

# CONNECTIONS

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All Features Issue

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# CONNECTIONS

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CONTRIBUTIONS are encouraged from members and colleagues: research papers of any length, reviews of applications of networks in different fields, comments and critiques, survey articles, computer programmes, conference information, abstracts, teaching aids, etc.

# NETWORK NOTEBOOK

## IN THIS ISSUE

This issue brings both more--and less--of CONNECTIONS than you've ever seen before. The more is articles: Clyde Mitchell's keynote address to the 1986 Sunbelt [hah!--it poured all the time], one of Phil Abrams' last papers [on neighbourhoods, networks & social support], a feature on the many lives of Anatol Rapoport, a nice teaching aid courtesy of Ron Rice & Ev Rogers, and a cornucopia of computer features from Ron Burt (x2), Lin Freeman, Al Klovdahl, & Sheila Weaver/Stan Wasserman. Plus the inside scope on the AMERICAN SOCIOLOGICAL REVIEW vs. structuralist Marxists, and our AMAZING T-shirt offer. Plus, the 1st ever announcement of INSNA's very own awards: the GEORG-IEs.

What you don't get is abstracts or book announcements. But wait 'til next issue when you will be swamped [we hope]. Why? Read 'CONNECTIONS Gets Computerized' below.

## CONNECTIONS GETS COMPUTERIZED

Thanks to Siegfried Schulte in Toronto & Peter&Trudy Johnson/Lenz in Oregon, INSNA's membership list has been computerized from our 1977 beginnings. Last year, the loss of APL when we switched mainframes (yes, you matrix crunchers, there are folks who don't like that magic language) pushed our membership files--and us--onto an IBM PC/AT (nothing but the best for INSNA; altho we're anxiously watching for our hard disk to crash). Now that we've gotten membership files running smoothly (the big problem was handling changes of address), we're ready to use the power of PCs in creative ways.

The next thing we did is to use our membership files in conjunction with our database program [CORNERSTONE] to help the organizers of the next Sunbelt Conference--Russ Bernard & Al Wolf--locate likely session organizers. [Note: This service is also available right now to help session organizers find likely paper presenters.]

Hopefully, you're reading the results of our next adventure in computerland: moving from typing each issue with our Selectric to word processing it (using Word Perfect). Right now it's a fun & frustrating toy, but we'll try not to go too crazy with fonts & stuff. And starting now, we can accept 'papers' on floppy disks written with either Word Perfect (preferably) or Word Star. (Nothing else, please.) While wordprocessing works well for things such as Network Notebook and long articles, it does not seem to us to be the most powerful way to handle abstracts & book stuff.

So we're going to spend the summer gearing up for computerized handling of abstracts, etc. Our current plan is to use Notebook II for this (& thanks to Nancy Chapman, Portland St. for her expert advice). The two big advantages of going this way is that we will eventually be able to compile custom specialized bibliographies, and we may be able to automate totally the acquisition of abstracts (from BRS to you untouched by human hands). Current plans call for all abstracts to be keyworded, so that searches can be done on standardized terms as well as on free text.

Our Associate Editor, Walter Carroll, has compiled a huge list of abstracts, and if all goes well, the next issue should dump the lot on your lap--as well as on our disk.

The power of computing is clearly in synergy--getting things going so that the sum is more than the parts. If you have any ideas of how we can use these nifty databases, please give us a shout.

## HELP INSNA!!--FIND NEW MEMBERS QUICKLY

*We now have 199 members--about 50 less than this time last year. We've been drifting downwards for several years. We need new members if we are to survive. Please send the enclosed New Members form to the most likely person you know. Some of the most likely are not members. A free T-shirt to the most successful network recruiter, and another free Tee to the sociometric star who gets the most solicitations. And new student memberships are only \$8 during this drive!*

### WHAT'S OUR PITCH?

As we switch to computerization, it seems a good time to consider the look of CONNECTIONS. Since the beginning, we've been typing in 12 pitch & then reducing that 25%--effectively 16 pitch. This has the virtue of low cost--it reduces both printing & postage rates. But as the Editor passes 40, he begins to worry more about readability. This section is printed in a larger 10 pitch & then reduced 25%. Should we go to a larger pitch? A different font? Should different sections be different sizes? Feedback please?

### E-MAIL ANYONE?

Speaking of computerization, INSNA has been getting lots of propaganda from SOcNET, an electronic mail/computerized conferencing service aimed at soc scientists. Frank Howell, the Director, suggests we put a version of CONNECTIONS online, chat informally in dyads & clusters, and exchange datafiles (including SPSS(X & PC) & paper drafts. SOcNET is available internationally via Tymnet or Uninet, and it also provides (extra-cost) connections to the DELPHI public info utility & the DIALOG bibliographic database (Soc Abstracts, ERIC, etc).

Costs look like US\$30 to join, plus US connect charges of \$10-\$19/hr (plus an additional \$9/hr for non-US users). We may be able to get a better deal by joining as an organization.

I'm ambivalent about this. Many of us had fun when the (US) NSF gave out free \$s & terminals some years ago for networkers to communicate via EIES. On the other hand, many potential participants didn't think it was worth their time. And it's easy enough to sign on to DIALOG directly.

Some key Qs:

1. Do we have enough to say to each other?
2. Is SOcNET the way to say it, given many folks already have (possibly free) e-mail access via BITNET, NETNORTH, EARN, etc?

Hence this is an open call for those interested in this to follow it up, thru INSNA. Give me/Wellman a call & go ahead. Info from Frank Howell, Director, SocNet, Soc Sci Res Ctr, Box 6303, Mississippi St U, Mississippi State, Miss. 39762.

### NEW NODES AT INSNA HQ

It's Spring, and that means new faces at INSNA. Alicia VanDerMeer has left to seek her fortune with her INSNA-learned database skills. Eric Weissman, a Toronto sociology grad. student, has signed on as principal Associate Editor on the editorial side. He'll have principal responsibility for 2 key projects: computerizing the abstracts & designing our nifty new T-shirts. Cyndi Rottenberg, a Toronto urban studies undergrad., takes Alicia's place as keeper of the database & searcher of abstract files. Sharon Bolt is still going strong as Associate Editor in charge of administration (including your subscription hassles), Susan Dentelbeck copes with getting orders in & copies out, while Jack Richardson still drops by to offer some editorial advice.

### GET YOUR OWN INSNA T-SHIRT

No network is complete these days without its own T-shirt. Great for jogging, beach wraps at the Sunbelt, blockmodeling in California or Cardiff.

So now there's the AMAZING INSNA T-SHIRT, complete with its own network. Designed by our new Assoc. Ed, Eric Weissman, who's working his way thru grad. school selling sweats & Tees. The shirts are white, with a blue INSNA logo on a multicoloured logo. They proclaim, "GET CONNECTED!"--just the thing for making friends. They come in all sizes in a high-quality 50/50 polycotton cloth (no schmates here).

Eric gives us wholesale rates--no one makes a profit but INSNA (a few bucks into the reserve fund). The price is supercheap--US\$12 (US\$14 outside of North America). Take a look at the stunning design on the last page & then order yours today!

## SOCIAL NETWORKS

Volume 8, #1 of SN is now out. Abstracts will appear in the next issue of CONNECTIONS. Contents:

Ole-Jorgen Skog, "The long waves of alcohol consumption: a social network perspective on cultural change"

Patrick Doreian, "On the evolution of group & network structure II: structures within structure"

Jeffrey Johnson & Marc Miller, "Behavioral & cognitive data: a note on the multiplexity of network subgroups"

Donald Hildum, "'Competence' & 'performance' in network structure"

Karen Campbell, Peter Marsden & Jeanne Hurlburt, "Social resources & socioeconomic status"

And should any authors wish to contact SOCIAL NETWORKS concerning editorial production matters (copyediting, reprints, galleys, etc.) their new editorial office is: Elsevier Science Publishers BV (North-Holland), P.O. Box 442, Brighton BN2 3LX, England. Tel: 0273-684736.

## INFO FLOWS

JOHN PADGETT promoted to Assoc Prof of Poli Sci at Chicago...WAYNE BAKER now postdoctoral fellow at Harvard Business School...LEE SAILER to Bus, Penn St...HARRISON WHITE moving to Arizona to be Chair of Soc...LISA BERKMAN a Fulbright Fellow in England, 1985-1986...CLAUDE FISCHER a Fulbright Fellow at Geog, Hebrew U, Jerusalem thru 6/86; from 9/86 he's at Ctr for Advanced Study, 202 Junipero Serra Blvd, Stanford CA 94305...MARK GRANOVETTER on year's leave (86-87) at Social Relations, Johns Hopkins...SARAH MATTHEWS has returned to Case Western from her sabbatical at Toronto...HARRIET FRIEDMANN has been invited to join the Editorial Advisory Board of the JOURNAL OF PEASANT STUDIES...LESLIE HOWARD featured in the TORONTO STAR [29 March 1986] as being a companionable California companion...Assoc. Ed. JACK RICHARDSON's house is being used as the interior set in the new CBS-TV movie, "Mother Courage" (starring Sophia Loren--not Berthold Brecht)...ANTHONY OBERSCHALL a Fulbright Fellow in China, 1985-1986...ANATOL RAPOPORT awarded honorary doctorate from the University of Toronto...MICHAEL USEEM's THE INNER CIRCLE won the 1985 C. Wright Mills Award for the year's best book on social issues...JODI KLIMAN writes that she & DAVID TODD are celebrating their 3rd anniversary--"it was ortho, networks and Toronto that brought us together"--they met at the Amer. Orthopsych. meetings [once again, CONNECTIONS pays off]...JUNE CORMAN (Soc, Carleton; former INSNA Assoc Ed.) has given birth to Daniel...MARK MIZRUCHI (Alb. Einstein C of Med) now is a father...PETER NEW (Beh Sci, S. Fla) died of a heart attack, 30 Dec 85.

## GEORG-IES ANYONE?

While all network papers & books are interesting, some are more wonderful than others. So now that we have official T-shirts, we're thinking of using them as prizes for our "GEORG-IES" (for Simmel, of course).

Some possible categories:

Best paper/book.

Most papers published (two categories: tenured & untenured).

Most slides/transparencies flashed at the Sunbelt conference.

Most unreadable methodological article.

Best use of the Sampson data set.

Most interesting paper/book in the field of social support/corporate interlocks/world systems/markets/community--you name it!

Most startling career change.

Biggest grant.

Most personal computer toys acquired from research grants.

Most research done without any grant support.

Perhaps you are thinking of likely winners already. And some more awards categories.

PLEASE SEND IN

YOUR NOMINATIONS BY JULY 31, 1986. Awards will be presented at the next Sunbelt.

## PUBLISHING OPPORTUNITIES

**SOCIOLOGICAL FORUM**, the new journal of the (US) Eastern Soc Sty, is planning a special issue on **markets & organizations**, edited by Mitchell Abolafia & Michael Hannan. "Submissions of empirical studies using a sociological perspective to examine financial markets, labour markets, & consumer & industrial markets are actively encouraged"--by 1 July 1986. Contact Robin Williams, Jr, Ed., Soc, Cornell.

**SIGNS** is having a special issue on "**Communities of Women: Perspectives from Europe & Byzantium before 1500**". They're specially interested in: how women's communities differed from society at large or from other communal orgs; the ramifications of women's communities for women within & without such orgs; the extent to which women's communities gave women power in medieval societies. Send prospectuses now (&35p. ms by 30 Sept 86) to **SIGNS**, 207 E Duke Bldg, Duke U, Durham NC 27708.

## AND WHAT DO THEY THINK CONNECTIONS IS?

"Dear Professor Wellman: Thanks for your interest in P.E.N. [a world assoc of writers]. The membership committee has concluded that you would not be eligible. If we are in error & you have published or edited works of literary value, please do advise us. Bonnie Mason, Canadian Administrative Secretary."

## 'BIG FLOYD', NETWORK ANALYSIS AND [MAYBE] YOU

The US Federal Bureau of investigation has just implemented a crime database system, using artificial intelligence. One of its major features is "a series of graphic displays that illustrate the reported connections between a suspected labor official & his banks, business activities, union connections & organized crime contacts." [NEW YORK TIMES, 4 April 1986]

## GRANT GETTING

**SOCIAL SCIENCE & HUMANITIES RESEARCH COUNCIL OF CANADA:** DAVID CHEAL (Soc, Winnipeg), "Moral Economy of Family Life," using survey data to study intrafamilial redistribution of incomes, goods & services...BEN GOTTLIEB (Psych, Guelph), "A prospective study of intergenerational caregiving relationships"...BARRY WELLMAN (Soc, Toronto) "Integrating the analysis of network structures, dyadic ties & personal attributes: implications for personal communities & social support" (with OVE FRANK & CHARLES JONES). [Also support from National Welfare Research Grants for "Delivering social support thru social networks"].

**BARRETT LEE & KAREN CAMPBELL** (Soc, Vanderbilt) have an Amer. Soc. Assoc 'problems of the discipline' grant for a small conference on "a meta-analysis of the community attachment literature".

(US) **NATIONAL INSTITUTE ON AGING:** Colleen Johnson (Cal-San Francisco), "Marital instability & family ties in later life," \$106,459...Beth Soldo (Georgetown), "Health, in networks & living arrangements of the elderly," \$129,823...Toni Antonucci (ISR, Michigan), "Social support over the life course," \$52,371...Sarah Matthews (Case Western), "Strength of tie with hi school classmates in old age," \$34,106...James House (ISR, Michigan), "Productivity, stress & health in middle & late life," \$1,179,378...William McIntosh (Texas A&M), "Social support, stress, the aged's diet & nutrition," \$269,899.

(US) **NATIONAL SCIENCE FOUNDATION:** Noah Friedkin (Ed, Cal-Santa Barbara), "Collaborative research on an experimental test of a formal theory of social control," \$37,225...William Gamson (Soc, Boston C), "Public thinking on political issues," \$47,190...Michael Hannan (Cornell), "Selection & competition in the life cycles of orgs.," \$196,674...Ivan Szelenyi (Soc, Wisconsin), "Private economic activity in Hungarian society & the transformation of social structure: the case of agricultural production," \$54,997...Karen Cook (Soc, Washington), "Collaborative research on an experimental test of a formal theory of social control," \$13,764...David Knoke (Soc, Minnesota-St. Paul), "Resource acquisition & allocation in associations," \$56,588...Ivan Light (Soc, UCLA), "Labor force & self-employment, 1970-1980," \$50,400...Howard Aldrich (North Carolina), "Collaborative research on the creation & persistence of business interest associations," \$4,099...Judith Blau (Soc, SUNY-Albany) & Peter Blau (Soc, Columbia), "Collaborative research on metro. social structures & cultural activities", \$50,000...Ronald Burt (Soc, Columbia), "Intercorporate relationships & market constraints," \$62,401...Joseph Galaskiewicz (Soc, Minnesota), "Nonprofit

response to shifting resource markets," \$5,859...Charles Kadushin (Soc, CUNY Grad Ctr), "Microcomputers & social networks", \$30,000.

(US) NATIONAL INSTITUTES OF MENTAL HEALTH: Ronald Kessler (Soc, Michigan), "Patterns of differential response to stress," \$63,669...Nan Lin (Soc, SUNY-Albany), "A 3-wave study of stressors, social support & illness," \$149,707...Donna Eder (Indiana), "Early adolescent interpersonal relations," \$23,708...James Rule (Ctr for Policy Res), "The new uses of information: impacts on organizations," \$195,000...Leonard Pearlin (Cal-San Francisco), "Sources & mediators of emotional disorders," \$112,833...Ronald Kessler (Soc, Michigan), "Stress models for hi risk populations," \$53,136...Susan Frank (Illinois Inst of Tech), "Aging parents, young adult children & mental health," \$145,580...William Vega (San Diego State), "Hispanic social network prevention intervention study," \$423,872...Robert Weiss (Massachusetts-Boston), "Work setting & the processes of relational support," \$52,910 & "Work & relational buffers against stress symptoms," \$55,137.

#### NETWORKERS ANALYZED

SAM GILMORE (Soc Sci, Cal-Irvine) has started to do research on the organization of the social network analysis community. In addition to interviewing key informants (make sure he buys you a beer), he's also thinking of doing a survey.

#### STRUCTURALISM IN DANCE

"'Stella' shows a firm understanding of how form can generate content & how movement can be structured to achieve that form." [from Anna Kisselgoff's NY TIMES review of Jean-Pierre Perrault's Montreal modern dance piece, 30 Sept 85].

#### AIDS RESEARCH GOES NETWORK

"All 4 Harvard AIDS researchers have abandoned much of the territoriality & competitiveness common among senior scientists. [They are] sharing their latest findings & insights, working together in an unusual collaboration. It is a symbiotic alliance. Yet the group's interaction has no official status. It is not a formal team; rather, each professor heads a laboratory & directs a team of his own. The best term for their relationship is a 'network'; they talk to 1 another almost daily, think in concert & turn to each other for advice & help by 2s & 3s write many scientific papers." [Edited from Morton Hunt, "Teaming up against AIDS," NY TIMES MAGAZINE 2 March 1986].

#### WHAT ARE THE IMPLICATIONS FOR INSNA?

INSNA recently received a letter addressed to the Editor of ERECTIONS, inviting us to attend the 2d World Meeting on Impotence, 16 June 1986, in Prague.

#### NOSTALGIA NOTE

DATATEXT was removed from the U of Toronto computer system 3/86. "It was a statistical package whose very low usage did not justify continuing to pay the annual license fee." R.I.P.--& thank you for helping many of us 40+ers get our early research done.

#### 'INTIMATE' IS NOW OFFICIAL

"Mr. Deaver, former deputy White House chief of staff, is an intimate of Mr. Reagan & his wife, Nancy." [NY TIMES, 6 May 1986].

#### A NETWORK MITZVAH

Many of us know ruefully that the annual royalties to the chapter authors of an edited book are exceedingly small. Last year, Peter Marsden & Nan Lin asked the contributors to their SOCIAL STRUCTURE & NETWORK ANALYSIS to contribute their royalties to INSNA. The roll of honour is: Ronald Breiger, Gwen Moore, Richard Alba, Barry Wellman, Steve Rytina, Mark Granovetter, Nan Lin, Charles Kadushin, Bonnie Erickson, Karen Cook, Peter Marsden, Ronald Burt, Joseph Galaskiewicz, David Knoke, Edward Laumann & Howard Aldrich. Bless you!

The amount came to US\$416.34 and will go towards our computerized database project (see "Connections Gets Computerized" above).

This is the 1st sizeable donation to INSNA. Up until now, we haven't bothered registering as a charity in Canada, the US, or elsewhere. Mostly folks--especially BW & EW now--contribute services, & they ain't deductible (alas!). Perhaps registration would do some good. Comments?

#### PERHAPS THE DONATIONS SHOULD GO TO OUR SUPERCOMPUTER FUND?

"In social science research, the supercomputer is only beginning to be used, but the potential impact is substantial. [For] example in sociology, modelling the interaction of individuals requires that the implications of learning & choice behaviour be worked thru for large groups of people connected by common or different social networks." [From a successful University of Toronto proposal to buy a CRAY X-MP/24, 1/86].

But be sure & see the specs in this issue for new microcomputer network analysis programs.

#### INTERPERSONAL SOCIETY FOR THE STUDY OF PERSONAL RELATIONSHIPS

This new outfit is composed (mostly) of psychologists whose work is quite congenial to network analysis--if only they'd get beyond the dyad! It's going to have meetings (this year in Tel Aviv), publish a newsletter (quite nice--INSNA member Robert Milardo [Human Dev, Maine] edits it), etc. The co-chairs are Steve Duck (Communic, Iowa) & Robin Gilmour (Psych, Lancaster). INSNA Coordinator Barry Wellman is on the Advisory Council. Membership info from Mary Anne Fitzpatrick, Communic, Wisconsin.

#### SUPPORT SYSTEM TERM OF THE YEAR

"sharing impaired" contributed by Jane Wagner & Lily Tomlin, "The Search for Signs of Intelligent Life in the Universe" [Broadway, 1985/86].

Runner-up: "a linkless node": definition of a lonely person according to Vikram Seth's new verse novel, THE GOLDEN GATE (NY: Random House, 1986).

#### CATS, NETS AND PHYSICS

"How have physicists made sense of the panoply of particles described in the standard model? First, the particles can be divided into 2 fundamental classes: fermions & bosons. Leptons [e.g., electrons] & quarks, the basic constituents of matter, are fermions. The basic particles that mediate the 4 forces are bosons [e.g., photons]...Fermions are 'anti-social' & tend to occupy different energy states; bosons are 'gregarious' & tend to clump together in the same energy states." [from Howard Haber & Gordon Kane, "Is nature supersymmetric," SCIENTIFIC AMERICAN, 6/86: 54].

Could a fundamental question of the universe be: which came 1st, the quark or the network?

#### WORST INADVERTANT ACADEMIC PUN OF THE YEAR

"So what happened to the sexual revolution?

"I think it's bottomed out," says Robert Sherwin (Soc, Miami U of Ohio). [AP news-wire as reported in TORONTO STAR, 13 Aug 85].

#### FREE NETWORK BOOK

The US government is giving away Joseph Morrissey, Mark Tausig & Michael Lindsey, NETWORK ANALYSIS METHODS FOR MENTAL HEALTH SERVICE SYSTEM RESEARCH: A COMPARISON OF 2 COMMUNITY SUPPORT SYSTEMS. It attempts to integrate network methods & concepts with an interorganization framework for understanding the structure of multiagency mental health programs in 2 communities. Write Public Inquiries Office, NIMH, Parklawn Bldg, Room 15C-03, 5600 Fishers Lane, Rockville MD 20857. [DHHS Pub # ADM-85-1383].



### THE ULTIMATE INTERLOCK THEORY?

High school principal James Keegstra, accused of willfully promoting hatred "denied that he had told students a [Jewish] conspiracy was out to control the world. He said that he had never told them that this conspiracy encompassed all Jews; it merely included a small percentage including powerful international financiers & revolutionary intellectuals...The aim was to create 1 world govt & a new order run by an elite group of Jewish financiers, with Israel as the centre of power." [TORONTO GLOBE & MAIL, 24 June 1985].

