

10 CLOSING THOUGHTS

We called the first chapter of this book 'starting points', but in many ways we hope this entire book will serve as a starting point for the wider engagement of medievalists with social network analysis and for the broader HSNA community to engage more with medieval history and with digital prosopography. The primary goal of this book has been to publicize and contextualize the results of the efforts by the Leverhulme Trust project 'The transformation of Gaelic Scotland in the twelfth and thirteenth centuries' (2013-16) to apply Social Network Analysis methods and theories to the People of Medieval Scotland database. At times, we have also looked for ways to test the SNA results through examining datasets critically and comparatively. Moreover, we have also tried to examine these results, where possible, against the historical backdrop of medieval Scotland. What this book has not endeavoured to accomplish has been a full-scale historical synthesis, a mature incorporation of network thought and data into our historical conversation about Scotland in the central middle ages. Ongoing research will seek to present these kinds of results in due course. For the moment, it is hoped that the methods, case studies, and concepts in the current volume will provide medievalists with plenty to chew on.

As the discussion of medieval history and SNA in chapter 1 made obvious, we are not the first guests to arrive at this party. This book does not represent the first look at medieval charters with SNA in mind. It is important to understand and recognise our antecedents. Of these, it is possible to divide them into two categories – SNA studies that used only a single type of source material as evidence and those that included a number of different source types. For the medieval period, it is arguable that those studies in the latter category have been more successful at producing results with real potential impact on the larger historical conversation. The projects of Robert Gramsch and Isabelle Rosé, for example, employed narrative, epistolary and diplomatic evidence. Bespoke, relatively small-scale networks characterised by multiplex ties (links existing in divergent social contexts) can then be constructed, which allow specific questions and issues to be addressed. Much larger, pre-existing datasets, like PoMS, the Charlemagne's Europe database, the French notarial acts dataset, and Ruffini's Egyptian papyri, are much more likely to be drawn from a single type of source: in this case, diplomatic or transactional acts, broadly defined. The two most relevant studies for comparison with the work presented in this book were Ruffini's study of Byzantine Egyptian communities and the project looking at French notarial acts. Both studies made two-mode networks of documents on one axis and people on the other, from which they produced affiliation networks. This is the same method employed for our co-witnessing studies. Ruffini discussed connections between individuals with 'high tie strength': this is the same method as our raising the threshold of co-witnessing acts to see who has the strongest

co-witnessing connections. Both Ruffini and the French notorial acts project examined centrality and incorporated this into their analysis of certain actors. The French project, like PoMS, covered a long span of time and identified concerns with the way that chronology was represented in the social networks.

The PoMS approach to SNA is built directly on the PoMS approach to digital prosopography, and that means going to some lengths to avoid losing sight of the social contexts in which the documents were produced and in which, crucially, the historical actors acted, and the networks themselves were created, lived, and broken up. There is a reason why some scholars have been wary of using medieval source material, which tends to be highly idiosyncratic and otherwise problematic, as fodder for social network analysis. How representative could such networks be? The following principles should hopefully give colleagues more reason to trust that network sociograms relate to actual historical settings. First, we should try and compare like with like as far as is practicable. In documentary terms, this means comparing texts which are as similar in form and function as possible. The widespread proliferation of the epistolary Latin charter in the twelfth century provides one obvious opportunity for the creation of such a dataset. The PoMS database is comprised of charters, chirographs and other such transactional documents. Second, it is important to distinguish sub-types of document within the broader corpus. SNA studies should seek to reflect the social context 'on the ground' by consideration of these types. For example, our co-witnessing studies did not include *briefes* (or *writs*) because these reflected a different social setting from the assembly gatherings which most charters recorded. Third, the roles played by historical actors must be defined and structured into the database. While it would be possible to include all individuals mentioned in a group of charters in an SNA study, comparing witnesses with neighbours, previous landholders, and people mentioned in *pro anima* clauses (wherein prayers for souls of relatives and others were specified) would be nonsensical and misleading. Thus, by looking at witnesses to five document types only (charters, charter/*briefes*, notifications, agreements and settlements) we sought to keep the social function of the actors and social context in which they operated as historically meaningful as possible. The social context of charter witnessing is a topic that historians have already explored in some depth (see for example Broun 2011). Fourth, these decisions need to be transparent and explained clearly to readers so that others can check the work and reflect on both the methodology and the results. Finally, the results need to be considered against broader historical issues, such as the reliability in terms of survival patterns and other potential biases in the evidence itself, as well as what we know about contemporary politics and society. We feel that SNA

