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The Council of the American Sociological Association has announced the selection of Tony N. Brown, Katharine M. Donato, Larry W. Isaac, and Holly J. McCammon as Editors-Elect of the *American Sociological Review*. On January 1, 2010, the term of the current editors ends and Professors Brown, Donato, Isaac, and McCammon become *ASR* editors.

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The 2004 GSS Finding of Shrunk Social Networks: An Artifact?

Claude S. Fischer

University of California, Berkeley

McPherson, Smith-Lovin, and Brashears (2006, 2008b) reported that Americans' social networks shrank precipitously from 1985 to 2004. When asked to list the people with whom they discussed "important matters," respondents to the 2004 General Social Survey (GSS) provided about one-third fewer names than did respondents in the 1985 survey. Critically, the percentage of respondents who provided no names at all increased from about 10 percent in 1985 to about 25 percent in 2004. The 2004 results contradict other relevant data, however, and they contain serious anomalies; this suggests that the apparently dramatic increase in social isolation is an artifact. One possible source of the artifact is the section of the 2004 interview preceding the network question; it may have been unusually taxing. Another possible source is a random technical error. With as yet no clear account for these inconsistencies and anomalies, scholars should be cautious in using the 2004 network data. Scholars and general readers alike should draw no inference from the 2004 GSS as to whether Americans' social networks changed substantially between 1985 and 2004; they probably did not.

In the June 2006 issue of *ASR*, McPherson, Smith-Lovin, and Brashears (hereafter, MS-LB) reported that in the 2004 General Social Survey (GSS), respondents provided substantially fewer names when asked to list the people with whom they discussed important matters than had GSS respondents in 1985 (McPherson et al. 2006). In particular, the proportion of respondents who gave no names at all more

than doubled from about 1 in 10 to about 1 in 4. The report drew widespread coverage in the general media—for example, in the *New York Times* story, "The Lonely American Just Got a Bit Lonelier" (Fountain 2006) and in a well-publicized book, *The Lonely American: Drifting Apart in the Twenty-First Century* (Olds and Schwartz 2009; see also McPherson, Smith-Lovin, and Brashears 2008a). MS-LB's recent erratum, which I discuss below, modestly corrected the estimates of "isolated" Americans to 8 percent in 1985 and 23 percent in 2004 (McPherson, Smith-Lovin, and Brashears 2008b), but the claim of substantial shrinkage remains unchanged. In this comment, I show that the question used in the 2004 survey to measure the size of respondents' networks yielded results that were so inconsistent with other data, and so internally anomalous and implausible, that they are almost surely the product of an artifact. These data do not provide a reliable

Direct correspondence to Claude S. Fischer (fischer1@berkeley.edu). Thanks to Tom Smith for sharing the GSS materials and responding to the concerns described here. I have incorporated the corrections he provided for 41 miscoded cases. Michael Hout supplied key pointers and Rob Mare gave a helpful reading. Exchanges with Lynn Smith-Lovin and Miller McPherson improved my understanding of the issues, and comments by anonymous *ASR* readers were helpful. No one else, of course, is responsible for the interpretations in this comment.

estimate of what happened to Americans' networks between the 1980s and 2004.

Readers deserve to know the history of this controversy. In August 2008, I presented both MS-LB and Tom Smith of the National Opinion Research Center (NORC) with an earlier version of this comment. Much electronic conversation ensued. Smith and his staff scoured their records for evidence of an error. In September 2008, Smith (2008) announced that NORC had discovered 41 cases that had been erroneously coded as giving no names; they should have been coded as missing data. MS-LB's recent erratum (McPherson et al. 2008b) produced tables and figures corrected for this error. But the erratum makes no reference to the wider concerns I had raised in August 2008. The 41 corrected cases help but hardly suffice to account for the anomalies in the 2004 results. The problem is much greater and calls into question the entire conclusion that Americans' networks have shrunk.

THE INITIAL RESULTS

The central network question asked in 1985 and 2004—and in slightly different form in the 1987 GSS—is the following (taken from the GSS codebook):

127. From time to time, most people discuss important matters with other people. Looking back over the last six months—who are the people with whom you discussed matters important to you? Just tell me their first names or initials. IF LESS THAN 5 NAMES MENTIONED, PROBE, Anyone else? ONLY RECORD FIRST 5 NAMES.

NAME1 _____
NAME2 _____
NAME3 _____
NAME4 _____
NAME5 _____

The question was followed by this coding scheme, turned into the GSS variable labeled “Numgiven”:

128. INTERVIEWER CHECK: HOW MANY NAMES WERE MENTIONED?

[answer]	[code]
0	0
1	1
2	2
3	3
4	4
5	5
6+	6

The interviewer then proceeded to ask detailed questions about each of the persons named in Q. 127. MS-LB analyze these responses in detail, but my concern is with Q. 128, “Numgiven,” and especially the number of respondents who were coded zero. (The 1987 GSS used a variant of Q. 127; it differed from the 1985 and 2004 surveys by having interviewers probe for “anyone else” if the initial offerings were fewer than three, rather than five, names.)

Table 1 shows the basic result, corrected for the 41 miscoded cases. In their erratum, MS-LB properly weight the data to describe the general population. They show the 1985 “isolated” as 8.1 percent and the 2004 isolated as 22.6 percent. This is a slightly smaller change, a 2.8-fold increase, than the one displayed in Table 1, a 2.9-fold increase. I do not weight the cases because my interest is in looking at specific, real interviews rather than “constructed” cases. This choice does not affect the conclusions.¹

INITIAL REASONS FOR SKEPTICISM

There are substantive reasons why many sociologists found this result hard to accept, notably (1) the scale of social change suggested by the nearly three-fold increase in social isolation is stunning and hard to explain sociologically and (2) most other indicators of social involvement did not change at all, or nearly as much, in the same period. In *Bowling Alone*, Putnam (2001) presents some evidence—albeit debated by critics—that Americans’ social engagement declined from around 1970 through 2000. The scale of change he reports is magnitudes less than the contrast in Table 1 or in McPherson and colleagues (2006).²

THE SCALE OF THE CHANGE

What sociologists know about social change and social networks make the MS-LB results—

¹ McPherson and colleagues (2006: note 4): “The weighting issues, while complex, do not influence the substantive conclusions of our analysis.”

² Moreover, many of Putnam’s (2001) noted negative correlations between year and social involvement appear only after respondents’ education is controlled for; the MS-LB results are raw differences (on critiques, see, e.g., Fischer 2005).

Table 1. Percentage of Respondents Giving No Names to Numgiven Question, by Year

Question: "Looking back over the last six months—who are the people with whom you discussed matters important to you?"			
	1985	1987	2004 ^a
Percent Giving No Names	8.9	5.4	25.0

^a Corrected by dropping 41 miscoded cases per Smith (2008). Unweighted.

a near-tripling of isolation—suspect. No social factors that might even plausibly cause such isolation (e.g., rising divorce, economic dislocation, demographic changes, residential moves, television-watching, or women's participation in the labor force) changed to any comparable degree in the same period. One noteworthy development was the introduction of the Internet; it may have been of an appropriately massive scale. Research shows, however, that the Internet has had few—and perhaps positive—effects on social ties (e.g., Bargh and McKenna 2004; DiMaggio et al. 2001; Wellman 2004).

MS-LB are themselves skeptical of the magnitude of the 1985 to 2004 difference; they provide cautions in both the original article and the erratum and they test for possible artifacts (discussed below). Accepting the change as real, MS-LB search for some historical explanation. In the end, they find no variable in the GSS data that can make the statistical effect of year go away—that can, in other words, explain the 1985 to 2004 difference. MS-LB can only speculate about what factors not measured in the GSS affected American society so much that Americans' networks crashed. We are left with no plausible sociological theory for such a drastic social change. Nonetheless, MS-LB told a wide audience—in sociology and beyond—that "the number [*sic*; percentage] of people saying there is no one with whom they discuss important matters nearly tripled. . . . Americans are connected far less tightly now than they were 19 years ago" (McPherson et al. 2006:373).

CONTRADICTIONS IN OTHER DATA

Four measures of social involvement in the GSS itself cast serious doubt on the MS-LB conclusion.³ I focus on the simple dichotomy, whether the respondent was "isolated" (i.e., provided

no names at all) versus not. This is the key difference between the 1985/1987 and 2004 results. The alternative measures discussed here differ from Numgiven and may not be quite as precise measures of network isolation. Still, the proportion of respondents who appear friendless, and the trends in these proportions, can provide a cross-check on the 2004 Numgiven measure.

(1) Social Evenings: The GSS has long asked interviewees how often they "spend a social evening" with relatives, neighbors, or friends outside the neighborhood. The percentages who said never (or once a year) hardly changed between 1985 through 1987 and 2002 through 2006.⁴

(2) Close Friends: In 1986, the GSS asked respondents, "Thinking now of close friends not your husband or wife or partner or family members but people you feel fairly close to . . . [1208.] How many close friends would you say you have?" Five percent said none, 6 percent said one, and 12 percent said two. Two points are worth noting: (1) Even though the question excluded relatives, only 5 percent were "isolated." (2) That 5 percent in 1986 roughly matches the percentage found as isolated using the Numgiven item in both 1985 and 1987. Twelve years later, in 1998, the GSS asked a similar question: "[386a.] Do you have any good friends that you feel close to?" Nine percent said no—identical to the 1985 Numgiven estimate of isolates, but about *one-third* of the 2004 isolates estimated by McPherson and colleagues, which

³ Michael Hout alerted me to some of the GSS items.

⁴ The percentages who said "never" in 1985 to 1987 and 2002 to 2006 were, respectively, relatives: 4 versus 3 percent; neighbors: 25 versus 29 percent; and friends: 10 versus 9 percent. Adding those who answered "once a year" to those who answered "never" yields the same pattern. (Cases weighted by "wtsall.") It would be informative to compare how these items correlated with Numgiven in 2004 versus 1985, but these questions were not asked in 2004 of the same subsample of respondents who were asked the network questions.

was 25 percent. As with the social evening questions, there is no evidence here of network shrinkage.

