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RESEARCH ARTICLE

Capturing Peer Group Contexts: In Defense of Socio-Cognitive Mapping Strategies to Identify Children's Peer Network Affiliations

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ABSTRACT

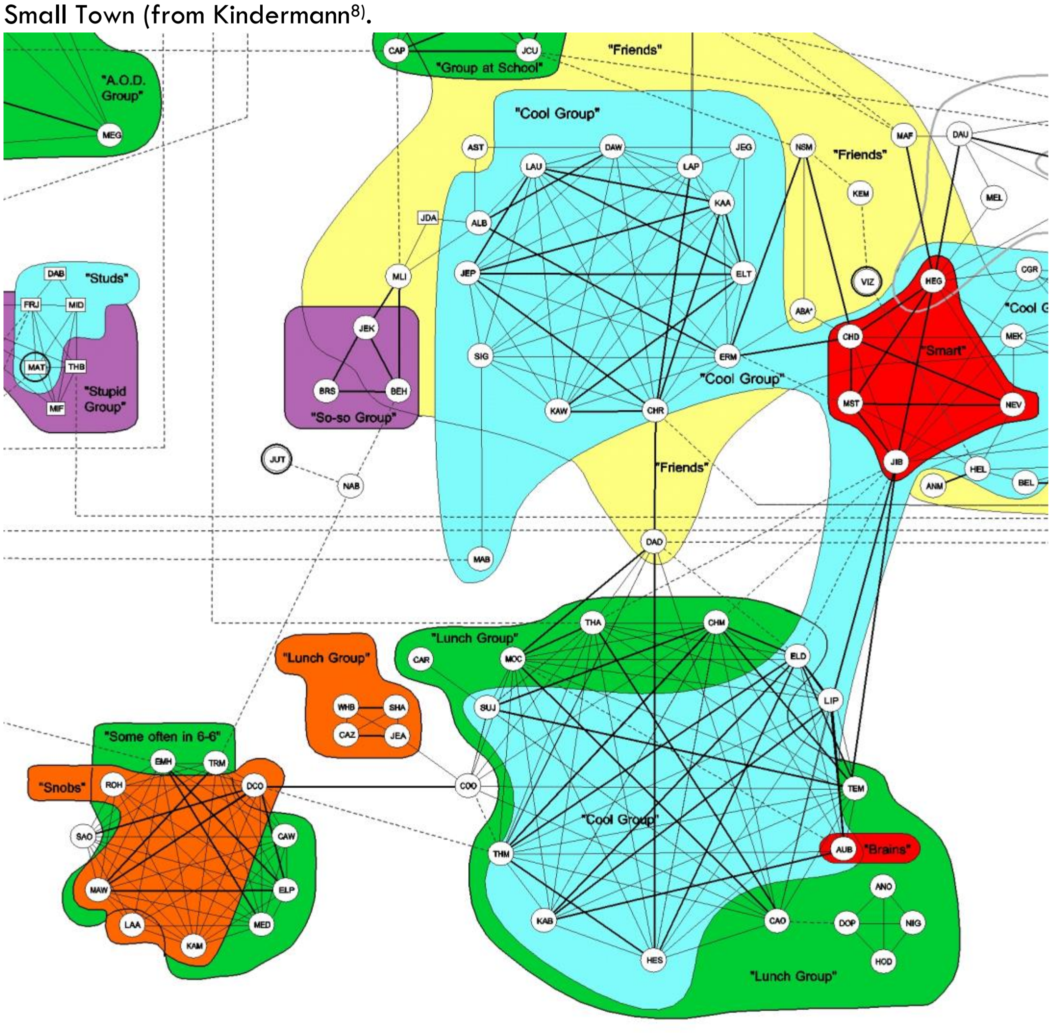
Socio-Cognitive Mapping is an observational method to collect information about people's social networks in settings in which participant observers know each other well, for example in school settings. Compared to traditional self-report data, observation reports make it possible to include (anonymized) network information about people who do not participate. In a series of papers, Neal and colleagues have criticized the methodology of Socio-Cognitive Mapping studies. However, the criticisms do not pertain to the data but only to a specific analysis program, SCM4, that was used in about 80% of the reviewed studies. To document their critiques, the authors introduce a new analysis strategy intended to correct some of the problems identified, and combine this with a promising new Community Detection method. They compare their results to SCM4 results and find in random simulations that, when using criteria that are more restrictive, fewer groups and fewer group members are identified. I highlight the extent to which the critique of the program is only justified under restrictive conditions, explain that the backbone of the proposed method has been used before, list problems of analyses that their method does not overcome, and outline avenues for their solution.

Capturing Peer Group Contexts: In Defense of Socio-Cognitive Mapping Strategies to Identify Children's Peer Network Affiliations

The goal of reliably identifying cohesive subgroups within people's social networks (i.e., cliques or peer groups) has been at the forefront of research on peer relationships since Moreno's pioneering efforts [1,2]. Two main data collection strategies are most prominent: People's self-reports of their own connections with others, and Socio-Cognitive Mapping strategies (SCM[3]), in which observers who know a setting well report on their own groups as well as on their observed affiliations of others. SCM has the advantage that network data are obtained on individuals who choose not to participate in a data collection. When care is taken to anonymize their data, the networks so identified become more inclusive[4]; Figure 1 gives an example (for the entire map, see https://web.pdx.edu/~thomas/graphics/jpgs/image002.JPG).

