Measurement Error and Attenuation Bias in Exponential Random Graph Models*

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Abstract

Exponential Random Graph Models (ERGMs) are becoming increasingly popular tools for estimating the properties of social networks across the social sciences. While the asymptotic properties of ERGMs are well understood, much less is known about how ERGMs perform in the face of violations of the assumptions that drive those asymptotic properties. Given that empirical social networks rarely meet the strenuous assumptions of the ERGM perfectly, practical researchers are often in the position of knowing their coefficients are imperfect, but not knowing precisely how wrong those coefficients may be. In this research, we examine one violation of the asymptotic assumptions of ERGMs - perfectly measured social networks. Using several Monte Carlo simulations, we demonstrate that even randomly distributed measurement errors in networks under study can cause considerable attenuation in coefficients from ERGMs, and do real harm to subsequent hypothesis tests.

Keywords: Exponential Random Graph Models; Social Network Analysis; Measurement Error; Monte Carlo Simulation
Word Count: 6,168

*Complete replication data and the appendix will be made available online at the author's website immediately upon acceptance for publication.

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In many ways, the study of social phenomena hinges on understanding how individuals interact with one another (McClurg and Young 2011). From studies of power, influence, and exchange (Cranmer, Desmarais and Menninga 2012; Desmarais and Cranmer 2013; Kinne 2014) to studies of collaborative, collective action (Box-Steffensmeier, Christenson and Hitt 2013; Box-Steffensmeier and Christenson 2014; Kirkland and Williams 2014), interdependent relational patterns form the core of a large portion of the questions that motivate social research. Traditionally, modeling such relational patterns has fallen under the scope of social network analysis, and thanks to recent computational innovations, statistical models of those patterns are increasingly estimated using the Exponential Random Graph Model (ERGM) (Heaney 2014; Cao 2012; Oatley, Winecoff, Pennock and Danzman 2013; Alemán and Calvo 2013; Ringe, Victor and Carman 2013). ERGMs have become the primary mode of analysis for social scholars interested in testing hypotheses and forming predictions about relationships between social actors.

In what follows, we examine the behavior of ERGMs when the network of interest suffers from random measurement error. Such errors may arise due to incomplete or inaccurate archival records, faulty recall by name-generator respondents, or simple corruption of data files. Our examination leverages a series of Monte Carlo experiments that demonstrate the influence of random measurement error in a network of interest on coefficients estimated by an ERGM. We then discuss how measurement error biases ERGM coefficients, noting that the degree of that bias is associated both with the strength of an association in the "true" network, and the degree of endogeneity in a network pattern of interest. However, in an analysis of the statistical power of ERGM hypothesis tests, we demonstrate that the hypothesis test power loss associated with this attenuation bias is most worrisome in small networks, and networks of moderate size ($\sim$ 75 actors or more) seem to have little lost power.

1 Exponential Random Graph Models

For many years, social and political scientists made heavy use of dyadic logistic regression models for relational data. For example, if we were predicting the probability that two countries would go to war, we could have dataset with $n*(n-1)$ observations with each observation representing a country-to-country dyad, and the dependent variable would be coded one when two countries were at war (Poast 2010). However, the dyadic logit model fails to capture interdependent relationships among observations because it assumes all observations of y are identically and independently distributed. In the face of interdependent dyadic relationships, such interactions are more properly viewed as a social network. In such a network, the relationships between dyads (and triads and other higher order groupings) can be influenced by other dyads through endogenous patterns like reciprocity or transitivity (Ohtsuki, Hauert, Lieberman and Nowak 2006; Newman and Park 2003).

Indeed, in many social network applications, it is the interdependence of relationships itself that is of interest. The existence of reciprocity in a network, and whether that level of reciprocity is unusually large, is often the primary inference of interest for network scholars. ERGMs accomplish this feat by treating the entirety of a network as the dependent variable (rather than each dyad in the network), and thus, often ask questions like "is the reciprocity in this network larger or smaller than expected across a distribution of possible networks?" By treating the network itself as the dependent variable, network analysts can use ERGMs to ask which endogeneous patterns are unusually common in the observed network, while also asking which exogenous influences seem to drive relational formation.[1]

ERGMs treat a network as a realization from a distribution of random graphs.[2] This network y is one particular pattern of all the possible sets of edges on the number of nodes in our network. The node and dyad covariates are attributes of the actors in the network. ERGMs predict y_m, including nodal covariates, x_n, dyad covariates, x_d, and endogenous patterns of relationship like reciprocity, R. ERGMs have the following general form:

$$Pr(y_m) = \frac{exp(-\sum_{j=1}^{k} \Gamma_{mj}\theta_j)}{\sum_{m=1}^{M} exp(-\sum_{j=1}^{k} \Gamma_{mj}\theta_j)} \tag{1}$$

[1]Throughout this research, we focus our discussion on reciprocity effects. This is not because we believe reciprocity to be the "most important" network effect, but instead is because reciprocity is also the simplest endogenous component of ERGM term.

[2]This introduction to ERGMs is largely drawn from Cranmer and Desmarais (2011), Robins and Lusher (2013), and Koskinen and Daragonova (2013).

where y_m is the network that we wish to model, j is a particular parameter, k is the number of parameters, and m is an index for a particular graph (i.e. network) within the distribution of random graphs, M. Γ is defined as any network statistic, and θ is the coefficient on Γ.

Based on maximum likelihood theory, we attempt to capture the value of θ that maximizes the probability of observing a particular network pattern, Γ, given the random distribution of networks. Thus, Γ represents the covariates in our network model, and θ represents the coefficients on those covariates. ERGMs attempt to find the distribution of random graphs (the denominator of equation 1) most likely to have produced the observed graph (the numerator). To do so, ERGMs attempt to generate a distribution of graphs in which the expected value of the denominator in equation 1 matches the numerator.[3] In a network model, Γ can be node-specific, dyad-specific, or higher order combinations of exogenous attributes and endogenous sets of relationships.

With this general form in mind, consider a dyadic covariate like homophily.[4] ERGMs estimate coefficients on dyadic covariates like homophily by counting the number of homophilous ties in the observed network via the following equation:

$$\Gamma_{x_n}(y, x_n) = \sum_{i \neq j} \delta(x_i x_j) * y_{ij} \tag{2}$$

That is, a dyadic pattern, $\Gamma_{x_n}(y, x_n)$ counts all $i \neq j$ situations in which $x_i = x_j$, and $y_{ij} = 1$.[5] Notice that, even in this formula based largely on characteristics of actors exogenous to the network itself, y_{ij} appears in the measurement of the covariate. Thus, any measurement error in y_{ij} will also appear on the right-hand side of the ERGM formula, potentially inducing bias in coefficient estimates on that largely exogenous covariate.

The potential problems associated with measurement error in the network become more severe as we move from considering largely exogenous covariates to the endogenous patterns in rela-

[3]This occasionally leads to a problem referred to as "degeneracy" in which the distribution of random graphs most likely to have generated the observed graph is the set of fully complete or fully empty networks. This is typically a sign of a poorly specified model that fails to capture the patterns of interdependence driving the data generating process.

[4]An example of a homophilious tie is party identification, where a tie exists from one Republican to another.

[5]In Equation 2, δ refers to Kronecker's Delta, a mathematical operation that returns a one when the two numbers inside the δ are equal to one another.

tionships commonly of interest to social network scholars. For example, in order to estimate a coefficient on reciprocal relationships in a network, ERGMs count up the number of reciprocal relationships in a network via the following equation:

$$\Gamma_R(y) = \sum_{i<j} y_{ij} y_{ji} \tag{3}$$

$\Gamma_R(y)$ then, is the count of dyads for which connections from i to j and from j to i exist. While the potential for measurement error in y to influence coefficients on homophily via equation 2 exists, this potential should be much higher for coefficients on reciprocity, as the mis-measured y now appears on the right-hand side of the ERGM model twice. This potential for bias via measurement error is only likely to increase as the complexity of endogenous patterns used as covariates in ERGMs increases. Triadic patterns like transitivity add up counts of triangles in a network, allowing a mis-measured y to influence the relevant covariate in three possible ways (each y_{ij} in the triangle). Thus, even for the simplest and most exogenous covariates typically included in ERGM models, measurement error in ERGMs is likely to generate mis-measured counts of patterns in the network, and thus, is likely to produce coefficient bias. The severity of this bias also seems likely to be greater for endogenous covariates like reciprocity than for somewhat exogenous covariates like homophily.