(3) ISSP Support Question: The GSS administers items from the International Social Survey Programme. In 1986 and 2002, the ISSP asked a question (numbered 1213 and 1239 in the GSS codebook⁵) that comes close to the gist of the Numgiven question: "Now suppose you felt just a bit down or depressed, and you wanted to talk about it. a. Who would you turn to first for help?" In 1986, 2 percent said no one, compared with 4 percent in 2002 (Chi-square $p < .05$).⁶ The upward trend is consistent with MS-LB, but the difference is much smaller.

(4) Keeping in Contact: In 2000, 2002, and 2004, the GSS asked: "[797.] Not counting people at work or family at home, about how many other friends or relatives do you keep in contact with at least once a year?" (Robinson and Martin [2007] report on this item.) Pooling those surveys, only 2 percent of respondents had no annual contacts in the 2000 to 2004 period; the median for those who gave any names is 15.

In 2002 and 2004, a follow-up question raised the standard for a close tie: "[798.] Of these [insert number] friends and relatives, about how many do you stay in contact with by: a. Seeing them socially, face-to-face?" In 2002 and 2004 combined, 6 percent either said they were in contact with no one or that they saw none of their contacts "socially, face-to-face." Recall that this question excludes coworkers and kin at home, including spouses. It is hard to reconcile this 6 percent in 2002 to 2004 with the 25 percent who named no one to the 2004 Numgiven question.

The questioning most comparable to the 2004 Numgiven item appeared in 2002. The introduction was the one above, "Not counting people at work or family at home, about how many other . . . do you keep in contact with." It was

followed in 2002 by this question: "Of these [insert number] friends and relatives, about how many would you say you feel really close to, that is close enough to discuss personal or important problems with?" One percent said no one in answer to the first part, Q. 797, about being in contact, and another 4 percent reported at least one contact but then said they felt "close enough" to discuss problems with no one. This two-part sequence yields only 5 percent isolated, without a confidant, in 2002. And recall that these respondents were told not to include coworkers and kin at home.

To be sure, the 2002 question about contacts respondents felt "close enough to discuss personal or important problems with" and the 2004 Numgiven question are different in various ways. For one, the latter specifies actually having discussed something of importance in the previous six months. Yet the 2002 questions throw out a major chunk of Americans' social networks—their immediate family and coworkers. The contradiction between the 2002 item on discussing problems yielding only 5 percent isolated and the 2004 Numgiven question showing 25 percent isolated is hard to explain except by artifact.

The reader may have noticed that both the Numgiven question and the "how many other . . . do you keep in contact with" question appeared in the 2004 survey. In 2004, 938 respondents answered both questions. (Unfortunately, the 2002 follow-up question—how many of these would respondents discuss important problems with—was not repeated in 2004.) Table 2 presents the cross-classification. For each level of Numgiven, it displays how many "other contacts" respondents reported to Q. 797. The table shows, for example, that of respondents who were coded as zero for Numgiven, 8.4 percent answered zero to the question about contacts. There is an association between the two questions; those who listed more names also estimated more contacts. But the association is remarkably weak. Most important for present purposes, 80 percent of the respondents who presumably gave zero names in answer to the Numgiven network question estimated that they had at least three people—aside from coworkers or family at home—with whom they kept in contact. In the full table (not shown here), the median for the 225 respondents in the zero column for Numgiven is 10. Borrowing informa-

⁵ The items differed slightly in the coding of the answers, not in the question.

⁶ Two similar questions appeared in both 1986 and 2002 and show the same slight trend. One asked who would help around the house should the respondent be ill. In 1986, 1 percent said no one, compared with 2.5 percent in 2002 ($p < .001$). In 1986, 4 percent said there was no one from whom they could borrow a large sum of money, compared with 12 percent in 2002 ($p < .001$).

Table 2. Percentage of Respondents by Number of “Contacts” They Estimated, by Number of Names They Gave to the Numgiven Question, 2004

Number Respondent Estimated in Answer to “Contacts” Question	Number of Names Respondent Gave in Answer to Network Question (Numgiven)			
	No Names	1 Name	2 Names	3+ Names
No Contacts	8.4	1.6	2.7	1.5
1 Contact	5.8	5.9	1.1	.9
2 Contacts	5.8	4.8	3.8	3.5
3+ Contacts	80.0	87.7	92.4	94.2
N	225	187	184	342

Notes: The contacts question is “Not counting people at work or family at home, about how many other friends or relatives do you keep in contact with at least once a year?” The network question is quoted in Table 1. Data corrected by dropping 41 miscoded cases per Smith (2008).

tion from the 2002 survey, we can estimate that, had the respondents in 2004 also been asked the 2002 follow-up about discussing personal problems, only 3 percent of them would have reported that they had no one to talk to⁷—compared with the roughly 25 percent figure from Numgiven.

Finally, McPherson re-interviewed 839 of the 2004 GSS’s original 1,467 respondents in 2006. These respondents agreed to be re-interviewed by telephone and were re-asked the Numgiven question. The results are that these

2006 respondents “in comparison with the 1985 respondents [show] modest significant changes, or negligible changes 1985–2004, depending on the model.”⁸ That is, the answers to Numgiven of 2004 respondents re-interviewed in 2006 were more like the 1985 respondents’ answers than to their own answers in 2004. McPherson points out reasons, in addition to the high attrition, that may explain why the re-interviewees gave substantially more names in answer to the Numgiven question in 2006.⁹ Nonetheless, these data further contradict the conclusions MS-LB drew from the 2004 data.

Other independent estimates of isolation for around 2004 are also much lower than that of MS-LB. For example, the 2006 Saguro Seminar Community Survey reports that 3 percent of their respondents said they had nobody to confide in (Q. 54; Saguro Seminar 2006). Cornwell, Laumann, and Schumm (2008) conducted a survey of 57- to 85-year-olds with a network question similar to Numgiven, but asking about the previous 12 months and leading off the survey. They found a mean size of 3.6 names,

⁷ Using the 2002 GSS data, I ask: What percentage of respondents who estimated keeping in contact with only one person then said they had no one to discuss problems with when asked “Of these, . . . about how many would you say you feel . . . close enough to discuss personal or important problems with”? What proportion of those who estimated two contacts then said they had no one to discuss problems with? What proportion of those who named three or more people? I apply the 2002 ratios to the 2004 distribution for the initial “how many . . . keep in contact” question. We can then infer that in 2004, about 22 percent of those who estimated one contact, 12 percent of those who estimated two contacts, and 3 percent of respondents who estimated three or more contacts would have gone on to say that they felt close enough to discuss personal problems with no one. Combined with Table 2, roughly 12 percent of those who were coded zero on Numgiven would have been coded zero on whom (outside of coworkers and coresident family) they felt close enough to discuss personal matters. All together, 3 percent of all the 2004 respondents would have been without such a confidant according to these questions, compared with 25 percent estimated from Numgiven.

⁸ Miller McPherson, personal communication, September 21, 2008; also sent to the GSS Board of Overseers on October 24, 2008.

⁹ McPherson points to the difference between using telephones in 2006 versus face-to-face interviews in 2004 (although see note 14 for a discussion of a possible telephone effect), the publicity about the MS-LB report at the time of the 2006 interviews, demographic differences between the 2006 and 2004 pools, fatigue effects in 2004 as a result of the preceding battery of questions (much discussed later in this comment), and attrition.

Table 3. Percentage of Respondents Who Gave No Names to the Numgiven Question, by the Number of Types of Organizations They Belonged to, by Year

Number of Types of Organizations Respondent Belonged To	1987	2004 ^a
No Organizations	8.3	33.8
1 Type	4.4	24.9
2 Types	5.4	15.6
3 Types	1.9	19.5
4+ Types	2.8	14.9

^a Corrected by dropping 41 miscoded cases per Smith (2008).

which they note (p. 192) is much higher than that of the 2004 GSS (2.1 for the same age group).

Clearly, there are both theoretical and empirical reasons for considerable skepticism about MS-LB's conclusion, even in their erratum, that there was "a significant increase in the number of people who report that they do not discuss important matters with anyone." The anomalies in the 2004 Numgiven data, to which I will now turn, are even more striking.

ANOMALIES

Some results from the 2004 GSS Numgiven data make little sociological sense; they render the main findings—that Americans' networks shrank greatly—not impossible, but highly implausible. Again, I focus on the simple dichotomous dependent variable: the respondent was coded as having given no names versus 1+ names. I analyze the 1987 data, as well as the 1985 data where possible. The three years' data are roughly parallel through the Numgiven question and then differ at the follow-up probe. (In 1985 and 2004, interviewers were supposed to ask for more names if the respondent stopped at fewer than five, and in 1987, if the respondent stopped at fewer than three.)

ANOMALY 1: ORGANIZATIONAL MEMBERSHIPS

The 2004 GSS asked the Numgiven question near the end of the interview and after a heavy battery of questions concerning organizational membership. This turns out to be important in trying to understand the artifacts in the data, and I will return to it again. For now, consider Table 3, which displays the percentage of respondents coded as giving no names to Numgiven cross-classified by the number of types of organizations to

which they belonged, repeated for 1987 and 2004. (The 1985 GSS did not ask about organizations.) We see, for example, that in 1987, 8.3 percent of respondents who reported no organizational memberships reportedly gave no names in answer to Numgiven, 4.4 percent of those who belonged to one type of organization reportedly gave no names, and so on. In striking contrast, in 2004, almost 15 percent or more of respondents who belonged to two, three, or four or more types of organizations supposedly listed no one in answer to Numgiven—14.9 percent among members of 4+ types ($n_{2004} = 221$), compared with under 3 percent in 1987 ($n_{1987} = 250$). Under what plausible sociological theory could five times as many hyper-social respondents, respondents who were willing to report in detail about their organizations, claim *no* confidants in 2004?