To analyze network data, many analytic tools are available. Most prominent is the framework UCINET (University of California Irvine Network Analysis[5]) which includes many tools and routines (e.g., n-clique, k-plex, Factions, Girvan-Newman algorithm). In addition, there are classical analysis programs (e.g., Negopy[6], CSS[7]) as well as recent developments (e.g., Neighborhood Detection algorithms; Islands algorithm; Moody's Crowds algorithm). All of these methods are primarily designed for use with self-reports but can also be used for multiple-reporter data. A specific program, SCM4, offers some advantages for such data, and it is this program that has come under concerted attack. The current paper is in defense of SCM data and in specific, in defense of this program. It examines the key critique points that were made about the program, disentangles the assumptions underlying the critiques, and clarifies those critique points that are justified.

Figure 1: Subsection of the Socio-Cognitive Map of Girls' Peer Groups in a Cohort of All 6th Graders in a Small Town (from Kindermann[8]).



The basic position of the current paper is that many of the critiques have been leveled at a "straw man," that is, at a monolithic "SCM method" that equates data collections and the nature of the data with SCM4 as their sole analysis method, and at a version of SCM4 that does not exist except in the minds of its critics.

Where Do Real Problems Exist with The SCM4 Program and Where Have Non-Existent Problems Been Created?

The evaluation of the critiques of Socio-Cognitive Mapping strategies is based on the latest attack, a paper by Neal, Neal and Domagalski[9] entitled *"False Positives Using Social Cognitive Mapping to Identify Children's Peer Groups*." The critique rests on four points. First, the authors argue that there exists a monolithic method -- "*the*" SCM method-- consistent and "dominant" (p. 1) that relies on the SCM4 analysis program or "variants" thereof (p. 3). Second, they contend that "the" SCM Method leads to the identification of large numbers of "false positives" in data on peer affiliations, and "always" identifies groups, even when none are present (p. 5). Third, they describe the method as consisting of five steps of data manipulations that, according to Neal and Neal[10,11], change the "meaning" of peer groups. Fourth, they argue that the method is not documented sufficiently and its inner workings remain obscure. As the solution to the problems, they suggest an alternative, which combines conditional probability analyses with a Community Detection algorithm.

On the surface, this seems to be a devastating critique. On closer inspection, however, most of these points in the paper[9] do not hold up to scrutiny. To give an example, SCM is depicted as the "dominant" method for identifying cohesive subgroups in naturalistic settings from data in which multiple peer observers report on groups (p. 1). One page later (p. 2), the authors state that forced-choice peer reports of Cognitive Social Structures[7] is the "most widely used" approach for analyzing multiple reporter networks. To make this point requires a very fine-grained distinction between reports of groups and reports of networks. "Dominance" of SCM is then a result of a narrow definition of research areas – and it is "dominance" in a very small pond: The vast majority of network analyses is based on self-reports of affiliations.

After addressing the four critique points, the current paper concludes that when researchers pay attention to some well-known minor problems with SCM4, they should still feel confident in using it. I end with a list of the real issues to be considered for future updated versions of the program, offer clarifications and corrections when these are needed, and weigh the value of SCM4 among alternative approaches.

Critique #1: Does "*the*" SCM Method Exist for Analyzing Peer Group Affiliation Data?

Socio-Cognitive Mapping is first and foremost a method of collecting data on children's and adolescents' peer groups in natural settings. This method was introduced in 1985 by Cairns, Perrin, and Cairns[12] as a way to identify peer groups from reports of resident expert observers (i.e., participant observations by children or adolescents in their own schools or classrooms). The goal was to reach a higher level of inclusiveness. In traditional self-reports of networks, people who do not participate are lost as individuals and as potential network partners. In SCM, participants are also observers and respond to a simple probe: "Who hangs out together in a group in your class/school? Please list the kids who hang out together in a group, for as many groups as you can, and include your own group(s)" (sometimes adding, "For each group, can you tell me what is it that makes these kids a group? What do they typically do together? Is there a name for this group?"). Observers report on known groups, including dyads. For example, observer A reports being in a group herself together with B and C, and also reports another group of D, E, and F. A key assumption is that reporters are experts about peer groups because they see them every day. Such data are 'multiple reporter data' (the term that Neal and colleagues[9] use); the difference is that SCM reporters are *study participants* and *expert observers* at the same time.

From these data, a *Co-Occurrence Matrix* can be formed, consisting of counts of the frequencies with which two students occur together across observers' reports. In A's report above, A occurs one time with B and once with C, B occurs once with A and once with C, and C occurs once with A and once with B. These are the counts of how often each child was observed together with any other child. The matrix is based on the program Networks[13]; the first version ran in 1986 on an Apple IIe. It was inspired by Bakeman's *Sequential Lag Analysis* [14], in which conditional probabilities of observed behavior sequences are compared to unconditional expected rates. With SCM data, because observers are reporting on the same visible groups, there is typically a high level of agreement among them. Then, various analysis methods are used on these data to extract composite maps of peer group membership.