2 Simulating Measurement Error and Coefficient Bias in ERGMs

The key point from this introduction is that it is typically the case that independent variables in ERGMs are measured as endogenous count statistics derived directly from the network under study. This immediately suggests that random measurement error in the network of interest (the dependent variable) simultaneously generates random measurement error in the independent variables. Random measurement error in independent variables creates attenuation bias in coefficient estimates for those independent variables in most regression approaches,[6] but we hope to determine whether this is also true for ERGMs. To evaluate the degree to which random measurement

[6]See the appendix for a review of this phenomenon

error in the network of interest generates attenuation bias in ERGM coefficients, we design a Monte Carlo simulation.

To begin our simulation, we create 25 artificial actors and assign them randomly to two membership groups with a probability of 0.5 of winding up in group one over group two. We then simulate a network of interactions between these actors with a baseline probability of connection, a fixed reciprocity coefficient, and a homophily coefficient creating an elevated probability of connection between two actors belonging to the same group assignment. This provides us with a social network with a known data generating process governed by a purely endogenous process (reciprocity), and an exogenous covariate (homophily). In our simulations we set both the reciprocity coefficient and the homophily coefficient to a value of two.[7] After creating this initial perfectly measured network, we run an ERGM on the network and record the coefficients.[8]

After recording the coefficients on our perfectly measured simulated network, we introduce measurement error into the network itself. We do so by considering each dyad in the network, and if an edge exists between the actors in the dyad we eliminate the edge with some pre-established probability, p. If an edge does not exist between the dyad members, we establish the connection using the same pre-existing probability. Thus, if actors i and j have a connection in the network and $p = 0.1$, then they retain their connection with a probability of 0.9 and lose their connection with a probability of 0.1. If i and j did not have an existing connection, they gain a connection with a probability of 0.1, and maintain their lack of connection with a probability of 0.9. Consequently, we are not simply adding connections to the network (i.e. missing edges), we are also eliminating some connections as though the actors in our network mis-reported to whom they were connected.

Having randomly introduced errors in the dependent variable, we then again run an ERGM containing covariates for homophily and reciprocity, which we can compare to the coefficients from the ERGM run on the perfectly measured network. It is important to point out at this point

[7] We utilize the "statnet" package in the statistical program R (Handcock, Hunter, Butts, Goodreau and Morris 2008; R Development Core Team 2013) to execute our simulations. In the supplemental appendix, we provide the full code necessary to reproduce our simulations.

[8] For all of our reported ERGM results, we use a burn-in period of 50,000, and record a posterior sample size of 2,048 using an MCMC interval of 2,048. Thus, we have 4,000,000 draws from the posterior distribution of random graphs.

that we have only introduced errors into the dependent variable of interest. In traditional regression approaches, random errors in a dependent variable of interest only increases the standard errors of reported coefficients, but because independent variables in an ERGM are constructed directly from the dependent variable, random errors in the network of interest are also likely to create bias in reported coefficients in addition to inefficiency. Our simulations begin by setting p to 0.05, and then executing 1,500 simulations. We then increase p to 0.1, 0.15, 0.2, and 0.25, with each value of p also receiving 1,500 simulations.

Figure 1 reports the results of our initial simulation efforts. The top panel of the figure plots the distribution of reciprocity coefficients from the perfectly measured network, and the distribution of reciprocity coefficients from the network with random measurement error. The bottom panel reports the same distributions for the homophily coefficients. Recall that we set the value of these coefficients to two during the data-generating process. The results from our initial simulations indicate two key findings. First, as random measurement error in a network increases, the coefficients from a model of that network increasingly attenuate towards zero. Indeed, by randomly altering only 25% of the connections in the network, we reduced the average magnitude of the reciprocity coefficient more than 80%.[9] Second, our results also indicate that the attenuation bias of the homophily coefficient is much weaker than that of the reciprocity coefficient. This could be taken in one of two lights. We could see this as evidence that exogenous network coefficients suffer less bias as a result of random network errors, or we could express relatively severe concerns that even our exogenous coefficients are attenuated in the presence of network errors.

2.1 Network Size, Attenuation Bias, and Power Loss

The attenuation bias we observe in our simulations raises considerable concerns about network scholars' ability to reject null hypotheses, particularly for endogenous network relationships. One way to combat a loss of statistical power in a hypothesis test is to increase the sample size of a study. In order to see whether increasing the size of the networks we consider reduces attenuation

[9]The "true" value of the reciprocity coefficient is 2, but the average ERGM reciprocity coefficient in networks where p=0.25 is only 0.36.

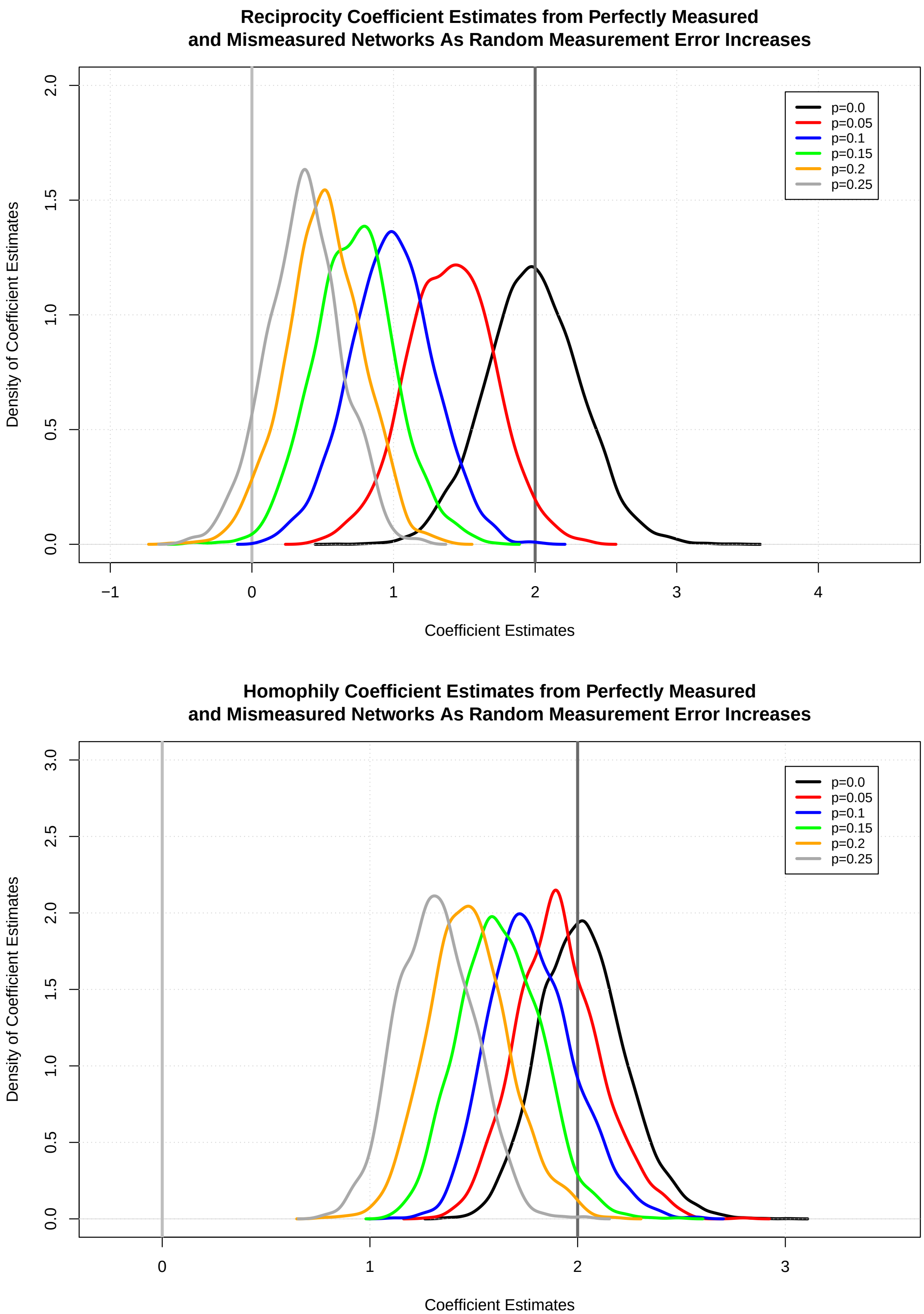


Figure 1: ERGM Coefficient Attenuation Bias in Networks of 25 Actors as Random Measurement Error in Networks Increases