results will become most relevant when combined with various other methods in the historian's toolkit. This approach has underpinned our approach to the first large-scale SNA study to show the workings of a medieval kingdom; we hope this will serve as a milestone of sorts. We have constructed case studies where the data allowed and highlighted points where the studies rubbed up against problems caused by the nature of the data. Having based our social network analysis on these principles, we hope to ensure the results are reflective of activities on the ground as they are reflected in the surviving sources.

We were very fortunate to have the People of Medieval Scotland database (www.poms.ac.uk) to work with. As already mentioned, the dataset already existed independently and does not have to be created by the network analyst. Further, as the database is public, and the prosopographical work had already been conducted, an unprecedented level of transparency is possible. Readers are able to check the SNA work against the publicly-available website. A great deal of data has been made available for public download as spreadsheets on the website. Further, many of the sociograms discussed in this book have been made publicly available on the website. As these are interactive, PoMS users can explore the sociograms themselves (<http://db.poms.ac.uk/sna/all/>). While other projects, like Trismegistos and Early Modern Letters Online have been making headway on this front, the range of visualizations available on the PoMS website is at this time unrivalled. Second, as discussed above, the database has been structured in a way that is in tune with scholarly concerns about constantly being aware of the uses and contexts of distinct document types. This has allowed SNA case studies to be easily designed in ways that reflect the medieval social contexts which provide the backdrop to the documents' production. The downside of this, however, is that there has hitherto been little attempt to integrate multiplexity of relationships into the social network analysis. This approach, while simpler, by highlighting diplomatic or record sources above epistolary and narrative sources, may be more appropriate for a time period when so many factors are in flux, such as uneven survival and distribution of sources, in terms of geography, chronology, and socioeconomic status/ 'class'. Moreover, with over 15,000 potential actors and over 6,000 potential documents, the PoMS database running up to 1286 offers a larger dataset than has so far been deployed in medieval SNA. As the previous chapters have demonstrated, the relatively high levels of interconnectedness between the actors mean that there are unparalleled opportunities for valued network data. Our method focuses heavily on the intensity of social relationships (ties) based on the numbers of interactions. Furthermore, as this database constitutes the only digital prosopography in the central Middle Ages to represent exhaustively the

diplomatic output of a medieval kingdom, we hope that what follows offers historians and other scholars and students of various medieval polities a set of models for what is possible using digital approaches with medieval charters. Perhaps chief among these are new ways of conceptualizing the social relationships around royal courts and assemblies. When seen in parallel with Robert Gramsch's structural model for analysing court politics and Preiser-Kapeller's work on the early fourteenth-century Byzantine court, we have the makings of a package of SNA methods and techniques for understanding political and public life – a package which will hopefully be added to, and adopted, applied, and adapted by the next generation of medievalists.