Multiple Methods of Analyzing SCM Data

Neal and colleagues' critique[9] raises no concerns at all about the data of SCM methods. It is focused solely on SCM4, a specific analysis program. They argue that all SCM data are analyzed by SCM4 or "variants" thereof (p. 7). To construct this argument, the authors used a Google search for papers with the term "Socio-Cognitive Mapping" or "SCM" in their titles and identified 42 SCM studies among 72 studies that aimed to identify peer networks. All used SCM data collection methods (collecting multiple reporter data on "who hangs out together in a group?" and counting co-occurrences) but only about 80% used SCM4 for analyses. As shown in Figure 2, there are several options for analyzing cooccurrences: Kindermann[8,15] used conditional probability analyses; Gest and colleagues[16- 19] used factor analyses. At most, there is dominance of the SCM *data collection* method. Further, this count of studies is an outcome of the decision to exclude more diverse journals in their review (e.g., *European Journal of Psychological Science, Journal of Educational Psychology, Merill-Palmer Quarterly, Psychosocial Intervention, Zeitschrift fuer Erziehungswissenschaft, L'Anee Pychologique*). If a wider net had been cast, 12 studies with SCM data would have been found that did not use SCM4. In short(see Figure 2), Neal and colleagues[9] constructed a strawman to make the case that there would be a single ("the") dominant SCM method. To create a big target, they conflate the data collection method with the analysis program, dismissing both when the program is the only actual target of their critiques. By overcounting studies that use SCM data collections and undercounting analysis alternatives, they mischaracterize the "dominance" of the program.

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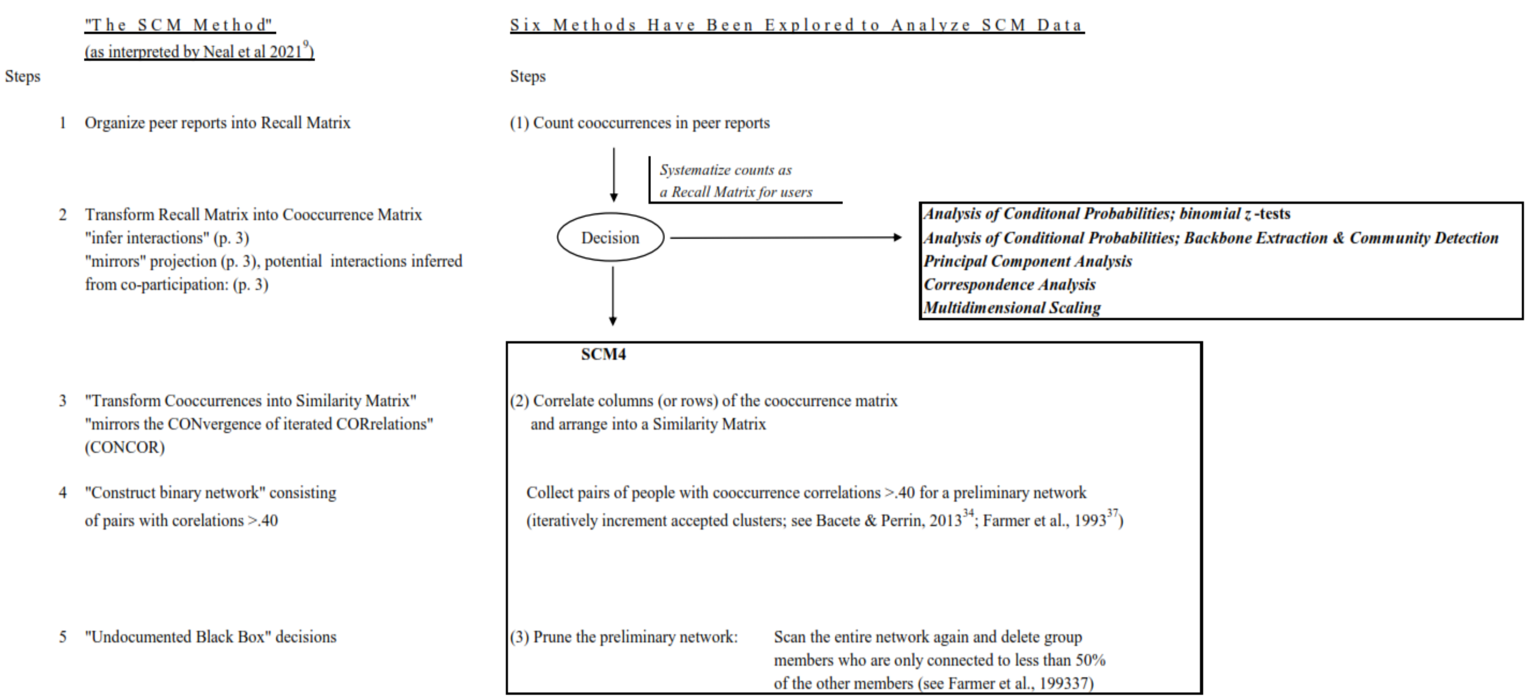
Figure 2. Socio-Cognitive Mappimg: Data and Analysis Methods



Critique #2: Does SCM4 Create "False Positives" When Detecting Groups?

Neal and colleagues' second criticism is that SCM4 produces "false positives". Implicitly, the authors assumed that all 42 studies found in their Google search used analyses that have in common a specific feature called "projection" (as hinted in Neal & Neal[10,11,20 21]). Projection combines two-mode network data into a one-node network (Breiger[22]). For example, if *A* observes B and C together in a group, *B* observes A and C, and C observes A and B, the observations can, according to Neal[23], be treated as a two-mode network and projected to observations that connect all into a triadic group, A-B-C (p. 85). However, *none* of the SCM studies does this. And, this would not be recommended because it can create affiliations between people who have no observed connections and inflate group membership.