Table 1: Frequency of Null Hypothesis Rejection As Sample Size Increases

Variable - Network Size	$p = 0.05$	$p = 0.10$	$p = 0.15$	$p = 0.20$	$p = 0.25$
Reciprocity - 25 Actors	1487	1366	1078	698	423
Reciprocity - 50 Actors	1500	1500	1500	1464	1226
Reciprocity - 75 Actors	1500	1500	1500	1500	1492
Reciprocity - 100 Actors	1500	1500	1500	1500	1500
Homophily - 25 Actors	1500	1500	1500	1500	1500
Homophily - 50 Actors	1500	1500	1500	1500	1500
Homophily - 75 Actors	1500	1500	1500	1500	1500
Homophily - 100 Actors	1500	1500	1500	1500	1500

Note: Cells report the number of Monte Carlo simulations which reject the null hypothesis in models of networks containing random measurement error. p represents the level of measurement error introduced into the network. For each network size-p pairing, 1500 simulations were conducted.

bias, recovers hypothesis test power, or both, we re-run our Monte Carlo simulations using exactly the same parameters as reported in the second section, but we increase the size of the networks we study from 25 actors to 50, 75, and 100 actors. We expect that as the number of actors in our networks increases, we will recover power lost in our hypothesis test due to network measurement errors.

Table 1 reports the frequency with which the ERGMs run on the simulated networks with measurement error reject the null hypothesis as random measurement error in the network increases for simulations with varying actor sizes. For each p-network size pair, 1,500 Monte Carlo simulations were conducted. Thus, the closer each cell is to 1,500, the more powerful the hypothesis test on the relevant covariate was. The results of the simulation indicate that there was considerable power lost on the hypothesis test associated with the reciprocity coefficient due to random measurement error in the network of interest, but with networks of 75 to 100 actors, nearly all of this power loss was recovered. Therefore, the hypothesis testing power lost to random measurement error seems to be of largest concern for scholars studying small networks. Again, our results also indicate that the hypothesis testing power lost by test on homophily were considerably lower. In fact, no level of random measurement error caused the ERGM to make a type II error across all actor sizes.

Thus, network scholars worried about hypothesis testing power loss as a function of random network measurement error can be assuaged. Type II errors resulting from random measurement error are only particularly common with rather small networks. However, the hypothesis testing power recovered by the ERGM as networks grow in size could be a function of two different processes. First, power may be recovered by increasing the network under study's size, which combats the attenuation bias induced by random measurement error in the network. In other words, larger networks may produce less bias in coefficient estimates. Alternatively, this recovered power could be a result of narrower standard errors reported by the ERGM as network size increases. ERGMs may still be reporting attenuated coefficients, but successfully reject the null because those downwardly biased coefficients have much smaller standard errors.

To evaluate which of these processes is driving the recovery of statistical power we see in Table 1, Figure 2 plots the distributions of both reciprocity and homophily coefficients on our networks incorporating random measurement error as network size increases. The top panels report the distribution of reciprocity coefficients in minimally and maximally mis-measured networks respectively. The bottom panels present the same distributions for the homophily coefficients. A close examination of the distributions reveals that increasing the size of the networks does *not* reduce the attenuation bias introduced by random measurement error in the network under study. However, the variance in the distribution of coefficients is reduced considerably as network size increases. This occurs for both the reciprocity coefficient and the homophily coefficients. This would seem to provide strong evidence that the recovered statistical power that results from increased network size reported in Table 1 is due to a reduction in variability rather than a reduction in bias.

Our Monte Carlo simulations reveal several things. First, as random measurement error in networks under study increases, both endogenous and exogenous coefficients are likely to suffer from attenuation bias. Second, this attenuation bias seems to be more severe for endogenous coefficients relative to exogenous coefficients. Third, the statistical power lost to this attenuation bias can be recovered by increasing the number of actors in the network under study. Finally, the recovery of statistical power that comes from increased network size is a result of decreasing variance, rather than reduced attenuation bias. Accordingly, random measurement errors in network studies us-

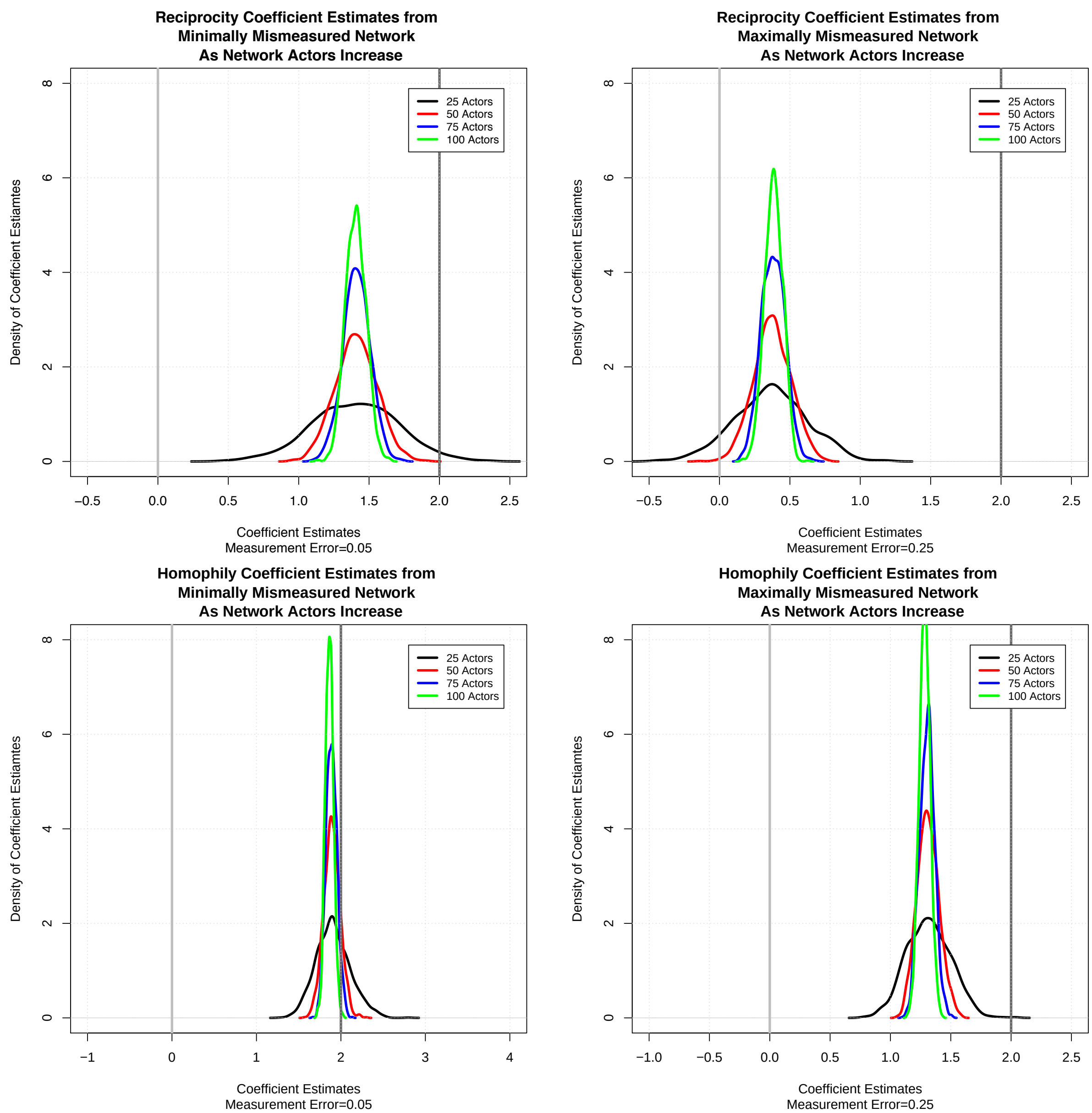


Figure 2: ERGM Coefficient Attenuation Bias in Minimally and Maximally Mis-Measured Networks As Network Size Increases

ing ERGMs can cause a loss of statistical power, and does cause large attenuation bias. Even if scholars of social networks can properly test their hypotheses in the face of random measurement error, their ability to engage in out-of-sample predictions regarding networks is hampered by the resultant attenuation of coefficients. To the degree that scholars hope to get the magnitudes of coefficients correct along with a proper evaluation of hypotheses, random measurement error in networks should be of considerable concern.

