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Impact of methods for reducing respondent burden on personal network structural measures

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Abstract

We examine methods for reducing respondent burden in evaluating alter–alter ties on a set of network structural measures. The data consist of two sets, each containing 45 alters from respondent free lists: the first contains 447 personal networks, and the second 554. Respondents evaluated the communication between 990 alter pairs. The methods were (1) dropping alters from the end of the free-list, (2) randomly dropping alters, (3) randomly dropping links, and (4) predicting ties based on transitivity. For some measures network structure is captured with samples of less than 20 alters; other measures are less consistent. Researchers should be aware of the need to sample a minimum number of alters to capture structural variation.

Keywords: Personal networks; Structure; Respondent burden

1. Introduction

Increasingly researchers who study personal networks are interested in collecting the data to calculate *structural variables*. By structural variables we mean measures that rely on the pattern of relationships within a network. These measures include network density (the most commonly applied structural measure for personal networks), centrality (degree, closeness and betweenness), centralization (degree, closeness and betweenness), components, core/periphery and isolates. These are in contrast to *compositional variables* that summarize the characteristics of alters within the personal network; such as the proportion of the network that are women, smokers, or family, the average age or the average intensity of the relationship between the respondent and their alters.

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Of course the problem with collecting data on personal network structure is the issue of respondent burden. An adjacency matrix for a personal network requires the respondent to assess some portion of the ties between their alters. This task grows geometrically as the number of alters increases. If we make the assumption that the best assessment of personal network structure can be achieved by having the respondent evaluate all possible alter–alter tie evaluations, a network of 10 alters requires 45 evaluations and a network of 50 alters requires 1225 evaluations.

We have two goals for this paper. First is to determine which method for reducing respondent burden best approximates the structural measures from the unabbreviated network. Second, using this method, what is the minimum number of alters necessary to approximate the unabbreviated network. We approach this problem empirically using two datasets.

2. Background

Unlike whole networks where the data for an adjacency matrix are either collected from network members through survey, observation or secondary data, personal network structural data are collected from respondents by asking them about the ties between their alters. Whole network data collection is therefore high on researcher burden, and low on respondent burden as the task of collecting tie data is distributed across network members who the researcher must observe or interview individually. In contrast, personal network data collection is low on researcher burden and high on respondent burden as the respondent provides the researcher with all information on the ties among their various alters. This is a key difference. The problems associated with sampling and missing data in whole network analysis stem from the inability of researchers to interview or observe network members. For personal networks, alters and ties are missing because respondents either did not recall them or were not asked about them in such a way as to fully capture the network structure. Both approaches result in adjacency matrices, and certain sampling issues affect these matrices similarly. Therefore, much of the literature on sampling and missing data in whole networks is relevant to personal network research.

Friedkin (1981) showed that the simplest network structural measure, network density, was sensitive to network size—as networks grow in size, density declines. He demonstrated the importance of normalizing social network metrics when comparing networks of varying size. Taken another way, he confirmed that if something less than the entire network were measured, the most fundamental structural measures would be affected. Galaskiewicz (1991) tested the effects of different sampling techniques on point centrality in whole networks. As one would expect, he found that the more ties sampled, the lower the error on the estimate of point centrality, regardless of network size. Anderson et al. (1999) considered network size and density as exogenous conditions of network data collection and examined how size and sparseness affected other network metrics (degree centralization, betweenness centralization, H–F hierarchy, Krackhardt hierarchy, connectedness and efficiency). They found that network metrics are affected by size and density, but that the pattern of the effect is not the same for different metrics. They proposed a test to determine if the observed values of the metrics were significantly different from what would be expected from the distribution given a specific network size and density. Frank (2002) measured the effect of different sampling strategies on network centrality. Using random graph models he showed that sampling vertices versus edges and the use of snowball sampling will result in different probabilities of selection for nodes with different centralities. A similar approach was used by Hoon Lee et al. (2006). Costenbader and Valente (2003) used a bootstrapping method to test the effect of non-response on eleven centrality measures. By successively dropping nodes then correlating the resulting centrality scores with the original, they found differences in the network structural metrics. In-degree was the most stable as 50% of the nodes could be dropped and still correlate at .90 with the whole network. More relevant for personal networks, asymmetric networks tended to be more stable than symmetric networks (personal network structural data are typically symmetric). Overall, most of the measures tended to degrade after 50% of the nodes were dropped. Robins et al. (2004) developed a set of methods that corrected for missing data from whole networks. These included re-specifying the boundary to include only those mentioned, assuming reciprocity, and an exponential random graph approach. They found that there can be a big effect to leaving out structurally important people. Kossinets (2006) also looked at the effects of different sampling strategies on whole network data, particularly non-response versus boundary specification issues. As with other studies he modelled the effects of random removal of actors, ties and contexts, as well as methods associated with reciprocation. He found that for whole networks with reciprocal responses, response rates of 70% were tolerable. However, focusing on non-response over boundary specification may severely impact network metrics. Borgatti et al. (2006) examined the effect on centrality of distorting a network by removing nodes and edges. They found that, while removing nodes and edges affected centrality scores, it affected them in more or less the same way. Accuracy declined monotonically and predictably as error increased. Node removal error was more forgiving than edge removal error.