On the observational level, contrary to

Neal and colleagues' earlier criticisms,[10,11] the co-occurrence matrix of the example is very clear: Three observers reported three dyads with tallies of '1'. Whether or not there could be a triad cannot be determined when observers do not agree. Beyond the observational level, analytics strategies for SCM data can broadly be grouped into probability-based and correlation-based analyses; the critiques of Neal and colleagues only apply to the latter methods. Correlation-based analyses (e.g., Factor Analysis, Correspondence Analysis, Multidimensional Scaling) assume that when people have similar co-occurrence profiles, these people can be seen as connected. This is sometimes justified, but sometimes not. For example, consider Max Weber's 1947 point that if a dozen people on a street open their umbrellas at the same time, this can denote a network effect, but it can also mean that, independently of interpersonal interactions, a rain shower occurred.[24]

This is where Neal and colleagues' erroneous assumption about projection comes in. SCM4 first correlates the columns of the co-occurrence matrix: People with intercorrelations above a certain level (usually .4) are preliminarily assumed to be in the same group. This can lead to projection-like results, but note that if so, this occurs via intercorrelations, not through inferences about the counts. Figure 2 includes quotes from Neal and colleagues' paper[9] suggesting that SCM's Step 2 data manipulations use projection and create artificially inflated counts. SCM4 does not involve any of these manipulations and matrix transformations. Instead, the process used by the program is simple and straightforward: It involves counting.

Neal and colleagues' assumptions about projection can explain several of the otherwise puzzling critique points they raised: If the co-occurrence matrix *were* a projected matrix, there would indeed be a coherent set of (42) SCM "projection" studies and it would be possible to group studies that use SCM4 with studies that do not. If projection occurred before correlations were tested, that could explain why one can end up with non-explainable results. And, because projection was never explained in any SCM4 manual or paper, one could have argued that there would be unexplained assumptions in a mysterious "black box" that produced non-replicable results. SCM4 does none of these things.

The "Creation of False Positives"

Even if SCM4 does not create false positives through projection, it is still possible that it identifies too many group members because of its correlational nature. As mentioned previously, similarity of co-occurrences does not guarantee affiliations. Neal and colleagues' paper[9] uses randomized data simulations to test whether SCM4 overestimates affiliations, and after a superficial reading, the verdict is concerning: The number of SCM4-identified group clusters seems much too large. However, this conclusion is far from justified.

First, the quality of an analysis program cannot be judged by its ability to deal with data it was not designed to analyze. There is a difference between SCM data and random data. Second, that pattern-finding programs find patterns in random data is reminiscent of the hurdles that Cluster Analysis proponents had to overcome before their method was accepted. To filter out illicit data, Cluster Analysis proponents developed tests to determine whether real clusters actually exist in a data set, and similar strategies have been developed for Factor Analysis (parallel analyses). Indeed, all pattern recognition rests on the assumption that there is something to be recognized. If there is something, a program should find it.

With observational data, the key criterion to determine whether there is something to detect rests on inter-observer reliability. In their earlier critique, Neal and Neal[10] presented artificial data in which no observer agreed with any other observer (i.e., A reports A and B to be in a group; B sees B and C; C sees C and A) and argued that SCM4 cannot distinguish whether there are three dyads or one triplet. It should not. Why? Because the observers do not agree on any groups; hence there aren't any (Kindermann[15,25] also excluded cooccurrences of 1 in order to exclude self-enhancing self-reports). Reliability needs to be examined prior to running group detection programs. If classrooms are the unit of analysis and expert participants observe their own classrooms, quick inspections of reporter agreement will suffice (as long as more than 50% of setting members participate[3]). In large samples, observer agreement will not be obvious, so Kindermann[25] recommended a *kappa*[26] (agreement corrected for chance) of .70 or higher.

In the above example, the determination of whether there are any groups will depend on additional observations. If a further observer also reports any of the dyads (raising co-occurrences from 1 to 2), that dyad is reliably observed and can be accepted. There would be a triad as soon as another observer (D) reports that A, B, and C are together (the dyadic reports are taken as errors of omission). Whether such patterns will be identified in SCM4 will also depend on the significance levels of the co-occurrence intercorrelations (i.e., none of the above patterns would be accepted if individuals also have frequent connections with other people, say E or F).

Although their overall criticism does not withstand scrutiny, Neal and colleagues' example does identify two valid critique points: First, SCM4 has no check for reliability built into the program. Researchers themselves have to know that they need to check it. Secondly, there is no alert when "significant" patterns are based on single nominations; researchers need to exclude those based on inspection of the output. To my knowledge, the published SCM4 papers took both of these steps. In Neal and colleagues' simulations, the authors chose not to do so.

However, there is also an elephant in the room. How many significance tests were conducted in the simulations? As results of their simulations with SCM4, Neal and colleagues[9] report that on average, two-thirds of children were found to be with a group when using SCM4 (Study 2, p.6). This means that more than 18000 tests showed significant correlations. How many of these should be accepted when adjustments for chance significance would have been made? Typically, peer researchers do not use Bonferroni adjustments when just a handful of classrooms are studied, but adjustments are needed with this many simulations (either constrained to specific classroom characteristics, see Neal and colleagues' Study 2[9], or unconstrained with completely random data as in Study 4). Even with adjustments by a factor of 100 (on a level of .0005), only a very small number of children, if any, would remain accepted in a group. In Neal and colleagues' Study 4 using Backbone Extraction and Neighborhood detection[9], only between 7% and 14% of children were found to be in a group; however, most of these connections would likely also disappear after adjustments for chance. In sum, one method would be criticized because it produced more pseudo-significances (noise) than another, when actually, likely none of the significances in the random data should be accepted.

