

Author's response to reviews

Title: The intellectual structure and substance of the knowledge utilization field: A longitudinal author co-citation analysis, 1945-2004

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Author's response to reviews:

15 July 2008

Martin Eccles

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Dear Martin,

RE: The intellectual structure and substance of the knowledge utilization field: A longitudinal author co-citation analysis, 1965-2004 (MS: 9116368171750532)

We have uploaded the above named manuscript which has been revised.

We have also uploaded our response to the three reviewers' comments.

Thank you for your consideration of this manuscript. I look forward to your response.

Yours sincerely,

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REVIEWER #1 (Carl May)

Major compulsory revisions

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 authors' even handed 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.

Response: We are not the first to argue that DoI is a paradigm. It may be of interest to the reviewer that Rogers himself felt that DoI research “conformed to the Kuhnian notion of a paradigm” (Valente, p. 292; in Backer, Dearing, Singh & Valente 2005 “Writing With Ev, Words to Transform Science Into Action”, *Journal of Health Communication*, 10:289-302). Valente was talking about the evolution of an earlier historical piece they co-authored about early DoI research (Valente and Rogers 1995). Also, in the same piece, James Dearing says “For diffusion scholarship, Ev Rogers’s impact is perhaps well summarized by asking yourself what the diffusion paradigm would look like if he had never gotten a PhD at Iowa State. Would there be a diffusion paradigm?” (p. 294).

We agree that diffusion is a “theory of the middle range”. It relates mostly, but not solely, to the social aspects of diffusion, and is neither a micro theory, nor a “grand theory.” However, we believe that Rogers representation of it and his ability to shepherd it through a half century of evolution and (re)framing not only kept it fresh and at the forefront of accessibility and prominence, but also elevated it to paradigmatic status. To view DoI theory as representative of a dominant paradigm as illustrated is not incompatible with it being a middle-range theory. We argue that a theory that acts to guide and inform most of the research in a field is paradigmatic. The reviewer argues that it is the case that 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 and thus by staying in the middle-range make his model highly transportable. We do not disagree but neither do we view this claim to be inconsistent with DoI being a middle range theory. Rogers’ representation of DoI achieved this paradigmatic status we might counter by virtue of assuming such a “middle range” position.

We have clarified our intended meaning of “paradigmatic status” in the text. The citation maps show clearly that Rogers’ work formed a central intellectual nexus for DoI inquiry for over five decades. The maps do not show the context of citation, just the fact that citing authors cited Rogers’ work more than any other DoI scholar.

To validate what the reviewer is arguing would necessitate that we examine the ways that people, including the EBM people, have cited Rogers’ work, which is a fundamentally different type of analysis. To tease out the ways in which Rogers and the wider KU literature are “normative” for EBM would require looking at the context of citation, and we did not do that. We think that the EBM group draws on Rogers largely through or due to border authors such as J Lomas. Until Greenhalgh’s 2004 MBQ work DoI was not as apparent in the EBM grouping of authors. EBM is methodological and normative insofar as it is highly prescriptive and so it is not surprising that the core set of authors in EBM has not cited

Rogers work widely – as his work is descriptive and not as directly relevant to the EBM agenda. Nonetheless the maps clearly show that there are co-citation linkages to Rogers’ work – enough authors in our sample cited Rogers with some of the EBM people that there are clear links between the two.

From a science studies point of view, Rogers stayed successful because he kept doing the same thing repeatedly – he kept analyzing all new diffusion studies, in a wide range of fields, and he kept publishing new editions of his book approximately every ten years. People had to cite him, because he had analyzed all known work in DoI. While it may be true from a sociological perspective – that Rogers’ representation of DoI theory was successful by implicitly drawing on “higher level” sociological theory, and then attaching itself to constructionist theory, again, to validate this would require a content analysis of the contexts of citation. This goes beyond what we can claim based on our data. Our maps show that Rogers keeps getting cited, together with others on the maps, more than anyone else, decade after decade. People in a wide range of disciplines cite him, along with other work. To try to impute their motivation – that the theory is a middle range theory, that it keeps its alliance with sociological theory implicit rather than explicit – extends beyond what we can claim.