To summarize, the study of sampling and missing data in whole networks suggests guidelines that are relevant to the goal of reducing respondent burden in personal network data collection. Network structural metrics are sensitive to network size, so to make comparisons across personal networks they must either be the same size or the metrics must be normalized. Sampling something less than the entire network can have a dramatic effect on network structural metrics—so size matters. Unfortunately, size may affect some metrics differently than others (say centrality versus components), although there is reason to believe that measures of centrality are affected more or less the same by changes in network size. The way in which data are sampled affects the metrics; that is, sampling of edges versus nodes is not the same, and snowball sampling of nodes can have peculiar effects that should be accounted for when interpreting social network metrics. Random removal of edges does not appear to be a good solution for whole networks, and random removal of nodes for asymmetric data are highly correlated with the whole network when as much of 50% is removed.

A few studies suggest ways to make adjustments for missing data in whole networks. These typically rely on the use of directed data in asymmetric matrices. If data from one node is missing, researchers can rely on reports from other nodes about that node and adjust the metrics based on that knowledge. This is a key difference with personal network structural data. In those cases where personal network structural data are collected from respondents, the question posed for evaluation of alter–alter ties represents a symmetric tie. While it may be reasonable to ask a respondent if two alters know each other, or talk to each other, in most cases it is not reasonable to ask them if one alter has a stronger tie to another. In the absence of directed data, the solutions for adjusting social network structural metrics for missing data are not relevant for personal networks.

In addition to the lack of asymmetric data, personal network data collection is subject to some unique problems. These stem from differences in boundary specification and respondent recall. Although this can also be a problem in whole network data collection, it is somewhat different for personal networks. In whole network research, boundary specification is often objective. For example, the members of a classroom of children are easily defined and agreed upon by all observers. At times, however, network boundaries can be less obvious, and measures to specify those boundaries can affect whole network structural metrics (Laumann et al., 1983). Personal network boundary specification is defined by one or more name generators, created by the researcher, and the respondent's interpretation of those generators (Burt, 1984; Marsden, 1990). It is common for name generators to invoke alters that cut across several social domains, such as family, work and social activities. Unlike whole network research, there is typically no way to independently verify who should and who should not be included in the network. Researchers must rely on the respondent to make that decision based on their interpretation of the name generators. Much of the methodological research in personal networks is focused on the effects of different name generators.

Bernard et al. (1990) demonstrated the compositional differences in personal networks using different name generators, later documented in other research (Fu, 2005; Ferligoj and Hlebec, 1999). The most comprehensive research in the area of network elicitation bias is by Brewer (Brewer, 1993, 1995a, 1995b, 1997, 2000; Brewer and Yang, 1994; Brewer et al., 1999; Brewer and Webster, 1999). Brewer's work demonstrates that there are significant order effects to the way alters are recalled (or forgotten) and how they are ordered in memory. Marsden (1993) demonstrated that biased recall and order effects of alter recall can affect network structural measures such as density. Over-reporting and under-reporting of alters (Feld and Carter, 2002), and social regard (Carter and Feld, 2004) were also found to affect personal network structural metrics. Brewer and his colleagues have developed methods for probing that may reduce these effects (Brewer and Garrett, 2001).

Marin (2004) found both compositional and structural effects when limiting alter elicitation to a single network generator. Alters were more likely to be named if they were closer to the respondent, had multiplex relations with the respondent, knew the respondent longer and were tied to more people in the respondent's network. The last point is of particular concern as it directly affects network structural metrics. Similar results were also found by Sudman (1988). Marin and Hampton (in press) compared network composition and network density for 487 respondents using two abbreviated methods for network generation compared to a full elicitation method. These were the modified multiple generator (MMG) that consists of the two most reliable generators from a list of generators, and the multiple generator random interpreter (MGRI) where alters were selected randomly from a comprehensive list using multiple generators. They found both methods strongly correlated to the entire list of alters on composition and density. The MGRI provided the maximum reduction of respondent burden and the best reproduction of the full set of generators. This finding is particularly relevant to the methods we test in this study. Kogovšek et al. (2002) examined the effects of different modes (face-to-face or telephone) and the way questions about alter attributes are asked (by question versus by alters) on the reliability and validity of compositional data, such as closeness and frequency of contact. Reliability was found to be highest for telephone interviews by alter.

To summarize, the way in which personal networks are defined given specific name generators can significantly affect network structural metrics. With personal networks, missing data are due to the failure of the respondent to list them. Unlike whole networks where we often know that we have not collected data from someone in the network, with personal networks only the respondent can say who has been left out. There is an additional problem that there is almost always an ordering to the way alters are elicited from respondents.[1] Respondents frequently start with very

[1] McCarty et al. (1997) attempted to randomly sample alters from respondents using first names. This method was at least free of order bias (Brewer, 1997), although it was not free of sex, age and ethnic biases.

close family, when free-listing alters. They also tend to list alters in groups so that the choice of the number of alters to interview can often introduce bias into the sample. The methods for analyzing the effects of or adjusting for missing data in whole networks does not take into account the biased ordering of network alter elicitation in personal networks. Based on this research, personal network research should use multiple name generators and probing, and/or ask about a sufficient number of alters to minimize order bias. Both of these recommendations significantly increase the respondent burden, particularly when structural data are being collected. Many of the problems associated with order bias and size effects are controlled for by eliciting the same number of alters from each respondent.