ANOMALY 2: COOPERATIVENESS

Table 4 displays the percentage of respondents who were coded zero in Numgiven by the interviewers' ratings of their cooperativeness. The striking point here is that 23.7 percent of "friendly, interested" respondents in 2004 were coded zero, compared with 6.0 and 3.7 percent in the two 1980s surveys ($N > 1200$ each year). This is a roughly five-fold increase among the most forthcoming respondents.

ANOMALY 3: EDUCATION

Educational attainment is the best predictor of whether respondents gave any names. Table 5 displays the pattern. Note, in the bottom row, that in 2004 about 16 percent of respondents with postgraduate degrees were recorded as giving no names ($N = 142$). In the 1980s, only two postgraduate respondents were coded as zero—one in each year ($N = 90, 86$). The data imply a roughly 15-fold increase in the likeli-

Table 4. Percentage of Respondents Who Gave No Names to the Numgiven Question, by Interviewers' Rating of Their Cooperativeness, by Year

Interviewer's Rating of Respondent	1985	1987	2004 ^a
Restless, impatient, hostile	32.1	20.7	57.5
Cooperative	17.6	7.9	25.9
Friendly, interested	6.0	3.7	23.7

^a Corrected by dropping 41 miscoded cases per Smith (2008).

Table 5. Percentage of Respondents Who Gave No Names to Numgiven Question by Educational Attainment, by Year

Respondent's Highest Degree	1985	1987	2004 ^a
Less than high school	18.2	11.1	34.8
High school	6.9	3.8	28.0
Junior or some college	1.7	5.2	25.7
Bachelor's degree	2.3	2.6	14.6
Postgraduate degree	1.1	1.2	16.2

^a Corrected by dropping 41 miscoded cases per Smith (2008).

Table 6. Percentage of Respondents Who Gave No Names to Numgiven Question by Marital Status, by Year

Respondent's Marital Status	1985	1987	2004 ^a
Married	6.6	3.6	22.2
Widowed	21.4	9.4	31.3
Divorced	8.3	5.5	25.7
Separated	20.0	10.8	29.8
Never married	6.7	6.3	28.7

^a Corrected by dropping 41 miscoded cases per Smith (2008).

hood of giving no names among that highly sociable group. This oddity accounts for a finding emphasized by MS-LB, the closing of the gap in network size by respondent education between 1985 and 2004.

ANOMALY 4: MARITAL STATUS

Table 6 displays the percentage coded as Numgiven equals zero by respondents' marital status. Notice (1) about 22 percent of married respondents presumably gave no names in 2004, compared with about 5 percent in 1985 and 1987 combined; and (2) the differences among marital categories essentially wash out in 2004.¹⁰

¹⁰ A simple test is to apply an anova to each column of Table 6. For 1985 and 1987, the marital effects are significant at least at $p < .002$. For 2004, they are not significant ($p = .083$).

The 22 percent rate for 2004's married interviewees is striking. Why would these respondents not at least mention their spouses? We can ask: What percentage of married respondents did *not* list their spouses? Table 7, line A, provides the answer: about 42 percent of the 2004 married respondents failed to name their spouses, compared with about 30 percent in the 1980s. Perhaps this finding reflects something about the way interviewers asked the question; maybe in 2004 they implied that spouses should not be named. But line B in Table 7 discounts that explanation: among respondents who gave any names at all to the interviewer, the proportion who failed to include their spouses was about the same across all years. This suggests that the 2004 application did not have an anti-spouse bias. More important, it strengthens the emerging pattern, that the critical difference between 2004 and the earlier years was in the mentioning of any names at all, in the zero category. To

Table 7. Percentage of Married Respondents Who Did *Not* List a Spouse in Response to Numgiven, by Year

	1985	1987	2004 ^a
(A) Among all married respondents	32.5	25.5	41.9
(B) Among married respondents who listed at least one name	27.8	22.7	25.3

^a Corrected by dropping 41 miscoded cases per Smith (2008).

return to the key point: one-fifth of married respondents in 2004 failed to mention *anyone* as a confidant. This represents a four-fold increase over the 1980s for a category of people who were living with a confidant.

PROVISIONAL CONCLUSION AND HYPOTHESIS

These four anomalies are most puzzling. Subgroups one would expect to be rarely isolated, and which were rarely isolated in 1985 and 1987, had rates of isolation in 2004 of about 15 percent or higher. *Indeed, among the 88 respondents in 2004 who were (1) married, (2) holders of postgraduate degrees, and (3) rated as "friendly" by the interviewers, 18 percent named no one in answer to the network question. In the 1980s surveys, none of the 102 respondents in that select group failed to give at least one name.*¹¹ As a consequence, differences in isolation by organizational membership, education, cooperation, and marital status narrowed in the 2004 survey. A logit model predicting the probability that a respondent gave no names explains considerably less of the variation in the 2004 data than in the 1980s data (even though the 2004 distribution is much less skewed).¹² It is, of course, possible that an as yet unidentified social change between 1985 and 2004 severely and disproportionately cut the social ties of educated, cooperative, married,

and club-joining Americans—but it is an implausible (and inefficient) explanation.

The most parsimonious explanation for these anomalous results is this: for some reason, a random set of the 2004 respondents, roughly 15 to 20 percent of them, were coded as having given no names to Numgiven even when they did or would have given one or more names. (Smith [2008] identified only 41 respondents who had refused to answer and were coded zero.) This would explain not only the gross change from 1985 to 2004 in percentage of respondents coded zero, but also why in virtually every subcategory of any size among the 2004 respondents—married, postgraduates, and so on—about 15 percent or more were coded zero.

SEARCHING FOR THE ARTIFACT

MS-LB are themselves somewhat dubious of their results; “given the size of this social change, we remain cautious (perhaps even skeptical) of its size” (McPherson et al. 2006:372). They consider several possible artifacts.

STUDY DESIGN

MS-LB reject the suggestion that the designs of the 1985 and 2004 surveys differed significantly: “Interviewer training and probe patterns also were very similar across the two surveys” (McPherson et al. 2006:364). They also find sampling frame and response rate differences to be unlikely explanations. More recently, McPherson examined interviewer effects within the 2004 survey (personal communication), but it is hard to see how that could explain much of the 1985 to 2004 difference.

CONTEXT AND TRAINING EFFECT

MS-LB point to a crucial difference between the 1985 and 2004 surveys: in 2004, the network item followed a sequence of questions asking

¹¹ I dropped two of the original 90 cases from 2004 as a result of the Smith (2008) correction.

¹² Regressing the dichotomy, gives no names versus any names, on cooperativeness, race (black), education (four dummies for degree attained), age and age-squared, and marital status (dummy for married versus not), yields the following results: (1985) Log Likelihood = -369, pseudo R-squared = .171; (1987) Log Likelihood = -325, pseudo R-squared = .116; and (2004) Log Likelihood = -755, pseudo R-squared = .050 (corrected for the 41 miscodes).

respondents about the organizations to which they belonged. Perhaps respondents learned during this segment that affirming membership in any organization led to their being asked many more questions, often intrusive ones. Some interviewees may, therefore, have subsequently reacted to the network question that immediately followed by giving no or few names to avoid detailed questions or simply to get the long interview over with. To test this possibility, MS-LB turned to the 1987 version of “Numgiven,” because that survey preceded Numgiven with a (shorter) set of organization questions. (MS-LB do not otherwise analyze the 1987 data, except in an appendix, because, as previously noted, the 1987 survey asked respondents to describe their first three, rather than first five, associates.) As we saw in Table 1, the 1987 results are comparable to the 1985 results and both differed greatly from 2004. MS-LB therefore conclude that the 2004 organizational module cannot explain the reduction in names from 1985 to 2004.

However, MS-LB significantly understate the differences between the 2004 and 1987 organizational questions; the 2004 series was much more burdensome. In 2004, but not in 1987, the GSS asked respondents to not only indicate what types of organizations they belonged to, but also to distinguish specific organizations within the types (e.g., to count how many hobby clubs they belonged to). Even more critically, in 2004 but not in 1987, the GSS asked for the name, address, telephone number, and Web site of one specific, randomly selected organization—and for the name and telephone number of a leader in that organization. One can reasonably suspect that the extended questioning and the greater intrusiveness of the 2004 version, although surely valuable for studying organizations, made some respondents reluctant to provide even just the first names or initials of many confidants, if any at all.

Such a “training effect” remains a leading explanation for the exceptionally high percentage of respondents coded as offering no names in 2004. Smith (2008) describes the 41 mis-coded respondents as disproportionately composed of organization members who refused to provide follow-up information on their organizations. He speculates that others may have opted out of the network question, not by refusing to answer, but by simply answering “no

one” to Numgiven.¹³ (At this writing, the GSS Board of Overseers, in an effort to explain the 2004 results, has approved a survey experiment in the 2010 GSS to test this hypothesis and the fatigue hypothesis discussed next.)

One comparison between the 1987 and 2004 results, however, suggests that the contextual difference may not suffice to explain the large difference in the percentage coded as zero on Numgiven. (The context may explain why the 2004 respondents who gave at least one name gave fewer names on average than did the 1985/1987 respondents, but I have not explored that issue.) If respondents gave no organizations in response to the question asking about memberships, then they were not exposed, in either 1987 or 2004 (nor, of course, in 1985 when membership was not asked) to the follow-up questions on organizations; they were not “trained” to avoid such questions. Table 3 shows that even among respondents who reported belonging to no organizations in 1987 or 2004, roughly four times as many respondents in 2004 gave no names in answer to “Numgiven.” This finding suggests that a training effect may not suffice to explain the 1985/1987 versus 2004 differences.