3 The Direction of Coefficient Bias in ERGMs

In the previous section, our Monte Carlo simulations revealed attenuation bias in both endogenous and exogenous ERGM coefficients when the network contains random measurement error. In this section, we explore whether measurement error-induced coefficient bias is always downward, or whether conditions exist in which measurement error might induce ERGM coefficients to be too large. As we will demonstrate, the bias in ERGM coefficients depends on the strength of a pattern in the "true", perfectly measured network, the degree of endogeneity in the relevant covariate, and the subsequent level of measurement error.

3.1 Homophily

Consider an example network with six actors in which the true number of homophilous edges is five, as illustrated in the left-hand side of the top panel of Figure 3. We begin with random measurement error that induces a 0.10 probability that a homophilous tie will appear if it did not exist in the true network or disappear if it existed in the true network. Here, we see the network will lose a homophilous tie with 0.50 probability (0.10*5). There is, however, only a 0.10 probability of gaining a homophilous edge in this network due to measurement error. In that sense, it is much more likely that we will lose a homophilous tie due to measurement error than it is that we will gain one. Thus, the count of homophilous ties in our network is likely to be depressed, and the coefficient on homophily in our model is likely to be attenuated.

Now consider a circumstance in which the number of homophilous ties in the true network is low as in the middle panel of Figure 3. The true network in this example only has one homophilous tie. Once we introduce the same level of measurement error, the mis-measured network is likely to have too many homophilous ties, as the true network has only a 0.10 probability of losing a homophilous edge due to measurement error, while there is a 0.50 probability that the network will gain a homophilous tie. Finally, consider a network with no overt tendencies towards or against homophily, as in the bottom panel of Figure 3. In this example, the true network has six actors and three homophilous ties. With the introduction of random measurement error, our mis-measured network is equally likely to gain a homophilous tie as it is to lose one. Thus, in expectation, random measurement error will not influence the total count of homophilous ties in this network, and the coefficient on homophily will be properly estimated.

These simple examples provide some clear indications to the direction of coefficient bias when random measurement error is introduced to a network of interest. When a network has an active tendency against homophily, random measurement error will induce ERGMs to overestimate the relevant coefficient, pushing a negative coefficient towards zero, while when a network has an active tendency towards homophily, random measurement error will induce ERGMs to underestimate the relevant coefficient and push a positive coefficient towards zero. When a network has neither a tendency toward homophily nor against homophily, random measurement error will produce no coefficient bias. Therefore, random measurement error seems to generate classic attenuation bias in homophily coefficients for ERGMs.

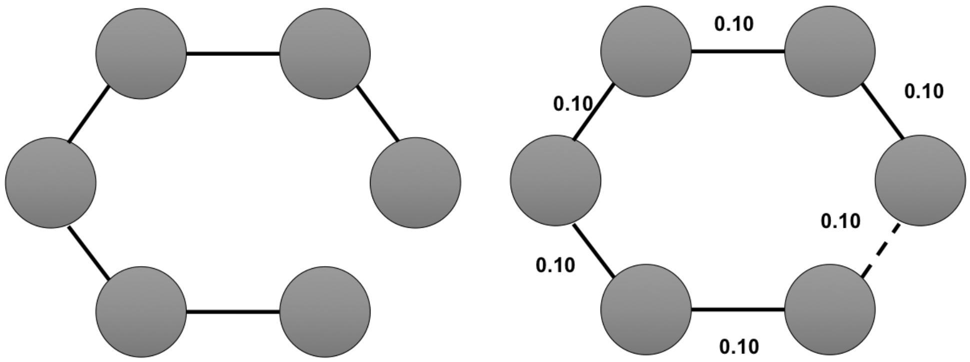
3.2 Reciprocity

Now consider an example network with five actors in which the "true" number of *reciprocal* edges is 3, as illustrated in Figure 4. When measuring this network, suppose a level of random measurement error such that there is a 0.90 probability that an edge in the true network will remain in the measured network and a 0.10 probability that a non-existing edge will be added to the measured network.

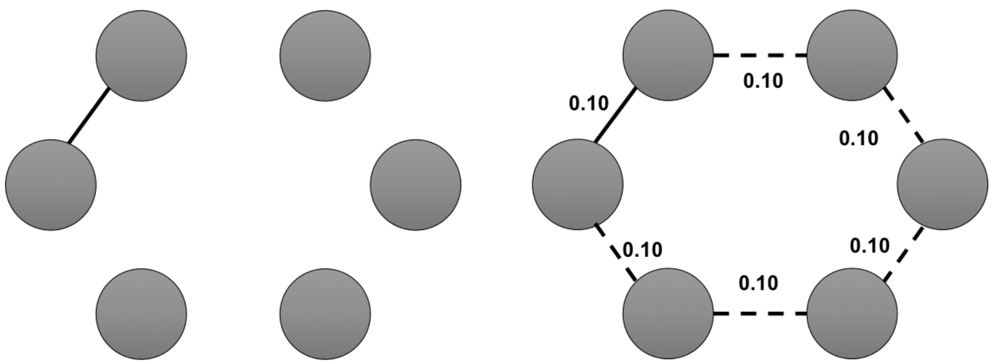
The right-hand side of the top panel of Figure 4 shows the probabilities of observing reciprocal ties in the measured network. By inducing a measurement error of 0.10, the probability of observing each existing reciprocal tie drops to 0.81 ($0.90 * 0.90$). However, we also observe a reciprocal

Figure 3: Measurement Error and Homophilous Ties for Three Different “True” Networks.

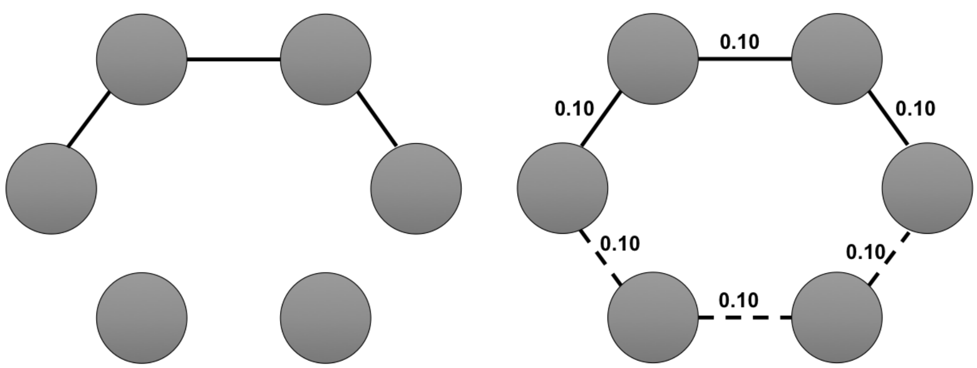
(a) 6-Person Network with Five Homophilous Edges



(b) 6-Person Network with One Homophilous Edges



(c) 6-Person Network with Three Homophilous Edges



Note: Left-hand side represents “true” network, while right hand panels demonstrate the probability of gaining or losing different ties as a 0.10-level measurement error is introduced. Potentially gained ties are dotted lines while potentially lost ties are solid lines.

tie where there previously was none with a probability of 0.01 ($0.10 * 0.10$). As is evident in this example, the expected number of lost reciprocal edges outpaces the expected number of gained reciprocal edges; indeed, there is a 0.57 probability that a reciprocal edge will be lost, but only a 0.02 probability that such an edge will be gained. This pattern is consistent with the simulated networks above, and uncovers the consequences to reciprocal edges of introducing random measurement error in an ERGM network.