In this article we explore four methods to reduce respondent burden in the collection of personal network data. The most obvious method is to ask for fewer alters. The literature on personal networks suggests that this would very likely introduce bias into network structural metrics. Another option is to elicit a large sample of alters, and randomly select a smaller sample of alters for tie evaluations. The literature on whole networks suggests this to be the most viable option. A third option is to elicit a large sample of alters and randomly select a smaller set of alter–alter ties for evaluation, an option not supported by whole network research. A fourth option is to predict alter–alter ties based on the notion of transitive triads. This option has not been tested in either whole or personal network research. Each of the four methods involve collecting less data from the respondent, which as with any sampling procedure, increases the variance of the metrics derived from the samples.

3. The data

We relied on two sets of data for this study. The first is a compilation of 447 adjacency matrices of 45 alters each from three separate studies. The three studies were a study of the relationship between personal network characteristics, race and depression ($n = 174$), a study about personal networks and smoking among college freshmen and sophomores ($n = 100$), and a study about personal network dynamics among interviewers in a telephone survey lab ($n = 173$). Although these three studies had different objectives, the respondents were all recruited from Gainesville, FL and were in many cases students. In all cases respondents came to a survey lab in downtown Gainesville, FL, USA to enter their data into a program called Egonet.[2] About two-thirds of the respondents were paid $40 for their participation, the remaining respondents worked in the survey lab where the interviews were conducted and provided their data free. For the depression and smoking studies, respondents were recruited through a variety of means, including newspaper ads, e-mail listservs and word of mouth. The sample of 447 is in no way representative of any larger population and is biased toward young adults.

The second study consisted of 554 respondents to a study about acculturation among migrants to the US and Spain. The respondents in the US consisted of Puerto Rican, Columbian, Dominican and Mexican migrants to New York City, and Cuban and Haitian migrants to Miami. In Spain the respondents were Moroccan, Dominican, Senegalese, Gambian, Equatorial Guinean and Argentinean migrants to Barcelona. Respondents were recruited mostly through advertisement in local newspapers and from community centers.

[2] Egonet is a questionnaire authoring and analysis program for personal network data. It is available for purchase from www.mdlogix.com.

In both studies respondents free-listed the names of 45 people they knew. Knowing was defined using the following definition:

> "You know them and they know you by sight or by name. You have had some contact with them in the past two years, either face-to-face, by phone, mail or e-mail, and you could still contact them if you had to."

Respondents were first asked to list people who were close to them, and then to fill out the remaining 45 alters with friends, family and acquaintances that fit the definition.[3] Each respondent was required to list 45 names.[4] It was not important that the names were exact, but that the respondent would recognize the name when it was presented to them again.

The structural data were the result of each respondent's assessment of the 990 possible alter–alter tie evaluations. These ties were assumed to be symmetric; in other words we did not ask respondents to evaluate asymmetric relations between their alters. The tie definition was evaluated on a three-point scale in response to the following question:

> "What is the likelihood that these two people talk to each other when you are not around? That is, what is the probability that these two people have a relationship independent of you?" [0 = Not at all probable (21% of responses), 1 = Somewhat probable (11%) and 2 = Very probable (68%)].

Despite the differences in the substantive questions about ego and about their alters, the method for collecting the 45-alter adjacency matrix was the same across all studies. We make the assumption that the structural features of the 45-alter adjacency matrix for each respondent represent some 'ideal,' and that comparisons of smaller samples represent degradation from that. This is similar to several of the approaches reviewed above that estimate the effects of sampling and missing data in whole networks.

4. Structural measures

All of the structural measures were normalized as they are all sensitive to the number of nodes in the network. The following structural measures will be considered (grouped on diagrams by the letter shown):

1. Point degree centrality:
   - average point degree centrality (network density);
   - degree centralization;
   - change of person with maximum point degree centrality.[5]

[3] Past experience with free-listing as a name generator has demonstrated that most respondents list close alters first. In some cases, however, respondents may list 45 alters and realize afterwards that they excluded someone very important in their network. The instruction to list close alters first reduces the possibility that important alters will be left out and introduces a consistent bias to the free-list, which is assumed to be better than varying order bias across respondents (see Sudman, 1985).

[4] Asking each respondent for the same number of alters controls for many of the problems observed by the literature on sampling and missing data in whole networks.

[5] There are three measures based on changes of the most central alter. This metric is very similar to the "Top 1" measure used by Borgatti et al. (2006) in their evaluation of the effect of missing data on centrality measures in whole networks.

2. Closeness centrality:
   - average point closeness centrality;
   - closeness centralization;
   - change of person with maximum point closeness centrality.
3. Betweenness centrality[6]:
   - average point betweenness centrality;
   - betweenness centralization;
   - change of person with maximum point betweenness centrality.
4. Number of components of size 3 or more, *and* number of isolates (shown together on diagrams).
5. Number of alters in network core from core/periphery procedure.[7]

The normalization follows Wasserman and Faust (1994) for the centrality/centralization measures.[8] The number of components is unnormalized; the number of isolates and network core members is normalized by the number of the alters. The approach used to compute the alters in the network core follows Borgatti and Everett (1999) closely, and uses their continuous model and a function maximizer. Not all combinations of datasets and numbers of alters found a useful maximum fit to the core/periphery structure (though usually on more than 80% of occasions), and we cite figures for this measure only over those datasets where solutions were found. The measure of whether the most central alter changes when network size is reduced is defined by creating a binary variable that begins at zero, and changes to a one when the most central alter changes as a result of data excluded from analysis. The values cited represent the average of these 0/1's across all 447 respondents, and so are the fractional number of changes as one more alter is added (or removed).