### THE IRON LAW OF NETWORK ANALYSIS?

Exiled Soviet dissident Alexander Zinoviev [former prof of math & philo, Moscow], claims that Chairman Gorbachev will be unable to implement any significant reforms because of "the objective laws" of Communist society. "The Communist system, he argues, is an immense, complex network of independent institutions linked by 'subordinate' & 'coordinate' relations. To move even 1 point in this network (i.e., to increase the output of even 1 factory or collective) requires moving all the points--a task that 'even God' could not accomplish." [TORONTO GLOBE & MAIL, 8 March 1986].

### SUPPORT AVAILABLE

Columbia U Schl of Public Health [Eugene Litwak, Dean] has post- & predoctoral socio-medical sciences fellowships in Social Stress & Mental Health. Courses in basic soc sci & medical soc scis, combo'd with a research placement lead to a PhD or a postdoctoral MPH. Postdocs must have MD, PhD, etc; predocs must have BA or MA in the health areas or soc scis. Info: Div of Sociomedical Sciences, Columbia U Schl of Pub Health, 600 W 168 St, New York NY 10032. Tel: 212-305-5656.

UCLA has postdocs in research on the delivery & evaluation of mental health services. \$16-30K. Info, Oscar Grusky, Soc, UCLA, Los Angeles CA 90024.

U of Chicago has 2 postdocs on the delivery & evaluation of mental health/services. Info, Donald Fiske, Beh Sci, U of Chicago, 5848 S University Ave, Chicago IL 60637. Tel: 312-962-8841.

Cornell Dept of Human Development has 2 postdocs in developmental psychology & family sociology.

(US) National Academy of Education gives out 5 Spenser Fellowships annually for-research on the improvement of education in all of its forms. For those who got their docs within past 5 years; \$10K x 3 yrs. Info, Gail Keeley, N.A.E., Harvard Grad Schl of Ed, 108 Longfellow Hall, Cambridge MA 02138. Tel: 617-495-9701.

Stanford has postdocs in a Res Training Prog on Orgs & Aging, including the effects of aging on orgs, the effects of orgs on aging, & the org of services for the aged. \$16-30K. Info, Richard Scott, Soc, Stanford U, Stanford CA 94305.

(US) National Research Council has \$15K awards available for studies on wage determination and inequities. Info, Heidi Hartmann, Panel of Pay Equity Res, Room JH852, Nat'l Res Council, Nat'l Academy of Scis, 201 Constitution Ave NW, Washington DC 20418. Tel: 202-334-3590.

### MAKE YOUR PARENTS HAPPY

Two Amer Soc Assoc sections are awarding annual prizes:

1. Aging--"Student of the Year", based on an unpublished article from dissertation research--Info, Anne Foner, 48-28 196 Pl., Flushing NY 11365.
2. Methodology--Lazarsfeld award, based on nominations--Info, Leo Goodman, Soc, U of Chicago, 1126 E 59 St, Chicago IL 60637.

Gerontological Society of America (Beh & Soc Sci sec)--Student research awards in pre/post dissertation categories. Info, Jon Hendricks, Soc, U of Kentucky, Lexington KY 40536.

Amer Assoc for Pub Opinion Res--student paper competition. Papers may relate to methodological questions in their field of survey or soc sci res, or emphasize substantive findings in a study advancing understanding of public opinion & social behaviour. Info, Lawrence Bobo, Soc, U of Wisconsin, Madison WI 53706.

Pacific Coast Council on Latin American Studies--Hubert Herring Awards for best paper, book, thesis, film, etc. Info, E Bradford Burns, Hist, UCLA, Los Angeles CA 90024.

SOCIAL NETWORK ANALYSIS, INC.

Network Consultants, run by INSNA members Jodie Kliman & David Trimble, offers "network interventions & consultations for families, individuals, groups & psychotherapists. These range from "full-scale assemblies" [meetings of 15-80 relatives, friends, etc. "who gather together for problem-solving & mutual support" to small group sessions & "coaching individuals & families on improving network relations". Info, 1415 Beacon St, Suite 322, Brookline MA 02146 USA. Tel: 617-232-5282.

The Network Inc. of America, run by INSNA member Mary Voell, is "an info. database & referral system corp. designed to make connections". It does info. searching, networking workshops, conference organizing & connections-making. Info, 819 N Marshall St, Milwaukee WI 53202. Tel: 414-289-7774.

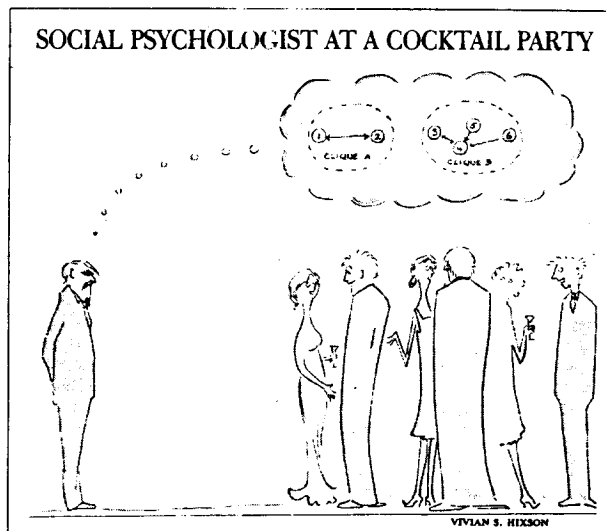
Interaction Network runs "Human Interaction Programs" on a Caribbean island. Their ad shows a gorgeous couple (SWM/SWF) embracing in the surf. It asks, "What better environment could you imagine within which to live 10 days of pleasure; developing yourself & connecting with others. Indulge yourself in the joy of true human contact. Trust & comfort with another are natural, & so are the freedom & intimacy they inspire. Utilizes activities such as focused discussions, role plays, experiential lectures, visualization, fitness activities, music & games." Course fee C\$450. POB 558, Station J, Toronto Canada M4J 4Z2. Tel: 416-429-2468.

Could this be a new approach for the Sunbelt conference?

SUPPORT SYSTEMS WIN JUDICIAL APPROVAL

Ontario Judge Robert Reid overturned welfare officials' practices of cutting off mother's allowance benefits because the woman is living with a man. "A single mother shackled with the responsibility of raising 2 children on meager govt assistance no doubt lives under very trying circumstances," Judge Reid said. "It would be a strong woman indeed who could manage this without daily emotional support from a close friend. To deprive her of her only source of income because her close friend happens to be a man is very harsh." There are many relationships between men & women which fall short of financial provider & recipient, he said, including "simple friendship". [TORONTO GLOBE & MAIL, 22 June 1985]

CONTINUED ON PAGE 13



CHRONICLE OF HIGHER EDUCATION 19 May 1986  
Courtesy of Stanley Wasserman

# MEETING CALENDAR

## MONSOONBELT NETWORK CONFERENCE, NEW DELHI, August, 18-22, 1986

The 1st ever Monsoonbelt conference--held conjointly with the 11th World Congress of Sociology (International Sociology Association)--will have three formal sessions, one business meeting, and lots of informal interaction.

### Interorganisational Relations (John Scott, Soc, Leicester, Chair)

John Scott, "Network analyses of shareholdings: a comparison of Britain, the USA and Japan"

Yoshiaka Ueda (Japan Securities Research Inst, Osaka), "Intercorporate networks in Japan: a study of interlocking directorates in modern corporations"

Hiroshi Okumura (Econ, Ryukoku U, Kyoto), title tba

Meindert Fennema (IWP, U of Amsterdam), "Kinship relations among finance capitalists in the Netherlands"

Michael Savage (Soc, Cape Town), "Interlocking directorships in South Africa"

### Interpersonal Relations (Barry Wellman, Soc, Toronto, Chair)

Wesley Shrum (Soc, Louisiana St), "Inter-racial relations in adolescence: a network analysis of friendship & interaction patterns during the school years"

Philip Bonacich (Soc, UCLA), "Power & centrality: a family of measures"

A. Ramachandra Rao & Suraj Bandyopadhyay (Soc Res, Indian Statistical Unit, Calcutta), "Measures of reciprocity in a social network: a study of some villages in India"

Leslie Howard (Soc, Whittier C, Whittier CA, USA), "Industrialization and community in an Indian context"

William Stevenson & Donald Wilson (Mngmt, Cal-Irvine), "The strength of ties & social distance in intraorganizational networks"

Albert Hunter (Soc, Northwestern), "Friends & associates: a blockmodel analysis of informal & formal status structures of community elites"

Community Networks (Organized by Joseph Galaskiewicz [Soc, Minnesota] & Barry Wellman for the Community Research Section of the International Sociological Association)

M. Luisa Tarres Barraz (Soc, Colegio de Mexico, Mexico City), "Middle classes' community networks & political opposition in Mexico City"

Max Heirich (Soc, Michigan), "Network processes in the restructuring of health care policies: the emergence of a wellness model for health care delivery in the USA"

Alexis Ferrand (Soc, Grenoble), "Maintained friendships & new acquaintances: a typological analysis of middle-class relational stars"

Barry Wellman & Paula Goldman (Soc, Toronto), "Getting social support through personal community networks"

## SUNBELT SOCIAL NETWORK CONFERENCE, 12-15 Feb 1987, Miami Beach

The next fun-filled Sunbelt conference will have (ad)vice for all. It will be in fabulous (North) Miami Beach, February 1987. {Perhaps we will find out the true nature of network vice.}

Conference organizers are Russ Bernard (Anthro, Florida; tel: 904-392-2031) & Al Wolfe (Anthro, South Florida; tel: 813-974-2345). Please call them immediately if you want to organize a session or give a paper (they will direct papers to likely sessions). [Given the locale, does anyone want to organize a session on "My son/daughter, the network analyst?"]

Among the features of this Sunbelt: Stan Wasserman will give a six-hour workshop on the practical conduct of network analysis.

AMERICAN SOCIOLOGICAL ASSOCIATION, 1-4 Sept 1986, Hilton Hotel, New York City

We don't have the complete schedule, but INSNA will hold an informal get-together Sunday, Aug 31, 8:30-10:30 PM, at Gramercy Room A. One other item we know about is a session on "The Community Question Re-evaluated," Thursday, Sept 4 with Barry Wellman, Al Hunter, Charles Kadushin, Melvin Oliver & Lisa Peattie.

GERONTOLOGICAL SOCIETY OF AMERICA, 19-23 Nov 1986, Chicago

For info contact Jenny Youngdahl, GSS, 1411 K St NW, Suite 300, Washington DC 20005; tel: 202-393-1411.

ENVIRONMENTAL DESIGN RESEARCH ASSOCIATION, 29 May-2 June 1987, Ottawa, Canada

Theme: "Public Environments: An International Forum on Environmental Design Research." Focuses on the appropriate design of environments used & owned by the public or designed & managed for public use. Public environments are complex places to plan, design & manage, given that they often need to accommodate multiple & varied users & uses, allow for a mix of public & private behaviour in public spaces, & balance user-owner demands. The sessions will be bilingual in English & French, with simultaneous translations.

Info on sessions from Conference Secretariat, 275 Bay St, Ottawa, Canada K1R 5Z5. Tel: 613-232-8228. Deadline for submissions is 1 Oct 1986.

THIRD INTERNATIONAL CONFERENCE ON PERSONAL RELATIONSHIPS, 6-11 July, 1986, Herzilia (Tel Aviv), Israel

Daily themes are: Growth of relationships; 'Normal' & 'abnormal' relationships; Relationships across the life-cycle; communication in relationships.

Invited speakers include Michael Argyle, Carl Backman, Steven Hobfoll, Ted Huston, Harold Kelley, Eleanor Maccoby & Irwin Sarason.

Reg: US\$140. Room: \$145 for whole conf. Info from Robin Gilmour, Psych, Lancaster, England.

WORLD CONGRESS OF VICTIMOLOGY, 9-13 July 1986, Disney World, FL.

Theme: Victims & the professions: new directions & services. Topics include child abuse & neglect, sexual assault, elderly victims, victim services, domestic violence, witnesses & bystanders.

Held at Hilton Hotel, Lake Buena Vista FL. Info. from World Congress, 2333 N. Vernon St., Arlington VA 22207.

SOCIETY FOR THE SOCIAL STUDY OF SCIENCE, 23-26 Oct 1986, Pittsburgh

Held in conjunction with the Hist. of Sci. Sty., Philosophy of Sci Assoc. & the Sty. for the Hist. of Technology. Info from Ruth Schwartz Cowan, Hist., SUNY, Stony Brook NY 11794.

PAST CONFERENCES

SUNBELT SOCIAL NETWORKS CONFERENCE, 14-16 Feb 1986, Santa Barbara

The 1 thing they forgot was the SUN--it poured the whole conference, with record-breaking rain flooding the California coast & exciting waves pounding our choice, beach front rooms.

Anyway, it gave folks more time to talk with 1 another. About 150 people showed up, with 3 or 4 sessions running simultaneously over 3 days. Biggest concentrations were on methods (Frank Sampson's monastery data seems to have become the standard example set) & social support. But the range ran from monkeys to Ronald Reagan's politics, with Clyde Mitchell giving an elegant keynote (& participating actively in the other sessions) & Kathleen Carley setting a record for most overheads presented in 1 paper. Only the rain, & the US immigration's barring of a leading network analysts (see ANNALS OF AMERICANISM, this issue) dampened our thoughts. Next year, Miami Beach.

WORKERS AND THEIR COMMUNITIES, 9-11 May 1986, Ottawa, Canada

Selected Papers:

- Meg Luxton (Soc Sci, York, Toronto) "Women back into Stelco: challenging the division of labour"  
Peter Usher "What the informal economy is & is not"  
Ted Jackson (E.T. Jackson & Assoc.) "Lessons from the North"  
Scott Clark (Clark & Assoc) "Lessons from marginal farming communities"  
Joy Mannette (Soc, St. Mary's) "The informal economy of black women in Nova Scotia"  
Susan Ilcan (St. Mary's) "Ties of dependency: fishermen & plant workers in a Nova Scotia fishing community"  
Ann Hewitt (Soc, Carleton) "The mutual dependency of structure & agency: escape & stabilities in a state-controlled single-industry town"  
Laura Hollingsworth (Soc, Toronto) "The strike of cleaners at 1st Canadian Place"

CANADIAN SOCIOLOGY & ANTHROPOLOGY ASSOC, 4-7 June 1986, Winnipeg

Selected Papers:

- Joel Novek (Winnipeg) "Grain industry automation: bureaucratic vs craft control"  
Ian Gomme (Memorial) "Informal contacts with formal state agents: the creation of fear"  
Andrew Wister (Waterloo) & Laurel Strain (Beh Sci, Toronto), "Social support & well-being: a comparison of older widows & widowers"  
Linda Muzzin (McMaster) "An empirical study of family physician-specialist consultations"  
Victor Owen (Carleton) "Computer as medium: social networks"  
Marneena Gonick (McGill) "Telecommuting: implications for women service industry workers"

The following thematic sessions are follow-up discussions of 'state-of-the-art' review papers presented in the CANADIAN REVIEW OF SOCIOLOGY & ANTHROPOLOGY 22,5 (12/85):

- R.J. Richardson (McMaster) & Barry Wellman (Toronto), "Structural analysis: the state of the Canadian art"  
Alfred Hunter (McMaster), "Doing it with numbers"  
Margrit Eichler (OISE), "And the work never ends: feminist contributions"  
Patricia Marchak (British Columbia), "Canadian political economy"

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*NETWORK NOTEBOOK (cont'd from page 10)*

MINI RESEARCH REPORTS

'EXPERIMENTS IN RESPONSE ERROR: THE SIZE OF NETWORKS OF NEIGHBORS & RELATIVES.' (Seymour Sudman, Survey Res Lab, Illinois). Examines alternative procedures for the measurement of network size using neighbors (varying distance & # of housing units) & relatives. Uses phone & personal interview data from various Illinois urban & rural sites.

'1986 GENERAL SOCIAL SURVEY'. (NORC, Evanston IL). This year includes questions on whom Rs turn to for help under varied circumstances (with same Qs being asked in U.K., W. German & Australia).

'PRODUCTIVITY, STRESS & HEALTH IN MIDDLE & LATER LIFE'. (JAMES HOUSE, Survey Res Ctr, Michigan). A key goal is to understand & document the ways in which a range of activities & social relationships that people engage in during middle & later life are productive, i.e., contribute toward maintaining or enhancing health & effective function. Based on large national phone sample. [Above 3 items from SURVEY RESEARCH NEWSLETTER]

**THE MAYDAY PROJECT: A NEW COMMUNICATIONS MODEL FOR A NEW LABOUR INTERNATIONALISM** (PETER WATERMAN, Politics & Dev Strategies Prog, Inst of Soc Studies, POB 90733, 2509LS The Hague, Netherlands. Tel: 070-502321.) The project is concerned with "1. the creation of a meaningful internationalism among labouring people at grassroots, shopfloor & community level; 2. the democratisation of international communications as necessary for this." It aims to create a book which will examine how the new communications media can further international worker-worker communications.

## NORTH AMERICAN AFFAIRS

(Barry Wellman, Soc, Toronto)

### US IMMIGRATION vs. BONNIE ERICKSON

Sunbelt conference members were surprised this February to find that Bonnie Erickson had been refused admission to the US by immigration authorities. (They stopped her at the Toronto airport). She had been scheduled to present 2 papers at the conference. Erickson is Prof. of Soc. at the U of Toronto. Her principal organizational affiliation is the INSNA Advisory Board.

At the Sunbelt, INSNA members unanimously passed a resolution deploring this attack on intellectual freedom and directing INSNA to support Erickson. Erickson is currently undertaking legal proceedings to overturn Immigration's action. Informal contacts are being pursued as well, with the kind network assistance of INSNA members. Clearly it will be practically and morally difficult to keep the Sunbelt an international conference if such actions by US Immigration persist.

### THE AMERICAN SOCIOLOGICAL REVIEW vs. 'MARXIST THEORY'

{Most of us have had the depressing experience of having a journal reject a paper. (Indeed, many of us have done a bit of rejecting ourselves.) However, the following story is a bit more complex than the usual ups & downs of academic life.}

1. Sometime in 1984, network analysts John Fox & Michael Ornstein (York) submitted to the AMERICAN SOCIOLOGICAL REVIEW their paper, "The Canadian State & Corporate Elites". This 38 p. ms. used Canadian corporate/state interlock data to compare "instrumentalist" & "structuralist" approaches to theorizing about the capitalist state.

Their abstract summarizes the research:

This study examines empirically the links between the boards of the largest Canadian corporations & the Canadian state, between the years 1946 & 1977. There are a total of about 23K entries in the database. The state institutions studied include the Federal & Provincial cabinets, crown corps., & top civil service, the Senate, the highest Federal courts, the boards of the largest univs. & hospitals. The analysis focuses on the numbers of individuals holding both corp. & state positions at any time during the study period, whether or not the positions are held concurrently. There are hi levels of interlocking between corps. & the state, altho the numbers are not nearly large enough to suggest corp. domination of the state. The densest corp. ties are with univs. & hospitals, followed by the Federal Crown corps. & Royal commissions, the provincial crown corps., the Senate & the Federal & provincial cabinets. The lowest level of ties is found for the Federal & provincial bureaucracies. There is evidence that the level of interlocking increased during the period of the study.

The 9 p. theoretical introduction discussed such writers as Miliband, Poulantzas, Domhoff, Mintz & (on the Canadian situation) Panitch & Clement. The methods section compared the database to the Schwartz/Mariolis/Mintz American interlock database. The data analysis itself used a lot of straightforward network matrix crunching.

2. On 18 Oct 1984, the ASR rejected the paper. One referee thought the paper was weakened by being too Canadian-specific, the other thought that this was a strength (but called for more theoretical development). Although I don't have their final (confidential) recommendation, I'd guess from their "authors' reports" that 1 referee gave an "accept, with revisions" & the other suggested that it be accepted as a research note or sent to a Canadian journal.

3. So far, just another routine author/journal encounter. But Sheldon Stryker, the editor of the ASR also made comments. In his rejection letter, he wrote Fox & Ornstein:

My own reading of your paper leads me to believe--as my detailed comments will suggest--that the framing of the paper is inappropriate for the ASR. That is, on the 1 hand the paper is framed & the 1st set of pages proceed in terms of issues within Marxist theory, per se; i.e., the paper is written as though its major motivation were via an interest in Marxism & in issues within Marxism rather than via an interest in sociological theory. In the 2d place, the paper is also framed in a way that suggests its major purpose is to understand the

specific Canadian situation rather than to advance more general sociological understanding of the relationships between the state & economic elites. Indeed, my sense that the framing of the paper is simply wrong from the standpoint of the ASR was so strong that I stopped making detailed notes on the paper at the end of the introductory section.

I, did, however, complete my reading of the paper. In the end, the sense of the paper announced in the preceding para. did not change. But I found myself agreeing that your data are in fact interesting & important, & that they could be used in the service of general sociological theory to a far greater extent than they are currently used. It would indeed be possible, as I see things, to prepare a research note that simply presented the data & the analyses with a minimum of framing of any kind; & it is at least reasonably likely that the ASR would find such a note acceptable. It would be better, again as I see things, to reorganize the intro. of the paper & the concluding discussion so as to speak less to Marxism & to the specifics of Canada & more to sociology.

4. Fox & Ornstein replied to Stryker on 20 Feb 1985, with copies to ASA pres. Kai Erikson & the ASA's Marxist section. While they found "the reviewers' comments generally reasonable," they objected to Stryker's:

What concerns us is your decision that our paper is inappropriate for ASR because it deals with Marxism rather than with sociological theory....It was our impression...that Marxism is 1 among several recognized theoretical traditions of sociological thought...Moreover, our paper addressed theoretical positions different from our own (what we term 'pluralist' & 'radical')....There are, of course, legitimate intellectual reasons for requesting that we convert our paper to a research note, but deleting the Marxist material to sanitize the paper for 'sociologists' is not 1 of them. We think that you owe us an apology which, in its most meaningful form, would entail a resolution not to practice intellectual McCarthyism in the future.

