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”The Social World of the Network”: A Reply to the Comments

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“The Social World of the Network”: A Reply to the Comments

by Nick Crossley

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I would like to begin by thanking the respondents to my paper. They have been generous in their support and gentle in their criticism. They have also raised a lot of points; too many for me to address fully in this brief reply. I will try, however, to address some of the main critical points raised.

There was a general feeling that I had caricatured the SNA tradition and underplayed the role of qualitative and mixed method work in its history. I accept this, excusing myself only by noting that my representation was rhetorical and intended to provoke debate. José Luis Molina raises a more serious point when he suggests that some of the flaws I have associated with quantification may derive rather from specific individual applications of SNA or more general trends in its usage unrelated to quantification as such. This point is important and deserves further consideration but I do not think (and do not think that he thinks) that it detracts from or negates the central thrust of my argument as it applies to only certain of my claims. The quantitative/qualitative interplay is only one of a number of issues that we might wish to reflect upon at this juncture but it is an important one all the same.

I would also say, in my defence, that my paper was aimed more at the future of social network analysis than at its past. I noted the considerable quantitative advances in SNA in my paper (not, as Deirdre Kirke suggests, implying that they had pushed qualitative work out so much as that they had not been matched on the qualitative side, by qualitative researchers) but this is only one aspect of what looks like being a much bigger development. SNA now sits alongside a broader “network science”

whose contributors are physicists, computer scientists and economists as much as sociologists and anthropologists. This is an important and exciting development which has much to offer but it also threatens the important qualitative and indeed social scientific thread in SNA that the responses to my paper identify. SNA is and has been a genuinely interdisciplinary project, with mathematicians and social scientists each keen to learn from and work with the other. The validity issues referred to by Chris McCarty have been kept in the foreground by all parties, as SNA has been conceived as a social science which explores patterns of connection between sentient beings whose ties are mediated by thought, emotion, interpretation etc. It is far from obvious that the new network science shares these priorities, however. Its main protagonists and particularly its popular advocates certainly use social data and seek to make claims about the social world but they often show little interest in social scientific ideas and have little comprehension of the concerns for meaning and validity which are common place within SNA and social science more widely. In this context there is a need to make explicit and discuss some of the features that distinguish *social* from other types of networks and some of the methodological precautions and practices that these differences necessitate. A discussion of mixing methods and incorporating qualitative dimensions is not all that is needed here but it can play an important part.

The need for this debate is further occasioned by the availability and possible future availability of many new sources of relational data. The archiving of transaction data from mobile phones, e-mail, Web 2.0, credit cards, etc., and the desire to have them analysed by those who gather them, opens many new avenues for network scientists. These are genuine opportunities, to be welcomed, but they too give social network analysts reason to think seriously about the *social* aspect of social network analysis and to raise and discuss the abovementioned validity issues. The transaction data potentially offer a fascinating insight into human social life but only if explored by way of a genuinely *social* network analysis; that is, a network analysis sensitive to the way in which such media are used, their meanings for their users and so on. Qualitative methods have much to offer here, if run alongside quantitative methods. Indeed they are necessary. Network scientists will be able to conduct very reliable studies of the network structures evident in these new data but a properly social network analysis will want to know what significance to attach to such structures and answering that question will entail a qualitative engagement with the communicative practices involved.

Of course we won't be able to conduct open-ended interviews with everyone involved in these mediated networks. They will be huge. Deirdre Kirke's reflections on network size are pertinent here. However, I am less pessimistic than her concerning the implications of network size for mixing methods. There is no reason why we

could not sample parts of a network in order to elucidate the issues that are better accessed by qualitative means.

Moving on, I agree with Deirdre Kirke that SNA affords us fascinating insights into “social worlds.” That is one of the things that attracted me to the method in the first place. Relations and networks are an important part of what social worlds are. My point, however, was that social relations between actors become entangled with other things in the context of social worlds; things that we cannot afford to ignore if we want to understand networks and *a fortiori* the wider worlds of which they form a part; and things to which qualitative methods afford us better if not exclusive access: e.g. social practices, rituals, stories, meanings etc. Social networks are embedded and we can better understand them if we understand their embedding, which qualitative analysis helps us to do.

In my gym work, for example, I was interested in the rituals that brought people together such that they began to form networks; rituals which also revived and transformed ties over time. And in more recent work on the networks of UK suffragettes I have been similarly interested both in the bonding rituals which generated *esprit de corps* and strong affective bonds between the suffragettes and also in the meanings of those bonds. Suffragettes who were imprisoned for their activism were revered, for example, and there was a special bond between those had been imprisoned, sometimes even if they had not been imprisoned together (although that was especially important). These meanings complicate the standard network theories of writers such as Coleman [1988], who argue, for example, that closed and dense networks are most conducive to the development of solidarity and self-sacrifice within social movements. The importance of closure, I suggest, varies with the meaning of the relationship, and the meaning of the relationships varies to some extent in accordance with the practices through which they are formed. Coleman is not wrong but his thesis can be refined by closer attention to phenomena (e.g. meaning and ritual) that come to light by means of qualitative analysis.

We may close the circle by noting that the practices, meanings etc. referred to here form through interaction within networks – networks embed practices as much as they are embedded by them – but that does not alter the fact that social movement networks and the worlds of which they form a part are better understood in context, in their interconnection with practices, meanings etc. And, to reiterate, qualitative methods afford us relatively unique access to those practices, meanings, identities, stories and, indeed, to many of the mechanisms and processes which are both produced by and productive of social relations and networks and the ground level.