5. Asking for fewer alters—Data Set 1

One obvious way to reduce respondent burden is to ask respondents to list fewer alters. A common question among those who are collecting structural data on personal networks for the first time is "What is the minimum number of alters I can collect and still capture the structural variability between respondents?" We simulate this in Fig. 1 using "Somewhat probable and Very probable" as the tie definition and in Fig. 2 using "Very probable" as the tie definition. The question we want to answer is, for each measure, what is the minimum number of alters a researcher can

[6] Two studies, Everett and Borgatti (2005) and Marsden (2002), examined betweenness centrality in egocentric networks embedded within a whole network and including ego. This differs from the current study as the egocentric networks here are not constrained to a single sociocentric space and exclude ego (see McCarty and Wutich, 2005 for a discussion about including and excluding ego in structural analyses of personal networks). Personal networks are unconstrained egocentric networks.

[7] Two reviewers raised concerns about using measures of core–periphery. One concern is that there is still debate about the reliability of core–periphery calculations given the possibility of multiple solutions (see Borgatti and Everett, 1999; Everett and Borgatti, 1999) for a thorough discussion on this problem. Despite its limitations, core–periphery is still used as a metric for whole networks (García Muñiz and Ramos Carvajal, 2006), and is therefore employed here. Another concern is that there is no tradition of calculating core–periphery for personal networks. While this is true (until now), there is a long tradition of core–periphery as a concept in personal networks (see Hammer, 1983). Morgan et al. (1996) examined the composition of the core–periphery by looking at the core of names included in longitudinal elicitations of personal networks.

[8] The definitions and normalizations can be found in Wasserman and Faust (1994): (1) pp. 178–180; (2) pp. 183–186; (3) pp. 188–192.

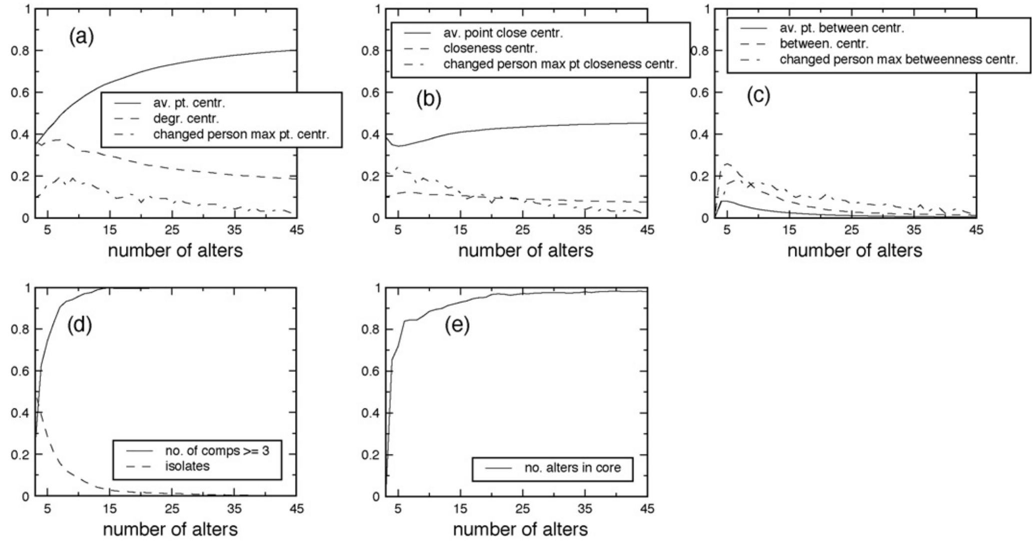

Fig. 1. Data Set 1: strong and weak ties.

elicit and still capture the structure represented with all 45 alters? We do this by recalculating the network structural metrics using matrices where we successively drop alters from the end of the list.

The curve for (normalized) average point degree centrality decreases until five alters are elicited, then rises asymptotically as alters are added (average point centrality is nothing more than network density). The asymptotic rise of average point degree centrality is even more pronounced when only strong ties define the structure (Fig. 2).

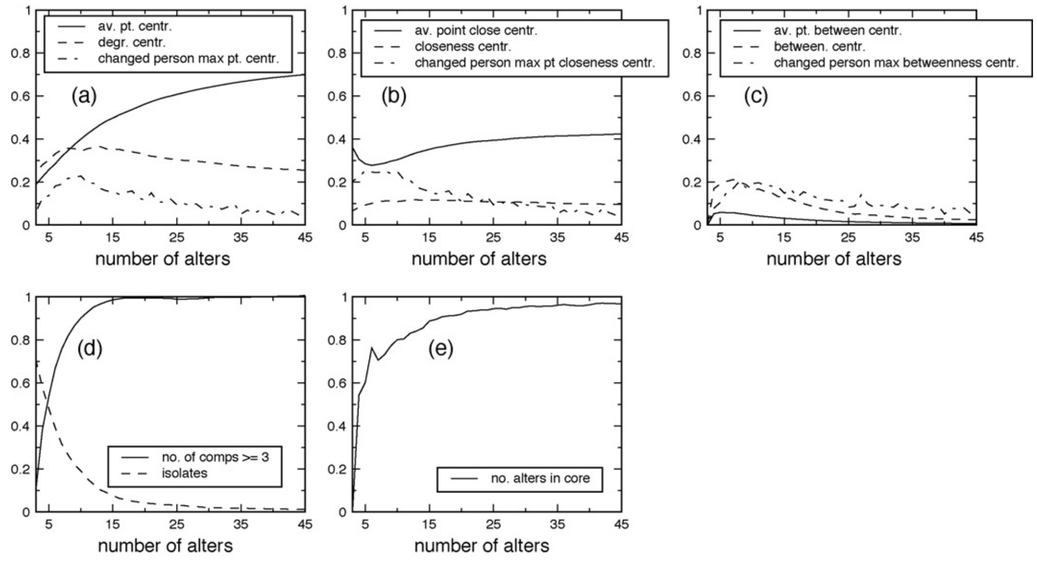

Fig. 2. Data Set 1: strong ties.