We have also edited our text to temper our claims re the predictive ability of DoI.

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.

Response: We agree, we strongly suspect that EBM (and its derivatives, e.g., EBN, EBP, etc.) create knowledge differently from everyone else on the map. These are highly normative and prescriptive practices emerging from disciplines concerned with criterion referenced practices. Their members tell clinicians and others what to do (best practices). EBM proponents often function at a meta-analytical level with strong emphases on syntheses of the scientific literature, to derive “gold standards” for treatment protocols. The explicit goal is intervention, to define the state of the art in terms of knowledge, and to change practice. Knowledge is not produced for the sake of knowledge. Their methods are different, and they proceed for the most part relatively atheoretically. As we understand it, EBM makes assumptions about what constitutes “good science” and then seek scientific studies that achieve this standard.

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.

Response: This criticism, while gentle, is in keeping with many of the negative reactions to EBM evident in the literature. These observations are at quite a high level of abstraction, and go beyond what we believe our data can speak to. However, it does constitute an invitation for further research in the field. It might even be possible to "prove" the claims by further bibliographic analyses, particularly the assertion that some innovations are prevented from diffusing – citations to those innovations would, we hypothesize, die out fairly quickly.

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.

Response: We have added a limited amount of text in the discussion in order to expand on our earlier somewhat tentative claims regarding EBM and its normative orientation.

We think that the reviewer is claiming that the relationship between KT and EBM is normative, but not in the sense of KU providing a paradigm that says "how" knowledge translation activities should be conducted, i.e., DoI theory does not provide a theory that is prescriptive of how to do KT in a normal [science] way. EBM is highly normative. In this sense, it is vastly different from anything else on our maps.

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.

Response: We do not understand this as a problem because the population of citing authors are the ones who have decided what goes with what. The co-citation maps are produced in effect by the people writing articles. We have a pretty good sense why Coleman popped back onto the maps, and it is consistent with the assertions above. The reviewer has a depth of knowledge of this field, but we believe that some of what he is asking us to do may extend beyond what ACA can provide:

"Because the data of ACA are merely noun phrases and associated citation counts, they produce history of the cliometric sort, which leaves out almost all the good parts, such as who had shouting matches, who slept with whom, and what

actually gave rise to the most significant work.” (White & McCain 1998, p. 327).

This passage remains in the manuscript for just this reason.

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 Dol inadequate.

Response: This is indeed correct, but again, it goes beyond what our data can support. We know which papers and books were cited, and with whom, but the citation maps cannot speak to why someone is or is not important. We can speculate, but not prove.

The point of these maps is to show who the population of citing authors cites together. The MDS program places nodes close together when authors are cited together frequently. When the nodes are far apart, authors are not cited together as frequently.

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 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.

Response: People are on the map together, but for very different reasons. It also takes more citations to get on the maps in the later decades than in the early ones. We argue that Merton may be less important to the field than the reviewer implies, because in the decade in the 1975-94 decade, he had only 25 co-citations (compared to 155 for Rogers).

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.

Response: We are not certain why the KU field is shrinking in the last decade. We re-ran the maps with 50 people on them, to check to see if this was an artifact of sample size, or if KU was being washed out by EBM. It is not an artifact; re-running the maps with 50 people showed that the domain of KU really is shrinking.

The reviewer argues it is due to theoretical fragmentation, and he may be right. It could also be as Kuhn says -- the proponents of paradigms eventually die out. Rogers has died, are the others getting older. Nonetheless Rogers really still is dominant in the 4th decade. If KU is finally fragmenting theoretically, this may be one of the reasons for the lower number of people in this domain. The continued dominance of Rogers is however real (reflected in actual co-citations) and not likely to wane because of theoretical fragmentation. Our maps do not (nor do we think they can) speak to theoretical fragmentation.

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 am 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.