This is not to say that readers will express no criticisms of our work. One obvious potential critique is how complete a picture can be presented by the social network analysis when we are dealing with medieval charters, a source for which the survival rate is incomplete and patchy. As Martin Düring asked in his paper at the 2012 Connected Worlds conference in Southampton, 'how reliable are centrality and clustering measures for data collected from fragmentary and heterogeneous historical sources?' (Düring 2016). To a certain extent, the problem of poor survival, or, in our case, uneven production and survival of charters and similar document types, is one that medieval historians are well accustomed to. Whether this makes it a less problematic question than it posed for Düring, who works with twentieth-century sources, remains to be seen. In any event, the particular 'landscape' around the corpus of Scottish charters was explored in detail as part of the production of the People of Medieval Scotland database, as was explained thoroughly in the volume published in 2013 (Hammond 2013, pp. 14-30). Because we understand the rate of adoption and production and the geographical and socioeconomic variations in the survival of sources, we have been able to consider these issues in our analysis of the social network results. For example, we know that the anomalous existence of large numbers of Coldingham charters at the level of individual holdings gives us a view of lay society in the southeast of Scotland which we cannot achieve elsewhere. This would make the Coldingham area an excellent case study for further SNA work, but when trying to view lay society across the kingdom, the existence of the material skews the results. Chronologically, we found that the reigns of David I (1124-53) and Alexander III (1249-86), bookending our period, had produced too few documents to provide the rich level of detail that we see in the intervening reigns. There were areas where the evidence is probably too fragmentary to see full fruition of the method: this is most evident in the studies of relationship networks (Chapter Two, above). We know enough about the familial relationships of the top aristocratic families to mean that the SNA method and visualization should provide real new

possibilities for historians, but the evidence of family connections among people further down the socioeconomic ladder are too scattered to be of much use. The tenorial and lordship relationships also explored in Chapter Two, collected as they are from casual and chance mentions of these kinds of connections in the charters, reveal even only a fraction of the tenorial and lordship relationships that we know as historians existed. The evidence for this issue is just too fragmentary. While the sociograms have some value as indicative illustrations, the numerical data produced by this study has little value. Tenorial and lordship relationships, however, also offer a good example of how initial case studies presented here could provide models for more sophisticated SNA work in the future. As additional evidence for tenorial and lordship relationships can be gathered by historians – evidence which was not captured in the relationship factoids – more nuanced and multiplex studies could be designed in future, which could render more trustworthy and useful results. Returning to Düring's question, however, the structured design of the PoMS database at least allowed us to avoid the issue of heterogeneity. This is principally organized around the form and function of documents. Brieves or writs were instructive texts, usually short, ephemeral and later discarded. On the other end of the spectrum, diplomas, of which there are very few in the PoMS database, were very formal documents to which associated individuals were technically signatories rather than witnesses. The five specified document types (charters, charter/brieves, notifications, agreements and settlements) which we included in our co-witnessing studies fell generally into the categories of one-sided quasi-epistolary documents – 'charters', more broadly defined, and two-sided documents, known as chirographs, both of which required a certain critical mass of witnesses in order to function properly legally. It would be possible to divided this body of documents into two general groupings, the first more likely to represent large public gatherings, such as royal public assemblies and church synods, while the second grouping would reflect the more routine activities of various courts and households, for example, of bishops or earls. There is no easy or clear-cut way to distinguish between these two social settings, however, although it would certainly be possible for a historian to tease these out and create new SNA datasets which more accurately reflected more specific social contexts.