They withdrew the paper from ASR consideration in order to focus attention on the issue of bias.

5. Stryker replied 5 March 1985:

You get no apology from me. In the 1st place, I abide by the distinction I drew in my letter, between an interest in advancing Marxist theory & an interest in advancing sociological theory, Marxist or otherwise. In the 2d place, anyone who could offer your 'intellectual McCarthyism' remark deserves none.

6. Kai Erikson referred Fox & Ornstein's letter to the ASA's Publication Committee. On 5 July 1985 Fox wrote Norval Glenn, committee chair, "I wish to reiterate that we do not question the reviewers' or the editor's judgement of the quality of the paper, but we do take exception to the editor's comment that the paper is inappropriate for ASR because it is 'Marxism' rather than 'sociology'."

7. Glenn replied on 5 Sept 1985 after the Publications Committee's meeting, re-affirming:

the previous policy of the ASA that the publications it sponsors be receptive to sociological work from all major perspectives. The members...declined to make a judgement about whether or not that policy was violated in the case of your paper, since there was agreement among the members that whereas much Marxist work is sociological, not all of it is, & there is no practical alternative to letting the editor...decide...[Members]...do not necessarily agree with Stryker's judgement in this case, but we felt that our role was limited to making sure that it is not his policy to exclude Marxist work which is clearly & unambiguously sociological. A review of his past editorial decisions indicated that he has not followed such a policy.

8. Glenn also wrote a personal P.S. saying that "your complaint was considered shortly before preliminary screening of candidates for the next editor of the ASR was begun, & I think it had an important sensitizing effect."

9. Walda Katz Fishman, Chair of the ASA Marxist section's "Ad Hoc Committee for Academic Freedom" also wrote to Glenn (25 Sept 85) expressing concern. She observed that the "evaluation of the research article in question...would seem to lend support to an increasing tendency in academic & research institutions of questioning the 'professional credentials' of faculty & researchers for promotion & tenure who teach, do research & publish within the Marxist sociological tradition."

10. Stryker received a copy of this letter and replied to Fishman 1 Oct 1985 stating his openness to diverse sociological perspectives. "Sociological scholarship conducted within the framework of Marxian theory is a legitimate & valid form of sociological sponsorship." He apologized for any "infelicitous or careless use of language" & pointed out a number of Marxian papers that the ASR had published during his term, Joel Devine (vol. 50), Robert Robinson (49) Ronald Aminzade (49), Michael Buraowoy (48), Luca Perrone (49) & Eric Olin Wright (47).

11. 30 Jan 86: The new ASA Publications Committee chair, Arlene Kaplan Daniels also replies to Fishman, stating that the "ASR's current openness to work using a Marxist approach cannot be judged in terms of a single editorial decision."

12. Spring, 1986. The ASA announces that at the end of Stryker's term William Form will be the new editor of the ASR.

13. A somewhat revised version of Fox & Ornstein's paper will appear shortly in the **CANADIAN REVIEW OF SOCIOLOGY & ANTHROPOLOGY**, "The Canadian state & corporate elites in the post-war period".

#### CANADIAN vs. AMERICAN STRUCTURALISM

Last December, Jack Richardson (Soc, McMaster) & Barry Wellman (Soc, Toronto) published a review paper, "Structural Analysis" in the **CANADIAN REVIEW OF SOCIOLOGY & ANTHROPOLOGY's** special issue on "The State of the Art in Anglophone Canadian Sociology". In 1 section, they contrasted Canadian & US structural/network analysis.

Wellman, recycling as usual, included much of this material in a recent grant proposal. One reviewer (while thankfully recommending approval) commented, "Are these comparisons of Canadians & Americans a joke? If so, it's a bad 1." The section from the Richardson/Wellman paper is printed below.\* What do you think. (\* CRSA 22(5): 774-6).

#### ***A Canadian Structural Analysis***

**Structural analysis is currently a much more prominent part of Canadian social science than it is of its British or American counterparts. Important elements in Canadian intellectual history and contemporary social structure have fostered and shaped structural analytic work to be much more than a branch plant extension of foreign enterprises. Its development here can itself be explained in structural analytic terms, related both to large-scale patterns of dependency, and to networks of chain migration.**

Canadian structural analysis has been powerfully influenced by our longstanding political economy tradition, since the time of Innis (e.g., 1930: 1956) relating the emergence of local phenomena to (neo)colonial relations of power and dependency between nations, regions, provinces, and interest groups (see Marchak in this issue).

Rather than being an intellectual accident, we believe that the emergence here of strong political economy and structural analysis traditions is directly related to visibly powerful dependency relations. In contrast, Americans at the dominant centre - perhaps blinded by the light - have tended to ignore the links of power which bind others to them, celebrating others' allegiance as the result of the estimable traits of their own society. They have been especially receptive to arguments that the process of development can be explained by the degree to which members of other societies adopt American values and traits (e.g., Parsons, 1966).

The structural differences between American and Canadian societies have affected their value systems and intellectual climates. While the United States has developed individualist ideology to a point approaching a national religion, Canadian ideology has more strongly emphasized collectivities and carefully structured allocations of resources between contending regional, linguistic, ethnic, religious, and occupational interest groups (Davis, 1971; Bell and Tepperman, 1979; Marsden and Harvey, 1979). Moreover, dominated societies are naturally less receptive to developmental explanations based on the shortcomings of their members. Alternative explanations - proposing that structural relationships between nations, regions, and interest groups are fundamentally the causal variables - may be more persuasive (e.g., Frank, 1967; Davis, 1971; Friedmann and Wayne, 1977; Friedmann, 1986; Matthews, 1983; Brym, 1985). Hence while neocolonial migration has influenced the development of structural analysis here, the Canadian social structure has transformed it.

Important centres of structural analysis have arisen in many parts of Canada, from Memorial University in Newfoundland through Laval University in Quebec to the Universities of Alberta and British Columbia. An especially active centre has developed at the University of Toronto, the institution which the authors know best through long association. It began with the migration to Toronto in the 1967-73 period of many of Harrison White's students and with the brief, but influential, stay of Charles Tilly (1966-69). Tilly's enterprising studies of the nature of community and collective political behaviour mobilized graduate students and young faculty in the systematic structural analysis of historical data.

By the mid-1970s, University of Toronto structural analysis had attained the supportive critical mass necessary to develop a scientific theory group and had put themselves on the map by organizing a major international conference in conjunction with the meeting in Toronto of the 1974 World Congress of Sociology. Two organizational developments helped maintain this momentum. First, the International Network for Social Network Analysis, founded by Barry Wellman in 1976 has kept Toronto as a main focal point of international information. Second, a university-supported Structural Analysis Programme, 1979-83, strengthened the scholarly community, bringing together a dozen Toronto faculty to discuss each other's work, train graduate students, acquire research tools, and develop links with scholars elsewhere.

Such efforts across the country have led to a flourishing, quintessentially Canadian, structural analysis:

First, it focuses on relations between members of a social system and not on the psychically driven behaviour of individuals. It looks at systems, not individuals, structural constraints, not internalized drives.

Second, compared to much American structural analysis, it is more interested in applying the network approach to a broad range of substantive questions than in developing network analytic methods for their own sake.

Third, unlike much American network analysis, it does not see the world as composed of voluntarily-chosen, egalitarian, symmetrical relations. The emphasis on resource flows leads researchers to study asymmetric ties unevenly distributing resources in complex hierarchical structures of power and dependency. In interpersonal analysis, this takes on an Atwoodian (1972) concern for how network members use these unevenly distributed resources for their own survival (e.g., Luxton, 1980; Wellman, 1985). In large-scale analysis, this brings analytic precision to the perennial Canadian preoccupation with relations between core and periphery (e.g., Friedmann, 1986). Thus Canadian structural analysis is quite 'Marxist' in its focus on exploitive structures, even though it avoids a full-tilt Marxist commitment to seeing the relations of production as the dominant explanatory variable of social relations and social structures.

Fourth, a concern for social structures leads naturally to work in conceptualizing and describing such structures. While the great majority of Americans have studied interpersonal network structures, Canadians have been much more apt to study links between large units. They have looked at the structure of relationships within communities, complex links between ethnic groups, and structural connections between regions and large corporations.

Born in Great Britain and Western Europe, developed in the United States, enriched by intellectual immigration, transformed by hardy native strains structural analysis is truly a Canadian product.



ETHNOGRAPHY AND NETWORKS \*

J. Clyde Mitchell (Emeritus Fellow, Nuffield College, Oxford)

The problem that I want address myself to is one which has arisen out of my past but which is still with me. It concerns, at the most abstract level, the relationship between methods of data collection and the subsequent operations upon those data when collected, but there are several preliminary issues to be resolved before we need to tackle that thorny one. The problem arises out of my past because I am commonly believed to be an anthropologist. My interest in social networks arose out of a dissatisfaction with the heavily 'institutional' method of anthropological analysis at the time when I was in the field. I mean by the 'heavily institutional' method of analysis that the behaviour of individuals was usually related to some overall embracing cultural or institutional feature - like kinship for example - particularly lineage structures. My own predilections at the time, arising out of my being trained in social work and psychology (as well as sociology), were to work within the framework of case-studies. The case study approach forced one to appreciate the complexity of behaviour of people even when operating within, say, the framework of a lineage system. It was when I had had experience of case data that I began to realize that I needed some other method of formal analysis to understand what was going on in a systematic way, especially in urban contexts where typical anthropological institutions did not appear to operate.

This early exposure to detailed case material left its mark on me. Similar exposure to detailed observational material, of course, had influenced John Barnes when he was making his study of Bremnes and was concerned with problems of social class. He found it more profitable to explore the social behaviour on that island using network concepts. So when I started talking about the possibilities of systematic analysis of social behaviour using the idea of social networks to the talented group of field workers who had gathered in Zimbabwe in the 1960's, they were all of them people who had assembled data about different types of social events primarily using observational procedures.

By 'ethnographic' I am here referring to those procedures which assemble data through an observer's being there and watching what goes on and how people justify their actions to themselves and to the ethnographer. This use of 'ethnographic' accords with how it is used today. I was originally taught, however, that ethnography is essentially a descriptive technique whereby anthropologists were able to set out the cultural features of some previously unrecorded people. Ideally good ethnographic studies, I was taught, should not be trammelled with theoretical preconceptions. I do not, of course, subscribe to that point of view myself simply because I believe that some selection of information always takes place when recording events and what one leaves out is bound to be affected by what one thinks is interesting or important. Yet at the same time the free recording of events and observations provides the possibility of systematically extracting information on relationships formerly considered to be unimportant but which, after subsequent consideration turns out to be highly significant. It is this 'richness' of data which leads Ron Burt ruefully to comment: "With the notable exception of ethnographers, network analysts rarely capture the complexity of naturally occurring relations" (1983:35).

Methods of Data Collection

At present there seem to be four basic procedures for gathering the field data amenable to analysis using formal network techniques. These are:

1. Survey methods. My impression is that this method, like much conventional sociological research dominates the field. The development of techniques for gathering the sort of information we need for network analysis has expanded enormously in the last few years (Laumann 1973, Fischer 1982; Wellman 1979) and is rapidly becoming the standard method of data collection. With the development of appropriate sampling procedures and methods of inferring features of the parent population from samples the technique is likely to gain in popularity. Another reason why survey techniques are popular may be because their users believe that provided that the sample is drawn satisfactorily then extrapolation from the sample to a parent population is justified. The inference which may be extrapolated to the parent population, however, concerns the range of measurable aspects of the sample population for which sampling distributions are known. Extrapolations about the necessary connections about theoretically interesting features of the data in hand, however, must be based on different principles - those relating to analytical induction rather than enumerative induction. (c.f. Mitchell 1983) These principles apply as much to

\*Keynote address at 1986 Sunbelt Social Network Conference, Santa Barbara CA, USA. February.

types of data other than those based on 'representative' samples.

2. Interviewing. Interviewing is of course part and parcel of the survey technique and I distinguish it only in order to separate extended deep interviewing which is characteristic of the survey method.

3. Diaries and personal journals. I have not seen much discussion of the possibility of this method of collecting network data but it has been used quite effectively for certain types of data (see Cubitt 1973, Boissevain 1974, Higgins et al 1985). This procedure can only be used, of course, in literate communities and usually the sort of behaviour tends to be limited to what the respondent is asked to record, such as: "who did you see today and what did you talk about?" It is heavily dependent on respondent cooperation and therefore not easily checked.

4. Observation. There is of course a significant difference between observational data and those collected by other techniques. This is that it is likely to include both behavioural and cognitive data. Data collected by survey techniques, extended interviews or diaries as all ego-centred in the sense that the data are essentially the respondent's construction of what is going on. There is nothing wrong with using data of this sort of course provided that in making inferences from them, their cognitive qualities are not lost sight of. The material collected by observation on the other hand ought to contain information about what the actors actually did- as against saying what they would do. An additional feature of course is that data collected by observational procedures will usually be set-centred, rather than ego-centred.

Observational network data differ from those collected by other means in another important way. In designing a survey the standardization of the methods of data collection is an important situation. This usually has the consequence that the analyst must decide in advance on theoretical grounds what particular network questions should be included in the questionnaire. The problem that the analyst is trying to examine, and familiarity with preceding research into the same topic, will almost certainly determine what these questions are. But once the questions are decided upon then the possibility of expanding or changing the study is limited.

Observational studies by their nature tend to be set-centred. In other words the full set of interacting actors fall under the eagle gaze (perhaps I should say the basilisk gaze!) of the observer. Not that this makes much difference to the formal analysis of results afterwards: techniques tend to be neutral to methods of data collection. But the set-centredness of observation shifts the focus from any one specified actor to their set as a whole.

Network analyses of observational data tend to be conducted after the event which means that interactional characteristics which at the beginning of the study may not have loomed large may be given full weight in the final analysis because these features may be coded into the network data before the formal analysis begins. This assumes of course that the field-notes are kept in sufficient detail for the analyst to recover the detail subsequently.

Everything turns, of course, on the quality of the fieldnotes kept by the ethnographer. Talented fieldworkers like Kapferer, all of whose network material was produced after the event, kept incredibly detailed records: not all ethnographers can maintain the standards he set. Nevertheless, much can be recovered from good detailed ethnographic accounts as is evidenced by the extent to which classical anthropological accounts may be analysed using network concepts.

It should not be thought from all this that I am opposed to other forms of data collection and certainly to formal methods of analysis (see, for example, Mitchell, forthcoming). The use of formal methods of analysis on data collected by methods which aimed to replicate ethnographic methods as far as possible may be illustrated by material from a study Elinor Kelly and I have been doing of the network links of families undergoing the crisis of homelessness in Manchester. Although we both would have preferred to have collected the data using observational procedures, in fact this proved to be impracticable. Firstly, we were both heavily involved in day to day academic activities at the time and could not give up sufficient time to devote to extended periods of observation. In addition, of course, there could be no easy way to use observational procedures when the precipitating event was totally unpredictable. A family squabble or a

violent assault on a wife, - the two most frequent features underlying homelessness in our data - were hardly the sort of events open to observation. Instead we were forced to use an interviewer interrogating the victims after they had been given temporary accommodation by the local authority and were therefore visible. The difficulty is that we know that what we are dealing with in using data collected by the methods we were forced to use, is the victim's own reconstruction of the events that befell her and are distinctly not interpretable, failing further checking as behavioural data.

The essence of the ethnographic approach lies less in the method of assembling data than in the embedding of the data in a naturalistic framework. The detailed network data, in an ethnographic approach, are located in a narrative account of a series of events in terms of which the characteristics of the structure of network links may be interpreted. The use of the 'ethnographic' approach may be illustrated from the research on homeless families to which I have referred. Observational methods were, as I have explained, hardly feasible in this study. Instead my colleague and I tried to approximate the detail of ethnographic data as closely as we could using detailed interviews in which the history of the family through its phases of homelessness and a detailed description of the latest episode of homelessness played an important part. Detailed network data, however, were not neglected and sufficient detail was collected about the links that the families had during the episode of homelessness. How the narrative and analytical components of the data may be combined may be illustrated from some of the case material from the homeless families study.

Mrs. Wyatt was a woman interviewed initially in a women's refuge and subsequently after she had been rehoused by the homeless families unit. After her marriage had broken up she started to live with a man who, after he had lost his job, became more and more violent until Mrs. Wyatt was forced to secure the protection of a women's refuge. An analysis of five different links (frequency of contact, self-defined 'closeness', practical aid, convivial links and emotional aid) with those with whom she was involved just before and during her period of homelessness using CONCOR procedure (see Light and Mullins, 1979) revealed the following structure and relationships as she saw them (Figure 1).

It is clear that Mrs. Wyatt saw her relationships with those with whom she had been involved during her crisis as falling into three categories. The first is composed of the co-habitee and his kin with whom Mrs. Wyatt had no contact. The second is composed of her own kin. The last is made up of those women and social workers who had been a source of support to her during her crisis, all of whom had come into her life after she had been forced to seek the protection of the refuge. More detail of the structure of Mrs. Wyatt's relationships as she sees them is provided by the sociometric diagram based on the ordering established by the CONCOR analysis (Figure 2).

The three categories of actors described emerge very clearly in this diagram. Note however that Mrs. Wyatt sees her intimate links as being located very firmly with the women in the refuge who are in a similar predicament as she is, and with the social workers attached to the refuge. She sees her linkage with her own kin as being somewhat limited. She has an unreciprocated link with her own mother but that is all. This feature is easily explained by the ethnographic fact that when her marriage broke up, her own kin sided with her husband against her and became estranged from her. Note that Mrs. Wyatt is out of all direct contact with her co-habitee. If she has to contact him she can do so either through her Friend A or indirectly through her sister via her mother.

Mrs. Wyatt's situation may be contrasted with that of Mrs. Maxwell. Mrs. Maxwell left Ireland with her husband when he decided to look for work in England. The employment position being what it was in England, they found themselves destitute. They squatted in unoccupied premises for several months until one of Mrs. Maxwell's acquaintances suggested that they put themselves in the hands of the Local Authority Homeless Families Unit. It was while the Maxwells were in temporary accommodation that we were first able to interview them. Figure 3 sets out Mrs. Maxwell's structure of relationships as she sees them.

It is quite clear that Mrs. Maxwell, unlike Mrs. Wyatt sees her kinship network, both personal and through her husband as being very much alive. Were it not that most of her kin were still in Ireland she would have been able to mobilize kinship links to help her out during her crisis. As it is two of her children were being cared for by kin in Ireland. Given that observational methods can be used in data collection there remain some ineluctable problems - ineluctable to me at any rate - of the process whereby out of the richness and complexity of ordinary social interaction certain aspects of behaviour and

the beliefs supporting them are isolated and represented as elements in a social network. Normally in survey work this process of data construction is consciously addressed: the designer of the schedule pays a lot of attention to how the equivalence classes are being created by the interviewers. This control over the process of data construction is both their strength and the weakness of survey procedures. Strength because the categories are very carefully constructed a priori, weak because the predefinition of the categories may lead to missing significant categories not anticipated by the schedule designer. The procedures we use for network analysis, as far as I am aware, depend on disaggregating complex patterns of behaviour into constituent elements and these are then analyzed, given that appropriate techniques are used, as a collectivity. But the problem which eludes me is that of reflecting the impact of specific combination of links of various kinds on behaviour - of capturing, if you like, the 'interaction' effects rather than the simple additive effects of different types of link in behaviours. I imagine one answer may be in the use of loglinear approaches but I have not myself done much work along these lines.

What I am pleading for, then, is for observational procedures to be given due weight as a method of collection of data to be used in network analysis. Ideally several different methods of data collection should be used to examine some particular problem. In this way the specific qualities of the different approaches should buttress one another. Hard reality in the form of limited resources more often than not, does not allow such luxury. My plea under these circumstances would be that observational procedures should be seriously considered as a method which can yield rich returns while at the same time enabling formal analytical procedures, which I consider essential to network methods, to be used to the fullest advantage.

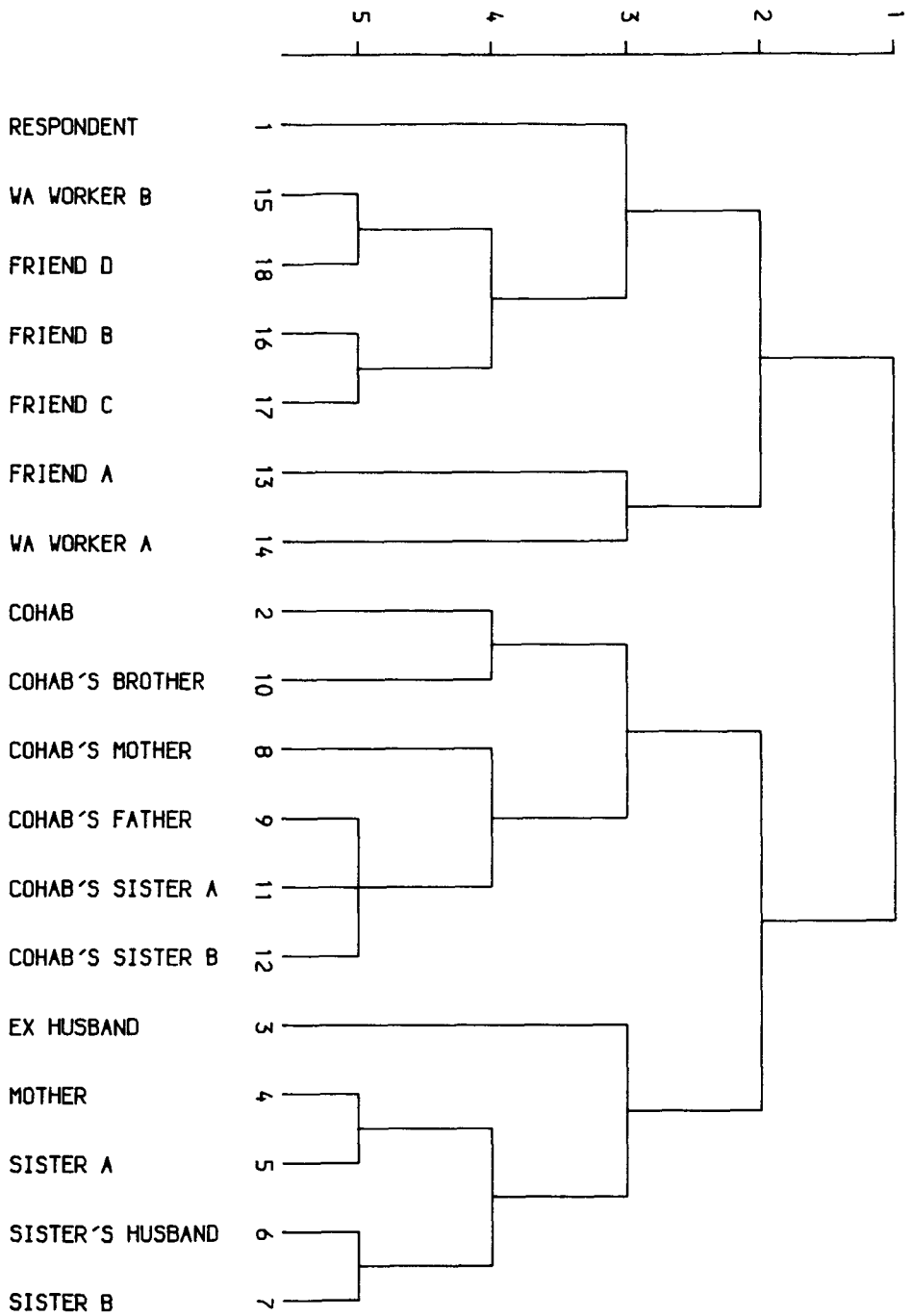
In other words I go along with Dr. Johnston who wrote in 1749:

Let observation with extensive view,  
Survey mankind, from China to Peru;  
Remark each anxious toil, each eager strife,  
And watch the busy scenes of crowded life.