Response: We have augmented this biographical detail in the discussion in response and because we believe it is relevant to the understanding of Rogers' dominance; in effect his personal biography embodied his theory. We have also added a limited amount of discussion framing this part of the discussion in terms of Knorr Cetina's discussions about practitioner's biographies "making" them into instruments.

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.

Response: These have each been rectified or completed in the text and/or tables, appendices.

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?

Response: This table has been deleted.

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?

Response: Since, as one reviewer pointed out, when Knowledge changed to Science Communication, it not only changed editorship but, in fact, the orientation of the journal and content as well – we have left it as it was in the original text and added a footnote.

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.

Response: Done

REVIEWER #2 (Katherine McCain)

Major Compulsory Revisions

1. The authors are mistaken about the content of the journal Special Libraries (in MS pg 19). Unless there were two journals by the same name, Special Libraries was a publication of the Special Libraries Association and the articles reflected the broad range of interests of this group of practitioners (and some researchers). This can be confirmed by examining the articles indexed in SSCA 1972-1974. Its appearance here is intriguing.

Response: We have noted this and modified the footnote.

Minor Essential Revisions

1. The title is a bit misleading, since the maps are based on a shorter time frame than the full set of article counts. The mapping only covers the 1965+ period although the lists span a longer time frame.

Response: Although our maps cover only 1965 forward, we did do some analyses of earlier decades. We have revised the title to “The intellectual

structure and substance of the knowledge utilization field: A longitudinal author co-citation analysis, 1965-2004”

2. How were the diagonal cells defined in the cocitation matrices? There are different views on this.

Response: We used a lower left matrix without diagonal values. White (2003) argues that “The strongest argument against treating the diagonal as missing data in statistical computation is practical: A number of computer programs useful to ACA will not run unless matrix diagonals are filled in” (White 2003, p. 1253 “Author cocitation analysis and Pearson’s r”). Our software handled a lower left matrix with no diagonal and we were in part guided by White’s comments as follows: “However, McCain (1990) argued that, in her experiments, Griffith’s ad hoc value sometimes produced results that were hard to interpret in a consistent way. She therefore decided to treat the diagonal as missing data and to rely on the rest of the counts over any two authors’ cocitation profiles as input to the similarity measure r . That is the procedure that has guided several other ACA studies since” (White 2003, p. 1253), JASIST, 54(13):1250–1259, 2003.

3. Most ACA maps are based on either Pearson correlations (White, McCain and colleagues) or Salton’s cosine (Leydesdorff’s and possibly Persson’s preference) – these measures tend to reduce the differences in scale of citedness and focus on pattern similarity = subject structure in the case of ACA and Journal CA at least. Authors with high cocitation pattern similarities are placed near each other, as a rule, and those with similarities to many authors placed in the central region of the map. This is interpretable based on subject or similar pattern relationships. The raw cocitation counts are more commonly visualized using a network analytic tool such as Schvaneveldt’s KNOT software (PFNets) which emphasize citedness and centrality in the network [1]. What is the effect of using MDS directly on the raw cocitation counts?

Response: We tested for whether the raw cocitation counts would have an effect by recalculating the maps using Salton’s cosine; there was no effect of using MDS on the raw cocitation counts. We have reflected this on page 17 of the text.

4. Non-metric approaches to scaling yield a higher % variance explained with a lower Kruskal Stress I (the measure of goodness of fit reported by the authors) than do metric approaches – which approach was used in the SYSTAT MDS mapping?

Response: We used a non-metric approach.

5. Reporting the % variance explained by the two-dimensional solution is important as well—a low % variance explained can suggest either the need for a higher dimensional solution or a problem in coding the data. In ACA studies, % variance explained below ~0.8 are generally not considered good except in unusual circumstances. What was the % variance explained in each map?

Response: Systat MDS does not calculate explained variance. Our stress values are low, as discussed in the paper; the reference we provide to validate that our

stress values are low is this reviewer's.