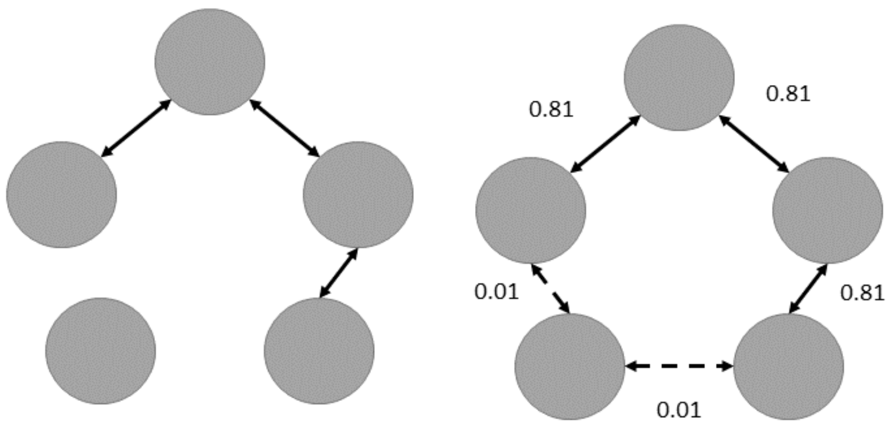
This example has several additional implications. First, as we saw in Figure 4, random measurement error also *adds* reciprocal edges to the measured network. Thus, by extension, a "true" network that has exponentially fewer reciprocal edges may, with the introduction of random measurement error, actually experience an *increase* in the number of measured reciprocal ties. However, given the low probability of this occurring, the amount of random measurement error would have to be rather large for this impact to be noticeable. Returning to our example, suppose that we began with no reciprocal ties and maintained the same level of random measurement error (0.10). The probability of observing a reciprocal edge is 0.05, an arguably negligible change.

A final scenario not covered in the examples above is the effect of asymmetric directed edges on reciprocal ties. The bottom panel of Figure 4 replicates the top panel of the figure, but replaces the reciprocal edges with asymmetric directed edges. In this scenario, the network has an active tendency to resist reciprocity. Actors form connections in this network commonly, but are hesitant to reciprocate those connections. Thus, this is a setting in which an ERGM ought to estimate a negative value on a reciprocity coefficient. As expected, the probability of observing a reciprocal edge where previously there was an asymmetric directed edge lies between the probability given a reciprocal edge and the probability given no edge. Here, the probability of adding a reciprocal tie is 0.29. Although this is slightly larger than the example in which the "true" network began with no edges, it remains relatively small. Nevertheless, because the true network in this example has no reciprocal edges, and the mis-measured network may have some reciprocal edges, what should be a strong negative reciprocity coefficient may instead be attenuated towards zero.

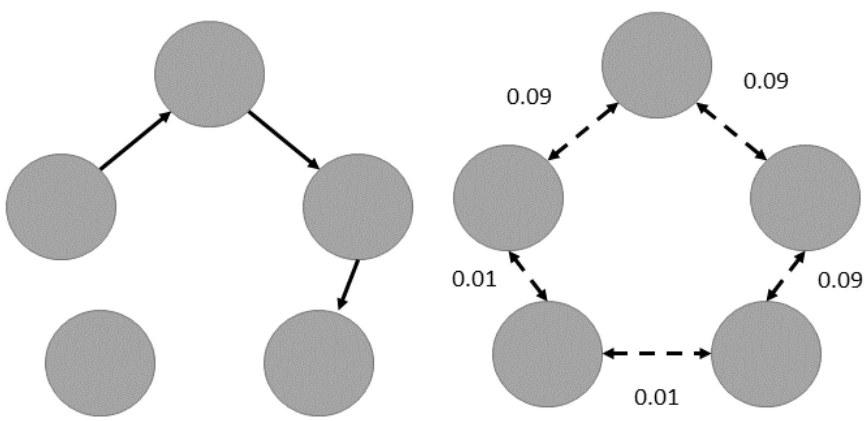
Moving beyond exogenous coefficients in ERGMs to endogenous network patterns, we again

Figure 4: Measurement Error and Reciprocal Ties for Two Different "True" Networks.

(a) 5-Person Network with Three Reciprocal Ties



(b) 5-Person Network with Three Asymmetric Ties



Note: Left-hand side represents "true" network, while right hand panels demonstrate the probability of gaining or losing different ties as a 0.10-level measurement error is introduced. Potentially gained ties are dotted lines while potentially lost ties are solid lines.

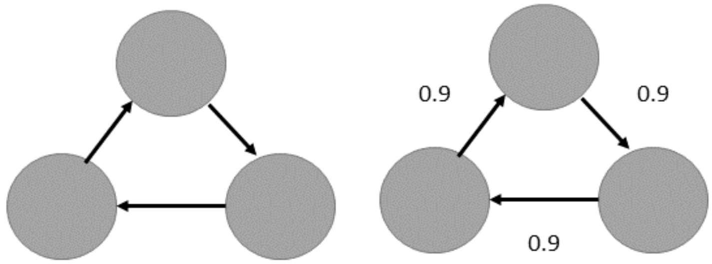
see that the introduction of random measurement error in the network of interest results in attenuation of coefficients towards zero. The attenuation of endogenous covariate coefficients works slightly differently than the attenuation bias associated with exogenous coefficients in that count statistics for endogenous network patterns are more likely to be eliminated by random measurement error than they are to be gained. Thus, positive endogenous relationships in a "true" network are severely attenuated by measurement error while negative endogenous relationships suffer from less attenuation towards zero. This is simply a function of the fact that it is much easier to eliminate one of the many conditions necessary to count an endogenous pattern as part of a network covariate by random chance than it is to gain those same conditions.

3.3 Triads and Beyond

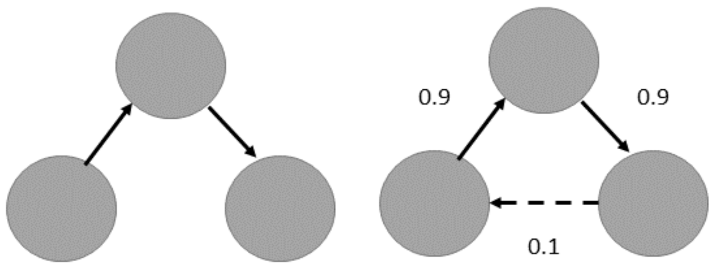
The jump in attenuation bias from homophily to reciprocity suggests that attenuation bias grows more severe as the complex dependencies in a pattern of interest grow. That is, the more ties of a specific nature are required to form a particular network pattern, the greater the likelihood that such a pattern will be lost to random measurement error. Additionally, patterns whose formation require a greater number of ties are less likely to materialize as a result of random measurement error. Take, for example, triadic ties. The left-hand side of the top panel of Figure 5 shows a group of three actors that are connected by a particular type of triadic tie; in this example, this is the "true" relationship between the three actors. The right-hand side of the top panel of Figure 5 introduces random measurement error of 0.10 and displays the probability of observing the edges from the "true" relationship. It only takes the loss of a single edge for the triadic relationship to disappear. Thus, there is a 0.30 probability (0.10 * 3) that the true triadic relationship will be missing once random measurement error is introduced. Contrasted with the examples above, the probability that random measurement error *introduces* a triadic relationship is even smaller, as show in the bottom panel of Figure 5. Here, the left panel shows three actors and two asymmetric directed edges – again, the "true" relationship. As before, the right-hand side introduces random measurement error of 0.10. To form a triadic relationship in this scenario, we need not only maintain the two existing edges, but also add an edge where there is none. Thus, there is a 0.081 (0.9 * 0.9 * 0.1) probability

Figure 5: Measurement Error and Triadic Ties for Two Different "True" Networks.

(a) Three Actors Connected by a Triadic Tie



(b) Three Actors with No Triadic Tie

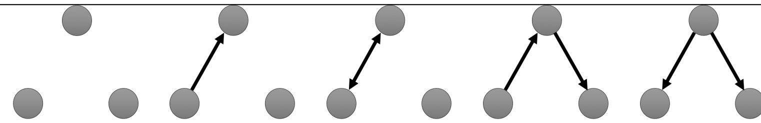
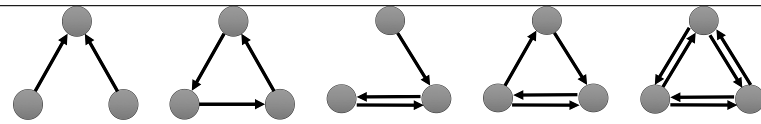


Note: Left-hand side represents "true" network, while right hand panels demonstrate the probability of gaining or losing different ties as a 0.10-level measurement error is introduced. Potentially gained ties are dotted lines while potentially lost ties are solid lines.

of creating a triadic relationship after introducing random measurement error, given the "true" relationship in the left panel. At the most extreme, we can imagine three completely unconnected actors. In that case, the probability of creating a triadic tie between those three actors, given the introduction of random measurement error of equal probability as in the examples above, is a mere 0.001 (0.1 * 0.1 * 0.1).