Do the Decisions Neal and Colleagues Made as Analysts Make the SCM4 Program Look Worse Than it Actually Is?

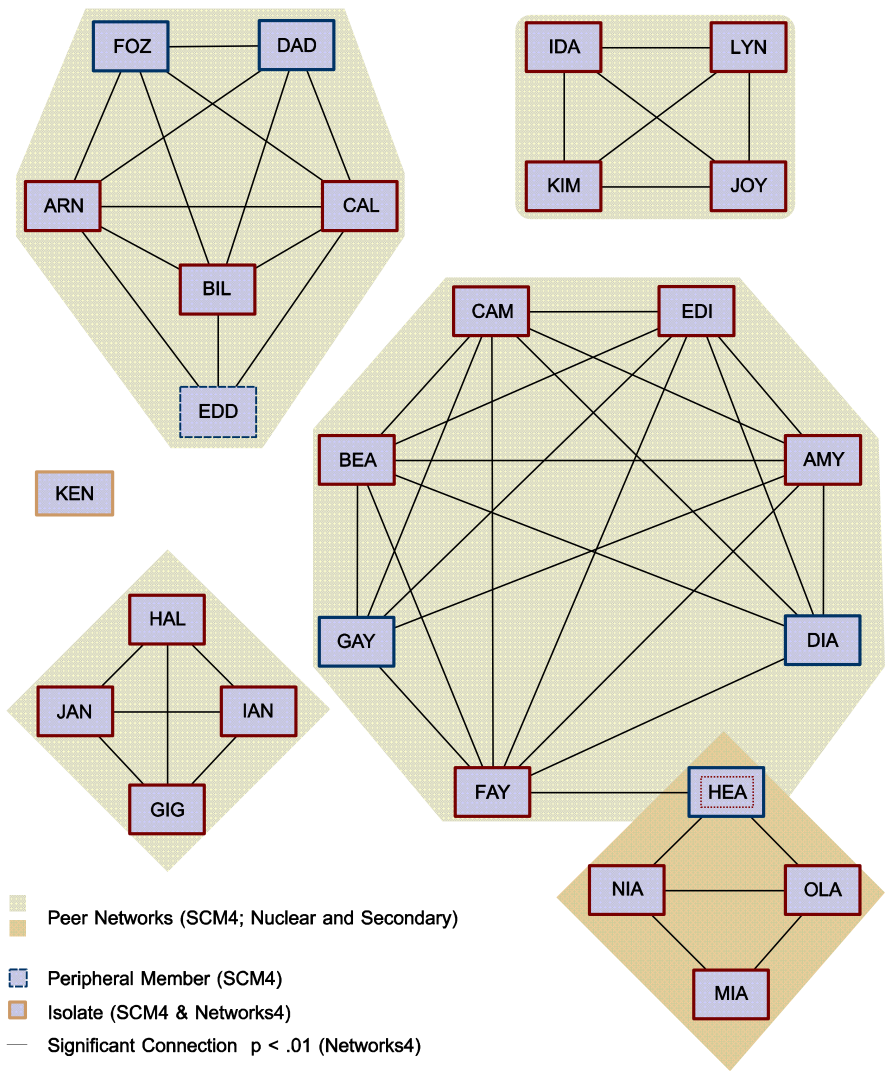
Neal and colleagues propose[9] a promising method for analyzing data collected via SCM that combines conditional probabilities with community detection (Study 4). The goal was to show that this is better than SCM4, and they conducted five simulation studies comparing the two analysis methods. I will take up the merits of their new method later, but when using SCM4, Neal and colleagues made decisions that informed analysts would not easily make; these decisions made the program look worse than it actually is. Neal and colleagues imply that most researchers also made these same decisions in prior studies. But they did not necessarily do so, and even if they did, it is not the fault of the program. The differences between SCM4 and Neal and colleagues' new method arise, not in the output of these two programs, but as explained in the next sections (1) in the input where, instead of reliable co-occurrence data, completely random data are used; (2) in their interpretation about how to treat dyadic connections; and (3) in their interpretations with respect to whether students can belong to multiple groups.

SCM4 in Simulations of Data it is Not Designed to Analyze. In their *Study* 3, Neal and colleagues[9] show that in 1000 random simulations, peer "groups" were identified about 800 times by SCM4, and 66% of children were identified as members of groups. This seems like a lot. However, should one really expect that there would be "no groups" in random data (p. 7)? Consider a game of poker: The probability of having a hand with a pair or higher is about .499 (almost 500 times in 1000 draws). If one only accepts three of a kind or higher, as in Neal and colleagues' Community Detection comparison, the probability is .029 (29 times). Importantly, the probabilities are never zero. Even in random data, patterns should be expected: Pairs and three of a kind in a poker game, and groups in randomized observations.

Nevertheless, SCM4 shows a high number of groups. One explanation rests on the fact that SCM4 was designed to analyze co-occurrence matrices representing expert observer reports on existing peer groups, that is, data that already reflect reliable reports of co-nominations from multiple reporters. When used with such data, SCM4 results were shown to be comparable to those of Neal and colleagues' new method. This is demonstrated in their *Study 1*, where they use reliable data (the sample of Cairns and Cairns[3]). Her, findings from SCM4 are consistent with results from the new combination of Backbone Extraction with the Community Detection method (BE & CD[28]). When reliable SCM data are used, the results match, and this is exactly as it should be. When the SCM4 program was first developed in the 1990's, the patterns from z-tests of conditional probabilities matched with SCM4 results (see Figure 3).