FATIGUE

MS-LB also consider the possibility that the 2004 network question had many defectors because it came so late in the interview—essentially at the end, before the income questions. To test that idea, they examine whether missing data in previous modules affected respondents’

¹³ “Thus what distinguishes the [41] errant cases is that they tended to have objected to providing detailed information on the groups they belonged to and people they discussed important personal matters with. . . . [T]his connection between the group items and the social-network questions raises the possibility that others who wanted to minimize follow-up questions asking about details of their interpersonal contacts in general and those who had answered group-membership, follow-up questions, including the hypernetwork battery [the questions asking for organizational names and places] in particular, might have reported they had not discussed an important personal matter with anyone as another way of skipping out of follow-up questions” (Smith 2008:2).

cooperation. The answer was: not nearly enough to explain the drop-off in names between 1985 and 2004. They also examine whether interviewers' ratings of interviewees' cooperativeness account for the 2004 results. That is, did respondents' resistance increase over the years? Again, the answer is no, as we saw in Table 4.

In these ways, MS-LB, skeptical of their own results, tried strenuously to check for artifacts. They failed to identify any, although they may not have adequately dismissed the possibility that the 2004 organization questions "trained" respondents to say no.¹⁴ The core finding of a huge decline in Americans' number of confidants remains both sociologically stunning and unexplained.

A RANDOM ERROR?

Earlier, I suggested there may have been a technical error—in the software for the computer-assisted interviewing, in interviewer procedures, or in coding—that, in effect, randomly recoded respondents to zero on Numgiven. I advance this hypothesis because of the pattern in anomalous results in which at least 15 to 20 percent of virtually every subgroup, including members of four or more organizations and the married, friendly, postgraduates, were coded as isolated. Here, I pursue this hypothesis by a rough simulation.

(1) Assume that the "true" 2004 distribution of respondents across categories of Numgiven, from 0 through 6+, is identical to that in 1985. (2) Assume that 20 percent of all respondents in 2004 were randomly coded as having given no names, whatever they actually said. I take the 1985 distribution and move 20 percent of the cases out of each category and move them all into the zero category as a simulation of what the 2004 distribution would look like if the only true change were this error. (This simulation cannot be done with the 1987 data, because in that survey interviewers did not encourage more

than three names.) Table 8 shows the resulting distribution.

Although the simulated and observed 2004 distributions still differ somewhat, with the observed one skewed more to lower categories, the simulation is nonetheless a reasonably close fit. This exercise does not take into account other factors—that only the 2004 survey asked respondents intensive questions about organizations, other methods differences, demographic changes, true historical changes, or the possibility that the random error was not exactly 20 percent (perhaps it was 14.9 percent; see Table 3). Given all that, the fit is close enough.

Another sort of simulation addresses the MS-LB finding (replicated above; see note 12) that in 2004 the associations between giving any names and predictor variables were weaker than in the earlier surveys. Table 9 presents a simulation of the association between college graduation and giving no names. It shows the results for 1985, a simulated 2004, and the real 2004 data. The simulated 2004 table is the same as the 1985 table, except that 20 percent of the respondents who gave one or more names to Numgiven are transferred to the no-names row. One sees the strong association between education and naming in 1985 ($\gamma = -.71$; Odds Ratio = .17). The effect of a random 20 percent redistribution to the "no names" category to create a simulated 2004 table weakens the association ($\gamma = -.18$, OR = .70) and approaches the observed 2004 cross-tabulation ($\gamma = -.39$, OR = .44).

Table 10 summarizes the results of conducting this sort of simulation for four predictor variables, using both 1985 and 1987 for the simulation. (I can use the 1987 GSS here because the dependent variable is not the number of names but simply 0 versus 1+.) The important comparison is between the last column, observed 2004 associations, and the two columns to the left, simulated 2004 associations based on the 1985 and 1987 results with their 20 percent redistributions. For the most part, the simulations come close to the observed. One noteworthy exception is the effect of being black, which is considerably stronger in 2004 than the simulation expects.

MS-LB's results, both the distribution of respondents by number of names and the associations of key variables with giving any names, can thus be approximated without assuming

¹⁴ An additional possibility is mode of interview. Although the GSS is primarily face-to-face, in 2004, 22 percent of the interviews were conducted or completed on the telephone with hard-to-reach respondents. There was no zero-order difference in the percentage of isolates in the two formats.

Table 8. Simulation: Percentage of Respondents by the Number of Names They Gave to the Numgiven Question; 1985, Simulated 2004, and 2004

Number of Names Given to Numgiven Question	Observed 1985	Simulated 2004 ^a	Observed 2004 ^b
No Names	8.9	27.1	25.0
1 Name	14.9	11.9	19.7
2 Names	15.3	12.3	18.4
3 Names	21.0	16.8	16.3
4 Names	15.2	12.2	9.0
5 Names	19.2	15.4	6.7
6+ Names	5.5	4.4	4.9

^a Simulated by subtracting 20 percent of each 1985 cell and moving it to the “No Names” row.

^b Corrected by dropping 41 miscoded cases per Smith (2008).

Table 9. Simulation: Cross-tabulation of Giving No Names to Numgiven by Respondent’s Highest Degree; 1985, Simulated 2004, and 2004

	Percentage Who Gave 1+ Names versus No Names					
	1985		Simulated 2004 ^a		Observed 2004 ^b	
	< BA	BA+	< BA	BA+	< BA	BA+
Gave 1+ Names	89.7	98.1	71.8	78.5	71	84.9
Gave No Names	10.3	1.9	28.2	21.5	29	15.1

^a Simulated by taking 20 percent of the upper row in the 1985 table and adding it to the lower row.

^b Corrected by dropping 41 miscoded cases per Smith (2008).

Table 10. Simulations of Associations Between Giving No Names and Predictor Variables (Gammas)

Predictor Variable	1985	1987	2004		
			Simulated from 1985	Simulated from 1987	Observed ^a
Respondent had college degree	-.71	-.49	-.18	-.09	-.39
Respondent was married	-.32	-.36	-.11	-.08	-.16
Respondent was black	.57	.44	.27	.12	.45
Respondent was very cooperative	-.60	-.50	-.27	-.14	-.18

Note: Simulations follow procedure in Table 9 for each underlying 2 × 2 table used to generate the associations.

^a Corrected by dropping 41 miscoded cases per Smith (2008).

any real historical change and by assuming only that (1) American social networks in 2004 were the same as those in 1985 and (2) for some reason a random 20 percent of the 2004 respondents were erroneously recorded as giving zero names. Any other effects, be they technical or substantive, would be relatively minor compared with this one. Perhaps both sorts of factors—a training or fatigue effect from the 2004 organizational questions and a random error—were at work. For example, one or more tech-

nical errors may have inflated the number of respondents coded as having no confidants, while a contexts or fatigue effect depressed the number of names given by those who gave any names at all. If we combine artifacts with any kind of modest substantive changes—for example, a growing gap between the more and less educated, or between blacks and whites—then we could imagine fully accounting for the observed 1985 to 2004 differences without assuming that “Americans are connected far

less tightly now than they were 19 years ago" (McPherson et al. 2006:373).

CONCLUSION AND LESSONS LEARNED

Although the "smoking gun" artifact has not yet been found,¹⁵ we can only conclude that the 2004 results, even after the MS-LB erratum, are *highly implausible*. The best estimate is that the "true" percentage of 2004 respondents who were isolated was roughly the same—or perhaps less—than the percentage in 1985/1987, somewhere under 10 percent.¹⁶

Beyond its details, this case reinforces a few lessons. One is the importance of early, close, exploratory data analysis to check one's data for anomalies, outliers, coding problems, and the like. Any scholar who has done much statistical work will have been tripped up by such a problem. (I have tripped and can expect to again.) Another is the importance of replication to ensure that our findings are robust. The public availability of researchers' data—and here, the GSS is especially commendable—is critical to the scientific cross-checking process.¹⁷ Results that seem too good or too strong to be true probably are and need particularly thorough scrubbing, especially those that will find their way into the general media.

One point on network research in particular: there are potential problems with using a single probe—discussing "important matters"—for

eliciting the people in respondents' networks.¹⁸ This item is vulnerable to significant "noise" and contextual effects (Bailey and Marsden 1999; Bearman and Parigi 2004). One might speculate, for example, that being questioned during a spring full of heated discussion about war and presidential primaries may have led many respondents to interpret "important matters" as political matters. In any event, a more diverse battery of questions would probably yield more robust measures.

Those skeptical of the MS-LB report owe the authors at least a suggestion for how those results could have obtained if Americans' social networks had in fact not changed. MS-LB certainly tried to explain the great contrast between the 1985 and 2004 results. Nonetheless, whatever the explanation for the 2004 results that will (hopefully) emerge, reports on those findings continue to hang out in public where the latest word is that American social networks crumbled between the 1980s and 2004. Because those results are hard to explain sociologically, are inconsistent with other findings, and have major internal anomalies, that conclusion now appears implausible. Pending the release of further results and further studies of the matter by the GSS, the best statement social scientists can make is that we do not know whether or how American social networks, as measured by the 2004 Numgiven item, changed between 1985 and 2004. I would further venture that our best estimate, drawing on other data, is that they changed little.

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¹⁵ To date, Tom Smith has been unable to track down any further technical problem beyond the 41 miscoded cases.

¹⁶ I made an effort to estimate the true 2004 value. In the 1985 data, I regressed respondents' network sizes on basic demographic variables and on four sociability questions (how often the respondent got together with relatives, friends, neighbors, or went to a bar). The equation explains 15 percent of the 1985 variance. Substituting the 2004 means for those of the 1985 predictor variables yields a predicted mean for "Numgiven" of 3.2 (versus an observed mean of 2.0). Even if we imagine that educational attainments did not change between 1985 and 2004, the predicted mean for "Numgiven" is 3.0, the same as the observed 1985 mean.

¹⁷ For earlier cases in this vein, see, for example, Kahn and Udry (1986) and Jasso (1986); Peterson (1996) and Weitzman (1996).