Discretionary revisions

1. The authors identified the author groupings in the maps based in inspection and the "highly cited/productive" lists rather than using cluster analysis or factor analysis (principal components analysis) or some other pattern extraction program independently on the data. This is reasonable but leaves out the opportunity for detecting additional interesting patterns and relationships and introduces a bit of additional subjectivity. The authors may want to comment.

Response: We didn't do FA or PCA because we did not use correlations in our matrices.

2. No explanation is provided for the need to keep the 1965-74 mapped author set to a count of 13 other than "lack of interpretability" with more (or fewer) authors. If the problem was noise introduced by large numbers of very small cocitation counts, that's one thing, but if there was structure that needed more exploration, then it would be good to present/discuss this. Reification is always a danger when dealing with these kinds of analyses and unexpected results can illuminate interesting aspects of a field.

Response: When we initially attempted the maps with 25 authors, they were indeed cluttered with "noise introduced by large numbers of authors with very low citation counts." Thirteen was a natural point to separate according to the frequency of citations.

Results & Discussion comments

3. With respect to Bradford Zones, while Bradford reported 3 zones, Brooks [2-3] later showed that the same article set could be partitioned into several different numbers of zones. How important is the existence of three zones?

Response: The existence of three zones per se is not central to the paper – we were illustrating that we could break the material into three zones as Bradford had suggested; and in particular were interested in the most prolific journals as Bradford suggests are in zone 1.

4. When Knowledge changed to Science Communication, it not only changed editorship but the orientation of the content as well. This likely accounts for its post-1994 disappearance from the core list.

Response: We have noted this in Table 3.

5. While White is certainly correct that the ACA maps are to be considered in the "light of the claims being made" it is also the case that these maps have been quantitatively validated as being quite congruent with data collected independently from scholars in the field (as is demonstrated in MS reference #22 and elsewhere).

Response: We have added this [italicized above] sentence in this section of discussion.

6. A limitation of the study that is not mentioned is the effect of partitioning on papers/authors/journals published toward the end of the decade. These have a lower likelihood of being cited than do those published toward the beginning of the decade. There's no good way around this without resorting to some kind of continuous interactive display, but it should be mentioned.

Response: We have mentioned this on page 29 of the manuscript, indicating it is not likely we have illustrated the state of the field past 2002; we expand that footnote somewhat using the suggestion above. We have prepared (pending acceptance of papers so that final changes can be incorporated) a technical report in which we can include material such as the Salton's maps, tables we have removed based on reviewer's requests and to shorten the manuscript. We have noted its availability in the manuscript

REVIEWER #3 (Thomas Valente)

1. First, the authors seem to neglect one area of diffusion research that might warrant comment or consideration. A lot of research on diffusion has been conducted about methods and models of diffusion research. By that I mean, work on using mathematical functions to fit diffusion data and social network research to understand how diffusion spreads via person-to-person communication. This occurred to me when seeing Mahajan and Peterson and Mansfield listed as technology transfer scholars. A major contribution of these authors, was to show how to fit mathematical models to diffusion data and many scholars have debated different modeling techniques. In a related vein, much of the citation to the Coleman Katz and Menzel (1966) study can be traced to Ron Burt's efforts to uncover these data and make them publicly available. So I'm not sure how it applies to the current narrative, but it seems to warrant comment.

Response: The points are well taken and we have made minor modifications the text on pages 25-26 to include reference to the mathematical modeling of these authors.

2. Second, the authors need to be clear that the data from this analysis are based on co-citation data not citation analysis. In that sense, the data are somewhat under-analyzed in that it is really a tallying of citations not who cited whom. The MDS plot is simply a graphical display of the data and are not so informative as such. I think this is fine, but I think authors should indicate why they do not analyze the data as a citation network, whether such analysis is possible, and how it might differ from the present analysis.