Dealing with Dyadic Connections. The second explanation is that SCM4 includes some dyads, purposefully, whereas the authors' new method does not, also purposefully. This is a decision, not a program feature; the authors' method could have included dyads, or dyads could have been excluded from the SCM4 output. Since inclusiveness is one of the goals of SCM4 (and SCM data collections), dyadic connections are included under certain conditions: The intercorrelations of their cooccurrences need to be above .40, there are no disconfirming observations, and the groups are reliably observed.

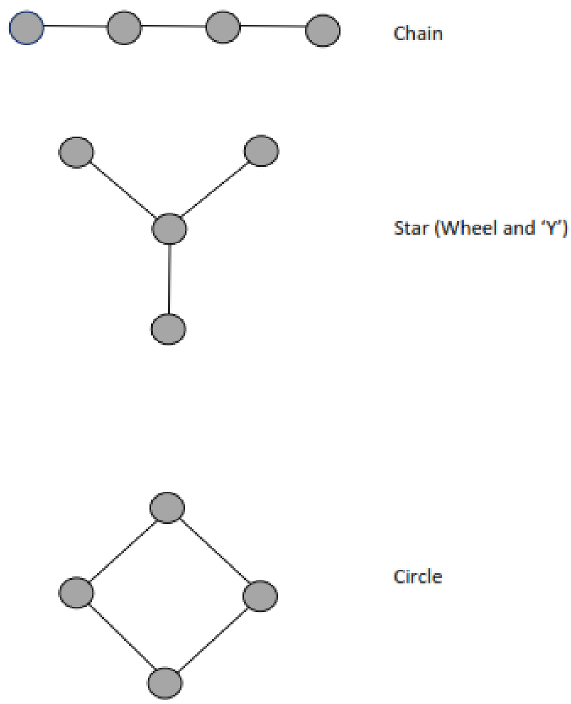
Figure 3: Group Contexts Identified by SCM4 and Networks4 in the data of Cairns et al[3,12]



One reason for including dyads is that Robert Cairns aimed to include Bavelas-like group constellations (see Figure 4). The goal was to include structures like "stars" or "chains" as peer groups. Many traditional group theorists, however, do not consider such patterns to denote groups; instead, a Bavelas-chain of three would be seen as a pattern of two dyads, not one group. SCM4, however, includes people in a single group when they share connections with 50% or more of its members. Readers can imagine this as an iterative process, in which first, dyads are incremented to triads, and then, two triads are combined (occasionally, researchers have also used 60% as the minimum, in order to exclude such patterns).

Bavelas groups can be real (e.g., in a school newspaper group, only the editor may be in touch with all the reporters). Thus, the patterns in Figure 4 may denote groups, even if they consist just of interconnected dyads (assuming there would be no observer who reported a triad). Many neighborhood detection methods will likely only include those as interconnected dyads.

Figure 4: Bavelas Groups: Dyadic Connections and "Groupness"



Neal and colleagues'[9] method explicitly excludes dyads (p. 5 footnote 7, and so do some SCM researchers[27]). SCM4, however, will include the patterns in Figure 4 as (secondary) quadruplet groups in its output if the criteria for inclusion are met (i.e., cooccurrence intercorrelations are above .40; little connections to outsiders; see HEA in Figure 3). When researchers want to exclude them, they need to be taken out separately from the output. This is a problem of SCM4: Bavelas-like dyads are hard to detect from the output. A future version of the program should aim to identify them separately and give an estimate of their reliability.

Interpreting Output on Multiple Group Membership. The third example of a decision made by Neal and colleagues[9] that needs to be questioned is that, in their Community Detection method, they added a decision rule: Only accept primary (non-overlapping) groups. (The authors state on p. 8 that this was done in "most" SCM studies -- they refer to four -- but I am less than sure that this is true.) This rule defies ecological validity, since in the real world, most children belong to multiple overlapping peer groups[29].

This decision produced a problem highlighted by Neal and colleagues (in their Figure 1; equivalent to Figure 3 here [9]) and revolves around child HEA who shows close connections to two different groups of children that are interconnected. The question is whether HEA belongs to multiple groups (and similarly KEN, not included in the Figure) or just to one most important group. Neal and colleagues argue that because HEA seems to have two groups, her primary group affiliation cannot be decided from the data, and her network pattern should be ignored. However, HEA is well-integrated in the classroom system (Figure 3) and should not be ignored. The outcomes of this decision are apparent in Neal and colleagues' *Study 4* which uses BE-CD[9] to contrast SCM4 analyses of real classroom data (data set 1), randomized classroom data (set 2), and freely simulated data (set 3). SCM4 performs badly when there are no experts and there is no reliability. But the comparisons actually look worse than expected. SCM4 aims to preserve multiple group memberships, whereas Neal and colleagues' BE-CD alternative decision rule explicitly excluded them. This is not a matter of programs but of analysts' decisions. To expect that each of the simulations contains two individuals with multiple group membership may be unrealistic, but if only a quarter of the simulations contained such cases, there would be dozens of multiple memberships. This could explain much of the differences.