This is not the case for degree centralization. For strong ties degree centralization plateaus around 13 alters, while for strong and weak ties it falls steadily. The fractional number of times the maximum degree central alter changes plateaus at around 10 alters for strong ties then falls steadily, while for strong and weak ties there is no clear breaking point. In both cases a strong argument can be made that sufficient structure is captured with fewer than 45 alters.

Unlike degree centrality, average point closeness centrality does not rise uniformly with number of alters. However, it is apparent from Fig. 1(b) that the difference between average point closeness centrality using 15 alters is not much different than that using 45 for both strong ties and strong and weak ties together. Closeness centralization falls steadily in both Figs. 1(b) and 2(b) after a plateau at around five alters.

Average point betweenness centrality plateaus after 5 alters in both Figs. 1(c) and 2(c) and is virtually unchanged after 25 alters have been collected. Betweenness centralization and the fractional number of times the maximum between alter changed both plateau at about 7 alters and are relatively unchanged after 25 alters have been collected.

Components of size three or more rise rapidly as alters are added in both Figs. 1(d) and 2(d) until approximately 16 alters at which point it plateaus. Similarly, the number of network isolates falls rapidly until about 15 alters have been collected at which point it is functionally zero.

For strong and weak ties (Fig. 1(e)) and strong ties only (Fig. 2(e)), the number of alters in the network core rises rapidly and stabilizes at around 25 alters. The core/periphery identifying routine was successful on 84% of occasions (strong and weak ties) and 87% of occasions (strong ties only).

Taking all of the measures and both strong and weak ties into account, it is clear that 35 alters will yield very similar results to an analysis using 45 alters. For most measures an analysis with 25 alters will also be adequate. The number of alter–alter tie evaluations drops from 990 with 45 alters to 300 with 25 alters.

6. Randomly selecting alters from a larger list—Data Set 1

Another possible solution to the respondent burden problem is to elicit a large set of alters and randomly select a smaller set for the alter–alter tie evaluations. This has the advantage of forcing respondents to extend their elicitation to both strong and weak ties, and to groups that they might not tap into if they are free-listing a smaller alter set. There are several potential disadvantages. Given that the selection is random, it is possible that key alters may not be selected and so the structure might change dramatically. Our measure of changed central alters can be modified if a central alter is removed or added, which is a potential artefact of this approach.

For this analysis we began with the 45-alter matrix as before, and then instead of removing the currently highest-numbered alter, we removed a randomly numbered alter and inserted the highest-numbered alter data into the now empty number. The results for the structural measures are depicted in Fig. 3 for strong ties only.[9]

Average point centrality and point closeness centrality are nearly uniform. Degree and closeness centrality are also almost constant. Compared with Fig. 2, there are higher probabilities of changing a central alter. The number of components at least 3 in size, and the (normalized) number of core alters, are essentially unity. The core/periphery identifying routine was successful on 88% of occasions.

[9] Overall there was not much difference between the graphs using strong and weak ties and those using strong ties alone (given that only 11% of the ties were weak ties).

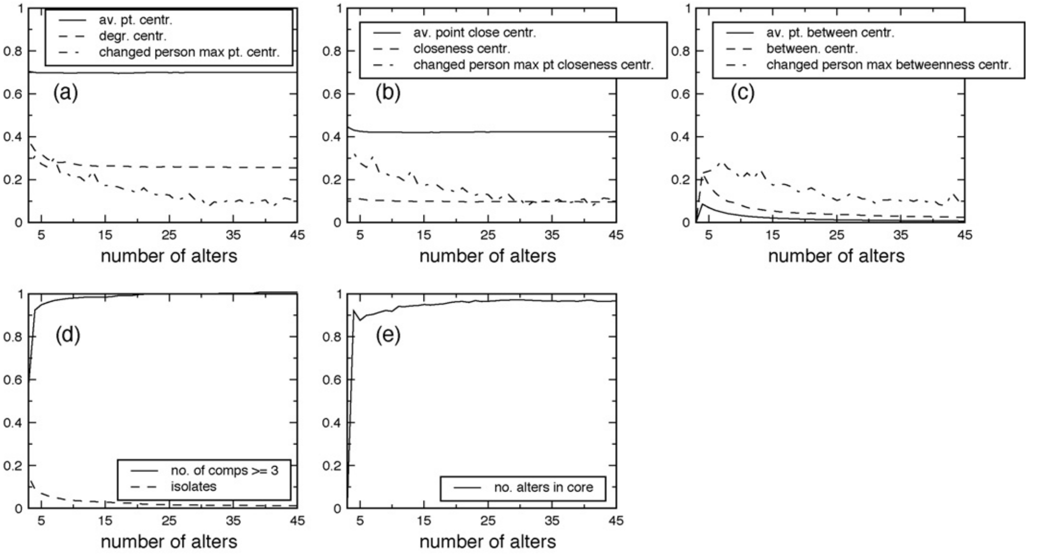

Fig. 3. Data Set 1: random alter removal.