Table 2 illustrates how attenuation bias affects different types of ties as the complex dependence/endogeneity of ERGM count statistics increases. The numbers below each figure indicate the probability of *losing* the depicted relationship given 10% random measurement error in the network. As the figures show, when the complex dependencies necessary for a particular pattern to be present in a network increase, the probability that said pattern will be disrupted by fixed amount of random measurement error also go up. This in turn implies that as the complex dependencies

Table 2: Attenuation Bias with 10% Random Error

Type of Terms					
Attenuation Bias	0	0.1	0.19	0.2	0.2
Type of Terms					
Attenuation Bias	0.2	0.3	0.29	0.39	0.57

in a pattern under study increase, the probability of observing that pattern in a network suffering from random measurement error decrease. Thus, unlike standard regression models, social network models suffer from attenuation bias in coefficients on endogenous model terms when the dependent variable (the network of interest) suffers from random measurement error.

From these examples, it is easy to see how attenuation bias is more severe as the relationship moves from triads to quadrilaterals, pentagons, and so on. In each instance, the number of edges required to establish a particular relationship increases. Thus, the probability of losing the relationship also increases, while the probability of creating a new relationship where previously there was none exponentially decreases. In sum, as the intricacy of the network relationships increases, so too does the effect of random measurement error on attenuation bias.

4 Comparing Logistic Regression and ERGMs in the Presence of Measurement Error

As has been repeatedly demonstrated, in the absence of endogenous network patterns, ERGM coefficients are identical to coefficients from a dyadic logistic regression model of the network of interest (Cranmer and Desmarais 2011). However, even in the absence of interdependence, the reported standard errors between the two models will be quite different. This is largely due to the differences in the sample size of ERGMs and dyadic logistic regression models. In dyadic logistic models, networks of 25 actors contain 25*24 observations, or 600 observations of the dependent variable, providing ample degrees of freedom for analysis and in many cases creating dramatically over-powered hypothesis tests (Erikson, Pinto and Rader 2014). ERGMs instead treat the entire network as the unit of analysis and each dyad as a dependent function of the other dyads in the network, thus reducing the sample size of the model, and lowering hypothesis testing power. The move towards treating the entire network as the object of inferential interest is typically seen as a real benefit to an ERGM approach to network analysis (Robins, Pattison, Kalish and Lusher 2007). Over-powered hypothesis tests resulting from dyadic approaches to network phenomenon typically result in an excess of false positives (Aronow, Samii and Assenova 2015).

In the presence of measurement error, however, the fact that dyadic logistic models are over-powered may actually be an asset. As we have observed, random measurement error in network attenuates coefficients in ERGMs, and reduces hypothesis testing power. If we can capture the necessary endogenous patterns of interest from our networks in a dyadic logistic regression model, that model's excess of statistical power may help us recover some of the hypothesis testing power lost to measurement error in our networks. That is, the dyadic logistic model's primary flaw in network analysis may actually be an asset in the presence of network measurement error.

In order to compare the hypothesis testing power of ERGMs and dyadic logistic regressions in the presence of measurement error, we again simulate a network among 25 actors. The simulated network has a strong tendency towards reciprocity and assortativity, mirroring our earlier simulations which produced the results in Table 1. We then generate measurement error in the network by randomly adding edges to formerly unconnected dyads or eliminating existing edges between dyads with some fixed probability. We then estimate both an ERGM and a dyadic logistic regression model on the resultant network. In both models, we estimate the effects of assortativity and reciprocity on the existence of a tie between two actors in the network. In the logistic regression model, we can capture reciprocity by incorporating a dummy variable for dyad ij coded one if dyad ji exists and zero otherwise.[10] Because this dyadic logistic model and the ERGM model both con-

[10] We should note here that dyadic logistic regression can account for reciprocity relatively easily, but higher order endogenous model terms are less easily adapted to the dyadic framework. Thus, the move from an ERGM to a dyadic model should only be undertaken when the endogenous network patterns of interest can actually be accounted for in the dyadic model framework. If the shift to a dyadic models results in the omission of critical endogenous network

Table 3: A Comparison of the Frequency of Null Hypothesis Rejection As Sample Size Increases Between ERGMs and Dyadic Logistic Models

Variable	ERGM Reciprocity	Dyadic Logistic Reciprocity	ERGM Assortativity	Dyadic Logistic Assortativity
Measurement Error = 0.05	902	972	611	508
Measurement Error = 0.10	658	823	505	445
Measurement Error = 0.15	437	662	408	363
Measurement Error = 0.20	268	476	284	271
Measurement Error = 0.25	151	326	236	225

Note: Cells report the number of Monte Carlo simulations which reject the null hypothesis in models of networks containing random measurement error. For each level of measurement error, 1000 simulations were conducted.

tain the same dependent and independent variables (though slightly differently operationalized), they should produce identical coefficients, but the standard errors from the models will be quite different, and the dyadic model should (correctly) reject the null hypothesis more frequently. We record the coefficients and hypothesis test results from each model for the simulated network. We run 1000 simulations for a fixed level of measurement error, and then increase the measurement error to a higher level. We consider measurement error of 0.05, 0.1, 0.15, 0.2, and 0.25.[11]

The results of our power comparison between ERGMs and dyadic logistic models appears in Table 3. These results suggest that a switch to the dyadic logistic model helps us reject the null hypothesis correctly much more often than the ERGM does in the presence of network measurement error. Curiously, however, the ERGM correctly rejects the null hypothesis on the assortativity coefficient much *more* than the dyadic logistic model. Thus, while the dyadic model's typically over-powered hypothesis tests assist us in recovering power on the reciprocity coefficient, they also lose power on the assortativity coefficients. This result suggests that while moves to models with a more dyadic orientation like dyadic logistic regression, latent space models (Hoff, Raftery and Handcock 2002), or QAP-logistic models (Krackhardt 1988) may be beneficial in the presence of network measurement error, those benefits are not certain, and are worthy of further simulation-

terms from the model, the increased hypothesis testing power will be of no benefit, as the estimated model will suffer from the more severe problem of omitted variable bias.

[11]The reduced number of simulations run is due to the limitations of computing power at our disposal. The addition of the dyadic logistic model requires a surprisingly high level of computational power for our simulations.

based and analytic investigations.

5 An Application to Inferred Policy Diffusion Networks

In order to compare the results of logistic regression modeling to ERGMs, this paper applies both methodologies to Desmarais, Harden and Boehmke's (2015) networks to demonstrate how random measurement error can affect network inference. Desmarais, Harden and Boehmke (2015) investigate policy diffusion networks among American states, specifying which state introduces a policy as "a source" and which state adopts the policy from the source as "a follower". In their analysis, the authors infer policy diffusion networks from a source state to a follower state by applying a latent network inference algorithm called NetInf to 187 policies from 1960 to 2009 using Boehmke and Skinner (2012)'s data.[12]

NetInf identifies a latent and directed network based on three criteria that decide the probability that a state, i, is a source for another state, j. First, NetInf considers an edge from i to j based on the number of times i adopts a policy before j. The edge is used only if i adopts a policy before j. Second, the length of time between i's adoptions and j's adoptions is used for the likelihood of the edge. NetInf prefers short times between adoptions. Lastly, the latent network inference algorithm takes into account the precision with which an adoption by i predicts an adoption by j. Thus, NetInf includes the edge that performs the best based on these three factors (Desmarais, Harden and Boehmke 2015)[p.394-5].

Since the authors use a latent network inference algorithm, it is certain that their inferred networks can contain measurement error. As with any statistical model used for drawing inferences, we have errors in the results or predictions due to the stochastic part of the model. Based on our simulations the, any ERGM-based analysis of Desmarais, Harden and Boehmke's (2015) networks are likely to suffer from attenuation bias. Thus, these inferred networks represent a good test case to examine the practical consequences of the attenuation bias we see in our simulations.

Table4 provides side-by-side analyses of Desmarais, Harden and Boehmke's (2015) networks estimated using both logistic regression and ERGMs. We use their network of 300 edges and

[12]This algorithm is introduced by Gomez-Rodriguez, Leskovec and Krause (2010).