¹⁸ This item is derived from the bank of about 10 name-eliciting questions used in Fischer (1982). Methodological tests of these questions show that the set could reliably describe respondents' networks, but that any single question has a high error rate (Jones and Fischer 1978). In particular, there is a "difference between the method's accuracy with regard to the names given in answer to specific questions and its accuracy with regard to the names as a whole. Our analyses of the pilot surveys show that there were notable reliability problems in clearly specifying who provided what. . . . But the reliability of the whole list was greater. . . . Associates missed by the specific question tended to be picked up somewhere else in the interviews" (Fischer 1982:289–90).

To Dwell Among Friends: Personal Networks in Town and City (1982); *America Calling: A Social History of the Telephone to 1940* (1992); *with five Berkeley colleagues*, *Inequality by Design: Cracking the Bell Curve Myth* (1996); *and with Michael Hout*, *Century of Difference: How America Changed in the Last One Hundred Years* (2006). *A forthcoming book is tentatively titled* *Made in America: A Social History of American Culture and Character* (University of Chicago Press).

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Models and Marginals: Using Survey Evidence to Study Social Networks

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Fischer (2009) argues that our estimates of confidant network size in the 2004 General Social Survey (GSS), and therefore the trend in confidant network size from 1985 to 2004, are implausible because they are (1) inconsistent with other data and (2) contain internal anomalies that call the data into question. In this note, we assess the evidence for a decrease in confidant network size from 1985 to 2004 in the GSS data. We conclude that any plausible modeling of the data shows a decided trend downward in confidant network size from 1985 to 2004. The features that Fischer calls anomalies are exactly the characteristics described by our models (Table 5) in the original article.

We are grateful to Professor Fischer and the *ASR* editors for the opportunity to revisit our 2006 article on social isolation (McPherson, Smith-Lovin, and Brashears 2006). We see two central themes in his comment: (1) there are over-reports of social isolation in 2004 and (2) the General Social Survey (GSS) data do not support the claim that confidant networks changed significantly from 1985 to 2004. We strongly agree with Fischer's (2009) first claim. We make exactly that point in the abstract of our 2006 article.¹ We disagree, however, that

these over-reports are confined to the 2004 data or that they are random. We use this opportunity to elaborate our analysis of the reports of social isolation.

We will show that Fischer's second (and most important) conclusion about the lack of a trend in social connectedness is extremely unlikely. We disagree with Fischer that the unweighted² cross-tabulation analyses that he presents are an appropriate approach to analysis, given the strong effects of cooperativeness and fatigue that we identified in our original Table 5. Such analyses are misleading because of omitted variable bias, among other problems. We review

We would like to thank Mark Chaves, Peter Marsden, and the Network Study Group at Duke University for useful suggestions. We also thank Jeff A. Smith for independently replicating our analyses. We, of course, remain responsible for our conclusions here. The data collection of network measures in the 2004 GSS was supported by National Science Foundation grant SES 0347699 to the first and second authors and a grant from CIRCLE to Tom W. Smith. The current analysis was supported by NSF grant BCS 0527671 to the first and second authors.

¹ "The data may overestimate the number of social isolates . . ." (McPherson et al. 2006:353).

² We do not emphasize the issue of weighting in this note because it does not affect the main substantive findings under dispute. We note, however, that without properly weighting for the complex sample design of the 2004 survey, Fischer's percentages refer only to the (two distinct) populations of respondents sampled in 2004 and are not representative of the non-institutionalized adult population of the United States (the population the GSS is meant to represent). All analyses of these data intended to reflect the general population must be weighted.

his findings to highlight these problems. We dispute his statement that the GSS trend data are inconsistent with other estimates and that no plausible social change could have produced a strong trend in networks. We conclude with thoughts about the perils of public sociology and the value of public data.

HISTORY AND THEORY

We began reviewing the 2004 data as soon as they were collected, because we initiated the NSF grant that supported the replication of the 1985 GSS network module. We immediately contacted the GSS to tell them there were too many reports of zero confidants in "Numgiven," the variable that codes the number of confidants in the 2004 data. Although we now know there were 41 miscoded cases (McPherson, Smith-Lovin, and Brashears 2008a), they found no problem at the time.

We modeled the over-reporting of zeros in the original article by controlling for artifacts of uncooperativeness and fatigue (see McPherson et al. 2006: Table 5). Because Fischer concentrates on social isolation—the reports of zero confidants—we use this opportunity to focus on the process through which zeros in particular might be over-reported in the data. We believe that there is actually a mixture of two processes in the data: a Poisson process of acquisition and loss of confidants as described below, and a binary mechanism (zero-inflation) that affects whether a respondent is coded into the zero category as a result of some independent process (e.g., respondent fatigue, interviewer effects, technical glitches in transcription, lack of rapport with the interviewer, or some substantive mechanism). We model these two processes explicitly with a zero-inflated Poisson analysis.³

How did we know immediately that there were too many zeros in Numgiven, and why do we now use an inflated Poisson model to model the data? Underlying any cross-sectional data like the GSS is a dynamic process, for which the cross-sectional measure is a snapshot at a single point in time. For the Numgiven variable, this process consists of discrete counts, which can be captured with a simple stochastic model. A person's confidants come from the very large number of poten-

tial partners in society, but one loses confidants from the relatively small set currently possessed. Under these conditions, the cross-sectional distribution of confidants will follow the Poisson distribution,⁴ with cross-sectional mean equal to the ratio of the rate of gain to the rate of loss of confidants (for a derivation of this result, see McPherson 1981, forthcoming). When the rates of gain and loss depend on the social positions of the actors involved, we model this process as heterogeneous Poisson, which allows us to take the sources of variability in those rates into account.

No simple Poisson process will generate the number of reported social isolates in the 2004 data, given the shape of the rest of the distribution, so we knew that the 2004 Numgiven zeros were inflated. In the course of reinterviewing some of the 2004 respondents, we discovered more evidence strongly suggesting there were misreported zeros for Numgiven. An intensive search by NORC discovered the 41 miscoded cases shortly after we reported this fact to the GSS.

FISCHER'S CORE CLAIM: NO TREND IN SOCIAL CONNECTEDNESS

The GSS data, even under the most conservative assumption that *all* of the zeros in excess of the Poisson process are artifactual, are inconsistent with Fischer's core claim that there was no change in confidant networks from 1985 to 2004. The zero-inflated Poisson analysis in Table 1 shows the effects of our independent variables on both the heterogeneous Poisson model and the binary process of zero-inflation.⁵ The zero-inflated model assumes there are two possible reasons for an observation to have a value of zero: (1) a Poisson count process, in

³ The zero-inflated Poisson model is available under the `zeromodel` option in SAS, `zip` in STATA, and `zeroinfl()` in R.

⁴ Readers familiar with the literature should note that this derivation of the cross-sectional distribution of Numgiven is very different from the standard Erdos-Renyi null model; we explicitly model the gain and loss of network ties as a stochastic process, while the Erdos-Renyi approach randomly assigns network ties in a static network.

⁵ Our 2006 article used the negative binomial model, which adds an additional parameter for heterogeneity for the original analyses because of overdispersion in Numgiven. Analyses of the now-corrected data set show that the Poisson is the preferred model after the explanatory variables are taken into account.

which parameters govern all of the values of the variable and (2) a binomial process, in which parameters govern an additional probability of the zero category versus all else. The parameters of both processes are estimated simultaneously, so that each process acknowledges the effects of the other. The predictors for the binomial process model the probability that a case will be an “inflated” zero, taking into account how many zeros there “should” be according to the entire Poisson distribution of Numgiven, while the Poisson estimates take the zero inflation process into account.

Table 1 presents an analysis that adjusts for zero-inflation in both 1985 and 2004. The change in Numgiven from 1985 to 2004 is documented by the Wave coefficient in the second

panel of Table 1, which models the dependence of mean Numgiven on our independent variables. The highly significant negative coefficient for Wave (1 = 2004) implies a mean decrease from 1985 to 2004 in Numgiven of around one confidant, on average, taking into account the excess zeros, the known threats to validity, and the substantive effects of sociodemographic variables. Figure 1 shows the estimated mean difference in Numgiven between 1985 and 2004 plotted across years of education.

This (unavoidably busy) figure contains a wealth of information. Each of the 2,957 respondents is indicated by either a larger square (1985) or a smaller dot (2004); the fitted mean Numgiven is the solid line for 1985 and the dashed line for 2004. The points have been jit-

Table 1. Zero-Inflated Poisson Model of the Number of Confidants (Numgiven) (using weighted data from the 1985 and 2004 General Social Surveys)

Independent Variable	Coefficient	Standard Error	Significance
Zero-Inflation Model Coefficients (binomial with logit link)			
Constant	−3.93	.67	.000
Wave (1 = 2004)	2.09	.37	.000
Cooperative ^a	−.34	.41	.411
Restless/impatient ^a	1.94	.37	.000
Hostile ^a	2.27	1.62	.162
Number of missing	.50	.13	.000
Years of education	−.05	.03	.136
Female	−.19	.21	.371
Age	.01	.01	.003
Married	−.44	.22	.045
Black ^b	1.24	.24	.000
Other race ^b	.13	.50	.796
Count Model Coefficients (Poisson with log link)			
Constant	.59	.07	.000
Wave (1 = 2004)	−.30	.03	.000
Cooperative ^a	−.19	.04	.000
Restless/impatient ^a	−.21	.10	.042
Hostile ^a	−.48	.36	.182
Number of missing	−.11	.04	.006
Years of education	.05	.00	.000
Female	.05	.03	.045
Age	−.00	.00	.014
Married	−.02	.03	.494
Black ^b	−.11	.05	.024
Other race ^b	−.22	.06	.001
Log-likelihood: −5.37e+03 on 2 Degrees of Freedom			

Note: We suppress statistical interactions between Wave and Education and Wave and Age for clarity in this table, but we take them into account in detailed analyses where appropriate.