For some measures (average point degree centrality, degree centralization, average point closeness centrality, closeness centralization, number of components greater than or equal to 3 and number of alters in the network core), random removal of alters appears to yield results similar to all 45 alters with far fewer alters than simply asking for fewer alters. In other words, by eliciting a large number of alters one can randomly select under 10 alters for the alter–alter tie evaluations and have similar structural estimates for several measures as would be attained with all 45 free-listed alters. This appears to be a better solution to reducing respondent burden than simply asking for fewer alters.

7. Randomly selecting ties from the 45 alters—Data Set 1

A third option to reduce respondent burden is to elicit a large set of alters, and then randomly select alter–alter ties to evaluate. The danger here is not that a key alter will be left out, but that a key connection would be left unevaluated. By definition that would default to null. Fig. 4 shows the results of this analysis, emulated by removing 5, 10, 15, . . ., 95% of ties randomly from the full set for 45 alters. Fig. 4 shows the structural results.

The core/periphery identifying routine was successful on only 41% of occasions. Clearly the average point centrality must (and does) decrease linearly with the percentage of ties removed. The measures of changed central persons are strongly affected by tie removal, being mainly over 0.8. (Note that these changes are produced by a full 5% tie removal and would clearly decrease if the increment of tie removal decreased.) Large amounts of tie removal produces large numbers of components, due to fragmentation of the alter matrix, and a corresponding reduction in core membership.

Compared with the previous two methods, randomly selecting ties produces much different results. This is not recommended as a way to reduce respondent burden.

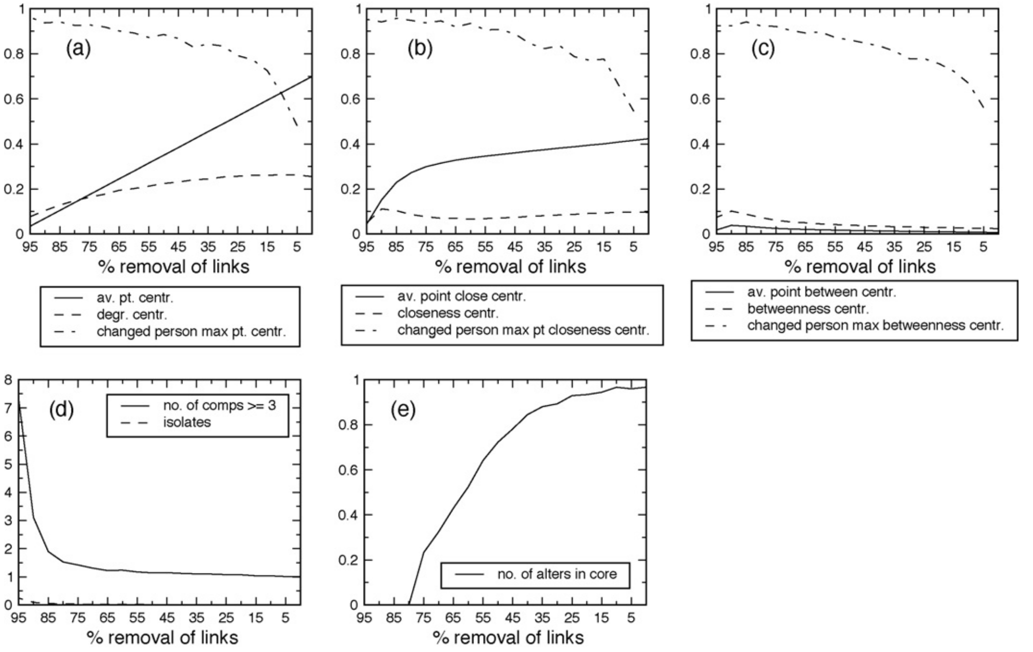

Fig. 4. Data Set 1: random removal of links.

8. Assuming alter–alter tie based on notion of transitive triples—Data Set 1

A fourth option is to somehow anticipate the existence of a tie based on social network principles. Some researchers have observed that in whole networks, triads are often transitive (Heider, 1946; Hallinan and Hutchins, 1980). Louch (2000) found transitivity to be a strong explanatory concept for structure in small personal networks. We tested this notion by examining the percent of triads that were indeed transitive after successively dropping alters from the list as was done in the first option. Fig. 5 shows that just over 40% (cutoff 2) or just under 60% (cutoff 1 or 2) of the triads are transitive for 45 alters, but this drops monotonically as the number of alters is reduced.

Adding in extra ties based on transitivity would strongly reduce the burden on respondents. We can test this effect as follows. We scan through respondents, reaching respondent i. If i knows j and k, then j and k must know each other for symmetric transitivity. Thus, we add (presumed) nodes to the existing matrix, emulating what might be done experimentally. These results are presented in Fig. 6. The core/periphery identifying routine was successful on only 17% of occasions, in line with the above comment.

Assuming transitivity strongly increases average point degree centrality, as it must (the procedure can only *add* ties) to nearly unity for the full matrix. Otherwise, with the exception of closeness centrality and core membership (both decreased from Fig. 2), there is little effect on measures.[10]

[10] An attempt to combine the assumption of transitivity and random removal of ties produced measures almost independent of the number of alters. We assume the matrices had become essentially homogeneous using this drastic approach.