Response: We have attempted to be clear that we did a first author co-citation analysis (ACA) tallying co-citation by first author. That is, first authors who were cited together by the population of citing authors. The MDS plots place authors who are frequently co-cited closer to each other than those who are infrequently

co-cited. Our understanding of the method is that the closeness of nodes suggests conceptual similarity among the first authors co-cited. We do recognize that authors can be placed close together for other reasons, such as in the case of a major controversy between two “giants” in a field, who then get co-cited by people who subsequently write about the field. Our matrices include the lists of who is cited with whom; the diagonal would be an author’s citations with themselves. The MDS program puts nodes closer together, or further apart, based on the frequency of their co-citation explaining why authors move about from decade to decade. We cannot analyze the data as a “citation network” because they do not in fact represent a citation network. So while the data may be under-analyzed from the perspective of citation network analysis, they are not under-analyzed from the perspective of ACA – to analyze more than we have done would be we argue in danger of over-interpreting our data.

We have attempted to be clear in the text that we have undertaken an examination of intellectual history not social network history. Our understanding of co-citation analysis in this paper is aligned with information scientists Small, White and McCain. Our belief that co-citation analysis can be used to represent the intellectual structure of a field originates with White and McCain’s work. White and colleagues argued that co-citations reflect intellectual structure more strongly than they reflect social structure. The co-citation patterns of a global group of scholars with varying degrees of social ties shows that citation patterns do not follow friendship or acquaintance ties: “Although cocitation may well have led to social or collegial relationships in the past – as cocited authors notice each other and develop contact through conferences and correspondence – it is the intellectual affinity reflected in cocitation that the regressions point to, and not the social ties. As a direct effect, it is intellectual affinity, what they know, that matters and not social ties, who they know” [White et al 2004 , p. 125].

3. Third, in one sense authors attempt to treat knowledge utilization, technology transfer, and diffusion as different labels for the same thing. Another view is that they are different activities and processes. I, and probably others, have made figures depicting how these terms relate to one another and have different foci. So it might warrant some presentation on how these domains relate to one another from a substantive perspective rather than being united by having 2 common citation to diffusion of innovations. Indeed, there may be other substantive areas that would also show overlap.

Response: These areas (knowledge utilization, EBM, technology transfer) are we believe all based on the idea of solving social problems with knowledge – the problems are different, the knowledge is different and the audiences are different, the modes of transfer may be different – but at core address a similar problem. The maps we have argued illustrate that the population of citing authors in fact treat the domains as separate. So while an expert in the field may readily see substantive similarities – the objective of this analysis was to see how the people using the publications in the broad KU field use them together. We have incorporated additional comments on page 5.

4. Fourth, I think the paper is a bit long and suggest that authors could reduce

some sections. Pages 9-13 can be reduced dramatically. Table 1 can be eliminated as it is impossible to read and really repeats what is in table 3. Tables 4 and 5 can be dropped as they add little to the paper.

Response: We have removed Table 1 and it will be available in the technical report. We elected to retain (old) tables 4 and 5 (now 3 and 4) in order to not have to increase text to explain them and the accompanying sections of the paper. We have made some reduction in pages 9-13 but did so in a limited way as the readership of this journal will generally be unfamiliar with this research method and to enable the readership to better assess the merits of the paper.

5. Fifth, authors repeatedly refer to Rogers' diffusion of innovations theory. This is incorrect. Rogers did not create the diffusion model and did not add any components to its formulation. Crane and Valente and Rogers show that the Ryan and Gross publication formulated the diffusion model. By the mid-1950s a cadre of rural sociologists had filled in the major elements. Lionbergers' 1960 "Adoption of new ideas and practices" book contains most of the elements of the diffusion model. Thus it is inaccurate to say Rogers' diffusion model. It is more accurate to say the diffusion model elaborated by Rogers or simply the diffusion model. Rogers' contribution was to see that diffusion research was being applied in many areas and comprehensively review those studies.

Response: This point is well taken. We have gone through the manuscript and corrected our text to reflect this. In particular we have inserted a footnote (#19, pg 40) to elaborate this point using the points offered by the reviewer.

6. Finally, I was not entirely clear on what recall and precision measured. Can you elaborate on this?

Response: This is elaborated on page 14-15 of the text.