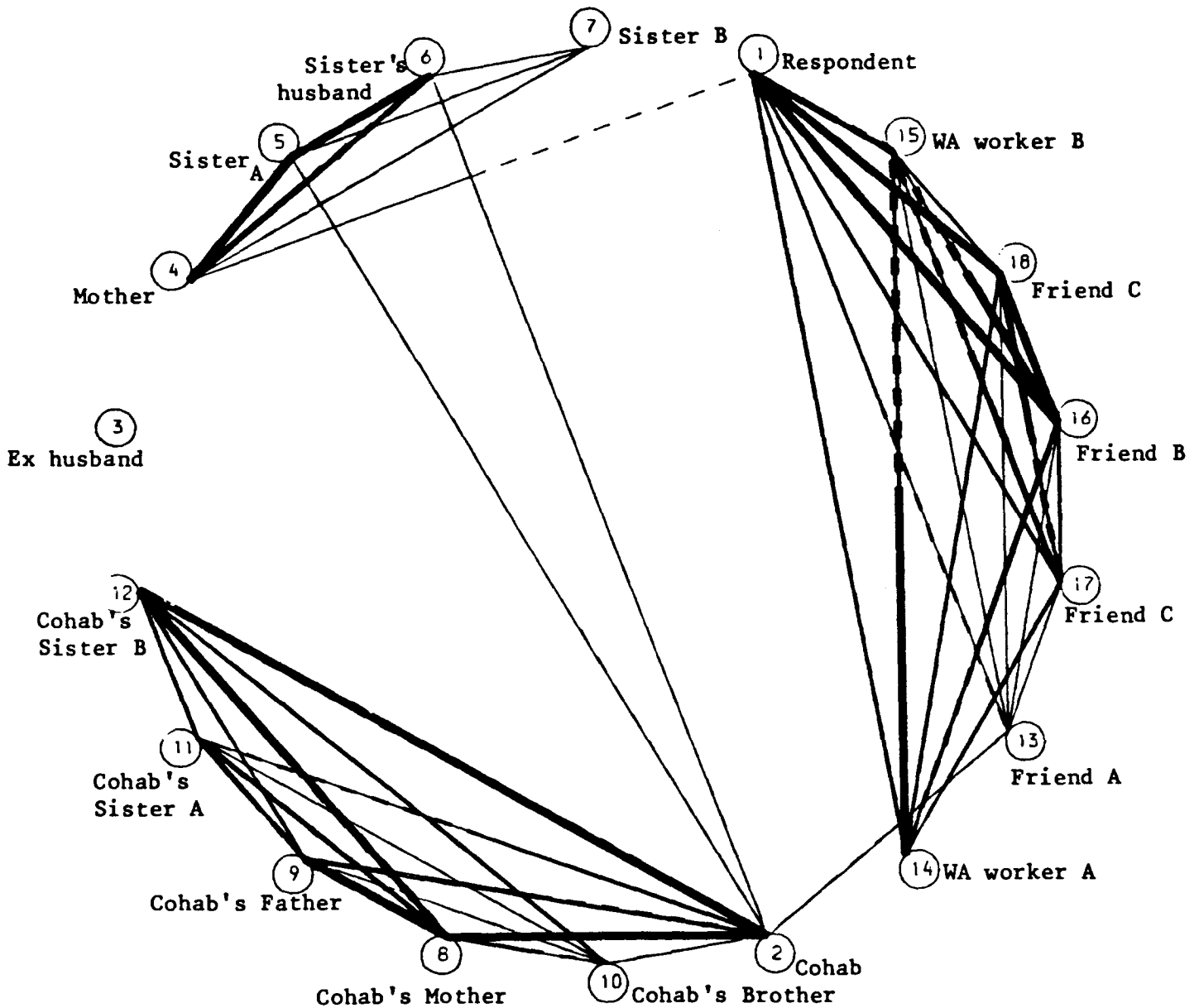
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HOMELESS FAMILIES, WYATT. WOMEN'S AID. CONCORD BLOCKING - FIGURE 1

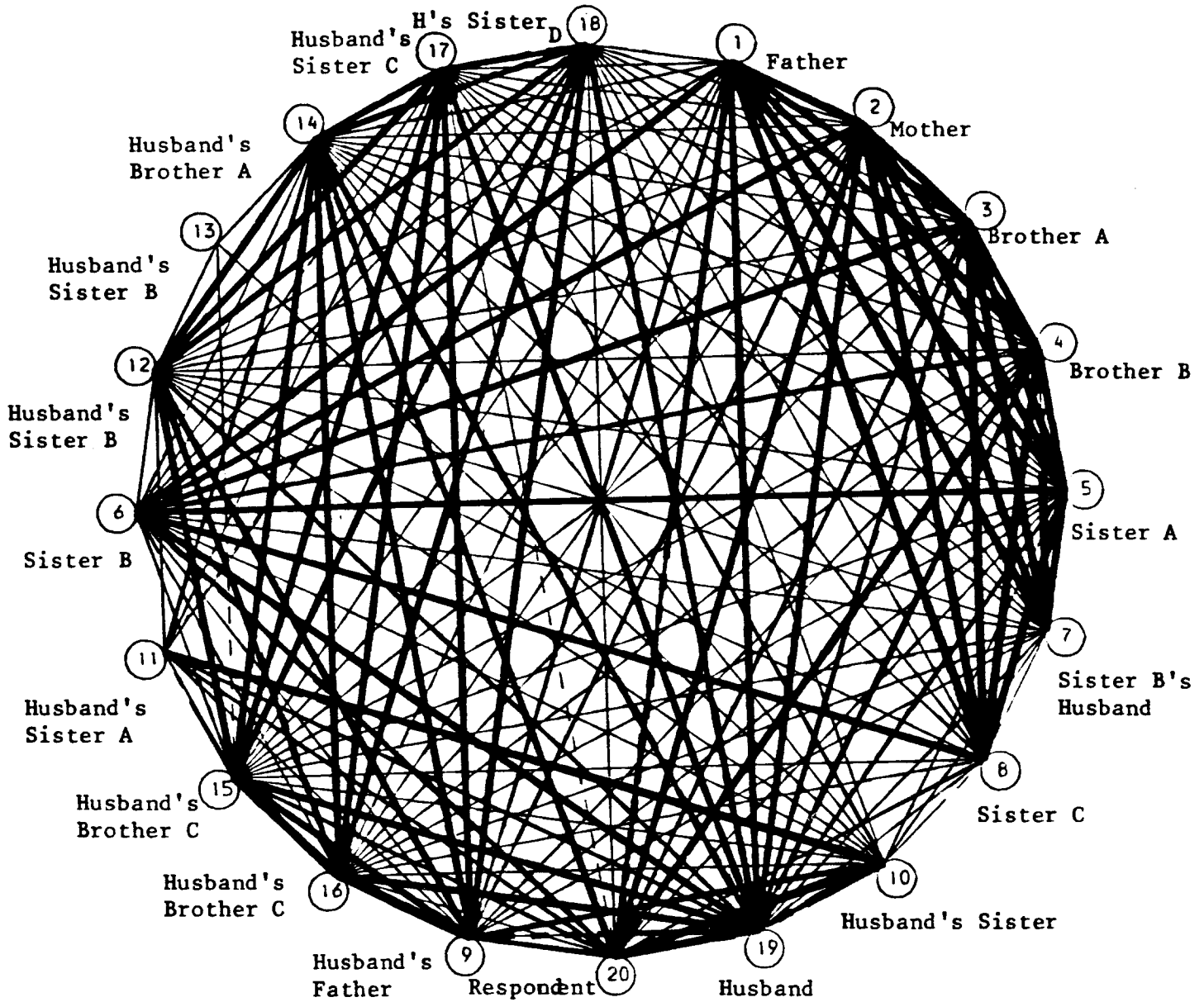


HOMELESS FAMILIES, WYATT. WOMEN'S AID. CONCOR BLOCKING - FIGURE 2



Note: The thickness of the lines joining pairs of elements reflects the multiplicity of the relationship linking them i.e. to what extent it is composed of frequent meetings, a feeling of closeness as defined by the respondent, whether they meet for convivial purposes, whether each gives the other practical aid and whether reach gives the other emotional aid. A broken line implies an asymmetric relationship.

HOMELESS FAMILIES. MAXWELL. HOSTELS. IRISH. VARIOUS MOVES. - FIGURE 3



Note: The thickness of the lines joining pairs of elements reflects the multiplicity of the relationship linking them i.e. to what extent it is composed of frequent meetings, a feeling of closeness as defined by the respondent, whether they meet for convivial purposes, whether each gives the other practical aid and whether each gives the other emotional aid. A broken line implies an asymmetric relationship.

# Evaluating Soft Findings: Some Problems of Measuring Informal Care

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*Abstract: This paper discusses the value of non-experimental methods of a qualitative kind for the study of informal care. Drawing on research on neighbouring conducted at the University of Durham, various strategies are suggested for obtaining confirmation of data gathered by observation, which would render the results more rigorous and 'hard'.*

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## Introduction

Professor Philip Abrams of the Department of Sociology and Social Policy at the University of Durham died in 1981 while his research on neighbouring (begun in 1976) was uncompleted. Several articles appeared in his lifetime on the subject (Abrams 1977; Abrams, 1978; Abrams, 1980; see also Robinson, 1981 and Bayley, 1981) as well as a systematic review of Good Neighbour schemes (Abrams *et al.* 1981, 1982; Humphrey & Snaith, 1982; see also Goldberg & Connelly, 1982, chs. 3 and 8). A full account is in preparation (Bulmer, forthcoming). The paper which follows reflects on the methodology appropriate to the study of informal care, in the light of the experience of the Durham team. It was originally presented to the Personal Social Services Research Unit Conference on Evaluating Interventions for the Elderly in June 1978. It has been edited for publication by Dr. Martin Bulmer of the London School of Economics who is currently preparing a full account of Philip Abrams's work on neighbouring with the support of the Joseph Rowntree Memorial Trust.

## The Nature of Informal Care

*'She's an awful old bully, and I'm not really strong enough to lift her; but she is my mother after all.'*

(50 year old spinster explaining her acute depression.)

*'Fred and Sue next door are always popping-in to see if I'm all right. I've known them for years and they've never let me want for anything.'*

(Arthritic 83 year old widower explaining his lack of need for social care.)

The informal sector of the universe of social care is more and more being looked to for a substantial – and ascertainable – contribution to the process of care as a whole. Once policy-makers and researchers had identified the sort of help that friends, neighbours and relations give each other on a day-to-day, private non-formal basis as a distinct 'sector' of their field of interest, it was perhaps inevitable that the demand for reliable quantitative information about just what is happening in that sector, how, in what circumstances, influenced by what causes and with what results should be voiced. Since the informal sector is typically assigned a highly important sustaining and linking function in relation to the voluntary and statutory sectors, understanding of the precise conditions under which, and the limits within which, those functions are performed can come to be seen as essential. Rational policy-making, no less than effective care, presupposes that both the actual and the optimal articulation of each sector with each of the others should be precisely determined.

For obvious reasons the informal sector is typically concerned with what can be called 'sub-clinical' levels of dependency and

need. The caring agents do not normally have the resources to sustain those they help *in extremis*, conversely the voluntary and statutory sectors do have such resources and are specifically organised to handle those personal troubles which have become so acute in their effects within the informal sector as to be made visible, complained about, referred or in other ways held up for attention as unmanageable within that sector. Obviously, too, the principal beneficiaries of informal care (apart from the very young) are the elderly – they are the people who most commonly experience the types of mild, chronic dependency with which the informal sector can best cope; and they are the people most likely to have long-standing reciprocal caring involvements with others to their credit (there is a great deal of evidence to suggest that long term reciprocity is a, perhaps the, crucial determinant of who does and who does not receive effective informal care). Less obviously perhaps, but on the basis of the work done by the Rowntree Research Unit in Durham I would say just as certainly, the distinctive conditions for which the informal sector provides care are those associated with various experiences of loss – lost company, lost mobility, lost bearings, lost opportunity, lost freedom, lost relationships, lost energy and so forth. Informal social care appears above all to remedy the social isolation which both results in and is caused by predicaments of loss.

## Experimental Research on Social Care

We are dealing, then, with a mode of care which despite great diversity can be said to be particularly expressed in the form of 'mere relating', relationship-work within well-established, trusted relationships. How could one pursue an experimental analysis of the outcome of such 'inputs'? More specifically, could one, for example, investigate the effectiveness of informal neighbourhood care for the elderly in anything like the exemplary manner in which Goldberg and her colleagues examined the effectiveness of social work in *Helping the Aged* (1970)? If not, what sort of non-experimental studies might offer us any comparable measure of evaluative understanding?

We know of course what a respectable evaluative social experiment should look like. Riecken (1974) has described the minimal features of such experiments quite neatly as follows:

'Besides the development of treatments, the experiment itself must be designed. A true experiment involves at least two treatments – perhaps one active treatment and a control treatment or two active treatments together which randomized assignment of treatments to experimental units. Methods of monitoring the delivery of treatments have to be worked out and methods of measuring changes in the factors that are supposed to be affected by the treatments must be perfected. Competent, trained technicians are required for testing, and the professional skills must be



applied to the problems of data collection and analysis. The final, crucial stage is to bring the results to bear upon treatment provision or the development of social policy.'

Sociologically, however, such a programme is only viable if all aspects of the experimental process can be constituted as finite, delimited and located items or entities. Not only should the condition to be treated be measurable but the treatments themselves should take the form of discrete acts, or better still the supply of a fixed quantum of material resources (walking frames, rail-cards, heating additions). And above all, the relationships through which the treatments are assigned and the hoped-for changes effected must be identifiable in terms of highly specific roles and encounters. It would thus, as Wicks has indicated, be entirely feasible to set up an impeccable field experiment to evaluate the effectiveness of alternative material treatments for hypothermia; indeed that is the logical sequel to *Old and Cold* (Wicks 1978). Similarly, the experiment described in *Helping the Aged* was successful in part because the social work inputs could be identified as distinct unit acts or services, and in part because the treatment as a whole was concentrated within the (largely) determinate role-relationship of social worker and client – a relationship organised socially and temporarily in such a way that it could be cut-out of the whole lives of the individuals concerned for purposes of observation and appraisal. The specificity of role gave the researchers an independent variable which they could both identify and manipulate with something like proper experimental precision and control.

### Difficulties with Experimental Designs

The world of informal social care, especially of neighbourhood care for the elderly, is unfortunately not like that. Its distinctive forms are embedded in relationships which, in the language of Parsons, have to be seen as diffuse, particularistic, affective and ascriptive; more naively one can say that in the informal sector people care for the people they care-for. The irreducible property of informal social care seems, unfortunately for social research, to be the fact that it is genuinely informal; and that it defies formalisation. Care is given because it has meaning and value, benefits and costs within long-term or deep relationships between whole individuals. The resulting problem for experimental evaluative research has to do, naturally, with the extreme difficulty of isolating inputs. The roles of kin, friend and even neighbour simply cannot be constituted in an economical finite manner as care-inputs in the way that those of both the social worker and the volunteer (to say nothing of the electric blanket or the meal-on-wheels) can be. A similar and consequential difficulty surrounds the problem of locating and isolating 'treatment' in informal care. The treatment is all too often evidently the whole loving, or at least engaged and supportive, way of life and not any ascertainable material or psychological care item or unit act. Children can sustain the morale of fragile widowed mothers just by being around, even if most of the time they are around they are criticising her for being so dependent. Neighbours can protect each other from the despairs of solitude just by dropping-in, even if they only drop in to moan or to borrow. Friends re-engage the isolated just by resonating old involvements. But they can only do so because they *are* children, neighbours and friends. The formal (statutory and voluntary) sectors can achieve the same effects. But not on the same basis; not by renewing the diffuse, particular, affective relationships of whole people. To speak of a continuum of social care is very misleading in this respect; the informal sector, the sector of private life, seems rather to embody principles that are in important ways at odds with, perhaps even antithetical to, those of the formal sectors, the sectors of public life.

By contrast, identification and measurement of at least some of the *outcomes* of informal social care do not present any special problem for experimental or quasi-experimental social research.

The outcomes of effective informal care are in many respects virtually indistinguishable from those of effective voluntary or statutory care and in principle are measurable in the same way in terms of individual functioning, sentiments and morale. Its results would be improved or at least sustained health, activity, social involvement and well-being; standard measures or indicators for these conditions have been available in social research practice for some time; and there is no peculiar difficulty about longitudinal analysis of the recipients of informal care in terms of such measures. The resistance of informal social care to experimental evaluation has entirely to do with the problem of breaking down the intractable informality of the treatment; of reducing informal caring relationships to the sort of unit acts, factors, events, variables, items needed if specifiable inputs are to be systematically related to specifiable outcomes. It is the problem of setting up an authentically informal 'intervention' that seems decisively to baffle experimental evaluative research in this field.

Of course the problem is not absolute. There are aspects of the process of informal care which can be subjected to experimental evaluation. One of the most important of these is perhaps the question of the 'costs' of care for the caring agents. If we were to assume that informal social care would be more effective and sustained and reliable to the extent that its costs in terms of physical and mental wear and tear for the care-giver were reduced we could clearly find populations on which we could conduct variable-cost experiments, relating various reductions of cost to outcomes in an entirely respectable manner. Such experiments would take a long time and might well be thought ethically invidious; furthermore their results might not be very surprising. But they certainly could be conducted. Their feasibility would turn on the fact that in selecting the populations to be studied we had side-stepped the much more enigmatic issue of identifying the types of people who do and who do not receive given measures and modes of informal care. We would be relating inputs to outcomes without any attempt to understand the social process of constructing caring relationships through which they were linked. From a policy point of view there is possibly not much wrong with that. From an intellectual point of view it does leave something of a gaping hole, however.

Nor is the deficiency just intellectual. The larger problems of the linkage of the informal sector with the statutory and voluntary sectors revolve very firmly around just this question of identifying the types of people who manage to fall through the net of adequate informal care. And it is here, as a practical no less than as an intellectual problem, that we have to face the issue of both conducting and evaluating non-experimental research. In my view the only realistic alternative to experimental evaluations in this field is the comparative *post-hoc* evaluation of case studies, that is to say, the substitution of the method of comparative history for that of the laboratory. But before saying something about the ways in which *post-hoc* evaluations of informal social care could be pursued with a reasonable degree of clarity and precision we ought to consider one other possible alternative to experimentation – namely practical or field simulation. There are many areas in which practical simulation is superior to *post-hoc comparison* as a substitute for experimentation. Is informal social care for the elderly, one of them? The question has some practical urgency in the light of the current proliferation of *soi-disant* 'good-neighbour schemes' which in varying degrees purport to be nothing less than field simulations of the world of informal social care. Specifically they seek to generate neighbourliness through formal organisational structures and the specification of neighbouring roles (see Abrams *et al.*, 1981). If their claims are justified they represent a revolutionary bridging of the formal and informal sectors and a site in which relationships at least strongly analogous to those of informal social care could be experimentally examined.

### 'Good Neighbour' Schemes

In practice, however, we are dealing with a metaphor rather than an analogy. In one all-important sense Good Neighbours are not good neighbours but something quite different. Neighbourhood care schemes certainly seek to activate localistic sentiment and the resource of proximity as bases for care-giving. One could allow, too, that the sort of need they seek to meet is also very similar to that which is distinctive of the informal sector – a mixture of short-term, very specific predicaments such as the need for shopping or child-minding and long-term needs for rather diffuse support, oriented in both cases however to the problems of loss and social isolation rather than to more acute or clinical conditions. It is interesting to note, though, that in neighbourhood care schemes as distinct from informal care relationships these two types of task are typically performed by different types of caring agent. This separation of function is not fortuitous. It indicates a principled dissimilarity between organised neighbourhood care and informal care. Although the needs typically catered for by the two modes of care are very similar the type of care given is fundamentally different. So far as the type of care they offer, and the conditions under which it can be elicited, are concerned Good Neighbour schemes or neighbourhood care schemes are firmly part of the voluntary sector of social care *not* of the informal sector. To that extent their effectiveness can perfectly well be studied experimentally – inputs and treatment can be isolated – but unfortunately such experiments will tell us nothing about the conditions governing the provision and effectiveness of genuinely informal care as provided by actual kin, friends or neighbours. The separateness of the two forms of care, despite the rhetoric of informality employed by the neighbourhood care projects, is even more apparent if we consider the recipients of care in each case. The neighbourhood care schemes are designed and function to deliver care precisely to those people who have fallen through the net of informal care, those who have *not* found friends, relations or neighbours to look after them. They represent a policy intervention to make good the deficiencies of informal care to do the things informal care cannot do. This may be the best that can be achieved in a society that has abandoned most of the structural and cultural circumstances strongly conducive to informal care. But it would be foolish to imagine that the extension of formalised neighbourhood care, however effective it can be made, is the same sort of thing as the extension of informal care. To put it crudely: caring mothers get caring children; caring Good Neighbours presumably get OBEs.