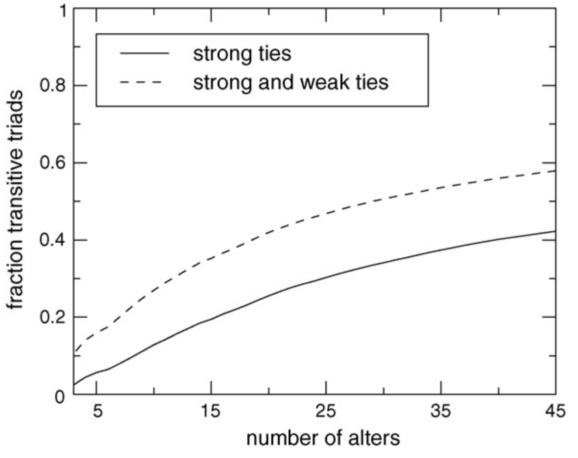

Fig. 5.

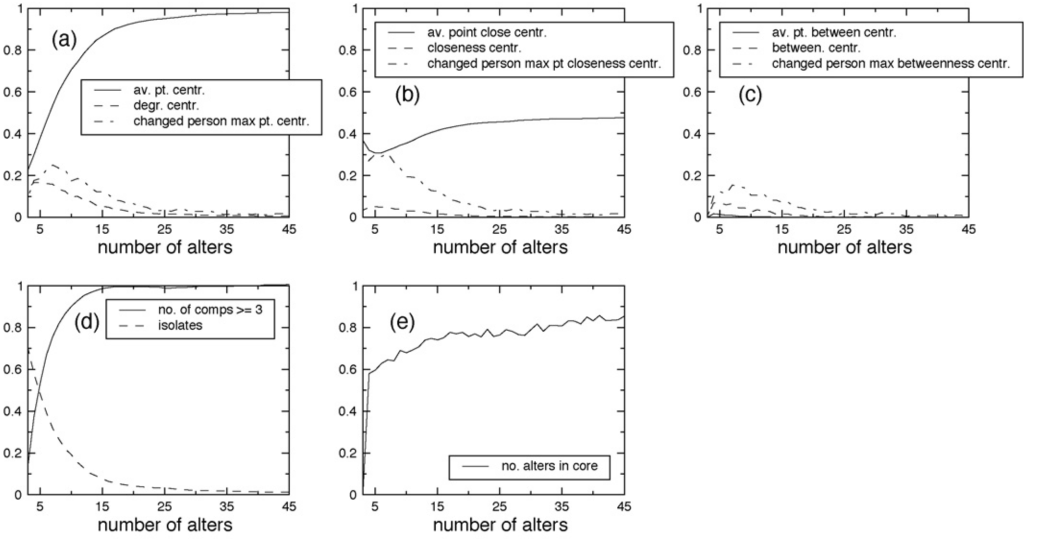

Fig. 6. Data Set 1: transitivity assumed.

9. Results for Data Set 2

Given the poor results for random selection of ties (Fig. 3) and assuming transitivity (Fig. 6) from Data Set 1, we restrict our analysis of Data Set 2 to removal of alters from the end and random selection of alters. The results are depicted in Figs. 7 and 8. In terms of evaluating methods for reducing respondent burden, it is once again clear from Data Set 2 that random selections of a subset of alters produces a better representation of the network metrics for the full 45-alter matrix than asking for fewer alters. For most measures very similar values can be achieved by randomly selecting fewer than 20 alters from a larger list.

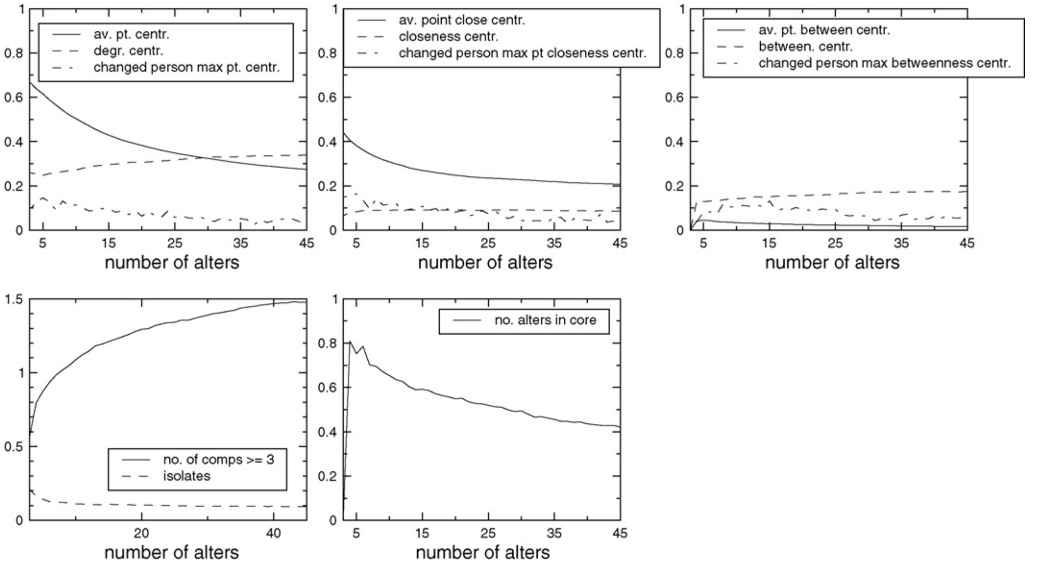

Fig. 7. Data Set 2: strong ties.