This point also allows me to touch upon two points on which I disagree with Chris McCarty. Firstly, although I agree with him that qualitative research is some-

times a matter of finding out about social worlds by asking the people involved I do not believe that this is always what it is about. It is often about observing people, in the widest possible sense of the world, and identifying aspects of what they say and do that they have not noticed for themselves. Their meanings, definitions and understandings are important but not always in the ways that they themselves recognise. In this respect, to pick on his discussion of the Krackhardt paper, I agree that it might have been futile for Krackhardt to seek to resolve his problem by asking his respondents what they think is wrong, but I disagree with the implication that this is the extent of the potential for qualitative analysis in this situation.

Furthermore, following on from this, qualitative research might actually be used to correct or at least problematise the biases that we know, from well established SNA studies [Knoke and Yang 2008, 38-41], affect survey and interview data. Some of the methods of data gathering more usually associated with qualitative research (e.g. participant observation and archival analysis) are less dependent upon the subjective recollections, impressions and understandings of the individuals analysed than the survey methods more usually associated with quantitative methods, which, in the final analysis, are entirely dependent upon what respondents understand and say.

My second disagreement centres upon the issue of generalisability, which Chris McCarthy associates with quantitative work. I have two points here. Firstly, although qualitative analysis does address particularity and that is important, it can and should equally engage at a more abstract and general level. My work on punk in London and Manchester is not just about punk in London and Manchester. It seeks to identify mechanisms, practices and processes which are potentially relevant to an understanding of the networks involved in many music scenes and, indeed, to collective action more generally. I have found commonalities in my work between punk and suffragette worlds, for example, and between both and other social movements and forms of collective action. Their use of rituals and symbols is similar, for example, as are their practices of demarcating group belonging and collective identity. Within the particularity of each we find evidence of the (potentially) generalisable. Of course we don't know how generalisable it is without further analysis but that does not mean that it is not generalisable and, as I note below, that is a limitation which qualitative analysis shares with much quantitative SNA.

Elisa Bellotti's reference to Goffman, Simmel *et al.* is pertinent in this context as these are writers who precisely sought to abstract generalisable findings about social interaction and relations from qualitative observations. Simmel's social forms, including "exchange," "conflict," and "domination," all of which are relevant to SNA, for example, are qualitatively informed reflections upon aspects of social life which, though historically variable, he believes to be generalisable within a given

society. He doesn't write about a particular instance of exchange or conflict but rather about exchange and conflict in general. He believes that the social world comprises such general (institutionalised) social forms and the implication of his work is that they can be accessed and analysed by qualitative means. Likewise Goffman's "total institutions" and "interaction rituals." The general often reveals itself in the particular but only to those who look in the open-ended way afforded by qualitative analysis.

This is a different sense of generalisability to the statistical sense normally associated with quantitative social science but, as Elisa Bellotti again reminds us, much social network analysis, at least when focused upon complete networks, is analysis of a single case and, as such, is similarly limited in its statistical generalisability. If our unit of analysis is the network then we have a single, non-randomly sampled case to infer from. If our unit of the analysis is the nodes of the network then we have multiple cases but again they are not randomly sampled and they do not meet the criteria of independence presupposed in many statistical tests.

Of course more recent statistical development in SNA seek to address some of these issues, and statistics, more generally, is a growth area within SNA. The basic point stands, however, that network studies often have more in common with the case study type of approach commonly associated with qualitative research and do not lend themselves straightforwardly to normal forms of statistical generalisation. Both study specific cases in detail and, at least insofar as our qualitative referents are the likes of Simmel and Goffman, both seek to abstract and analyse social forms. Qualitative analysis analyses the form of relations (exchange, conflict etc.); SNA analyses the form of the networks arising from such relations.

None of these points are intended as criticisms of quantitative methods in SNA. My call is for mixed methods, not qualitative methods *per se*, and my reasoning is that qualitative and quantitative approaches can each enhance the other. I would argue no less vigorously that qualitative work can often be greatly enhanced by way of quantitative input, indeed, that is what I argued in my paper.

The debate on mixing methods and incorporating further qualitative innovations within SNA is important. SNA is a living method. It is always evolving, growing and refining, and we need to be sure that the whole range of sociological possibilities plays into that dynamic. One of the most exciting things about SNA is that fact that it constitutes a space where the most quantitatively inclined of researchers can engage and dialogue with the most qualitative, with the majority, myself included, sitting somewhere between and drawing from both. My paper was offered in the spirit of furthering this dialogue and hopefully therefore making a small contribution to SNA's very healthy evolution. Thank you again to my fellow contributors for furthering it with me.

Crossley and , “*The Social World of the Network*”: *A Reply to the Comments*

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Abstract: This paper reflects upon the value of mixing methods, and in particular of integrating qualitative and quantitative approaches, in social network analysis. It argues that each has different strengths and weaknesses but that these are broadly complimentary, thus supporting the argument for their combined use. It also seeks to rebuff any claim that they may presuppose different and incompatible epistemological standpoints. The author supports and explores his claims by way of examples from his own empirical work.

Keywords: social networks, social worlds, mixed methods, qualitative methods, quantitative methods.

Nick Crossley is a professor and currently Head of Sociology at the University of Manchester (UK). He has researched and published in a number of areas but most recently has been exploring the role of social networks in relation to various types of collective action. His latest book, *Towards a Relational Sociology*, will be published by Routledge in August 2010.