In sum, neighbourhood care schemes may well be the *substitute* for informal care which we shall increasingly adopt as our society becomes more mobile, fragmented and pervasively bureaucratised. But if our research problem is to understand the nature of informal care and the conditions in which it is reliably available experimental evaluations of neighbourhood care schemes will mislead rather than help us. We must rely on the evaluation of non-experimental analyses of informal care itself. We must, that is, follow Durkheim (1938) and have recourse to comparative *post-hoc* studies and/or the comparative manipulation of aggregate data: 'When ... the production of facts is not within our control and we can only bring them together in the way they have been spontaneously produced, the method employed is that of the indirect experiment, or the comparative method.' But how can such evaluations proceed in this field? And how far can they achieve a precision comparable to that claimed by the best experimental social research?

As it happens, although the interventions represented by voluntary neighbourhood care schemes do not provide sites for simulation studies of informal care, our understanding of both informal care and voluntary neighbourhood schemes will probably come mainly from retrospective comparative evaluations for some time to come. For just as we cannot isolate

the inputs in informal care so we do not really know what sort of interventions to make in voluntary neighbourhood care. We are at a highly exploratory stage of policy development, not having identified the 'at risk' population we wish to help, the measures that will effectively help them or, least of all, the complex and ramified intervening factors by way of which inputs and outcomes are related. We are rapidly accumulating a mass of data about the incidence, structures, personnel and problems of neighbourhood care schemes to match the mass of data previously assembled about neighbouring, family life and community – most of it now as then derived from observational, often naively observational, case studies. Our limited knowledge and the exploratory condition of social policy both mean that the urgent research task is not experimentation but the *interpretation* of the studies available to us in ways that will reconstitute their findings (that is, their observations) as plausible organising hypotheses for more rigorous, possibly experimental, work to be carried out later on. The interpretive transformation of non-experimental observations into hypotheses is a peculiarly delicate and problematic phase of the research process. Unfortunately it is that sort of evaluative exercise which now seems to be imperative both in the field of informal care and in that of organised neighbourhood care. And it is here that, in both fields, the comparative method provides a life-line.

### The Comparative Method

By the comparative method I mean, following Durkheim again, the procedure which begins by seeking concomitant variation. Opportunely, that is precisely the sort of thing that most observational studies of both neighbours and Good Neighbours have sought. Thus we have some reason to believe, for example: that in large cities the distribution of neighbourhood care schemes is significantly skewed away from both the most deprived and the most privileged areas; that Catholics are about three times as numerous among the members of neighbourhood care schemes as they are in the population as a whole; that there is a relationship between the social class of members of care schemes and the types of care they provide; that both effective access to informal care and effective exclusion from it are 'transmitted' from generation to generation; that the incidence of informal care between neighbours is inversely related to social mobility and to geographical mobility; that people who have been trained to give care give more care than those with otherwise identical social attributes; that neighbourhood care schemes flourish best in areas where there are strong informal care networks; that individuals use different informal networks for meeting different types of need; that the provision or withholding of informal care is strongly related to positive affect, and that positive affect is in turn strongly related to reciprocity. Some of these findings, and the host of comparable observed relationships thrown up by the existing body of research in these fields, are surprising or puzzling, others are perhaps banal. But all of them invite, even demand, interpretation. They direct attention to the problem of identifying the intervening or ulterior, concealed and possibly causal factors that forcibly constitute the relationships the first generation of researchers claim to have observed.

As what I now have to say about the findings of research in these two fields is highly critical, I shall so far as possible draw for examples on the work done by myself and my colleagues in Durham. That work is representative, however, of a much larger body of enquiry in that it has tended to be praised for the 'sensitive ethnography' it embodies while being trounced for the 'imprecision and vagueness' of its findings; in other words it is commonly seen as interesting but useless. The problem that arises is to find some way of proceeding from soft data to hard evaluations – if we can.

### The Generation of 'Soft' Data

The way in which the soft data tend to be generated is something like this. A local neighbourhood or neighbourhood scheme, or sometimes more than one, is selected for study on the basis of a more or less clearly formulated problem: Is the extended family declining in importance? What are neighbourhood care schemes doing and is it needed? What conditions are conducive to close relationships between neighbours? What sorts of people join neighbourhood care schemes, and why? Do patterns of informal primary group care vary by social class? A fairly close and usually rather careful observational study with or without formally specified questionnaire procedures is then carried out. In the course of the study it becomes clear that the issues and problems underlying the research are a great deal more complicated than had at first been supposed. Behaviour and attitudes fail to correlate with attributes in the way first postulated for example; so we set off on a search for intervening or underlying factors that might explain the failure; personality, life-history, opportunity-costs, norms. And when we get to norms we find we need an indicator and double-back to attitudes and behaviour. Qualifications accumulate and the research focus blurs. The fine web of conditions within which social action occurs is discovered in the course of the research instead of having been provided for in the research design. Findings accordingly emerge in the form of very weak generalisations and highly conditional propositions. The sense of determinate relationships is defeated by the apparent variability of social practice and by the sheer weight and diversity of the 'quasi-inputs' which appear to intervene between effects and their presumed causes. The more meticulously the observational studies are conducted the worse this problem becomes.

Thus, characteristically, one of our own studies of neighbourhood care schemes claims to have identified three distinct types of Good Neighbour, three sources of Good Neighbouring: status, altruism and reciprocity. The argument, which already involves a good deal of interpretation of the available evidence, goes as follows:

Of these three views the third was much the most common. The images of a secular moral community in which helpers and helped were linked by a shared 'ordinariness' and thus by actual or potential reciprocity occurred in over half the interviews; that of a dominant and un-common minority moved by the gratifications of self-importance was offered in a further third; and the suggestion of people responding, usually through religion, to an exigent norm of altruism was very much a minority theme. Our own observations and conversations confirm this impression. There is a small group of long term residents who 'do everything' in the village, who are aware of each other and themselves as doing everything, and who have created their identities very definitely through constant activity on a public stage - expecting their performances to receive due applause: 'I like to help and I like to be appreciated'; 'I'm surprised how ungrateful people are sometimes - when you think what we've done for this village...'; 'It's the newcomers I mind - I mean, you'd think they'd show some respect...'; 'There was a time when I'd have done anything for this village, but since they dropped me from the Council I'm not so sure'. For them, involvement in the scheme is simply one more call on their local audience to affirm that they are indeed active, useful people. The fact that the affirmation is often given a little grudgingly hardly matters: 'some think you're nosy but they like a visit really'. These people see it as up to them to make things happen and the return they expect is the confirmation from others than things are indeed happening as a result of their efforts. They are seen as people who have 'always run this village', 'domineering types', 'full of their own importance', 'in it for the glory', or at least 'genuinely anxious to help but it's got to be done their way'. By contrast the majority of the Good Neighbours have little or no sense

of the public importance of the care they give. For them, the decisive consideration is a much more direct actual or possible exchange of services between themselves and those they help. The notion of 'ordinary people wanting to help each other' evokes neither social esteem nor altruism as its sanction but a simple expectation of reciprocity. These respondents saw themselves as 'just ordinary people' and that perception was in turn closely linked to an awareness of the ways in which Good Neighbouring could contain reciprocity: 'They're much the same as me; you're going to get old yourself one day and I'd like to think someone was going to visit me'; 'nothing special about them - they get some kind of fulfilment, company, you know; and so do I, it's as much for me as for them'; or perhaps more concretely, 'I've looked after an old lady of 76 but since I had the baby she's been more active than me; she's been doing things for me and bringing me bits of things; she comes in every day to see that I'm alright; I think that's how it should work, don't you?' Or, 'I really value having someone to talk to; it does me as much good as it does her'. The negative side of that is a fairly apparent belief among this type of Good Neighbour that help should earn help, that payment is due. As one respondent put it, slightly disgruntled because she herself had not received help during a recent illness, it would be better 'if the helpers weren't quite so concerned in getting something out of it - it doesn't cost anything to go and see someone; Mrs. B. came to see me but she wanted me to look after her son in return'. But even if the exchanges are not always made with appropriate sensitivity or symmetry we have been left in no doubt that for the majority of Good Neighbours in this particular scheme the basis of help is a well understood norm of reciprocity. And finally, there is the small minority who offer care on the quite different basis of a morally absolute altruism: they give help 'for God'. 'If you've got any faith at all you should help people who are worse off'; 'It's just the Christian thing to do'. (*Unpublished research paper*)

Among studies in this area the conclusions offered in this particular example would have to be seen, for all their impressionistic elusiveness, as relatively sophisticated. An attempt is being made to proceed from the observation and reporting of complex phenomena to some sort of morphology. The question is, of course, whether one has any good reason to trust the interpretive lead that is offered. In this instance the device of interpretation is essentially rhetorical; one would have to burrow back into the mass of interview material from which the tendentious quotations have been selected to know whether the proffered analysis could really be taken seriously as a basis for hypotheses for more exact and policy-oriented investigations. Even then one would really be doing nothing more than checking quantitatively on a qualitative reading of people's accounts of their own attitudes. The real problem of distinguishing motive from rationalisation, cause from interpretation would not have been grasped. Faced with another study of the same type carried out by researchers with different presuppositions and offering conclusions which identified the sources of social care in terms of social class, life history and relevant skills we would have no way of deciding on the basis of the studies themselves what to do next.

The step from detail to morphology is crucial. The best studies of this type try to make it but, given the sort of evidence they assemble they cannot make it definitively. Typically, honest researchers who have engaged in this kind of research end up admitting their own uncertainty. Even if one starts out with an apparently clear focus on an apparently specific variation mounting ambiguity relentlessly sets in as complex observed detail undermines conceptual clarity. Consider the following:

There is a strong tendency to believe that it is possible and useful to talk of working class or middle class areas, with which are associated distinct collective patterns of behaviour, attitudes and norms. But the research record suggests several

objections to this belief. How do we recognise an area as homogenous in class terms? How do we deal with the presence of both status-assenting and status-dissenting individuals, let alone the (alleged) existence of a 'new' working class with middle class styles of life and aspirations? Can we reasonably compare as similar realities the middle class in Britain and the class identified under the same name in the United States? What about all those varieties of the lower middle class? In one sense the problem is, it seems, still one of clarifying the concept of class, of identifying significant variations in class culture and, only then, of venturing hypotheses about the relationships of class and neighbouring. In another sense it is a matter of being forced to recognise the seemingly endless fine shades and gradations of status and class that are actually presented to one in the field. On both counts the unreflecting equation of class and type of residential areas has as yet conspicuously failed to bring unequivocal patterns of neighbouring to light. We are left with a dilemma. On the one hand it seems theoretically desirable to try to isolate the class factor in neighbouring – through more meticulous conceptualisation and the deliberate dissociation of class from locality. On the other hand the weight of currently available empirical evidence suggests that class is significant only in association with other factors and that it is those other factors which should be our main focus of attention in future research. The contradictions and qualifications to general statements about class and neighbouring that the literature impels all seem to spring from the ways in which the meaning of class is itself bound up with and affected by, for example, age, income, mobility, residential arrangements, social service provision, occupation, time available and need for neighbouring and a host of other factors. On the whole the observation of class variations in neighbouring does not, as yet, justify the attribution of a significant independent class influence on neighbouring but, rather, seems to point to the presence of more specific influences operating within and across the worlds of class. (Robinson & Abrams, 1977:24-6)

And if the problem in locating class influences could be thought to be at least in part an effect of conceptual untidiness, how sobering to find that the same inconclusiveness seems to prevail in relation to such conceptually clean categories as age and sex:

McGahan's (1972) (unusually specific) findings indicate a lack of significant difference between men and women in both the extensiveness and the intensity of their neighbouring. This conclusion is supported by Tomeh's (1964) work on a wide range of informal activities: but it is disputed in several other studies. Kuper (1953) for example, argues that women, particularly those with young children, are bound to the locality in their movements and, therefore, their social relationships. Men were found to be less involved with their neighbours in terms of friendliness but many borrowed household equipment from them. Kuper concluded that for the most part men satisfied their need for sociability at work, in clubs and so forth and hence, unlike their wives, did not 'need' the company of neighbours. A similar view could be adduced from Bott's (1957) study – in which male respondents were actually confused when asked about contacts with neighbours since that was a domain of relationships confined to women. On the other hand, there are instances where the reverse is true: in the pit village of 'Ashton' men were active as neighbours as an extension of their working relationships while on the outlying farms (but not in the village) of Gosforth, Williams (1956) found that it was again men who were outgoing and neighbourly, the collectors and bearers of news, the traders of small services, while the women remained in the farmhouse. In sum, the research evidence points in no single direction and it appears that differences in neighbouring

between the sexes are dependent upon other factors. (Robinson and Abrams, 1977:31-2)

The endless reappearance of these subversive 'other factors' in field after field of this area of study is in the end a powerful invitation to irrationality and despair; and to a wilful pragmatism in social policy. Perhaps we should just recognise that we are dealing with relationships so entangled, ramified and minutely varied that they either cannot be ordered at all or can be ordered only by an effort quite out of proportion to any conceivable results.

While it is indeed true that any large body of undigested data from open-ended interview research or observational studies is likely to have that effect I am not persuaded that we need to throw in the sponge just yet. To begin with, no-one would argue that, in the setting of policy research at least, such studies are ever likely, or can properly be expected, to achieve more than a preliminary orientation. On the other hand if they are necessarily preliminary they are also a necessary preliminary. While they cannot carry us to the precise appraisals of costs and effectiveness which policy-makers require, they plainly are the best available means of initially mapping any field of social relationships into which policy-makers might wish to intervene. The question is whether one can proceed from the detailed ethnographies provided by open interview and observational studies to the sort of social morphology from which more sharply focused hypotheses can be derived. It is not a matter of abandoning observational studies in favour of something superior; but of appreciating their specific role within an *extended process* of research and policy and of seeing how, within that process, they can be made usable. What matters from that point of view, however, is not so much the extent and exhaustiveness of the observational or interview data available to one but rather their analytic concentration. However wide-ranging and open-ended pilot inquiries and observations may seem when in the field, their value as a stage in a research sequence depends on the degree to which, whether by accident or design, a comparative examination of their findings will yield a classification of *types* of relationship or variation on which further action – policy or research – can confidently be based.

### Inductive Construction of Generalisations

The problem is not new. It was recognised by Durkheim: 'a satisfactory method must above all aim at facilitating scientific work by substituting a limited number of types for the indefinite multiplicity of individuals.' And, rather more sharply formulated, it has become a commonplace in anthropologists' discussions of the 'new' ethnography, where the issue ceases to be one of merely reducing cases to types and becomes a matter of selecting the right types, of deriving the most appropriate classification among the many classifications that could in principle be derived from the data – of establishing the reliability of the favoured classification as against others. This in turn is commonly seen as a question of demonstrating that one's scheme of analysis is indeed grounded in the understandings and purposes of the observed population. Thus, in a characteristic statement Ward Goodenough declares, 'ethnographic description requires methods of processing observed phenomena such that we can inductively construct a theory of how our informants have organised the same phenomena.' The difference between good and bad non-experimental social research could be said to lie precisely in the fact that the former does contain such methods for validating observations and responses whereas the latter does not. And fortunately we have some understanding of what those methods are. The crux of the matter, as Goodenough implies, is that the research should be designed and carried out in such a way as to permit the *inductive construction* of theory. It is on that basis that other, more specific, validating procedures (triangulation, response prediction and so forth) are most effectively brought into play.

The nature and scientific status of analytic induction is of course a standard issue for the textbooks of research methodology (cf. Bulmer, 1979:661-6; Hammersley and Atkinson, 1983:200-4; Mitchell, 1983 - ed.). And its function as a transformation device linking data and theory is generally acknowledged. However, statements of principle are one thing, practical implementation another. If we allow that analytic induction is the essential procedure for the evaluation of observational and other kinds of 'soft' data we still need to know just what that injunction requires us to do when embarking on a particular study or, worse, when faced with the jumble of ambiguous and indefinite findings typically thrown up by such studies. Here practical examples of what other researchers have actually done to master their data are perhaps more helpful than abstract formulations of good method.

### An Example

In this context I have found an article by Bloor (1978), spelling out the measures he took to achieve an inductive interpretation and validation of observational data in the course of a research project concerned with medical assessments and disposals, rather more useful than anything in the methodological cookery books. Certainly the particular measures of classification, interpretation and grounding he adopted were to some extent peculiar to the particular research task he was attempting. Nevertheless, his account of the steps he took to establish the reliability of his own reading of his observations is an impressive indication of what *can* be done to order and 'harden' soft data even if it cannot serve as a model for what in every case should be done. I will end this paper by briefly presenting Bloor's procedure and then by returning to the Good Neighbour study cited earlier and suggesting how its findings, too, could be consolidated.

Bloor's 'problem' was to find an explanation for marked geographical variations in the incidence of adeno-tonsillectomy among children. Setting aside all other possibilities he organised his research on the basis of a single, sharply formulated hypothesis—that the variation could be attributed to geographical variations in the assessment and disposal procedures of the relevant medical specialists. It seems quite clear that the brutal crispness of this initial formulation of the research issue itself had a great deal to do with its subsequent success – non-experimental studies should be exhaustive *within* the limits of a tightly defined question, not just exhaustive. The essential data he needed therefore had to take the form of descriptions of specialists' assessment practices. But, the problem of validation was anticipated: 'what is required is a description of specialists' assessment practices informed by a knowledge of specialists' definitions of the situation' and of their practical purposes at hand'. Given that concern, procedures of inductive interpretation and of respondent validation were built into the research from the outset. In effect this exercise in self control involved nine distinct steps, seven designed to facilitate coherent induction and two designed to bring the observed population's own judgements to bear on the validity of the interpretations thus achieved. The simplest possible presentation is to quote at length:

'The analytical system that I adopted was as follows:

1. For each specialist separately, cases were provisionally classified according to the disposal-category into which they fell.
2. The data on all a specialist's cases in a particular disposal-category were scrutinized in order to attempt to produce a provisional list of those case-features common to the cases in that category.
3. The 'deviant cases' (i.e. those cases where features common to many of the cases in the disposal-category were lacking) were scrutinized in order to ascertain whether (a) the provisional list of case-features common to a particular category could be so modified as to allow inclusion of the

deviant cases; or (b) the classificatory system could be so modified as to allow the inclusion of the deviant cases within a modified category.

4. Having thus produced a list of case-features common to all cases in a particular category, cases in alternative categories were scrutinized to discover which case-features were shared with cases outside the first category considered. Such shared case-features were thus judged necessary rather than sufficient for the achievement of a particular disposal.
5. From the necessary and sufficient case-features associated with a particular category of cases sharing a common disposal, the specialist's relevant decision rules were derived.
6. For each decision rule the cases to which it applied were re-scrutinized to derive the search procedures associated with that decision rule.
7. The above steps were then re-enacted for each of the other specialists' disposal categories until a set of decision rules and associated search procedures for that specialist was derived which accounted for the disposal of all of the specialist's cases for which data had been gathered. The analysis was then repeated for each of the remaining specialists.'

Having thus, on the basis of his own clearly specified search procedures arrived at a grounded and accessible classification of medical decisions, and so put himself in a position to relate types of decision to other variations in the characteristics of the specialists (or more dramatically to make the theoretical claim that the fate of patients depends on the decision rules of doctors rather more than on the objective condition of the patients themselves) he might well have thought his research task fully accomplished. In fact he went on to make two further efforts to consolidate his own reading of his observations by presenting it directly to his original informants for validation or criticism. There are a variety of 'respondent validation' techniques available in the literature. The one chosen by Bloor was simply to write out his analyses for his subjects and record their reactions to them.

8. For each separate specialist I wrote out a report describing his assessment practices; with each report I sent a covering letter requesting the specialist to read through the report and see how far it corresponded with his own impressions of his clinic practice. The discussion of the report then formed one of the themes of a tape recorded interview..... I was hoping for a sort of self-recognition effect.'

In effect Bloor found himself negotiating with his informants for the assent to his analysis, a procedure which frequently involved significant modification of the analysis itself. Clearly, however, such a procedure has its own difficulties; it depends both on the ability of the researcher to present his analysis in a fully comprehensible way to the informants and on the readiness of the informants to take that much trouble to help the researcher. Bloor's final step therefore involved an effort to check the validity of his validation exercise by further exploration of those instances in his study where he had found himself unable to choose between alternative explanations.

9. I took the opportunity to fill in the resultant gaps in my analysis by the use of hypothetical cases or interview material ... inserting a few questions about some of the specialist's routine assessment practices, the answers to which were already incorporated in the written report. Where I found a specialist describing his practices in terms which contradicted the report he had previously endorsed I asked more and more questions ... I ended up triangulating my validation exercise by the interview method.'

The success of an exercise such as this obviously depends heavily upon the open-mindedness and thoroughness of the researcher. But it depends, too, on other conditions which are perhaps more

easily guaranteed. Respondent validation, at least of the kind attempted by Bloor, is possible only with respondents who are themselves rather actively interested in the research. Inductive analysis can be pursued effectively only if (a) the question to be answered is clearly and tightly specified at the outset, (b) within the terms of that question data collection is extensive and open-ended and (c) provision is made within the population studied for control or comparison groups in relation to which the status of necessary and sufficient conditions for effects can be assessed. Even if all of these conditions are satisfied the method does not, as Bloor allows, *guarantee* reliable interpretation: it simply increases its probability by injecting a systematic attempt to eliminate alternatives into the analysis of soft data. It offers the possibility of quasi-experimental onslaught on the necessary ambiguity and complexity of such data. If the onslaught is, as it was in Bloor's case, intellectually coherent, sustained and methodologically many-sided it can win at least modest victories.