There is, however, another difference in the results between Data Sets 1 and 2. This is best illustrated by focusing on the curve for average point centrality (density) in Figs. 2 and 7. In Fig. 2, representing mostly students in Gainesville, FL, density increases as respondents add more alters. In Fig. 7, representing migrants in the US and Spain, density declines as alters are added. This illustrates an important point which, frankly, we had not anticipated. The structure of the personal networks of respondents in Gainesville is not the same as for migrants. In all likelihood, as

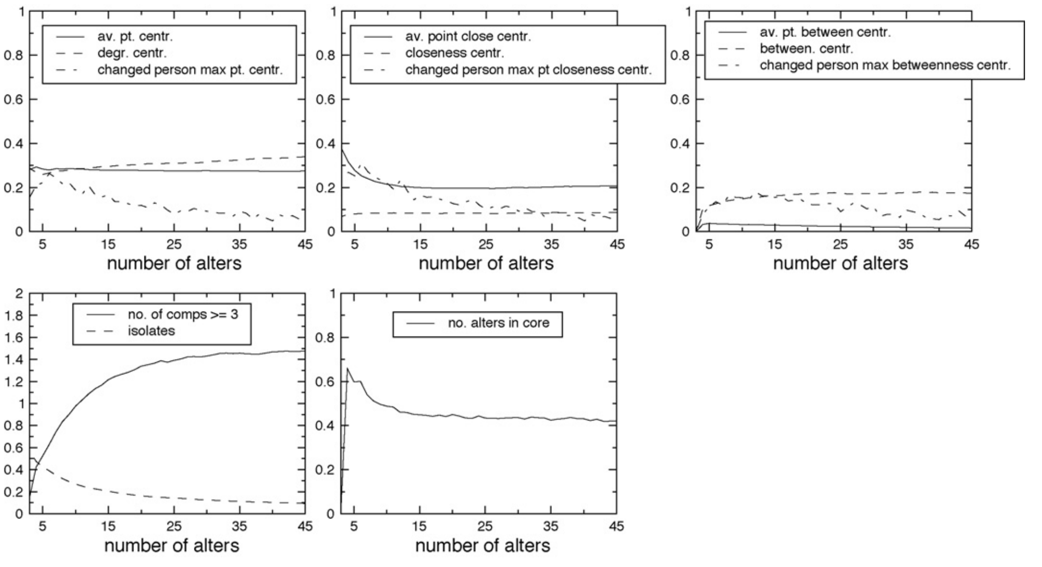

Fig. 8. Data Set 2: random alter removal.

migrants are forced to add alters, they tend to populate their network with alters from both their home and their host countries. In contrast, students and other residents of Gainesville, Florida tend to list mostly people nearer to them, giving their alters the opportunity to know each other.

Based on these results, we think it would be a mistake to assume that the modal or average structural properties of personal networks in different populations would be the same. Indeed, we can imagine several populations that, for various reasons, would tend to exhibit structural properties different from each other. For example, much of the literature on social support suggests that the core networks of those that are depressed or schizophrenic may be much smaller than those without those illnesses. The homeless may have more isolates. When stratifying personal network studies, researchers should consider how those strata may affect personal network structure.

10. Discussion

In this article we have examined the changes in personal network structural measures as a consequence of various strategies for reducing respondent burden in collecting structural data. The most surprising result is that for some measures, structural variability may be captured with fewer alters than we originally anticipated.

For most measures, a free-list of 25 alters will capture the same structural pattern as a network of 45 alters. The number of alters may be reduced to as few as 10 alters for most measures by randomly selecting alters from a larger list. For some measures, anticipating ties based on transitive triads produces similar results, but for others like number of alters in the network core, it produces very different results. Randomly selecting ties from a large set of alters is not recommended.

It is important to note that the results reported here say nothing about changes to personal network composition as a consequence of dropping alters from the end of a free-list task or randomly selecting alters from a larger list. Changes to personal network composition may be more sensitive to the way respondents have been sampled than personal network structural variables. This dataset was limited in our ability to test changes in composition given the dissimilar set of questions respondents answered about each alter. However, from the consistency of our results, we assume that the random selection of alters from a larger list would also be the best method for reducing the respondent burden of providing alter attribute data. In a personal network study this is typically the most time-consuming part. We do not know the number of randomly selected alters necessary to represent the composition of the 45-alter network. This probably varies by the particular alter attribute.

In practice the process of alter–alter tie evaluation is less burdensome than intuition might suggest. When alter pairs are presented to respondents in a systematic way (such as one alter with all other alters, the next with the rest and so on), respondents can get a feel for the way their alter lists are organized and anticipate their responses. In personal networks where there is less cohesion, many of the ties will be null, and having software that defaults to a null tie can save a lot of time. In the studies conducted here it was not unusual for respondents to complete all 990 tie evaluations in 30 min. Far more burdensome is the task of answering a set of questions about each alter. These cannot be easily anticipated. For a personal network of 45 alters, ten alter questions results in 450 different questions. Researchers must be acutely aware of the consequences of asking for too much alter information.

One caveat with the results presented here is that these adjacency matrices are the result of alters elicited by free-list and using a tie definition that focuses on independent interaction. It is entirely possible that the pattern in the change in structural measures will be different using different elicitation technique(s) and a different tie definition.

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