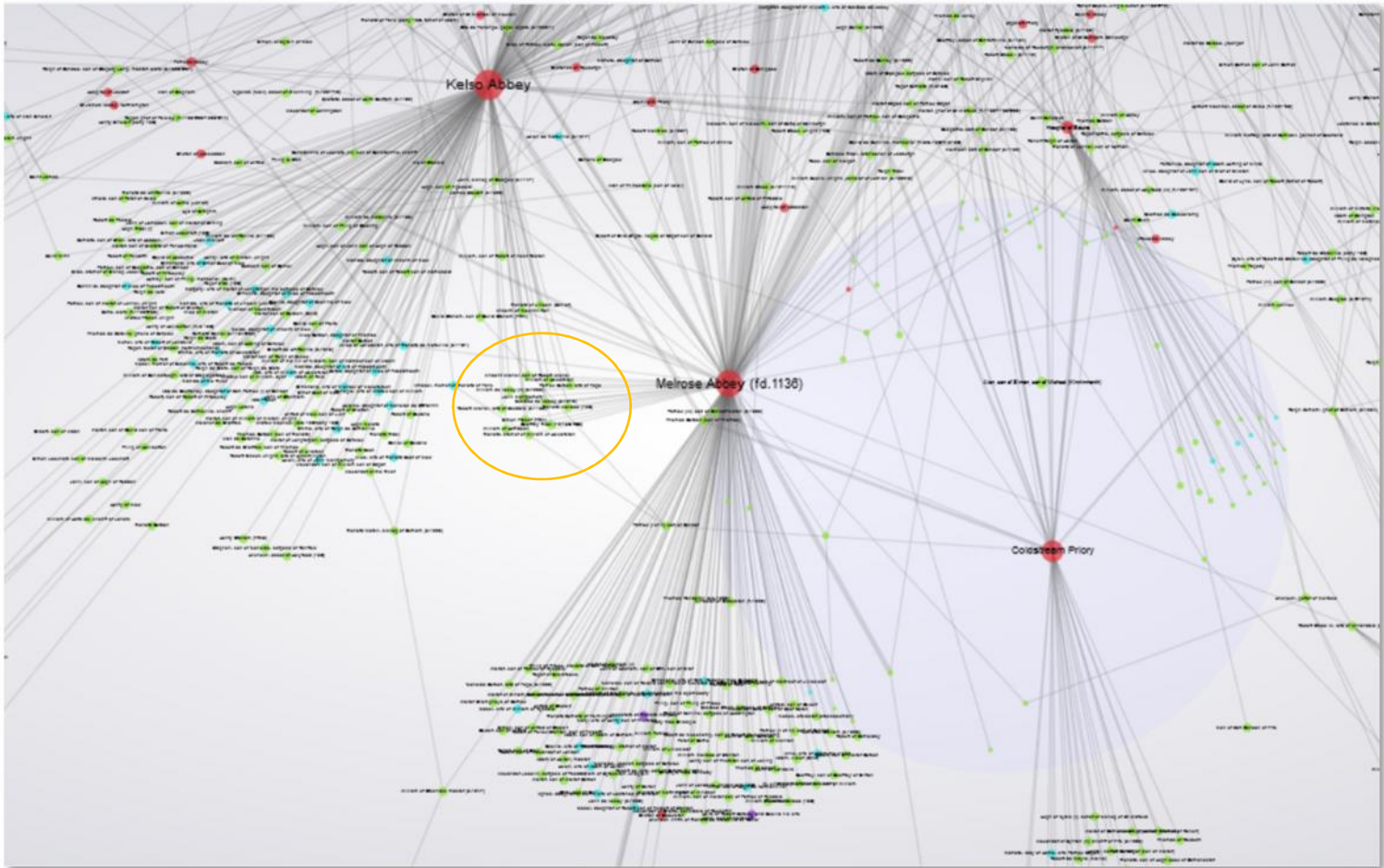
We also went to some effort to ensure that our results were meaningful, even if that meaning was not always obvious, and to rule out results that were untrustworthy. Many of these techniques were meant to counter the effects of chronological change. In Chapter 4, the chronological dimension to the pattern of witnesses was explored, and the 'sweet spot' of the late twelfth/early thirteenth century identified. A number of methods were explored for countering the effects of the 'sweet spot' on the centrality