### Validating Findings

Finally, then, we can consider whether the sort of control achieved by Bloor – in what was after all a study of a highly structured and determinate social setting – can also be hoped for in studies of the much less determinate field which immediately concerns us: the provision of neighbourhood care for the elderly. Returning to the research on neighbourhood care schemes mentioned previously, and remaining properly sceptical about the rather grandiose claims concerning the fundamental bases of care quoted earlier, I would like to take some rather more straightforward and specific 'findings' of that study which strike me as genuinely embodying the possibility of a controlled development of research and analysis. The findings in question concern the crude distribution of care among the population of the neighbourhood for which schemes are ostensibly responsible. In none of the cases we have studied was care distributed uniformly or comprehensively among the known population in need. Moreover, we felt confident that we had observed a process of twofold selection on the part of the members of our neighbourhood care schemes. Good Neighbours appeared to be making choices both about the types of care they would offer and about the people to whom they would offer it. In neither case did it seem possible to match the pattern of care to a similarly structured pattern of need. Some needs, such as the need for occasional shopping, appear to be over-satisfied; others, such as the need to accompany disabled people on outings, are manifestly and severely under-satisfied. And some people with needs of a given type, solitary old people needing company for example, have their needs amply met, while others with identical needs are virtually ignored. Both forms of this mal-distribution of care are striking features of all the neighbourhood care schemes we have so far examined. We naturally wanted to move from this basic observation to some explanation of the deeply problematic situation it seemed to indicate. The obvious thing to do was to investigate both the explicit care-allocating decisions of the members of neighbourhood care schemes and the criteria of selection implicit in their care-giving practices. One major strand of our subsequent research was accordingly focused on precisely these issues.

Unfortunately, care allocating decisions are not made with the formality of medical disposals; nor is there any one site or moment at which one can observe them. We had therefore to make what we could of records and observations of visits and meetings on the one hand and of transcripts of discussions and interviews with Good Neighbours and their clients on the other. This already introduced an element of uncertainty into the research which Bloor had not had to face. To master it we had to define the decisions in which we were interested as clearly as possible. As it happens the exclusion of clients occurs in two steps: some people known to be in need never get visited at

all; others receive an initial visit and are then effectively abandoned, either to the statutory social services or to their own devices; yet others, the majority so far as we can tell, receive an initial visit which is then followed by a more or less extensive treatment in the form of further visits, social activities, particular types of help and so forth. In effect, for each case that comes to their attention Good Neighbours make one of three decisions: not to act at all; not to proceed beyond an initial visit; to engage in extensive caring and helping. We decided to make an effort to account for these variations paying particular attention to the two exclusion decisions, not visiting at all and not following up an initial contact. Even though the 'decision' was in most cases unlikely to be firmly located in time and space our research task thus in principle constituted one which could be controlled both by systematic inductive analysis and by procedures of respondent validation.

### Conclusions

In the event we have indeed been able to pursue this particular analysis through the whole gamut of Bloor's programme. We encountered a number of awkward but usually informative difficulties on the way, and at some stages the processes of comparison, scrutiny and evaluation of cases and of modification of categories had to be much more open-ended, protracted and devious than we had originally expected them to be. I would like to comment on some of the procedural difficulties we encountered, which seem to have been peculiar to this field of study and this type of enquiry. The initial classification of cases presented few problems – although we were impressed to find that of the 90 Good Neighbours included in the study 75 had cases in all three decision categories while 84 of the 522 cases had to be abandoned in the face of adamant insistence that no decision had been made about them at all, the case being, rather, forcibly removed from the Good Neighbour's hands by the scheme's organisers, a social worker, or another Good Neighbour. By contrast, the second stage, in which we tried to achieve a listing of the features common to the cases in each of the three decision categories, proved more complicated because we were relatively dependent upon the Good Neighbours' own accounts of their cases rather than on independent observational evidence. Unanimously – and there are considerable advantages in this respect in being part of a research team rather than a solitary investigator – we formed the impression that the Good Neighbours were inclined to indulge in a good deal of rationalisation in accounting for the exclusion decisions. The two commonest rationalisations (as we judged them) were that the client didn't really need help, or conversely that the help needed was so substantial and specialised that it was a matter for the statutory services not for the Good Neighbour. Because we had not included any direct study of the excluded clients in our initial research we had no independent basis on which to check these claims. All we could do was to note them and to wait and see whether other evidence that we did have tended to confirm them or call them in doubt. Another problem at this stage was that many of the features peculiar to cases in different categories only existed on the basis of inferences made by ourselves about the social attributes of the Good Neighbours and their clients. We had not, for example, systematically established the social class position of either the Good Neighbours or their clients. Nevertheless we formed a strong impression that the exclusion categories were to a significant degree populated by people who belonged to a social class different from that of the Good Neighbours assigned to help them. At this point it became clear to us that Bloor's requirement for exhaustive data-collection is almost certainly utopian in the context of problems of this type.

Somewhat to our surprise the remaining steps in the process of inductive analysis proved remarkably straightforward and the next major difficulties occurred when we tried to embark on respondent validation exercises. The trouble here was that the

analytic procedure had left us with a set of interpretations of Good Neighbours' exclusion decisions which was not at all flattering to the self-esteem of the informants whose validation we now needed. Brutally put, we had concluded that exclusion decisions sprang from three types of consideration which we had identified as : inconvenience, fear and a perceived lack of relevance on the part of the client to the care-giver's own needs. Such interpretations were, quite predictably, deeply offensive to most of our informants. Their involvement in the business of giving care and their consequent interest in the research were quite insufficient to persuade them of the credibility of an analysis which we had derived in part from their own accounts but much more from judgements about the independently observed characteristics of our informants and their clients. Accordingly, the validation exercise, insofar as it required informants to give overt assent to our analysis, was something of a disaster. Far from being the gratifying venture in self-recognition reported by Bloor, our respondent validation project turned into a series of furious arguments, wrangles and recriminations. The lesson seems to be that overt respondent validation is only possible if the results of the analysis are compatible with the self-image of the respondents. Fortunately for our own morale we had decided to attempt covert validation as well in the form of hypothetical case constructs. Here we found that our informants splendidly confirmed our interpretation of them rather than their own account of themselves: that is, young, female, middle class Good Neighbours did regularly say that old, incontinent, smelly, working class males were a problem for the social services and not for them. Rightly or wrongly we concluded that that was better evidence (of course it confirmed our earlier conclusions) than the insistence of our informants that such things as class, age and smell could not possibly affect their altruistic practice. We end, then, by conceding that there is a debate between ourselves as researchers and the people whose lives are the object of our research. I am fairly confident, thanks to the meticulous way in which we have pursued the programme of inductive analysis of our admittedly soft and ambiguous findings, that our (base) interpretations of the sources and meanings of neighbourhood care are correct. On the other hand neighbourhood care is a substantial and valuable contribution to social care as a whole. Perhaps research which suggests that neighbourhood care systematically neglects those who have nothing to offer the caring agents should be suppressed. After all, Good Neighbours also have their needs. The problem for the policy-maker seems to be not to apply research mechanically to practice but to strike a balance between the undeniable truths established by research and the equally undeniable but quite different truths embodied in the lives of those who make up the neighbourhood care sector of our welfare system.

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## ANATOL RAPOPORT

(Barry Wellman, Soc, Toronto)

Most network analysts know of Anatol Rapoport's work on snowball networks & biased graphs. It was 1st published as "A study of a large sociogram," with W.J. Horvath, BEHAVIORAL SCIENCE 6: 279-91 (1961)--looking at cumulative friendship choices in a hi school--& revised in several versions (with commentary) since then. Yet this is just 1 of the many Anatol Rapoport's.

The accompanying article in this issue gives some idea of Rapoport's work for peace. In his many years at the U of Michigan, Rapoport was a key member of the Ctr for Conflict Resolution, an editor of the J. of CONFLICT RESOLUTION, & extremely active in applying game theory to issues of peace. (According to John Sonquist he invented the concept & term of the "teach-in" in 1965 when Michigan profs. were searching for a way to protest meaningfully the Vietnam War.)

This June [1986] the U of Toronto is awarding Anatol Rapoport an honorary degree. At present, he is Professor of Peace Studies at University Col of the U of Toronto: an unpaid emeritus position which he fills actively until the university decides it can afford a paid chair. He teaches the two core course of the program: "Intro to Peace & Conflict Studies" & "Decision-Making Processes"--applying his game/decision theory skills.

Rapoport has just finished a vigorous term as head of the Canadian "Science for Peace" Association. When I interviewed him recently, I got a clear, forceful lecture on how academic analysis could link with praxis for peace. He identified 3 possible approaches to peace studies:

(1) psychological: "What are the factors which make for war, peace & conflict resolution? Is it natural to connect war with hostility? This might have been the case at 1 time but current technology decouples war from personal aggression. Now it is simply people doing their job."

(2) systemic: identifying the socioeconomic interests that lead to the arms race. "The conventional view of war of war--I don't mean the old view, which was that war was a legitimate extension of foreign policy--but the conventional anti-war view, is that war is a disaster, a visitation, like an earthquake or flood. I don't agree with that at all. My view is that war as an institution is imbedded in modern social structure."

(3) strategic: critiquing the zero-sum calculations that go in politico-military decision-making circles. He didn't feel there was much point in try to influence present decision-makers--they're too decoupled from the ordinary populace & too linked to arms race interest. Rather, he advocates setting up communication networks to mobilize the populace & bypass existing decision-makers. "Like physicians, diplomats have tended to look for the sufficient causes of the disease that is war. This is not necessary because the necessary causes are already known: weapons. Since there can be no war without weapons, the 1st order of business is to find a way of eradicating them."

Before retirement, he was Prof. in Toronto in both the Math & Psych depts & had headed Vienna's Institute for Advanced Studies, 1980-1984. This polymath activity goes back a long way: he was Prof of Math'l Biology & Sr Res Mathematician at the U of Michigan, 1955-1970, & in the U of Chicago's Committee on Math'l Bio, 1947-54. He has been 1 of the central figures in the General Systems Theory movement & edited its journal for 20 years.

There are 13 books, mostly in game theory & math models. One early book is intriguingly titled, OPERATIONAL PHILOSOPHY (1953). His vitae does not list individual papers, instead noting "journal articles (about 400), entries in encyclopedias (about 10), chapters contributed to books (about 40).

Yet there's even more to his accomplishments. Born in Lozovaya, Russia just before the revolution [1911], he soon immigrated & went to Chicago public schools. But from 1929 to 1934 he was back in Europe, training to be a concert pianist at Vienna's Staatsakademie fuer Musik und darstellende Kunst. His vitae lists his wartime service as "U.S. Air Force, 1942-1946, lieutenant, then captain;" rumour tells me that this was liaison work in Alaska with the Russian allies.

In his interview, Rapoport confessed that he hadn't kept up with most recent network analytic work. He noted approvingly the corporate/govt interlock genre, especially when ("like Levine's") it moved beyond descriptive mapping. His suggested future direction? "We need more work on non-egalitarian graphs".



## ANATOL RAPOPORT: CRITIC OF PEACE RESEARCH

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Anatol Rapoport became involved with peace research in the middle of the 1950s. He conducted his famous game experiments in conflict and cooperation during the 1960s. He was President of the Peace Research Society (International) and of the Canadian Peace Research and Education Association in the 1970s. He received the Lentz International Peace Research Award in 1975. He has been the Director of the Institute for Advanced Studies in Vienna since 1980. During the last 35 years he has taught mathematical biology, mathematical psychology, mathematical sociology, and statistics at Chicago, Stanford, Michigan, Copenhagen, Toronto, and Vienna.

His critiques of peace research have taken many forms over the years. We shall first describe several of these forms, and then conclude with a description of his ideas on effective peace research.

### Critique of Positivist Thinking:

Although Rapoport started his academic career in the 1940s as a "pure logical positivist," he abandoned this position by 1950 because he believed that science could not be value-free and true to itself at the same time: "The scientist, in order to be consistent, must subscribe to certain values (and discard others) . . . because he is a scientist."<sup>1</sup> Scientific sanity implied freedom from fear and rage which, in turn, required love and cooperation as opposed to conflict, punishment, and revenge: "These values are not arbitrary. They must be used by the scientist if he is to behave consistently as a scientist."<sup>2</sup> In addition to love and cooperation, effective inquiry implied free inquiry and freedom implied equality: "An open society must be egalitarian . . . Egalitarianism, therefore, in operational social philosophy appears not as a postulated principle but as a conclusion, flowing out of the only premise of that philosophy — ethical principles must be arrived at as a result of free inquiry . . . Under conditions of free information flow, egalitarian structures seem to be the only stable ones and capable of being the embodiment of a universal ethics."<sup>3</sup> The revolutionary trinity of freedom, equality, and love, required for a just society, was also required for a true science, according to Rapoport.

In addition to criticizing positivism for its value-free factuality, its predictability theory of truth was also criticized. The trouble with this theory of truth was its assumption that our theories do not influence the facts which they predict. This assumption may hold true for nature but not for humanity: "The observations we make of human beings and the assumptions we make about their conduct often do affect human behavior . . . The hypotheses we choose may determine the course of history, and so the 'truth' which we are seeking."<sup>4</sup> "as a rule the assumptions made in natural science do not affect the material under study; in behavioral science they do."<sup>5</sup> "The assumptions themselves are facts . . . The

concept of value-free science becomes meaningless in this context."<sup>6</sup> "The scientist must not - in fact, cannot - be morally neutral . . . He must choose either to serve or not to serve . . . those whose goals are not morally neutral."<sup>7</sup>

Where values, rather than facts, have to be used to test our theories, what values shall be used? "We are committed to making and displaying maps of reality that are more conducive to sanity than to un-sanity."<sup>8</sup> And scientific sanity implies the values of love and cooperation, freedom and equality. In human relations, then, theories which promote these values are to be preferred to those which degrade them. These basic values defined not only Rapoport's philosophy of science, but also his concept of collective rationality, his political philosophy of social democracy, and his moral theory of peace.<sup>9</sup>

### Critique of Strategic Thinking:

The trouble with strategic thinking is its belief that arms races and wars could be won in a nuclear age. Strategic thinking defines rationality in terms of self-interest. Strategic thinking is defined as "psychopathic, that is, thinking utterly devoid of moral sense."<sup>10</sup> Strategic thinking, like positivist thinking, claims to be value-free in a field laden with values. The trouble with criticizing strategic thinking is its eminent display of rationality in the form of mathematical game theory. Intellectuals like Herman Kahn, Thomas Schelling, Robert Osgood, Henry Kissinger, etc., were quick to pre-empt the concept of rationality for the war community, thus making the peace community "irrational" by subtraction: "To this image of 'rationality' we owe to a great extent the acceptance by the mainstream liberals of the criminal aspects of American policies during the 1960's."<sup>11</sup> Rapoport was so distressed by these policies that he and his family left the United States in 1970.

Game theory professed to rationalize choices and decisions in conflict situations, defining rationality in terms of self-interest. In zero-sum games, where one loses whatever another gains, and where there is no room for cooperation but only pure conflict, then the self-interested definition of rationality seemed to fit very well. However, when this definition is applied to non-zero-sum games, where there is room for cooperation as well as conflict, the result is paradoxical: Acting in one's self-interest defeats one's self-interest! This is hardly the rational thing to do.

For example, in the game called Prisoner's Dilemma, if only one of two prisoners confesses, that one goes scot free while the other one gets a very severe sentence. If both confess, both get a heavy sentence. If neither confesses, both get a light sentence. Consequently, the temptation to confess is very high. It is rational to confess. However, if both prisoners make this rational (self-interested) choice, both get a heavy sentence! Where it is more rational to be irrational, the concept of rationality becomes absurd. And this is the concept of rationality championed by strategic thinkers.

For another example, in the game called Chicken, two players are driving their cars toward each other at breakneck speed. The last one to turn aside is the winner. The first one to turn aside is chicken! If winning is the highest value (which it is in strategic thinking), the only rational choice

is to be ready to die. If you are ready to die, and commit yourself irrevocably to this choice (such as by throwing your steering wheel out of the window so that you cannot turn aside), then your opponent has no choice but to turn aside (if he wants to live), and you will win. However, if both drivers reason so rationally, and throw out their steering wheels at the same time, then neither will be able to turn aside, neither will win, and both will be dead! This kind of rationality is not merely absurd, but downright deadly.

When we realize that nonzero-sum games (like Prisoner's Dilemma and Chicken) are more typical of real-life situations than zero-sum games, the limitations of the self-interested definition of rationality become most limiting indeed. In spite of these limitations, game theory and strategic thinking were largely supported by the military establishment since 1950.<sup>12</sup> Much of this research was done for military agencies by the Rand Corporation.<sup>13</sup> "In fact, game theoreticians were recruited into the think tanks of the United States military establishment . . . . The attractiveness of game theory to a certain type of military mind is to be sought not only in the possible concrete applications of the theory but also in the intellectual support it lends to the idea that war can be fought 'rationally'."<sup>14</sup> "A perusal of the literature on applications of game theory to logistic and military problems reveals that all of them are cast in the form of two-person zero-sum games."<sup>15</sup> What's wrong with strategic thinking is that it assumes pure conflict with no possibility of cooperation, while most real-life situations contain possibilities of cooperation as well as conflict. Consequently, strategic thinking is neither so realistic nor so rational as it claims to be.

#### **Critique of Psychological Thinking:**

Rapoport found neither hereditary (instinct) nor environmental (frustration) theories of aggression very convincing. He argued against instinct theory because intraspecific human aggression seemed to serve no biological nor survival purpose.<sup>16</sup> He argued against frustration theory because historical and experimental evidence suggested that "aggression is as likely, or more likely, to be instigated by success than by frustration."<sup>17</sup> Another problem with instinct theory is that its acceptance would tend to support belief in the inevitability of war.

#### **Critique of Systemic Thinking:**

Turning from individual to systemic theories of war and aggression, Rapoport reviewed seven such theories: Hobbes, Hegel, Clausewitz, Marx, Lenin, Richardson, and Kahn.<sup>18</sup> As a pacifist, Richardson was interested in preventing wars. As a militarist, Kahn was interested in winning them. Richardson proposed that some wars were functions of arms races which, in turn, were functions of interaction.<sup>19</sup> This process of interaction was reminiscent of the competitive interactions in the Prisoner's Dilemma game. Contrary to Richardson, Kahn believed that arms races (even nuclear arms races) could be controlled and used to blackmail and threaten opponents into submission.<sup>20</sup> This process was reminiscent of the game of Chicken.

In spite of the different evaluations, orientations, and motivations of systemic thinkers, a common thread ran through all of their conflict theories: "Psychological traits of entities **need** not be involved to explain the behavior of the system . . . . In summary, systemic theories of conflict cover an immense range of conceptions and attitudes, from the mystical idealism of Hegel to the austere materialism of Marx, and from the pacifism of Richardson to the sadistic enthusiasm of Kahn. All of them, however, point to the same conclusion: The 'psychology' of the system may be entirely independent of the psychology of its human components. If this conclusion is correct, we need not search the human psyche for attributes that explain the murderous tendencies of certain forms of human organization."<sup>21</sup>

In spite of his attraction to systemic theories, Rapoport recognized their limitations. In his review of Richardson's arms race theory of war, Rapoport found that Richardson's "results . . . are of uncertain scientific value."<sup>22</sup> There was only one weak empirical corroboration of this arms race model.<sup>23</sup> Systemic thinking, like strategic thinking, led toward the "conclusion that war is inevitable" and both failed to recognize "the indivisibility of the vital interests of the human race as a whole."<sup>24</sup> In Richardson's statistical study of war, "About the only positive finding was the apparent role of 'common government' in damping the severity of conflicts."<sup>25</sup> "Hardly any of the indices examined by Richardson could be shown to be related to either the incidence or the severity of wars."<sup>26</sup> Other systemic studies by Rummel (1963), and by Singer & Small (1972), for examples, showed no **outstanding** correlates of war, so that none of these studies provided any **outstanding** clues for the prevention of wars or for the mitigation of their severity.

#### **Critique of Ideological Thinking:**

In his analysis of the Big Two (USA and USSR), Rapoport saw this conflict as an ideological one which might be at least partially resolved by an ethical debate: "A clash of ideologies can hardly be the underlying basis of large-scale social and international conflicts. But it **may** play an important part and so overshadow strategic considerations as well as bias the operation of the 'blind' forces. Therefore, the roots and the role of ideology should be understood."<sup>27</sup> By 1980 the ideological theory of war was dismissed: "This theory is severely impaired by counterexamples."<sup>28</sup> "Today ideology seems to have lost its salience . . . . Preservation of the war system takes precedence over 'ideology'."<sup>29</sup>

#### **Critique of Utopian Thinking:**

Rapoport's most general critique of peace research was that there were no institutions authorized to apply the results of such research, even if the results were applicable: "While there are institutions specifically organized to put medical knowledge to use (boards of health, hospitals, etc.), comparable institutions do not exist to test and put to use whatever may be discovered concerning the conditions of war and peace. This absence of appropriate peace-keeping institutions (endowed with authority as well as with good intentions) reveals the inadequacy of the analogy between medical research (or any other research aimed at preventing or

controlling natural calamities) and peace research."<sup>30</sup> "Where are the institutions empowered to take appropriate measures if a 'cure for war' is ever discovered?"<sup>31</sup>

"The avowed ultimate goal of peace research is to uncover the 'causes of war' . . . . The limitation of peace research so conceived is in the circumstance that there are no institutions empowered to make use of the knowledge about 'causes of wars' in the way medical institutions make use of knowledge about the causes of disease."<sup>32</sup> A store of knowledge already exists, but no agencies to apply it.<sup>33</sup>

#### **Effective Peace Research:**

So much for Rapoport's critiques of peace research. We shall now turn to how he thinks peace research can be effective.