35-year periods, pooling together all of the years under study into a single network. Our models incorporate both an endogeneous model term in the form of a reciprocity coefficient, and two exogenous covariates.[13] The first covariate is an indicator variable for geographic contiguity between two states, and the second is the absolute value of the difference of professionalism scores for each state pairing.[14]

Looking first at the coefficients estimated by the ERGM in Table 4, our results indicate very few systematic relationships. There does not appear to be an unusual number of edges in the network, nor is there a general tendency towards reciprocity. Were we to study this network via an ERGM, we would also conclude that contiguity plays little role in policy diffusion across states. This would be particularly concerning, given the lengthy history of policy diffusion research stressing regional and contiguous influences on policy adoption (Mooney 2001; Berry and Baybeck 2005; Baybeck, Berry and Siegel 2011; Grossback, Nicholson-Crotty and Peterson 2004; Boehmke and Skinner 2012). Finally, the ERGM would also suggest to us the levels of state legislative professionalism play little role in the diffusion of policies across states. Thus, our ERGM-based analysis of the network would suggest to us that none of these factors play a role in the spread of policies across states. However, when we examine the policy diffusion network using dyadic logistic regression, we see markedly different results. Despite having much smaller coefficients, our dyadic logistic model suggests that both reciprocity and contiguity play a pivotal role in the spread of policies. The attenuation bias that is present with certainty in both of these models is offset to some extent by the shrinkage in the standard errors in the dyadic logistic model. Our prior simulation-based work would suggest that the random error in the inferred policy diffusion network may lead us to be more confident in these dyadic logistic results than we are in the results of the ERGM. Thus, this application demonstrates quite clearly that the choice of network modeling approach in the presence of random measurement error can have serious consequences for the inferences an

[13]This is not intended to be a complete account of the policy diffusion process across states. We use this model setup purely for illustrative purposes.

[14]We took the average professionalism score across the entire time period under study. Professionalism scores for all states but Nebraska were taken from Desmarais, Harden and Boehmke (2015). The professionalism score for Nebraska – which was absent from the aforementioned data set – was taken from Kirkland (2014).

analyst draws about the processes driving relational formation.[15]

Table 4: Logistic Regression Model ERGM Estimates using Network of 300 Edges

	Dependent variable:	
	Desmarais et al. Network	
	Logistic Regression	ERGM
Edges	0.579^{***}	-0.050
	(0.019)	(1.399)
Mutual	0.059^{***}	0.112
	(0.020)	(0.125)
Contiguity	0.060^{*}	0.208
	(0.034)	(0.154)
Professionalism	0.105	0.572
	(0.079)	(1.398)
Observations	2,500	
Log Likelihood	$-1{,}713.608$	
Akaike Inf. Crit.	3,435.215	3,183.081
Bayesian Inf. Crit.		3,206.297
Note:	*p<0.1; **p<0.05; ***p<0.01	

6 Discussion

Over the last decade, the statistical estimation of patterns of relationships in social networks has exploded. Thanks to increasing data availability, computational power, and statistical theory, social network scholars are now well-positioned to study their network of interests with all of the traditional tools of hypothesis testing. The advent of the ERGM has opened up new areas of human behavior to observational modeling approaches, but as with all statistical methods, the ERGM makes some strong assumptions about the data generating process. In this research, we demonstrate that practical researchers confronting violations of at least one of those assumptions

[15] We should note hear that Desmarais, Harden and Boehmke (2015) using a dyadic logistic regression with some standard error modifications in their analysis of the diffusion network. They find some weak evidence for both contiguity and distance-based accounts of diffusion.

need to be wary of their own hypothesis tests. In particular, we demonstrate that the introduction of purely random measurement error into a network of interest can induce large attenuation bias in ERGM coefficients, and can rob hypothesis tests on those coefficients of their power. This attenuation bias grows increasingly severe as the endogeneity of the network covariates being examined increases.[16]

The manner by which a network is constructed varies greatly. This variation presents different challenges within the context of the bias discussed above. In some studies, data collection is systematic and, arguably, more reliable. For example, a study that looks at bill cosponsorship relies on official congressional records to construct the network. Assuming these records are meticulously kept, and the information is freely available, these types of networks may be less likely to suffer from the problems described in this paper. Studies that assess political donor networks (e.g. Masket and Shor (2015); Dowdle and Yang (2014); Masket (2015)) are also less likely to suffer from problems of random measurement error. While it is true that they rely on reports of donations from individual donors, campaign finance laws likely encourage meticulous record-keeping, mitigating the risk for such errors to occur.

Other networks, however, rely on less standardized methods of data collection, and are not accompanied by potential legal ramifications in the event of erroneous reporting (intentional or otherwise). This is especially true for research that relies on surveys to construct the network. For example, in a study assessing how political discussion networks are formed, Song (2015) utilizes surveys of students in a US university to determine the frequency of discussions, political and otherwise, of actors in the network. These types of self-reporting lend themselves to random measurement error, given that they rely on respondents' abilities to recall past events, often under time constraints (i.e. time is finite, even if the surveys do not have time limits for completion). However, as illustrated by the Monte Carlo simulations contained herein, increasing the number

[16] Systematic measurement error leads to significant changes in the structure of a network, such as reducing network centralization, increasing heterogeneity, falling density, and decreasing transitivity (Fowler, Heaney, Nickerson, Padgett and Sinclair 2011). For example, if junior congressmen consistently overreport their ties to senior congressmen in a survey, this results in a type of bias known as "expansiveness bias" for junior congressmen as well as "attractiveness bias" for senior congressmen. (Fowler et al. 2011)

of actors may help mitigate problems associated with random measurement error, at least in terms of testing hypotheses. This may explain why the above-cited studies, though perhaps likely to suffer from random measurement error, ultimately obtain statistically significant coefficient estimates. However, the downward bias of these coefficients may still be present, such that the "true" relationship is stronger than estimated.[17]

Our research also suggests that network scholars may wish to take the time to carefully consider how ERGM estimates are influenced by common violations of the ERGM's assumptions. ERGMs have a strong connection to traditional logistic regression models, and so it is conceivable that ERGMs behave much as logit models do in the face of violations of regression assumptions; however, given the underlying complexity and endogeneity of ERGM covariates, it is also possible that ERGMs take on entirely different properties when confronted with the practical limitations of real data.

[17]For other examples of networks constructed from self-reported ties, see Goodreau, Kitts and Morris (2009); Fowler et al. (2011); Lazer, Rubineau, Chetkovich, Katz and Neblo (2010), and Leider, Castrucci, Harris and Hearne (2015).

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7 Supplemental Online Appendix

7.1 Measurement Error in Standard Regression Analyses

We now briefly review the effects of measurement error in traditional regression contexts.[18] Begin with a simple regression model

$$y = \beta_0 + \beta_1 x_1^* + u \tag{4}$$

where x_1^* is an unobserved explanatory variable captured by x_1 and u is the model's error term, here assumed to have a mean of zero and a variance of σ_u^2. The measurement error in the explanatory variable is thus

$$e_1 = x_1 - x_1^* \tag{5}$$

where E(e_1) = 0. If e_1 is uncorrelated with x_1, then the sole consequence is that the variance of β_1 will be larger when x_1 is used in place of x_1^*. Assuming, however, that x_1 and e_1 are correlated (and that x_1 and u are uncorrelated, referred to as the classical errors-in-variables [CEV] assumption), the result is attenuation bias, in which the OLS estimator will approach zero. As a consequence, we are more likely to observe Type II errors. Given the assumption that x_1 and e_1 are correlated, the covariance between x_1 and e_1 equals the variance of the measurement error:

$$Cov(x_1, e_1) = E(x_1 e_1) = E(x_1^* e_1) + E(e_1^2) = \sigma_{e_1}^2 \tag{6}$$

Since x_1 and u are uncorrelated, the covariance between x_1 and $u - \beta_1 e_1$ is

$$Cov(x_1, u - \beta_1 e_1) = \beta_1 Cov(x_1, e_1) = \beta_1 \sigma_{e_1}^2 \tag{7}$$

Thus, we observe a biased and inconsistent estimator in the OLS regression of y on x_1. Continuing,

[18]The overview on measurement error in independent variables for simple OLS models that follows is largely based on Wooldridge (2009), as well as Guolo (2007) and Carroll, Roeder and Wasserman (1999). More in-depth discussions can be found in these sources.

we can estimate how our estimates will be biased by calculating the probability limit of $\hat{\beta}_1$:

$$Plim(\hat{\beta}_1) = \beta_1 + \frac{Cov(x_i, u - \beta_1 e_1)}{Var(x_i)} = \beta_1 - \frac{\beta_1 \sigma_{e_1}^2}{\sigma_{x_1}^{*2} + \sigma_{e_1}^2} = \beta_1 (1 - \frac{\sigma_{e_1}^2}{\sigma_{x_1}^{*2} + \sigma_{e_1}^2}) = \beta_1 (\frac{\sigma_{x_1}^{*2}}{\sigma_{x_1}^{*2} + \sigma_{e_1}^2}) \quad (8)$$

Our estimate of $\hat{\beta}_1$ is therefore always smaller than β_1, as we are always multiplying it by a ratio smaller than one. Note, however, that if the variance of x_1^* is large relative to the variance of e_1, the above ratio will approach unity and the resulting bias will be relatively minimal.