This decision rule needs to also be questioned. First, to identify distinct and separated groups is not always a good goal of research questions in the real world: Groups tend to be naturally overlapping. Secondly, because many network researchers have trouble with groups that overlap, if ignoring a person is not an option, a decision will need to be made about a primary group and a secondary group affiliation. Often, such decisions are made based on levels of connectedness and favor membership in the more interconnected group. However, statistics may give bad advice here: It will make a difference whether a student is assigned to the Journalism Club or the Rock Band at school (which will likely be more cohesive) when he or she is a member of both. The decision should not be made based on statistical considerations but should depend on the research question under study. Thus, when academic development is examined, choosing the most cohesive subgroup can be a mistake; it should be the group that is most important for academic development (or, if in doubt, both groups should be included). If social integration, group selection, or group influences are considered, individuals with multiple group memberships can also not be ignored and their connections need to be included.

Over-identification or Under-identification? In sum, looking at the distributions in Figures 3 and 4 of Neal and colleagues'[9] critique, there are multiple reasons why SCM4 would show more groups and more members. However, major differences between the results from the new community detection method and SCM4 do not stem from peculiarities of SCM4, but from the decisions made in the comparison itself. On the one hand, SCM4 may overidentify groups when input data are not reliable. If so, then the verdict is clear: Do not use SCM4 with data that it was not designed to analyze. When there are no experts, "findings" should not be trusted. Researchers need to check for data reliability by hand (just as when using Cluster Analyses or Factor Analyses). An automated check may be preferable in future revisions. On the other hand, when differences in identification are based on decision rules that analysts impose about whether to count dyads and multiple group memberships, information on these rules needs to be provided. From the information provided by Neal and colleagues, this possibility cannot be evaluated. To do so, readers would need to know how many of the patterns identified by SCM4 were dyads and how many were cases of multiple group memberships. Based on Neal and colleagues' use of restrictive criteria, the comparison may not indicate that SCM4 overidentifies groups, but that their choice of decision rules lead to under-identification of groups.

Critique #3: Does the "SCM method" consist of five steps of "manipulating" collected data, and do the manipulations change the meaning of "peer groups"?

The authors seem to be invested in their own preferences for network analysis, as evidenced in the steps they attribute to SCM methods. The actual SCM data collection and analysis methods remain close to the observational nature of the data, and the steps that SCM actually uses are depicted in Figure 2. In the SCM studies that did not use SCM4, analyses methods consisted of two steps: (1) creating a co-occurrence matrix and (2) analyzing it with conditional probability methods or factor analysis. In studies where SCM4 was used, there are three steps: (1) creating a co-occurrence matrix, (2) correlating columns of the co-occurrence matrix and collecting individuals with correlations lager than .40; and (3) pruning the network. In comparison, Neal and colleagues' description [9] consists of five steps (see Figure 2).

Changing the Nature of Peer Groups

One can argue that it is the second step in SCM4, namely, correlational analyses, that could change the nature of groups, as inferred from Neal & Neal[10,11]. The authors are correct in that correlating bivariate connections in the co-occurrence matrix does not always lead to the best representation of groups; combinatorial or Markov approaches would be preferable. SCM4 does not do this, because there are usually not enough data to count and compare triplets, quadruples, and so on (but such strategies have been tried[30]). At the same time, the use of correlations is not uncommon and not always considered problematic. For example, proponents of factor analyses of network data[31] do not see a problem with a correlational approach. Even Neal and colleagues[9] seem to view this as less problematic now than years ago. The Community Detection method introduced in their paper builds on conditional probabilities with which two people are observed in the same group report (as in[15,16]) and then applies a community detection algorithm. Most community detection algorithms use similarity matrices and thus correlational methods. This suggests that the use of correlational methods in SCM4 would not be considered a serious problem any more. In sum, counter to Neal and colleagues' critiques, analysis of SCM data does not use projection and not five steps of data "manipulation." Instead, it employs two or three steps, all of which keep analyses close to the original observation data.

Critique # 4: Is the "SCM Method" a "Black box" that is Not Sufficiently Documented, and whose Inner Workings are Not Transparent?

Neal and colleagues[9-11] have argued that SCM4 is a "Black Box" with obscure inner workings. It is important to note again that this concern applies only to the SCM4 program. Nothing was ever unclear about SCM data collection methods or the co-occurrence matrices that are subjected to analyses. It is actually possible that this critique about the workings of SCM4 had some validity decades ago. When Robert Cairns died suddenly in 1999, SCM4 was finalized but its documentation was not. There existed a provisional Manual[32] and an unpublished co-authored paper[33]. Understandably but unfortunately, the programmers wanted to leave the program as it was and did not want to see it changed (e.g., efforts to combine it with Networks4 did not succeed because the source code was not shared; this may have been the same for Neal and colleagues,). So, the authors' frustration in this regard is understandable. And this protectiveness perhaps contributed to the misguided assumption that projection was a part of SCM4, when clearly it is not. Today however, little remains uncertain and published descriptions are available[32 - 37]. Note that years ago, information about the program was already sufficient to allow Z. Neal to design his own version of SCM4[38].

Documenting Decisions Surrounding the Use of SCM4

Based on the confusions and misinterpretations found in Neal and colleagues' critiques, it seems possible that additional documentation would be helpful, not so much about the SCM4 program itself, but about the series of decisions that need to be made by researchers surrounding its use. Drawing together information from previous sections of this paper and several decades of study using SCM, the following aims to clarify two main decision points.