^a Relative to respondent coded “friendly and interested” by the interviewer.

^b Relative to white respondents.

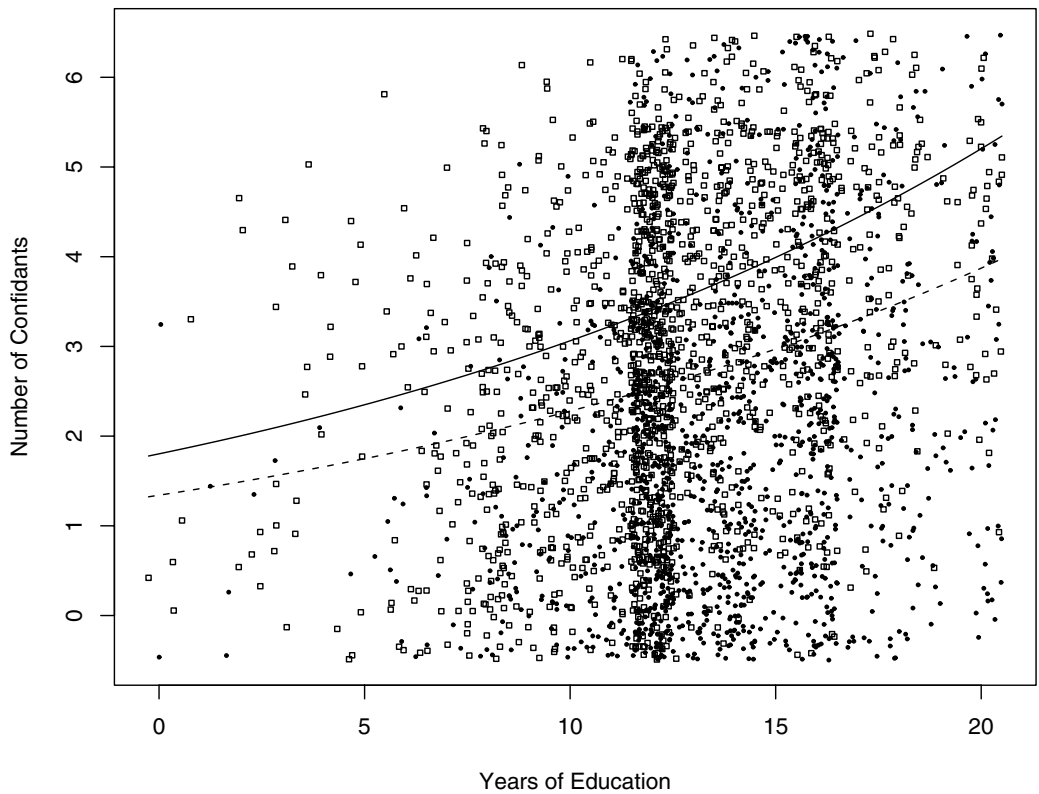


Figure 1. The Relationship between Education and Number of Confidants, 1985 and 2004

Legend: Squares indicate 1985 respondents, dots indicate 2004 respondents. The solid line is fitted mean for 1985, the dashed line is fitted mean for 2004.

Source: General Social Survey.

Note: Fitted means from zero-inflated Poisson regression model controls for presence of inflated zeros, fatigue, cooperation, age, gender, marital status, and race, as in Table 1.

tered to reveal their density in the scattergram. We first point out the need for humility in interpreting our model (and by implication, any summary of the data in the form of simple cross-tabulations); clearly, there is a great deal of variation in the data not explained by the model. However, close inspection reveals an unambiguous tendency for the 2004 values of Numgiven to inhabit the lower range, while the 1985 values are higher, on average. Note, for instance, the sparseness of 2004 observations in the upper-left quadrant in comparison with the lower-right quadrant. This impression is aptly summarized by the fitted curves for mean Numgiven, which are strongly different for 1985 and 2004 ($p < .000001$). It is important to remember that the fitted curves are adjusted for the fact that there are more inflated zeros in

2004 than in 1985 (see below), and for all the variables described in Table 1.

Our best model for Numgiven, controlling for the threats to validity posed by fatigue and cooperation; the known sources of variation due to years of education, age, marital status, and race; and taking into account the inflated number of zeros in the 1985 and 2004 data, shows a substantively and statistically significant difference between 1985 and 2004 in the GSS data.

As the table and figure conclusively show, the differences between 2004 and 1985 are extremely unlikely (less than one chance in a trillion) to have been due to sampling error, since the fitted means of network size are very different, taking into account all the available control variables. The trend is significant, even under the most conservative assumption that none of

the reported social isolation in excess of the Poisson process of gain and loss is substantively meaningful.

Our analysis rules out Fischer's (2009) simulated random mechanism, since the excess zeros in 2004 are not sufficient to have produced the 1985 to 2004 difference. In fact, one can easily demonstrate that there are major differences between 1985 and 2004 in Numgiven by simply throwing out all the zeros for both years. (One can either do this literally, or model it with zero-truncated count analysis. Both approaches lead one to conclude that there are substantial differences in Numgiven between 1985 and 2004 whether or not there are too many zeros.) If the 1985 to 2004 change were due to a process in which some positive counts were randomly coded as zero, as Fischer suggests, then ignoring the zeros would destroy the mean difference in number of confidants from 1985 to 2004. Since that mean difference remains great after eliminating the zeros, it is clear that the data show strong evidence for change.

As we repeatedly point out in the original article, we are unable to find any combination of variables that destroy the difference between 1985 and 2004 in the number of confidants (McPherson et al. 2006:367–71). We are forced to conclude that the data show a decline in social connectedness from 1985 to 2004. As Fischer's many cross-tabulations and our original Table 5 reveal, this decline does not depend on any variable that either he or we have been able to discover in the available data.

Fischer suggests (as does our original abstract) that there are too many zeros in the 2004 data. We show below that this is almost certainly true. He ignores the possibility of inflated zeros in 1985; we correct this omission. He posits a purely random mechanism for the inflation of zeros, but we show that his random mechanism can easily be ruled out.

TOO MANY ZEROS?

The coefficient for Wave, the dummy variable representing the change in inflated zeros from 1985 to 2004, appears in the second row of the first panel of Table 1. This significant positive coefficient implies that there are more inflated zeros in 2004 than in 1985. Because this coefficient is a logistic regression estimate, the value

of 2.09 implies that the odds of an inflated zero in 2004 (i.e., a zero in excess of the Poisson-predicted zeros) are more than seven times the odds of such an event in 1985. This estimate takes into account both the main effects of the independent variables from our original Table 5 on the Poisson process, and the effects of those variables on the probability of zero inflation.

Using the results of Table 1, we can go much further than asserting that there are inflated zeros in 2004. We can (1) estimate the effects of known threats to the validity of the items due to fatigue and non-cooperation on the probability of inflated zeros, (2) estimate the effects of known substantively relevant variables such as age, marriage, and education on the probability of inflated zeros, (3) estimate the number and proportion of such zeros in both 1985 and 2004, taking into account the above effects, and (4) describe the characteristics of individuals who are likely to be coded as inflated zeros. We take on the last two of these tasks first.

Although there are several ways to estimate the number of inflated zeros in each year, we use an informal Bayesian approach to the posterior distribution of Numgiven (cf. Gelman and Hill 2006). We estimate that there are 42 excess zeros in 1985 (with a 95 percent credible interval from 17 to 76), and 208 excess zeros in 2004 (95 percent interval from 171 to 244). Our preferred model thus projects that the 2004 data have roughly 166 more inappropriate zeros than the 1985 data, with a very high degree of confidence that 2004 has more than 1985. Put qualitatively, we are pretty sure that there are inflated zeros in both 1985 and 2004, and we are pretty sure that there are more in 2004 than in 1985, but the best estimate for the number of such zeros has a substantial amount of uncertainty.

The awareness of this uncertainty is one reason to be skeptical of Fischer's claim that no change has occurred, based on his strong assumption that there are exactly 200 randomly generated excess zeros in 2004 and exactly none in 1985. Taking the most conservative estimates of non-inflated zeros at face value, we would still be left with roughly a 70 percent increase in social isolation from 1985 to 2004. In 1985 there are 136 reported zeros, of which we estimate 42 are inflated, leaving 92 out of 1,531 cases for a proportion isolated of .06. In

2004 there are 356 reported zeros, of which we estimate 208 are inflated, leaving 148 isolates out of 1,428 respondents, for a proportion isolated of .10. The ratio of .10 to .6 is 1.7, suggesting a 70 percent increase in non-inflated zeros. Once again, we need to emphasize that we are removing the influence of *all* inflated zeros from this comparison, leading to an extremely conservative estimate of the increase in social isolation.⁶

There are many other reasons to be skeptical of Fischer's analysis. Turning again to Table 1, we see that the excess zeros, rather than being randomly distributed across social categories, as Fischer asserts, are systematically related to our measures of cooperativeness, fatigue, age, marital status, and race. Black respondents appear to have more inflated zero responses than do whites, as do older respondents, those with missing items preceding the Numgiven item, and those rated less cooperative by the interviewer. Married respondents are marginally less likely to give an inflated answer of zero. While it is beyond the scope of this note, it is possible to use the predicted probability of zero inflation for each respondent to search for coding problems or other patterns of inflation (e.g., subtle uncooperativeness or satisficing) in the data.

To sharpen the issues of agreement and disagreement with Fischer to this point, we agree that there are too many zeros in the 2004 data (as our original abstract says), we disagree that we should assume there were no such cases in 1985, and we disagree that a simulation assuming purely random error is the way to approach this question. We argue for a model-based approach to assess the substantive and artifactual variables that influence both social connectedness and potentially inflated reports of social isolation. Fischer's cross-tabulations ignore variables that we know influence reports of Numgiven, resulting in omitted variable bias. We now turn to these problems in more detail.