"Any research which can seriously claim to be directed toward the prevention of war or especially toward the establishment of peace must be **essentially** concerned with psychological matters, with man, his motivations, and his social behavior,"<sup>34</sup> because perceptions and attitudes "are among the decisive determinants of war and peace at the present time."<sup>35</sup> This statement might seem to contradict the critique of psychological thinking but, as we shall see, psychology here refers to the social psychology of conformity rather than to the individual psychology of aggression.

"Research as a social activity can have an impact on the society which supports it. If we start asking different questions, we may become different people."<sup>36</sup> Research as a social activity can be most effective simply by being itself, instead of an adjunct to some policy-making bodies. As a social activity, peace research would simply promote the free communication of peace research results. In this sense, peace research would require no other agency or institution outside of itself in order to be effective.

"If peace research becomes established as a sector of science, its first impact will almost certainly be ideational, not technological . . . . This task of peace research seems to be purely a destructive one; but it needs to be done, since some of the most formidable obstacles to conflict resolution on the international scale are the encrusted ideas of the past."<sup>37</sup> Some of the conventional ideas that need to be challenged are military strength leads to national security; balance of power leads to international stability; etc.

Simply by being itself and studying the war-making institutions of sovereign states, peace research may be able to delegitimize these institutions: "It is precisely the legitimacy of the sovereign state, including the legitimacy of war-making, that makes modern war at all possible. It may be, therefore, that while peace research is powerless to produce techniques which will be applied to the prevention of wars (for lack of institutions empowered to test and to apply such techniques), it may bring about fundamental changes by undermining the legitimacy of war as an instrument of national policy . . . . In our own day, legitimacy derives from the conviction that the rulers make decisions which are roughly in the collective interest of the rules . . . . The dissolution of 'individual rationality' as a meaningful concept in complex conflict situations serves to undermine the legitimacy of national interest."<sup>38</sup>

This argument against the legitimacy of military nationalism, based on the critique of strategic thinking, was soon joined by another argument based on the likening of war to "organized crime."<sup>39</sup> Both of these arguments were designed to change attitudes and perceptions so that habits of obedience and loyalty to military nationalism would dissolve.<sup>40</sup> The attitudes to be changed were not those of individual aggression, but rather those of social conformity.

These themes were further developed by Rapoport throughout the 1970s: "Peace research ought to be directed toward finding effective means of destroying or rendering less effective the war-making institutions."<sup>41</sup> "Parasitic crime syndicate" was proposed as "a good model for the existing military establishments, at least of the Great Powers."<sup>42</sup> This model would make the task of peace research "the accumulation and wide dissemination of knowledge about how war-making institutions plan wars, how they sell them to their populations, and how they themselves wax fat and prosperous in the process."<sup>43</sup> "Neither conspiracy nor wickedness is the issue. The issue is the institutionalization of war by deliberate intellectual effort . . . of people like Herman Kahn, Thomas Schelling, Robert Osgood, Henry Kissinger, and scores of others, intellectuals all . . . . One of its (peace research's) tasks ought to be, in my opinion, that of counter-acting the pre-emption of rationality by the war community . . . . To this image of 'rationality' we owe to a great extent the acceptance by the mainstream liberals of the criminal aspects of American policies during the 1960's."<sup>44</sup>

The war-making institutions were likened to parasites of war. Peace research needed to find ways to get rid of these parasites: "Undermine . . . the habits of obedience, the bonds of loyalty, and the bases of confidence that mask the malignancy of these institutions . . . . Reveal the fraud that hides behind the alleged principle of rational decision."<sup>45</sup> "Undermine the alleged 'rationality' and 'objectivity' on which modern power elites and their technical experts claim to base their policy decisions."<sup>46</sup> At this time, the most significant contribution to peace research was believed to be the Pentagon Papers, whose publication eroded confidence in policy-makers.<sup>47</sup>

"Peace research can perhaps, after all, contribute to inhibiting war. But if it does, it will be through its effects on attitudes and expectations, rather than by uncovering a 'cure for war' to be applied rationally in the way drugs are applied against diseases."<sup>48</sup> "In summary, the product of peace research ought to be not another technological invention placed at the disposal of the power-wielders, but a direct effect on our own (everyone's) attitudes and expectations, not the least important of which is a dissolution of the habits of obedience and attitudes of loyalty to that monstrous organism . . . the war-making state . . . . Seek out lines of research conducive to changes of attitudes and expectations resulting from enlightenment."<sup>49</sup> "The needs of power are inevitably destructive . . . . Hence, erosion of loyalty to entrenched power is one change in the semantic environment that ought to be encouraged."<sup>50</sup>

These quotes clearly illustrate that the psychology of war for Rapoport had nothing to do with the individual psychology of aggression, whether it was innate or learned, but rather with the social psychology of conformity,

loyalty, and obedience to authority. Rapoport presented no systematic evidence to support this social psychological theory of war, but there is some evidence to show that interpersonal conformity is more closely related to militarism than is individual aggression.<sup>51</sup>

It would be most misleading if we did not keep clearly in mind that this theory of war implied no conspiracy nor wickedness on the part of the military-industrial-academic complex: "The only justification for the existence of the military establishment . . . is the military establishment of another . . . . The goal of total destruction is being served not by design but as a consequence of the technocratic orientation."<sup>52</sup> It is in the light of this technocratic orientation that the following quotations should be interpreted: "Wars occur because they have been deliberately planned and prepared . . . . A profession exists whose task is to plan wars."<sup>53</sup> The existence of this broadened, prosperous and influential profession is not conducive to the abolition of war as an institution."<sup>54</sup> "The most valuable result of peace research would be a change in the conception of war, from that of a political instrument or a natural catastrophe or a disease to that of organized crime."<sup>55</sup>

Rapoport was well aware of the trouble with his analogy between war and organized crime, which presented war as "a normal activity of military establishments, in the same way that organized crime is a normal activity of criminal syndicates . . . . The conception of war as an analogue to organized crime, unlike other academically more respectable conceptions, would lead peace research into areas of activity traditionally avoided by scholars and scientists practicing their calling."<sup>56</sup> Peace researchers should study and expose the "crime syndicate" of war, breaching its secrecy and undermining its legitimacy.<sup>57</sup>

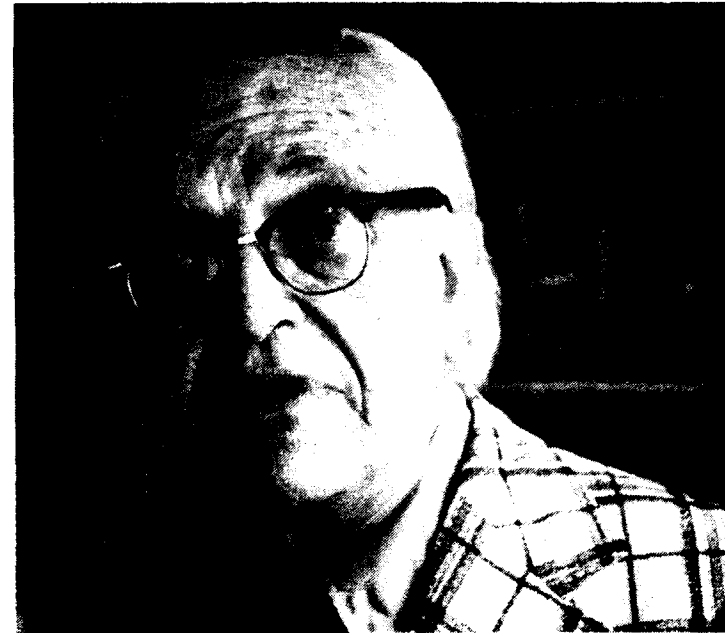
Rapoport's theory of war vs peace boils down to power and strategy vs humanity and conscience: "It is absurd to imagine that the interests of humanity as a whole can be served by wars . . . . Preoccupation with power, rather than with the fate of humanity, is still foremost in all circles where decisions on the allocation of resources, talent and effort are made."<sup>58</sup> Rapoport was convinced that neither war nor revolution was an effective means of ousting power elites: "Perhaps, instead of being overthrown and supplanted by other power, entrenched power can be eroded. The most brutal power depends, in the last analysis, on the prevalent conviction that it is 'legitimate'.<sup>59</sup> Organized warfare is the "crassest use of power."<sup>60</sup> It should be kept clearly in mind that military power involves "numerous interlocking agencies, the 'arms' of the defense establishment, research teams, institutes of strategic analysis, procurement agencies, etc."<sup>61</sup>

Finally, the most important contribution to war today is "the role of the military (and its business and academic entourage) in keeping the global war system going."<sup>62</sup> It is in the study and exposure of this role that Rapoport believes that peace research can be most effective.

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Professor Anatol Rapoport

REL TWO:

Interactive Loglinear Model Fitting for  
Pairs of Sociometric Relations

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I. INTRODUCTION

REL TWO fits loglinear models to a multivariate social network consisting of two sociometric relations measured for a group of actors. This FORTRAN program was used by Wasserman (1986). Different models focus on comparisons of the two relations or comparisons among the actors. Further, actors may be partitioned into blocks or subgroups, and models may be considered that focus on differences and similarities between these subgroupings. Finally, each relation in the pair of relations may be composed of two or more component relations. For example, consider the study of 18 monks in an isolated American monastery, conducted by Sampson (1968), and also discussed in Fienberg, Meyer and Wasserman (1985). Data were taken on the compound "positive-negative affect" pair of relations, which were composed of a "like-dislike" pair of component relations, an "esteem-disesteem" pair, an "influence-negative influence" pair, and a "praise-blame" pair. These relational components are aggregated to form the compound pair of relations in a manner described later.

Let us first consider the case of a simple pair of binary sociometric relations. A single binary relation is denoted by a  $g \times g$  binary sociomatrix  $X_r = (x_{ijr})$  such that:

$$x_{ijr} = \begin{cases} 1, & \text{if actor } i \text{ chooses actor } j \text{ on relation } r \\ 0, & \text{otherwise} \end{cases}$$

where  $g$  is the total number of actors. A simple pair of binary sociometric relations is defined by a pair of such matrices:  $X_1 = (x_{ij1})$  and  $X_2 = (x_{ij2})$ . We now describe how to fit loglinear models to this pair.

Consider the  $g \times g \times (2 \times 2)^2$  matrix  $Y = (y_{ijklmn})$  associated with  $X_1$  and  $X_2$  such that:

$$y_{ijklmn} = \begin{cases} 1, & \text{if } (x_{ij1}, x_{ji2}, x_{ij2}, x_{ji1}) = (k, l, m, n) \\ 0, & \text{otherwise} \end{cases}$$

Note that the  $(2 \times 2)^2$  submatrix associated with each dyad consists of a 1 and 15 0's; hence

$$\sum_{k,l,m,n} y_{ijklmn} = 1.$$

Let  $\pi_{ijklmn}$  be the probability of observing that  $y_{ijklmn}$  equals one for the pair of actors  $(i, j)$ , so that

$$\sum_{k,l,m,n} \pi_{ijklmn} = 1.$$

If we define  $\mu_{ijklmn} = \log \pi_{ijklmn}$ , a class of linear models in terms of  $\mu_{ijklmn}$  may be developed such that fitting a model to the  $Y$  matrix, using an interactive proportional fitting procedure corresponds to fitting the same conceptual model to the pair  $X_1$  and  $X_2$ .

The loglinear models above are specified simply by listing the margins of the  $Y$  matrix corresponding to the model parameters, representing indices  $i, j, k, l, m, n$  by integers 1, 2, 3, 4, 5, 6. For example, the model:

$$[12] [13] [24] [16] [25]$$

implies fitting the  $(ij)$ ,  $(ik)$ ,  $(jl)$ ,  $(in)$ , and  $(jm)$  margins. Fitting the model is done by creating a matrix  $\hat{m} = (\hat{m}_{ijklmn})$  that is the table of estimated expected cell counts, essentially the fitted  $Y$  matrix. That is, we iteratively update this matrix  $\hat{m}$  until:

$$\begin{aligned}
 \hat{m}_{ij++++} &= y_{ij++++} && \text{for all } i \text{ and } j, \\
 \hat{m}_{i+k+++} &= y_{i+k+++} && \text{for all } i \text{ and } k, \\
 \hat{m}_{+j+l++} &= y_{+j+l++} && \text{for all } j \text{ and } l, \\
 \hat{m}_{i++++n} &= y_{i++++n} && \text{for all } i \text{ and } n, \\
 \text{and } \hat{m}_{+j++m} &= y_{+j++m} && \text{for all } j \text{ and } m,
 \end{aligned}$$

where "+" indicates summing over the corresponding subscript. In this particular case--a simple pair of relations with no subgroupings--the matrix  $\hat{m}$  is the matrix of fitted probabilities ( $\pi_{ijkln}$ ). Conceptually, this model introduces the following "effects": The "dyad effect"--called  $\lambda_{ij}$ --associated with the margin totals  $(y_{ij++++})$ , is unique to each particular pair--or dyad--of actors. The effect itself is usually not of interest; it is fit to ensure that the probabilities sum to one for each dyad. The "choice effect on relation 1", determined by the margin totals  $(y_{i+k+++}) = (y_{+j+l++})$ , measures the productivity or expansiveness of each actor on relation 1; that is, how likely is actor i to relate to other actors on relation 1. The "chosen effect on relation 2", determined by the margin totals  $(y_{i++++n}) = (y_{+j++m})$ , measures the popularity or attractiveness of each actor on relation 2; that is, how likely is actor i to be related to by other actors on relation 2. These effects can be quantified by model parameters whose sufficient statistics can be expressed in terms of the Y matrix margin totals.

Now, if actors are to be grouped, loglinear models can be considered that investigate differences and similarities in and between these groups of actors, rather than the individual actors. Let us identify group membership by the function  $g(i)$  where

$$g(i) = a, \text{ if actor } i \text{ is in group } a.$$

Loglinear models for fitting grouped data may include terms specifying either group effects, or individual actor effects, or both. For example, the model:

$$[12] [13] [24] [g(1)6] [g(2)5]$$

is composed of a dyad effect--the term [12], an actor choice effect on relation 1--terms [13] and [24], and a group chosen effect on relation 2--terms [g(1)6] and [g(2)5]. This group effect measures the popularity or attractiveness of each group of actors on relation 2. It is determined by the margin totals  $(y_{g(i)++++n}) = (y_{+g(j)++m})$ . These types of models are fit to the Y matrix as above. For each "group term" (e.g., [g(1)6]) in a model, a generalized iterative margin fitting scheme--based on the "margin fitting table"--is used (see discussion in General Procedure section).

Finally, consider the case of a compound pair of relations. The compound pair of relations is defined by two or more component pairs of binary sociomatrices  $X_1^{(n)} = \{x_{ij1}^{(n)}\}$  and  $X_2^{(n)} = \{x_{ij2}^{(n)}\}$ . For example, consider the "positive-negative affect" pair of relations mentioned above. This compound pair is composed of four pairs of relations. We will denote them as follows:

- $X_1^{(1)}$  and  $X_2^{(1)}$  represent the like-dislike relation,
- $X_1^{(2)}$  and  $X_2^{(2)}$  represent the praise-blame relation,
- $X_1^{(3)}$  and  $X_2^{(3)}$  represent the esteem-disesteem relation, and
- $X_1^{(4)}$  and  $X_2^{(4)}$  represent the influence-negative influence relation.

We construct the compound matrices  $X_1^{(+)} = \{x_{ij1}^{(+)}\}$  and  $X_2^{(+)} = \{x_{ij2}^{(+)}\}$  by aggregating or simply summing over component pairs as follows:

$$x_{ijr}^{(+)} = \sum_{n=1}^4 x_{ijr}^{(n)}$$

In order to form the compound  $Y^{(+)}$  matrix, we first construct the  $Y^{(n)}$  matrix for each component pair (i.e.,  $Y^{(n)} = \{y_{ijklmn}^{(n)}\}$ ), then sum over the four  $Y^{(n)}$  matrices so

$$y_{ijklmn}^{(+)} = \sum_{n=1}^4 y_{ijklmn}^{(n)}, \text{ where}$$

$$y_{ijklmn}^{(n)} = \begin{cases} 1, & \text{if } (x_{ij1}^{(n)}, x_{ji1}^{(n)}, x_{ij2}^{(n)}, x_{ji2}^{(n)}) = (k, l, m, n) \\ 0, & \text{otherwise.} \end{cases}$$

Models are fit to the compound pair of relations exactly as outlined for a simple pair of relations, using the compound  $Y^{(+)}$  matrix in place of the Y

matrix discussed above. Note, however, that the sum

$$\sum_{k,l,m,n} y_{ijklmn}^{(+)}$$

does not equal 1, but equals the number of component pairs of relations--4 in the case of this example. The entries of the fitted values matrix  $\hat{m}$ , the  $(\hat{m}_{ijklmn})$ , are no longer the fitted probabilities  $(\hat{p}_{ijklmn})$ , but four times these probabilities.

The following types of data have been discussed: a pair of simple relations or a pair of compound relations, each with or without actor grouping. In each type of analysis, RELTWO computes an  $\hat{m}$  matrix, fitted to the observed Y (or  $Y^{(+)}$ ) matrix, according to a user-specified loglinear model. Using the  $\hat{m}$  matrix, RELTWO calculates fitted sociomatrices  $\hat{X}_1$  and  $\hat{X}_2$ . If the pair of relations is a compound one, RELTWO only computes the fitted aggregated sociomatrices  $\hat{X}_1^{(+)}$  and  $\hat{X}_2^{(+)}$ . If actors have been grouped, RELTWO also computes the "group fitted sociomatrices"  $\hat{X}_1 = (\hat{x}_{ab1})$  and  $\hat{X}_2 = (\hat{x}_{ab2})$  such that:

$$\hat{x}_{abr} = \sum_{i \in a} \sum_{j \in b} x_{ijr}$$

## II. GENERAL PROCEDURE

RELTWO first creates the observed  $g \times g \times (2 \times 2)^2$  Y matrix corresponding to the pair of sociomatrices  $X_1$  and  $X_2$ . If pairs of sociomatrices are to be aggregated, the Y matrices associated with each pair are summed, yielding an aggregated  $Y^{(+)}$  matrix.

When the model is entered, RELTWO creates a margin fitting table for each term in the model. Each margin fitting table is a  $g \times g \times (2 \times 2)^2$  matrix of integers. Each entry in the margin fitting table is associated with an entry in the observed Y matrix. Y matrix entries associated with the same integer value in the margin fitting table are summed to create observed margin totals for each term.

Fitted values are created via an iterative algorithm. RELTWO begins with an initial fitted values matrix  $\hat{m}^{(0)} = (\hat{m}_{ijklmn}^{(0)})$ . Entries of the  $g \times g \times (2 \times 2)^2$  matrix  $\hat{m}^{(0)}$  are all equal to a single value  $v$  such that

$$v = \sum_{k,l,m,n} y_{ijklmn} / 16,$$

where the sum

$$\sum_{k,l,m,n} y_{ijklmn}$$

equals the number of aggregated pairs of sociomatrices.

On each iteration, the margin fitting tables are used to create fitted margin totals using the current fitted values matrix  $\hat{m}^{(t)} = (\hat{m}_{ijklmn}^{(t)})$ . The  $\hat{m}^{(t)}$  matrix is updated so that

$$\hat{m}_{ijklmn}^{(t+1)} = \hat{m}_{ijklmn}^{(t)} \times \frac{(\text{fitted margin total})}{(\text{observed margin total})}$$

for each entry in  $\hat{m}^{(t+1)}$  and  $\hat{m}^{(t)}$ . The observed and fitted margin totals are obtained using the margin fitting table and the Y and  $\hat{m}^{(T)}$  matrices, where T is the requested number of iterations; hence,  $\hat{m}^{(T)}$  is the final iterate.

On each iteration,  $G^2$  and a tolerance value, epsilon, are computed such that:

$$G^2 = \sum_{i < j} \sum_{k,l,m,n} y_{ijklmn} \log (y_{ijklmn} / \hat{m}_{ijklmn}^{(t)})$$

and

$$\text{epsilon} = \sum_{i < j} \sum_{k,l,m,n} | \hat{m}_{ijklmn}^{(t-1)} - \hat{m}_{ijklmn}^{(t)} |$$



The user may choose an appropriate number of additional iterations based on  $G^2$  and epsilon values.

When the requested number of iterations has been completed, RELTWO computes the fitted sociomatrices  $\hat{X}_1$  and  $\hat{X}_2$  (aggregated over pairs of relations and/or groups of actors, if necessary). As output, RELTWO prints these matrices as well as the  $\hat{M}^{(T)}$  matrix and the fitted and observed margin totals.

We note that in order to save computer memory, the [12] model term-- corresponding to the  $(\lambda_{ij})$  parameters is fitted in a different manner than other terms. It is always the last model term to be fit on each iteration. Observed and fitted margin totals are not printed for it.

### III. INPUT

Input to RELTWO consists of the following three types:

- A. Sociomatrices  $X_1$  and  $X_2$
- B. General information
- C. Specifications for each problem

On each run, you may execute one or more problems. Each problem utilizes the same sociomatrices and subgrouping partition, but may fit a different model.

- A. Each sociomatrix  $X_r$  should be in a file in the following form (i.e., an I1 FORTRAN format):

```
010000
101101
110111
101000
111101
001010
```

where each row corresponds to a sending actor, and each column corresponds to a receiving actor. For example, the entry in the 3rd row and the 2nd column represents the presence (if the entry is 1) or the absence (if the entry is 0) of a relation sent from actor 3 to actor 2.

- B. General information for the run is all entered interactively by the user. It consists of:

1. names of files containing sociomatrices
2. the number of actors (i.e., the number of rows and columns in each sociomatrix)
3. the number of groups in actor blocks
4. the subgrouping or aggregation vector--the actual entries of the vector  $g(i)$  discussed above, where  $g(i) = a$  if actor  $i$  is in group  $a$ --the values are entered in the order  $(g(1), g(2), \dots)$
5. whether or not you would like a copy of the observed matrix  $Y$  written to an auxiliary file

You will be prompted for this information.