results for the study of all witnesses. These were a) weighted degree, b) eigenvector centrality, c) ranking by a quotient of degree divided by number of documents witnessed, and d) number of triangles, in particular comparing where individuals fell on a graph compared to a tracking line. Each of these techniques offered some sort of more refined view of the centrality results. In Chapter Five, in order to handle the issue of differences in king's reigns in terms of lengths, numbers of documents, and average numbers of witnesses per document, various techniques were developed. The longer reigns were divided into shorter 'time slices': although the nature of the historical evidence meant it was impossible to create exactly equal date ranges, the time slices were much more comparable with each other and with the shorter reign of Malcolm IV. It was also suggested that in future the reliability of these time slices could be tested, by creating different 'slices' with the lines drawn in different places, and comparing the results. A number of techniques were deployed in a comparison of the time slices, including comparison by average numbers of witnesses, average numbers of documents, graph densities, average degree per 'slice', as well as comparison of edges and cliques. Another trend, the changing length of witness lists, was factored into these considerations. These produced meaningful historical results. The high density of Malcolm IV's reign (1153-65) reflected high levels of trust and cohesion as the kingdom's elites shepherded a young king through a period of crisis and instability. By comparison, the low density of the last time slice of William I's reign (1195-1214), reflected diminishing degrees of trust and cohesion but new opportunities for new actors to emerge in positions of power. The creation of bespoke datasets in future should allow for more nuanced historical interpretation. Careful ongoing historical work on assemblies, for example, could mean that social network analysis of exclusively large-scale political assemblies across, for example, the period of 1150 to 1230, may yield important results. Other bespoke datasets could be created to look at co-witnessing in other more specific social, documentary, chronological, or geographical settings and contexts. There is ample room here for further development of this model, both within the confines of the People of Medieval Scotland database and with charters from other medieval kingdoms.

In our analysis of ego-networks drawn from the study of all witnesses (in Chapter 8), we considered the potential effects of variation in network size (equal to whole network degree) on the density results. We found that while the higher-degree actors tended to have lower network densities, actors with much smaller degrees often frequently found their way into the list of the lowest-density ego-networks. Degree ranges were created in order to examine the trendline of average densities according to degree (Figure 8.2). The relationship of density and betweenness was explored. A key factor was identified in

differentiating wide variations in density between two actors with otherwise similar traits: this was the variety of social contexts in which the actor moved, according to our surviving evidence. The relationship of an actor's position or office to his network density was considered. Archdeacons tended to have lower ego-network densities than episcopal chaplains or clerks, because they operated in a broader array of social environments. The concept of structural holes was explored: actors who bridged two distinct groups or clusters in some cases may have filled a real historical function of, for example, linking up local landholding communities to much broader-range institutions, like the bishopric of St Andrews. With the comparison of the ego-network characteristics of Bishop Andrew of Caithness (d. 1184) and Bishop Matthew of Aberdeen (d. 1199) on page 456, we have outlined a methodology with much broader potential for future research. Why does one actor have a higher-than-expected ego-net density, while another actor has a very low density, when these actors seem quite comparable on historical grounds? The answer seems to lie in the variety of social contexts in which the person witnessed. We also considered whether the surviving numbers of documents were the determinative factor in these results and concluded that they were not. Our discussion of Mark Granovetter's 'strength of weak ties' noted the existence of areas of greater and lesser density within an ego-network, and there is potential for future cluster analysis along these lines. We believe that the analysis of ego-network density is one of the most potentially fruitful new areas of academic inquiry with the most potentially valuable relevance to historical interpretations. This is intimately wound up in the growing awareness of, and need to reflect, the distinct social contexts in which an actor witnessed and in which documents were produced. The colour-coded ego-network sociograms created for Thomas of Galloway, earl of Atholl, offer one possible model for representing visually the range of settings in which an actor moved. More sophisticated and nuanced ways of depicting the 'profile' of an actor's witnessing activity deserve to be explored.

