theories for example others (e.g. Jeremy
Grimshaw) are not theorists at all but are “methodologists.” Grimshaw is important because he and others in his circle have integrated psychological theory into prospective experimental studies and have applied rigorous scientific methods to the business of theory testing in the wild. They have actively promoted both this approach and a specific body of psychological theories, partly I suspect, because they thought DoI inadequate.

The different roles of contributors to the field seems to me to be an important point and one that needs to be gently made in the text. Indeed, this becomes more important as time goes on, and many of the key writers in the fourth decade are writing much more procedurally and methodologically than writers in in the first decade did. In part this may be because EBM is primarily thought of in methodological terms by its practitioners, or it may be because at a macro-level procedure is more important when constraint is at issue.

The apparent continued dominance of Rogers in the 4th decade belies the theoretical fragmentation of KT, and especially the rise of competing psychological theories (especially the theory of planned behaviour) that have a much more limited scope. In fact, if this analysis is done again in ten years time, I would expect to see Ajzen and Fishbein appearing in the next bibliogram. Even though they are not in any way KT researchers, their theory seems to fit well with a medical model of behavioural change. As an aside, I found myself wondering as I reached the end of the paper about the importance of non-theoretical attributes of theories and their proponents. Rogers had a theory of diffusion founded of social roles and action, expressed this qualitatively as far as possible, built strong personal relationships and alliances. He treated graduate students and junior colleagues with real generosity of spirit. According to all who knew him, he was a really nice guy and good fun to work with. At the same time, James S Coleman developed a similar kind of theory using similar theoretical and empirical resources, but was rather more aloof and, I’m told, not an easy man to work with. So it might be that Rogers’ theory has triumphed over Coleman’s because of their different personal qualities and differential investment in the emotional labour of building and maintaining a community of theoretical practitioners.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

The list of experts in the field (Footnote 2) itself constitutes a kind of invisible college and they should be named in an appendix.

The author of ref 75 is more commonly cited as D de Solla Price (on this side of the Atlantic, at least).

References to different editions of the Diffusion of Innovations should be distinguished by edition number as well as year of publication (refs 93, 152 and 154).

References to Merton’s book (125 & 126) should be distinguished by edition if
they are not duplicates.

References to Thomas Kuhn (170 and 171) appear to be duplicates.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

Table 1 is hard to read. Is there a better way of presenting this information in graphic form?

Table 2 divides between Knowledge: Creation, Diffusion, Utilization and Science Communication. If these are the same journal then I think there is a strong argument for aggregating the two. Unless, that is, there was a profound change in policy about publication at the point which the name changed?

Table 3 shows most prolific journal by decade. I think there is a very strong argument for showing the date of first publication for each of these journals. For example Technological Forecasting and Social Change was first published in 1971, while Technovation was first published in 1981, but both published 23 articles in the period 1985-1994. Doing this will make the point about the diffusion of diffusion more strongly.

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.