First, there are decisions about the input; these focus on whether the data are sufficiently reliable to justify any further analysis. Second, there are decisions that researchers have to make from the SCM4 output; these involve determining which identified groups they actually want to accept. Since these decisions can vary based on the specific research question, researchers typically only describe the decisions they actually made in a particular study.

Are the Data Reliable? Observations rely on observer reliability. In small datasets, this can be gauged from the convergence among observer reports. In larger data sets, this needs to be examined separately (e.g., Kindermann's kappa indices). Future versions of the program should include such tests so that random data cannot be used.

How Should One Treat the Diagonal of the Cooccurrence Matrix? In SCM4, the use of the diagonal (which technically speaking should include the number of times A co-occurs with A) was never much scrutinized and is not well documented. The solution of SCM4 was to include in the diagonal of the cooccurrence matrix the raw observation frequencies of each observed person (i.e., the number of times each person was named as being a member of any group by the whole set of observers). This was a corrective measure to account for signal strength (dampening of correlations when signal strength deviates from co-occurrences), and makes the correlations more conservative for people whose co-occurrences differ much from their overall observed counts. The inclusion of signal strength in the correlations was, perhaps, audacious 30 years ago, but it has since become common practice, for example, in text mining[38,] and health statistics[39]. There may nevertheless be situations when this is not an optimal solution and Neal offers in his SCM4 version[38] an option to normalize the data that fills these values. This, however, will often smoothen the distributions and can then lead to higher correlations (and larger groups) than researchers may want to accept. So, caution should be used and researchers should be asked to explicitly justify their decisions if they adjust these values.

Do researchers want to accept dyads and multiple (overlapping) group memberships? SCM4 was designed to be inclusive, with the idea that children who belong to dyads cannot be considered social isolates, and that overlapping peer groups are the rule rather than the exception. If researchers decide not to include dyads and/or multiple group memberships, these need to be pruned from the extracted group reports. Thus, SCM researchers exclude them manually if they need to and it is considered problematic to exclude them by default. If these decisions are going to be made, they should be should be made explicitly (and justified) by researchers and not by an analysis program.

Conclusions: Letting Go of a Strawman

In their latest critique of SCM[9], Neal and colleagues created a strawman-- a picture of SCM that is inaccurate -- and then knock it down. Their criticisms are based on four premises. First, they argue that SCM is a monolithic method that dominates the identification of peer groups. However, it is neither monolithic nor dominant. SCM is not primarily a data analysis method, but first and foremost a data collection method, and SCM4 is not the only way to analyze data collected via this method. Second, Neal and colleagues contend that SCM4 identifies large numbers of "false positives" in data on peer group affiliations, and "always" identifies groups, even when none are actually present. Their evidence rests on the use of random data that a real peer group researcher would not subject to further analysis, as well as on comparisons between results from SCM4 and from a Backbone Extraction and Community Detection method in which they excluded dyads and insisted on non-overlapping groups. This may lead to the under-identification of how connected individuals really are with their peers. Most importantly, these criteria reflect analysts' decisions and not features of the program.

Third, the authors describe SCM methods as consisting of five steps of data manipulations that change the "meaning" of peer groups. In reality, the analyses consist of counting co-occurrences and aggregating the counts into matrices, followed by one or two further steps (depending on the specific method), all of which keep the data close to their observational base. The only step where questions may arise is the use of correlational methods—which, however, have become widely accepted in community detection methods. Fourth, Neal and colleagues argue that the method is not documented sufficiently and its workings remain obscure. Today, the code is available and the program was documented well enough in 2014 that Neal and colleagues were able to create their own version of SCM4.

Valid critiques rely on an accurate portrayal of the object to be criticized. The current reply aimed to paint a more accurate picture of SCM by: (1) providing an overview of the method and its roots in the collection of observations; (2) explaining how important it is to use the kind of data the program was intended to analyze (i.e., reliable co-occurrences); (3) reminding readers of the several alternatives to SCM4 in analyzing such data; and (4) describing the steps researchers go through in making decisions about which parts of the program's output to use and which to prune. These decisions should be based on investigators' expertise and their specific research questions, and should not be delegated to an analysis program. To sharpen the critiques, it might be helpful for Neal and colleagues to reorient themselves to a broader understanding of SCM as an area of observational research and to the facts of how that research is conducted. Or, they may wish to go beyond criticism. Given their expertise, and perhaps in collaboration with researchers who are experts on the method *and* the substance of the questions it is designed to address. This could make real and lasting contributions to this area of research.

Epilogue

It may seem surprising that this paper addresses critiques of SCM4, when the author has never used the program in any published paper. However, SCM4 is based on the analysis of the co-occurrence matrix that was co-developed together with Robert Cairns, and the current paper objects to the portrayal by Neal and colleagues of SCM4 as a complicated method that involves a large set of transformations, matrix manipulations, and controversial assumptions. From the critiques, readers get the impression that SCM data are problematic to analyze and SCM4 is almost impossible to understand. The goals of the current paper were to show that SCM data are straightforward and that SCM4 provides one good alternative for analyzing them. Personally, the experience of looking deeper into SCM4 gave me new respect for this 30-year-old program. It is essential to have accurate portrayals of various methods in order to compare their advantages and disadvantages, and SCM4 is still a good alternative that works reasonably well and so deserves to be considered as a valid alternative.

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