ARTIFACTS IN THE DATA

The coefficients for our fatigue variable (Number of Missing Values) and the cooperativeness dummy variables of Cooperative, Restless/Impatient, and Hostile (compared with Friendly/Interested) in our original Table 5 display major effects of these threats to validity that must be taken into account. These effects' very large size means that any analysis that excludes them will lead to biased and inconsistent results. Cross-tabulations such as those by Fischer, which produce estimates of the percent isolated not taking these artifacts into account, will be misleading because omitted variables will confound the analysis. The Missing Values variable (the count of the number of missing items on the 10 questions preceding the Numgiven variable) has a highly significant coefficient of .372 in our original Table 5, which means that the odds of reporting social isolation increase roughly 50 percent for each additional missing item in the preceding 10 items ($\exp(.372) = 1.5$). Because the observed range of this variable is 0 to 10, one only has to exponentiate 3.72 (10 times .372) to see that the odds for reporting social isolation for someone with 10 missing items are more than 40 times the odds for someone with no missing items. Of course, there are few cases with many missing preceding items, but an approach that relies on simple cross-tabulation will not be able to tell where those extreme cases will be in the high dimensional multivariate space created by considering many independent variables simultaneously.⁷ Because many of Fischer's results involve a very small number of cases (see our discussion below), large percentage shifts will occur with small changes in the independent variables.

Another way of illustrating the artifacts uncovered in our original article is to compare the fitted probabilities of social isolation for a typical case with no missing items to one with 10 missing items, as in Figure 2. The bottom two curves show the results of our original Table 5 for a representative individual with no missing items (2004 and 1985); the top two curves show the results for such a person with 10 missing

⁶ A roughly comparable comparison derived from our original Table 5, column 3, produces .04 isolated in 1985 and .13 in 2004.

⁷ About 10 percent of the respondents have one or more missing items. The means of the Missing Values variable are .077 for 1985 and .223 for 2004.

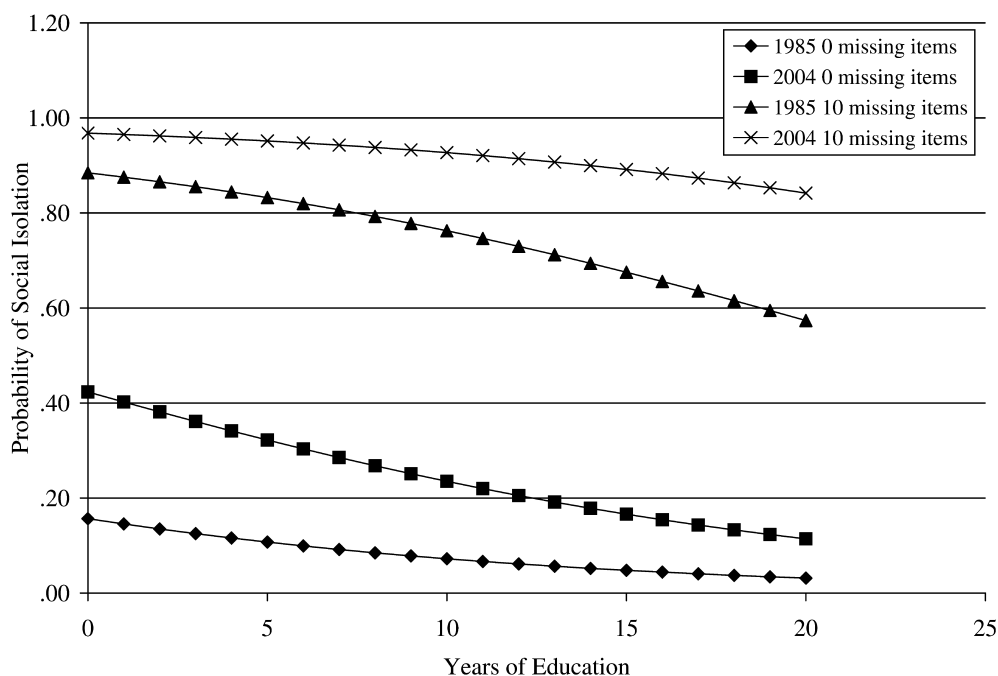


Figure 2. Fitted Probability of Social Isolation from Model IV (2006 Article)

items. The effects of the fatigue variable alone could have changed the apparent amount of social isolation in some of Fischer's cross-tabulations by over 80 percent. Our original analysis takes this striking artifact into account, while Fischer's does not. (The Missing Values variable we used in the 2006 article to represent fatigue was highly predictive of the subsequently discovered 41 miscoded cases. This fact is why our corrected tables published in the December issue of *ASR* [McPherson et al. 2008a] show so little change in our parameter estimates of the 1985 to 2004 change.)

In summary, Fischer's claim that we do not identify artifacts in the GSS data is based on a profound misunderstanding of the results in our original Table 5. The essence of our disagreement is whether or not to take into account the omitted variable bias due to the artifacts that we demonstrated in Table 5. As Figure 2 demonstrates, these effects alone could easily have caused the observed differences in reported social isolation from 1985 to 2004. But as the bottom panel of Table 1 shows, *they did not*.

FISCHER'S "ANOMALIES"

RELATIONSHIPS TO OTHER SOCIATION MEASURES

Fischer's first table, showing the proportion of people who report zero confidants in 1985, 1987, and 2004, seems to show that 1987 is not consistent with the trend downward in confidants. We avoid extensive analysis of the 1987 data because scholars generally agree that these data are not comparable: the 1987 question was somewhat different, and it is clear that the fact that further information on only three alters was collected influenced interviewers' motivation to probe for more alters.⁸ The mean values of Numgiven in 1985, 1987, and 2004 (3.03, 2.56, and 2.12, respectively), however, clearly illustrate our general point that one must pay attention to all of the data, not just a single cell or contrast. While the proportion responding zero in 1987 is small, the mean value of Numgiven for 1987 is even closer to the 2004 value than a simple linear trend would suggest.

⁸ An extensive analysis by Bruce Straits (personal communication) confirms this fact.

In an analysis of convergent validity, Fischer relates Numgiven to other social contact variables. He finds instances in which Numgiven and other social contact variables have patterns that seem implausible to him. We point out that these other measures are of very different types of social contact. We know that the GSS question about discussing important matters in the past six months does not capture all kinds of social contacts. Instead, it tends to elicit extremely close ties (Bailey and Marsden 1999). One may discuss important matters with a 4-year-old child or an investment banker, but such contacts are seldom included in response to the Numgiven question. Instead, GSS respondents tend to mention confidants with whom they interact frequently, whom they have known for long periods of time, with whom they disclose much, and whose opinion they respect.

The other measures of sociation that Fischer uses to establish (lack of) convergent validity are quite different from Numgiven. The question about social contact (Numcntnt: "Not counting people at work or family at home, about how many other friends or relatives do you keep in contact with at least once a year?") occurred in the context of a long module about the use of Internet technology that would prime a cognitive search for people with whom one had contact over that technology. The period of "at least once a year" is broader than "in the last six months" (used in Numgiven). Numcntnt has a range of up to 500, a mean of 27.9, a standard deviation of 43.3, and significant clumping at 10, 20, 50, and the 100's. Clearly, respondents are estimating rather than thinking of specific alters (as they are required to do in Numgiven).

Another measure of sociation Fischer uses is the sum of yes/no answers to a list of 16 types of voluntary associations (Memnum). This is even less similar to Numgiven: people report memberships in groups that never meet face-to-face or in which they have no close ties.⁹ Contrary to Fischer's assertions, the other sociation measures have very similar relationships

with Numgiven in 1985/1987 and 2004. The Pearson's correlation of contacts (Numcntnt, logged to reduce the effects of outliers) and confidants (Numgiven) in 2004 is .17 ($p < .001$). The correlation between number of voluntary association membership types (Memnum) and confidants (Numgiven) is .23 in 2004, almost identical to the .22 relationship in 1987 (both $p < .001$).

Obviously, firmer statements about measurement of sociation and networks require more data. We can be fairly certain, however, that (1) all of these variables have measurement error in them, (2) they measure different types of social activity, and (3) the number of confidants in 1985 and 2004 is significantly related to other measures of sociation. There is no indication that 2004 Numgiven is less related to other measures of sociation than are relationships among other similar variables in other years or other data sets.

MARRIAGE AND EDUCATION: HOW CAN THESE PEOPLE BE ISOLATED?

Fischer spends significant effort discussing the relationships between social isolation, marital status, and education (his Tables 5, 6, and 7). We believe that his analysis of education is a good illustration of the pitfalls of tabular analysis of bivariate relationships. Fischer focuses on one cell of his Table 5, the respondents with postgraduate degrees. We apply our zero-inflated Poisson model (Table 1) to predict zero inflation in that cell. Given a married, friendly/interested, male respondent with 18 years of education and no missing values on preceding variables, our model generates predicted values of 2 percent inflation of zero responses in 1985 and 29 percent inflation of values in 2004. Fischer is surprised that there are so many zeros in this latter cell. Our model not only explains why there are so many zeros here but adjusts for this effect in the other estimates in the model.

To illustrate this fact, consider that the fitted mean number of confidants for these highly educated respondents, after the fatigue artifacts and the overinflation of zeros are taken into account, is 4.4 in 1985 and 3.2 in 2004. There is thus a strong trend downward in the number of confidants for the individuals in those cells, even though the marginal numbers studied by Fischer seem confusing to him. Our models

⁹ Some dynamic evidence for the relationship between Numgiven and Memnum variables appears in McPherson, Popielarz, and Drobnic (1992), which finds that weaker confidant ties are more predictive of changes in memberships than are the very strongest ties.

and Fischer's tables both reinforce the main point of our 2006 article: the trend across these two time points (1985 and 2004) is significant and applies to the major subgroups that we use to divide the population.