Figure 10.1. Close-up of beneficiaries, with Kelso Abbey and Melrose Abbey shown



There are many possibilities for further SNA work with the People of Medieval Scotland database. One of these could involve cluster analysis. Two issues complicate the use of cluster analysis in our co-witnessing studies. Charters ‘speak the language of unity’ and seek to present consensus and agreement. Analysis of the assembled witnesses to royal charters in particular is unlikely to reveal distinct clusters, although further investigation is merited. The issue of chronology also complicates matters, however, given that people who were alive and colleagues at a given moment in time could appear as a cluster. Further, when we know from narrative historical sources that there were two distinct factions in royal government, such as during the minority of Alexander III (1249 to circa 1258), documentary survival is low, especially for one of the factions (that of Alan Durward). Although we have avoided bespoke multiplex studies so far, it is possible that particular cases could be carefully constructed which would allow the identification of clusters or blocks, but this remains to be seen. The area where cluster analysis could be the most fruitful is in the study of grantors and beneficiaries. As Figure 10.1 illustrates, religious houses like Kelso abbey and Melrose abbey had large numbers of donors. This visualization shows where these donors were unique to one monastery and where they

supported both. A group of individuals who gave to both monasteries has been circled in the illustration. Further, due to the survival of confirmations enumerating gifts to the religious houses, we enjoy something much closer to certainty that the surviving evidence for many monasteries is nearly complete. Viewing landholding society, especially in the region between the Tweed and the Tay, as networks of monastic donors, could produce very interesting results for the discipline more broadly.

Another potential future direction, one which could have major ramifications for the chronological issues in large datasets like PoMS which span decades or centuries, is the development of dynamic graphs. Dynamic graphs show change over time in the network. This approach makes the 'time slice' method look rudimentary, because dynamic graphs show actors entering and exiting the scene with fine-grained detail. Gephi allows for dynamic sociogram animations to be created according to timestamps or intervals. The problems with dating of many medieval documents, of course, cannot be solved by this approach, but it offers huge potential for dated charters, such as Scottish royal charters after 1222, or for the witnesses to English royal charters from an earlier date. Finally, as a general point, PoMS still offers the possibility of finely-crafted bespoke studies which could reflect even closer the social context 'on the ground', for example, by creating datasets restricted to large political assemblies and colloquia.

As Christine Carpenter pointed out in 1994, 'where the theory of networks can help is in focusing the questions to be asked and the means of asking' (Carpenter 1994, 366). Social network analysis and theory has more to offer the historian than just new quantitative data to play with and jazzy new graphs to show off. It shifts the discussion from the characteristics of individuals to the ways in which actors connected and experienced things as groups. It prompts us to ask questions about social cohesion, embeddedness and trust, about opinion leaders, brokers and social capital, about structural holes, power vacuums, and opportunities for changes in the balance of power in networks.

Considering these networks does not mean reducing well-known historical figures to a series of numbers and graphs. Conversely, it offers a chance to breathe new life into them. Padgett and Ansell's words here are astute:

'We close on this methodological note: to understand state building, we have argued, one needs to penetrate beneath the veneer of formal institutions and apparently clear goals, down to the relational substratum of people's actual lives. Studying "social embeddedness," we claim, means not the denial of agency, or even groups, but rather an appreciation for the localized, ambiguous, and contradictory character of these lives. Heterogeneity of localized actions, networks, and identities explains both why aggregation is predictable only in hindsight and how political power is born.' (Padgett and Ansell, 1993, p. 1310)

This observation gets at the paradox of Historical Social Network Analysis: while it seems to turn individuals and their actions into mere statistics, it actually holds the potential for helping us to a) understand that such themes and marriage, family and landholding, politics, trade and commercialization, dispute and coercion unfolded as part of the interplay between individuals; in other words, they were created and enacted in and through social networks, and b) the individual characteristics, traits, attributes and circumstances of actors or players in these networks had real effects on how this historical drama played out. Students of medieval history will be familiar with the frustration of having back-row, obstructed-view seating in the theatre where this drama is playing. While SNA cannot whisk us away to front-row seats – nothing can – it may offer the potential for new, albeit still distant and obstructed, vantage-points, with new ways of conceptualizing the agency of these actors, ways that are wholly distinct from our engrained habits of thinking about institutions and ideas. SNA offers a way of trying to factor in human behaviour, as individuals and in groups.

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