When multiple explanatory variables are present, attenuation bias on the OLS estimators is still present for variables with measurement error, but the size and direction of the effect of measurement error on variables without measurement error are less clear. For correctly measured independent variables that are orthogonal to the independent variables with measurement error, Monte Carlo simulations suggest that their estimates will be unbiased, but will lose some efficiency as their standard errors grow. For those variables that are not orthogonal to the variables with measurement error, Monte Carlo simulations suggest that even those variables without measurement error will have biased estimators.

Measurement error in the dependent variable is less worrisome than in the independent variables. Beginning with Equation 4 above, assume that y is an imperfect measure of the unobserved y^*:

$$y = y^* + e_0 \quad (9)$$

Thus, the error term of the equation is $u + e_0$. A standard assumption in regression analysis is that e_0 is independent of the explanatory variables x_i; thus, a model with measurement error in the dependent variable retains its unbiased estimators. However, since

$$Var(u + e_0) = \sigma_u^2 + \sigma_0^2 > \sigma_u^2 \quad (10)$$

it follows that measurement error in the dependent variable leads to larger variances in the model's estimators. The major concern with measurement error in the dependent variable is, thus, inefficiency in our estimates.

The consequences of measurement error extend to non-linear models as well.[19] Begin with a logistic model

$$Pr(Y=1|\mathbf{X}^*) = \left(\frac{1}{1+e^{-\beta X_i}}\right)^{yi}\left(1-\frac{1}{1+e^{-\beta X_i}}\right)^{1-yi} \tag{11}$$

where $\mathbf{X}^*$ are unobserved covariates captured by $\mathbf{X}$. The measurement error in the explanatory variable is thus

$$e_1 = \mathbf{X_1} - \mathbf{X_1^*} \tag{12}$$

Similarly to the OLS model described above, the expectation is that the coefficient estimates will be attenuated. However, this is not always the case, and estimating the magnitude and direction of the bias is not easy, as in the multivariate OLS case.

7.2 Monte Carlo Simulation Code

Below we provide the Monte Carlo simulation code necessary to replicate out results from Figure 1. This code was executed on a cluster computer, and is time-intensive, but executing this code should reproduce that figure exactly.

```
#Clear Memory
rm(list=ls())


#load libraries
#library(arm)
library(statnet)
```

[19]The brief overview on measurement error in independent variables for logistic regression models that follows is largely based on Stefanski (2000), where interested readers can find a more in-depth discussion.

```
#set seed
set.seed(1234)

#set parameters
initial.density<-0.2
actors<-25
error<-c(0.05, 0.1, 0.15, 0.2, 0.25)
sims<-2500
update.density<-0.1
update.mutual<-2
update.assortative<-2
#update.transitive<-1.25

#Create Place to store things
TrueCoefsE25<-matrix(NA, sims, length(error))
ErrorCoefsE25<-matrix(NA, sims, length(error))
TrueCoefsM25<-matrix(NA, sims, length(error))
ErrorCoefsM25<-matrix(NA, sims, length(error))
#TrueCoefsT<-matrix(NA, sims, length(error))
#ErrorCoefsT<-matrix(NA, sims, length(error))
TrueCoefsA25<-matrix(NA, sims, length(error))
ErrorCoefsA25<-matrix(NA, sims, length(error))
RejectNullE25<-matrix(NA, sims, length(error))
RejectNullM25<-matrix(NA, sims, length(error))
RejectNullA25<-matrix(NA, sims, length(error))
```

```
#Loop over Changing Error level
for(k in 1:length(error)){

  #Repeat Simulation for same error level many times
  for(l in 1:sims){

    #Actor Categories
    aa<-rbinom(actors, 1, 0.5)

    #Intialize Network
    Net <- network.initialize(actors, directed=T)
    Net <- san(Net ~ edges, target.stats=(actors^2-actors)*initial.density)
    set.vertex.attribute(Net, "category", aa)

    # the edge coefficient is logit of density, if there are no other terms
    edge.coef <- log(update.density/(1-update.density))
    coefs.new<-c(edge.coef, update.mutual, update.assortative)

    #simulate new network with known coefficients; this SHOULD create a netw
    sim.new<-simulate(Net~edges+mutual+nodematch("category"), coef=coefs.new
    set.vertex.attribute(sim.new, "category", aa)

    #estimate ergm on new network with known coefficients
    summary(mod.new<-ergm(sim.new~edges+mutual+nodematch("category"), contro

    #Store Coefficients
    TrueCoefsE25[l,k]<-coef(mod.new)[1]
```

```
TrueCoefsM25[l,k]<-coef(mod.new)[2]
TrueCoefsA25[l,k]<-coef(mod.new)[3]

#Extract adjacency Matrix of controlled network
matrix.new<-as.matrix(sim.new)

#Insert Measurement Error
matrix.error<-matrix.new
for(i in 1:nrow(matrix.new)){
  for(j in 1:ncol(matrix.new)){
    if(matrix.error[i,j]==1){matrix.error[i,j]<-rbinom(1,1,prob=(1-erro
    if(matrix.error[i,j]==0){matrix.error[i,j]<-rbinom(1,1,prob=error[k
  }
}
diag(matrix.error)<-0

#Estimate ERGM on measurement error Network
Net.Error<-network(matrix.error, directed=TRUE)
set.vertex.attribute(Net.Error, "category", aa)
summary(mod.error<-ergm(Net.Error~edges+mutual+nodematch("category"), co

#Store Coefficients
ErrorCoefsE25[l,k]<-coef(mod.error)[1]
ErrorCoefsM25[l,k]<-coef(mod.error)[2]
ErrorCoefsA25[l,k]<-coef(mod.error)[3]

#Calculate Null Rejections for Power Analysis; 1=null rejected
```

```
    RejectNullE25[l,k]<-ifelse(coef(mod.error)[1]-2*sqrt(mod.error$covar[1,1
    RejectNullM25[l,k]<-ifelse(coef(mod.error)[2]-2*sqrt(mod.error$covar[2,2
    RejectNullA25[l,k]<-ifelse(coef(mod.error)[3]-2*sqrt(mod.error$covar[3,3
  }
  print(k) #for tracking
}

save.image("MutualErrorsMC25C.RData")



##############Plots 25 person results############
plot(density(TrueCoefsM25), xlim=c(-1,4.5), ylim=c(0, 2), ylab="Density of C
grid()
abline(v=2, col="dimgrey", lwd=3)
abline(v=0, lwd=3, col="grey")
lines(density(ErrorCoefsM25[,1]), lwd=3, col="red")
lines(density(ErrorCoefsM25[,2]), lwd=3, col="blue")
lines(density(ErrorCoefsM25[,3]), lwd=3, col="green")
lines(density(ErrorCoefsM25[,4]), lwd=3, col="orange")
lines(density(ErrorCoefsM25[,5]), lwd=3, col="darkgrey")
legend("topright", inset=0.05, c("p=0.0", "p=0.05", "p=0.1", "p=0.15", "p=0

plot(density(TrueCoefsA25), xlim=c(-0.25,3.5), ylim=c(0, 3), ylab="Density o
grid()
abline(v=2, col="dimgrey", lwd=3)
abline(v=0, lwd=3, col="grey")
lines(density(ErrorCoefsA25[,1]), lwd=3, col="red")
```

```
lines(density(ErrorCoefsA25[,2]), lwd=3, col="blue")
lines(density(ErrorCoefsA25[,3]), lwd=3, col="green")
lines(density(ErrorCoefsA25[,4]), lwd=3, col="orange")
lines(density(ErrorCoefsA25[,5]), lwd=3, col="darkgrey")
legend("topright", inset=0.05, c("p=0.0", "p=0.05", "p=0.1", "p=0.15", "p=0
```