Fischer's analysis of marital status (his Tables 6 and 7) shows the same problems of focusing on single cells of a table, rather than controlling for other features that might have changed during the period, and using statistical tests to tell whether the patterns are chance variations or statistically significant. Fischer (2009:663) claims that "the differences among marital categories essentially wash out in 2004." He is arguing here that there is a statistical interaction such that marital status has an effect in 1985 but not in 2004. In fact, a zero-inflated Poisson analysis with just the two independent variables Marital Status and Wave *does* show an interaction effect between these two variables in their effect on Numgiven. When we control for the other variables of our original Table 5, however, this statistical interaction is not significant. Once again, a model that takes multivariate effects into account leads to substantively different conclusions.

We also note that some of the distinctions Fischer stresses in his comment are based on a very small number of cases, a fact that is largely hidden because he reports percentages without reporting the corresponding numbers of respondents. For example, the 15-fold increase in the postgraduate respondents who gave no names in Fischer's Table 5 is the result of a difference in 22 cases between 1985 and 2004, out of the 2,967 respondents who answered the question in those two years.¹⁰

We end our discussion of Fischer's analysis of marital status and confidants with two more subtle points. First, while the average number of confidants has decreased, even taking into account the inflated zeros, the relationships among variables have remained remarkably consistent. A careful examination of Fischer's Table 7 shows that among married respondents who named at least one confidant, the proportion who do *not* list a spouse is relatively stable

between 1985 and 2004 (27.8 and 25.3 percent, respectively). This pattern strongly suggests (as we demonstrate above in the zero-inflated Poisson model) that the inflation of zeros that we mention in our original abstract is the primary measurement issue in the data; the rest of the data structure remains similar. We make a similar point in our discussion of Tables 3 and 4 in our original article.

In an attempt to call all the 2004 data into question, Fischer (2009:664) argues that married respondents are "a category of people who were living with a confidant." The reader should note that all four of the authors here (including Fischer) are members of academic couples who work in the same department as their spouses. It is difficult for people like us to imagine not reporting a spouse as someone with whom we "discuss important matters." But notice that in Fischer's own Table 7, roughly a fourth of all married people who gave an answer other than zero in response to the Numgiven question did not name a spouse. Furthermore, 198 respondents in 1985 and 100 respondents in 2004 named their spouse only *after* first naming someone else as a confidant. Their spouse was not the first person who came to mind. One of the valuable things about representative survey data is that they help us transcend our egocentric view of the social structure created by strong homophily in social relations (McPherson, Smith-Lovin, and Cook 2001).

SIMULATIONS AND IMPUTATIONS

Rather than accept the 2004 data as evidence for change in network size, Fischer tries two more techniques to argue that his best estimate is that no change occurred in confidant networks between 1985 and 2004. First, he conducts simulations that randomly assign 20 percent of the 1985 cases to zero. Our zero-inflated Poisson analyses show that the inflated zeros are clearly not random ($p < .00001$).

Fischer's second method of imputing data to explore the change in network size is more interesting (although reported only briefly in his footnote 16). He uses demographic variables and a set of questions about sociability (getting together with relatives, friends, and neighbors or going to a bar) to predict network size in 1985. He then substitutes the 2004 mean values on those variables into the prediction

¹⁰ One graduate-educated person answered zero in 1985, and 23 did so in 2004 (weighted analysis). Two of those 23 respondents in 2004 were rated "restless and impatient" by their interviewers.

equation to get an estimate of network size in 2004. The predicted mean for 2004 Numgiven using his imputation procedure is 3.2, which is actually higher than the observed 1985 mean of 3.06. Fischer's imputation looks plausible until one realizes that virtually all of the variance explained in his imputation equation comes from the demographic variables. Adding the sociability variables to the demographic variables in the 1985 equation only increases the explained variance by .01. Fischer's imputation equation essentially assumes no change in network size, other than that which can be explained by demographic shifts. His finding of no change in the imputations is therefore not surprising.

OTHER EVIDENCE ON NETWORK TRENDS AND SOCIAL CHANGE

Fischer ends his comment with the suggestion that our 2006 results are implausible because they are inconsistent with other studies and cannot be explained sociologically with other major social changes during the same time period. We address these claims very briefly.

EVIDENCE FROM OTHER DATA

The major work positing a trend in social connectedness is, of course, Putnam's *Bowling Alone* (2000). In reviewing that book, Fischer (2005:158) asked, "Have many forms of sociality declined since about 1970?" He concludes that "given the wealth of data in *Bowling Alone*, the burden of proof is on the critics [who claim there is no decline]." Putnam (2000) analyzed political, civic, and religious participation, as well as informal social connections both in and out of the workplace. He argues for downward movement in all of them. Several of his figures show the steepest declines after 1985: the league bowling of the title (p. 112), daily informal socializing activity (p. 108), going to friends' homes (p. 99), active organizational involvement (p. 60), and attending a public meeting on town or school affairs (p. 43).

OTHER SOCIAL CHANGE, 1985 TO 2004

Fischer (2009:659) argues that the social change our models describe is implausible because "no social factors that might even plausibly cause such isolation . . . changed to any comparable

degree in the same period." First, we remind the reader that our GSS variable measures only the closest of social ties. A subtle shift in the social structure toward a more extensive set of weaker ties could lead to a decline in closest confidants. Our people who report zero confidants are not totally isolated; they just lack these very strong ties. We briefly note below several social changes that occurred during this period that might have led to such a restructuring.

Since 1985, the Internet has come into vogue and been adopted (to some extent) by roughly two thirds of the U.S. population (Pew Research Center 2009). There is little reason to suppose that individual usage has strong effects on socializing (Robinson and Martin 2009), but one can imagine macro-level shifts in communication patterns as a result of such a sweeping technological change. Weaker ties might be fostered and maintained at a higher rate while strong ties are diffused, a pattern that Mayhew and Levinger (1976) suggested would occur with increasing system size. In a sense, the inexpensive ease with which we can now contact others without regard to physical distance has expanded the size of our personal social systems, but possibly at the cost of intimacy.

Evidence of other major social changes from 1985 to 2004 can be found in Fischer and Hout's *Century of Difference* (2006). They document growing inequality during this period, especially based on educational differences (Figure 6.4, p. 146). Family work hours rose as a result of women's employment (Figure 5.13, p. 125); college graduates are working longer hours now than in the mid-1980s (Figure 5.12, p. 123). The overall diversity of our society by race, religion, ethnicity, and nativity has increased. People are more likely to live alone (Figure 4.10, p. 84), with the change in the past two decades especially notable among the middle-aged. We would not specifically argue for the causal impact of any one of these factors. We do think, however, that many important features of social life that are not well documented in the GSS changed between 1985 and 2004.

PUBLIC SOCIOLOGY AND PUBLIC DATA

We have argued in this reply that parameter estimates (including percentages) that fail to model data appropriately will produce mis-

leading results. Public sociology is particularly susceptible to this pitfall. In hindsight, it was not a good strategy to emphasize the raw mean number of confidants and the marginal proportions of social isolates in our original article. The 1985 to 2004 differences estimated by our models in Table 5 are much more meaningful numbers, although not as vivid.

Our 2006 article received a great deal of press attention when it was first published. It received more than 12,000 hits on Technorati (evidence of significant discussion on the Web) and the second author did hundreds of interviews with print and broadcast media. The media buzz seems to have focused the public's (and Fischer's) attention on the marginals rather than the model.

We compounded the issue by accepting an invitation from the editors of *Contexts* to furnish an abridged version of our findings for that journal, aimed at a larger audience. In that version, we presented several charts that emphasized easy-to-understand marginals (McPherson, Smith-Lovin, and Brashears 2008b). While the last two figures presented our model-adjusted estimates, our text described descriptive statistics that could have led readers to an exaggerated conclusion. We would like to alert others to the perils of trying to (over)simplify complex phenomena; public sociology has both pitfalls and promise when isolated phrases from research articles in the *ASR* may appear on the front page of *USA Today*.

While the public attention to our results has given rise to the current controversy, the public nature of our data represents a clear advance in the scientific enterprise. By the time our paper was under review at the *American Sociological Review*, the data were already publicly available. Any reviewer or reader could download the data for free on the National Opinion Research Center's GSS Web site (<http://www.norc.org/GSS+Website/>) or do quick and easy analyses at the Survey Documentation & Analysis (SDA) Web site at Fischer's own institution (<http://sda.berkeley.edu/archive.htm>). Given the surprising nature of our findings, many researchers did analyze aspects of the data and began a conversation with us about the findings. Scholars can now debate evidence in real time while manuscripts are actually under review. We hope that the support of these large infrastructure data sources continues, because

it fosters both the continuity of design that allows us to observe social trends and the open use of data to argue about evidence for those trends.

CONCLUSIONS

Fischer (2009:668) concludes his comment by saying that "the best estimate is that the 'true' percentage of 2004 respondents who were isolated was roughly the same—or perhaps less—than the percentage in 1985/1987, somewhere under 10 percent." We categorically disagree that the data show no change. Neither we nor Fischer have been able to destroy the 1985 to 2004 difference without assuming it away. Even accounting for the inflated number of zeros in 2004, there is a major decline in Numgiven in the data. If the 1985 to 2004 difference is illusory, it is due to the effect of variables that we have not been able to discover in those data. We are working on a survey experiment in the GSS to study the effects of fatigue and context on the network item in 2010. We expect that the next round of data on Numgiven will offer some new answers, and some new puzzles.

Miller McPherson is Professor of Sociology at Duke University and Professor Emeritus at the University of Arizona. His evolutionary model of affiliation, originally published in this journal, has been extended to the study of a broad range of social and cultural phenomena, including occupations, musical genres, and churches. Current projects include a test of his ecological theory with nationally representative data from the *Niches and Networks* project, funded by the Human and Social Dynamics Initiative at the National Science Foundation.

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