- C. Problem specifications, also entered interactively, are:

1. the problem title
2. the output file name
3. the model to be fit: Enter the model in the form 12,g134,g256,3456. This represents the model [12] [g(1)34] [g(2)56] [3456]. Spaces don't matter, but each term must be separated by comma and the model must end in a period.
4. the number of iterations: After the number you specify is completed, you will be given the option to request more.

You will also be prompted for this information. After each problem,

you will be given the option to execute another problem. If you choose to do another, you will be prompted for this problem information again.

IV. OUTPUT

RELTWO generates three types of output:

- A. General information
- B. The result of each iteration
- C. Results of requested number of iterations

More specifically:

A. General information includes (for each problem):

- 1. Observed matrices  $X_1$  and  $X_2$  (aggregated over component relations if necessary).
- 2. Observed matrices  $\hat{X}_1$  and  $\hat{X}_2$  (aggregated over component relations and/or subgroups of actors if necessary).
- 3. Observed matrix  $Y$  (aggregated over component pairs of relations if necessary).

B. For each iteration,  $G^2$  and epsilon are printed.

C. Result of requested number of iterations includes (for each problem):

- 1. Observed and fitted values of each fitted margin (the [12] margin excluded).
- 2. Fitted values matrix  $\hat{M}^{\wedge}(T)$ , where T is the total number of iterations performed.
- 3. Fitted values matrices  $\hat{X}_1$  and  $\hat{X}_2$  (aggregated over component relations if necessary).
- 4. Fitted values matrices  $\hat{X}_1$  and  $\hat{X}_2$  (aggregated over component relations and/or subgroups of actors if necessary).

Additionally, RELTWO will output an auxiliary file containing the observed matrix  $Y$  in a one-line-per-dyad format. Each line of the file contains the entries  $y_{ijkimn}$  in the order:

$y_{ij0000} y_{ij0001} y_{ij0010} y_{ij0011} y_{ij0100}$ , etc.

The values are printed using a FORTRAN 15 format.

V. Maximizing Efficiency

In order to maximize efficiency of computer memory allocation, array limits in the program code of RELTWO may be manipulated as follows:

A. Let the limits marked with a "\$" below equal the number of actors your models have:

```
PROGRAM RELTWO(INPUT,OUTPUT,TAPE5=INPUT,TAPE16=OUTPUT)
REAL MC($,$,0:1,0:1,0:1,0:1),FITSUM(4,12)
REAL ML($,$,0:1,0:1,0:1,0:1)
REAL FXG1(3,3),FXG2(3,3),FIT1($,$),FIT2($,$)
INTEGER OBSUM(4,12),LARGEST(4),IY($,$,0:1,0:1,0:1,0:1)
INTEGER MFT(4,$,$,0:1,0:1,0:1,0:1),A,B,RELNUM
INTEGER MARG(4,6),G($),ENT,T,FIT12,CATS
INTEGER IX(2,$,$),IXG(2,3,3),IXT(2,$,$)
CHARACTER*8 TERM(4)
```

B. Let the limits marked with a "\$" below equal the margin limit for your next run. The margin limit is the largest number of terms other than [12] that you will fit in one model.

```
PROGRAM RELTWO(INPUT,OUTPUT,TAPE5=INPUT,TAPE16=OUTPUT)
REAL MC(18,18,0:1,0:1,0:1,0:1),FITSUM($,12)
REAL ML(18,18,0:1,0:1,0:1,0:1)
REAL FXG1(3,3),FXG2(3,3),FIT1(18,18),FIT2(18,18)
```

```

INTEGER OBSUM($,12),LARGEST($),IY(18,18,0:1,0:1,0:1,0:1)
INTEGER MFT($,18,18,0:1,0:1,0:1,0:1),A,B,RELNUM
INTEGER MARG($,6),G(18),ENT,T,FIT12,CATS
INTEGER IX(2,18,18),IXG(3,3),IXT(2,18,18)
CHARACTER*8 TERM($)

```

C. Let the limits marked with a "\$" below equal the maximum number of margins per term in any model you will fit in the next run.

```

PROGRAM RELTWO(INPUT,OUTPUT,TAPES=INPUT,TAPE16=OUTPUT)
REAL MC(18,18,0:1,0:1,0:1,0:1),FITSUM(4,$)
REAL ML(18,18,0:1,0:1,0:1,0:1)
REAL FXG1(3,3),FXG2(3,3),FIT1(18,18),FIT2(18,18)
INTEGER OBSUM(4,$),LARGEST(4),IY(18,18,0:1,0:1,0:1,0:1)
INTEGER MFT(4,18,18,0:1,0:1,0:1,0:1),A,B,RELNUM
INTEGER MARG(4,6),G(18),ENT,T,FIT12,CATS
INTEGER IX(2,18,18),IXG(3,3),IXT(2,18,18)
CHARACTER*8 TERM(4)

```

D. If you plan to assign actors to groups, let the limits marked with a "\$" below equal the maximum number of groups to which the actors in your data may be assigned. If you do not plan to group actors, let the limits marked below equal the total number of actors.

```

PROGRAM RELTWO(INPUT,OUTPUT,TAPES=INPUT,TAPE16=OUTPUT)
REAL MC(18,18,0:1,0:1,0:1,0:1),FITSUM(4,6)
REAL ML(18,18,0:1,0:1,0:1,0:1)
REAL FXG1($,$),FXG2($,$),FIT1(18,18),FIT2(18,18)
INTEGER OBSUM(4,6),LARGEST(4),IY(18,18,0:1,0:1,0:1,0:1)
INTEGER MFT(4,18,18,0:1,0:1,0:1,0:1),A,B,RELNUM
INTEGER MARG(4,6),G(18),ENT,T,FIT12,CATS
INTEGER IX(2,18,18),IXG($,$),IXT(2,18,18)
CHARACTER*8 TERM(4)

```

VI. EXAMPLE OF INPUT

The following are two examples of interactive sessions with RELTWO. Each consists of a series of computer prompts and sample responses.

SAMPLE 1:

First, let us consider the case of a simple pair of relations (i.e., not aggregated), with no subgroupings of actors. For this example, we will use the "like" and "antagonism" binary relations discussed in Section I. The "like" relation is defined by the 18x18 binary sociomatrix:

```

011010000000001000
001011000000000000
010000110000000000
011010000000000000
010101000000000000
010000100010000000
001110000000000000
000000001100100000
000000010010000100
000000011000100000
000000011001000000
000000010100100000
000000000101100000
010000000000100001
0000000010000001011
000000000100000101
000000000100000110

```

stored in a file called XLIKE. The 18x18 sociomatrix defining the "antagonism" relation is stored in a similar file called XANTAGO. In order to fit the loglinear model

```
[12] [13] [24] [15] [26] [34]
```

we would answer the computer prompts as follows (Note that computer prompts are capitalized; responses are underlined):

WHAT WOULD YOU LIKE TO CALL THE OUTPUT FILE? sample1

WHAT TITLE WOULD YOU LIKE FOR THIS RUN sample1: like/antagonism relations  
 WOULD YOU LIKE TO OUTPUT THE OBSERVED Y MATRIX TO A FILE? n  
 ENTER NUMBER OF ACTORS? 18  
 ENTER NUMBER OF GROUPS? 18  
 WOULD YOU LIKE TO AGGREGATE OVER X MATRICES? no  
 ENTER NAME OF FILE CONTAINING FIRST X MATRIX? xlike  
 ENTER NAME OF FILE CONTAINING SECOND X MATRIX? xantago  
 ENTER NUMBER OF ITERATIONS DESIRED? 6  
 ENTER MODEL IN FORM: G13,G24,34.  
 ? 12,13,24,15,26,34.

ITERATION NUMBER= 1  
 G2= 510.14542 EPS=172.44215380

ITERATION NUMBER= 2  
 G2= 510.14597 EPS= .18702939

ITERATION NUMBER= 3  
 G2= 510.14486 EPS= .00358656

ITERATION NUMBER= 4  
 G2= 510.14486 EPS= .00000720

ITERATION NUMBER= 5  
 G2= 510.14486 EPS= .00000005

ITERATION NUMBER= 6  
 G2= 510.14486 EPS= .00000000

WOULD YOU LIKE TO DO ANY MORE ITERATIONS(Y OR N)? n  
 WOULD YOU LIKE TO TRY ANOTHER MODEL WITH THIS DATA? n

SAMPLE 2:

Next, consider the "positive affect-negative affect" compound pair of relations also discussed in Section I. The four pairs of component relations are defined by eight binary sociomatrices. These sociomatrices are in files similar to the XLIKE file of sample 1. The files are called XLIKE, XANTAGO, XPRAISE, XBLAME, XESTEEM, XDISEST, XINFLUE and XNEGINF.

The 18 actors are to be grouped so that the first 7 are in group one, the next 7 are in group two, and the remaining 4 are in group three. First, the loglinear model

$$[12] [g(1)3] [g(2)4] [g(1)5] [g(2)6]$$

will be fit, and then the model

$$[12] [g(1)36] [g(2)45] [g(1)45] [g(2)36]$$

will be fit. Note that when we fit a second model in the same computer run, the actor subgrouping as well as the data remains the same.

WHAT WOULD YOU LIKE TO CALL THE OUTPUT FILE? sample2  
 WHAT TITLE WOULD YOU LIKE FOR THIS RUN  
sample2: compound positive/negative affect relations  
 WOULD YOU LIKE TO OUTPUT THE OBSERVED Y MATRIX TO A FILE? y  
 WHAT WOULD YOU LIKE TO CALL THIS FILE? ymatrix  
 ENTER NUMBER OF ACTORS? 18  
 ENTER NUMBER OF GROUPS? 3  
 ENTER AGGREGATION VECTOR ON ONE LINE SPACED BETWEEN  
 ? 1 1 1 1 1 1 1 2 2 2 2 2 2 2 3 3 3 3  
 WOULD YOU LIKE TO AGGREGATE OVER X MATRICES? yes  
 HOW MANY PAIRS OF RELATIONS? 4

PAIR 1: FOR RELATION 1....  
 ENTER AN X MATRIX  
 ? xlike

PAIR 1: FOR RELATION 2....  
 ENTER AN X MATRIX  
 ? xantago

PAIR 1: FOR RELATION 1....  
 ENTER AN X MATRIX  
 ? xpraise

PAIR 1: FOR RELATION 2.....  
ENTER AN X MATRIX  
? xblame

PAIR 1: FOR RELATION 1.....  
ENTER AN X MATRIX  
? xesteem

PAIR 1: FOR RELATION 2.....  
ENTER AN X MATRIX  
? xdisest

PAIR 1: FOR RELATION 1.....  
ENTER AN X MATRIX  
? xinflue

PAIR 1: FOR RELATION 2.....  
ENTER AN X MATRIX  
? xneqinf

ENTER NUMBER OF ITERATIONS DESIRED? 3  
ENTER MODEL IN FORM: G13,G24,34.  
? 12,q13,q24,q15,q26.

ITERATION NUMBER= 1  
G2= 1539.05872 EPS=686.65307544

ITERATION NUMBER= 2  
G2= 1539.05872 EPS= .000000000

ITERATION NUMBER= 3  
G2= 1539.05872 EPS= .000000000

WOULD YOU LIKE TO DO ANY MORE ITERATIONS(Y OR N)? n  
WOULD YOU LIKE TO TRY ANOTHER MODEL WITH THIS PAIR?  
WHAT TITLE WOULD YOU LIKE FOR THIS RUN?  
? sample3: another model with positive/negative affect data

WHAT WOULD YOU LIKE TO CALL THE OUTPUT FILE? sample3  
ENTER NUMBER OF ITERATIONS DESIRED? 3  
ENTER MODEL IN FORM: G13,G24,34.  
? 12,q136,q245,q145,q236.

ITERATION NUMBER= 1  
G2= 1425.88069 EPS=724.74525970

ITERATION NUMBER= 2  
G2= 1425.92563 EPS= 5.40332867

ITERATION NUMBER= 3  
G2= 1425.91900 EPS= .20101111

WOULD YOU LIKE TO DO ANY MORE ITERATIONS(Y OR N)? y  
HOW MANY MORE? 7

ITERATION NUMBER= 4  
G2= 1425.91802 EPS= .01368758

ITERATION NUMBER= 5  
G2= 1425.91795 EPS= .00106678

ITERATION NUMBER= 6  
G2= 1425.91794 EPS= .00008787

ITERATION NUMBER= 7  
G2= 1425.91794 EPS= .00000719

ITERATION NUMBER= 8  
G2= 1425.91794 EPS= .00000058

ITERATION NUMBER= 9

G2= 1425 91794 EPS= .00000005

ITERATION NUMBER= 10

G2= 1425 91794 EPS= .00000000

WOULD YOU LIKE TO DO ANY MORE ITERATIONS(Y OR N)? n  
WOULD YOU LIKE TO TRY ANOTHER MODEL WITH THIS DATA? n

#### ACKNOWLEDGEMENTS

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Sampson, S.F. (1968), A Novitiate in a Period of Crisis: An Experimental and Case Study of Social Relationships, unpublished Ph.D. thesis, Cornell University, Dept. of Sociology.

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#### NETWORK NOTEBOOK (cont'd from page 13)

**HEALTH CARE UTILIZATION OF CANADIAN 'SNOWBIRDS'** (Victor Marshall, Beh Sci, Toronto; Richard Tucker, Psych, Central Florida; Charles Longino, Miami; Larry Mullins, South Florida). Thru a survey mailed to 4400 (mostly elderly) respondents, data is being gathered on the support nets & health care utilization of Canadians who spend large portions of any year in Florida. The study is investigating health status & use of health services in Canada & Florida, as well as the 'strategic' approaches respondents are taking in utilizing the 2 health care systems.

#### NEW MEMBERS

KAREN ALTERGOTT, Family Res Inst, Purdue U, W Lafayette IN USA 47906. 317-494-7859.

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JOHN HIRDES, 33 Nichol St, Elora Ont Canada NOB 1S0. 519-846-0225.

EDWARD YARRISH, 749 N 28 St, Allentown PA USA 18104. 215-434-2648.

{Actually we have lots more, but we only got the idea for this just before we went to press.}

#### JOURNAL NEWS

**CONTINUITY AND CHANGE** is concerned with past societies in a diverse range of European & American cultures, & time-spans. It especially seeks to publish studies that combine methodologies from various social sciences, both qualitative & quantitative. Topics of special interest include Household, family, kinship & social relations in their cultural, demographic & economic contexts (including connections with local economies; gender-specific roles of work, migration & life-cycle); Social structures at the level of community, region or nation (including long-term demographic change, especially as related to major transformations such as industrialization, urbanization or transitions from medieval to modern; integrated studies of changes in economic organizations; relations between occupations, sexes & status groups). The first issue includes Peter Laslett on microsimulation of kinship networks.

*Interactive Computer Graphics and Network Analysis: VIEW\_NET*

*A.S. Kloudahl, Department of Sociology, Arts, The Australian National University, Canberra, Australia*

Visual representations can facilitate interpretation of complex network data and help to convey the key points of research to relevant audiences. The work described here follows on from the author's earlier interest in the potential of interactive computer graphics for facilitating network analysis (Kloudahl, 1981). The main motivation for the work is the belief that viewing a large social network as a whole, from different perspectives in the context of interactive analyses, can provide a stimulus to theoretical insight not otherwise available in a world in which individuals - social scientists not excepted - are quite literally the captives of their personal networks and rarely able to see beyond to the larger networks in which all - or at least most - persons are enmeshed.

Two approaches to the development of computer graphics capabilities for network analysis can be identified. The first is to aim for basic tools that run on readily available personal computers. The second is to work towards more advanced tools using very sophisticated interactive graphics systems. Each approach has advantages as well as disadvantages. More importantly, the two are not mutually exclusive: In fact, they can be seen as ends of a spectrum, and likely to merge with the progress of computer technology.

The second approach was chosen here and an Evans and Sutherland PS 300 system with associated host employed. The PS 300 is a vector, calligraphic stroke, random scan or line drawing computer graphics system. (The characteristics of random in contrast to raster systems, i.e. TV technology, have been discussed elsewhere, e.g. Newman and Sproull, 1979; Foley and Van Dam, 1982.) In brief, the PS 300 system is capable of producing 10,000 to 90,000 vectors, depending on length, allows real-time interaction, and displays images on a screen with 8192 x 8192 resolution. (By comparison, in recent times 1024 x 1280 has been considered "high" resolution for raster systems.) Real-time interaction means that a visual image may be manipulated instantaneously, for example in response to turning a dial. In this context it might be noted that the PS 300 represents a move to distributed graphics, and thus allows most picture generating and manipulation to be carried out independently of the host.

Although this level of sophistication is not inexpensive, for some time Evans and Sutherland systems have been the systems of choice for many molecular modelling projects (especially for large proteins) and thus availability is more widespread than might be imagined. In part, this is due to the fact that these systems build on the early recognition of Sutherland (1963), in laying the foundation for the computer graphics era two decades ago, of "(t)he power obtained from a small set of generalized functions ...". Further, though some less expensive graphics systems may seem attractive at first glance, the relative stability of this upper end of the market would seem to reduce the likelihood that substantial investments of resources for programs would have to be written off because of market turbulence and product discontinuations. The same applies to the VAX, PDP and - more recently - IBM hosts to which a PS 300 system may be connected. Moreover as the more advanced computer technology and programming methods of the present tend to become the standard of the less expensive technology of the future, learning how to operate a more advanced system may well provide knowledge and skills useful for a relatively longer period of time.

Typically an interactive graphics program for network analysis on this kind of system would contain two basic elements: the PS 300 commands written in the system's high-level language, and one or more host-resident programs written in Fortran 77 or PASCAL. For example, a program to display and manipulate a network image would be contained in the PS 300, and the host would contain a program for preparing the data for down-loading to the PS 300, one or more cliquing programs, a program for calculating structural properties, such as centrality, and so on. Various interfaces to allow communication between PS 300 and host would also be developed.

In harnessing the power of computer graphics for network analysis, two stages might be suggested. The first stage is to develop an input/manipulation/output package that would facilitate the input of network data, allow the manipulation of this data in real time, and make possible the output of good visual representations for papers, articles, presentations and so on. That is, the first requirement is to be able to produce with minimal pain visual representations to help to convey the results of research on social networks. The second stage is to develop the interfaces (and host resident programs) to permit fully interactive data analysis. Thus, once a basic graphics package has been developed, the interfaces allowing network data to be passed back and forth between visual display device and network analysis programs can be written. The focus here is on the first stage.

VIEW\_NET is an input/manipulation/output interactive graphics package. When loaded a menu appears on the screen, and the use of a stylus and tablet allows the selection of different modes. In the present version (1.5) there is a 'Draw/Input' mode, a 'Rotate-Translate-Scale' mode, a 'Pick & Move' mode, and a 'Copy/Output' mode.

The *Draw/Input* mode uses function keys, and stylus and data tablet, to allow the user to draw a network consisting of points and lines on the screen. Once drawn a network can be sent to the host to allow various manipulations and/or calculations to be carried out. For example, when developing a new measure of a structural property of social networks, one might draw different networks on the screen, send each to the

2.

host for calculation of the proposed measure, and then observe how it behaves for different structures. As another example, one might find a network diagram in an article or book, use the *Draw/Input* mode to enter the data visually, and then carry out additional calculations or analyses using one of the currently available network analysis packages. For example, at the time Coleman wrote The Adolescent Society (1961) very few cliquing algorithms were available and a very simple definition of cliques was employed. Accordingly, if one wished to re-examine these data with one of the more recent cliquing programs, it would be a simple matter to input the data directly from the diagrams provided by Coleman (1961:175-182), as illustrated for his Figure 7.2.1 in the appended *Draw/Input* Figure and then to send the data to the host for further analyses. Also, this mode might be used to input network data in a digitalizer fashion in preference to the slower, and more error-prone, keyboard approach.

The *Rotate-Translate-Scale* mode allows the visual representation of a social network to be rotated and translated, on any axis, and to be scaled, in real time. Either continuous manipulation is possible, using dials, or manipulation in discrete intervals by picking the appropriate menu operations and values. The usefulness of this kind of manipulation comes from being able to view complex relational data from different perspectives, so to speak. For example, one might display a network in which positions were determined by the occupation, education, and income of respondents, for comparison with a representation in which the position coordinates were determined by wealth, political party affiliation and class. The *Rotate-Translate-Scale* Figures show test data with and without the 'labels on' switch activated.

The *Pick & Move* mode allows nodes to be "picked" and moved. In the simplest case, after data from a large social network has been run through various clustering-cliquing programs, etc. and returned for display, the stylus can be used to pick a particular node, the relevant dials used to move the node and its attached bonds to a new position, instantaneously in real time. Thus, an otherwise unintelligible visual representation of a large and complex social network can be rearranged in a relatively short time to make its salient structural features more readily apparent. The final representation may then be sent to a plotting program, perhaps after using the *Rotate-Translate-Scale* mode to determine the best angle of view and scale, and a publication quality plot produced. Alternatively, of course, the best angle of view might be determined first, and then any desired repositioning carried out. The *Pick & Move* Figures illustrate the four (small) viewports in this mode, before and after the nodes were rearranged.

The *Copy/Output* mode is intended to allow a user to choose the particular format for outputting the results of an interactive session, e.g. a listing, a PS 300 file for subsequent use, or a file formatted for input to a plotter. Although networks are represented as points and lines on the screen, a plotter may be instructed to draw nodes of larger diameter, i.e. circles or spheres, and thicker bonds as well. The remaining Figure shows the result of a simple rearrangement of nodes and bonds, together with the *Copy/Output* menu.

VIEW\_NET can be used by itself as a means for network data input, as a way of manipulating network data, and as an aid for obtaining high quality visual representations of social networks for papers, articles, books, and presentations. Equally important, it can be interfaced with other network analysis packages and programs to provide a powerful new tool for interactive, visually-assisted analysis of complex data on social networks.

VIEW\_NET is written in the Evans and Sutherland programming language. It is capable of handling a virtually unlimited number of nodes and bonds. (The present version, 1.4, allows up to about 2000 nodes, though this could be increased quite easily.)

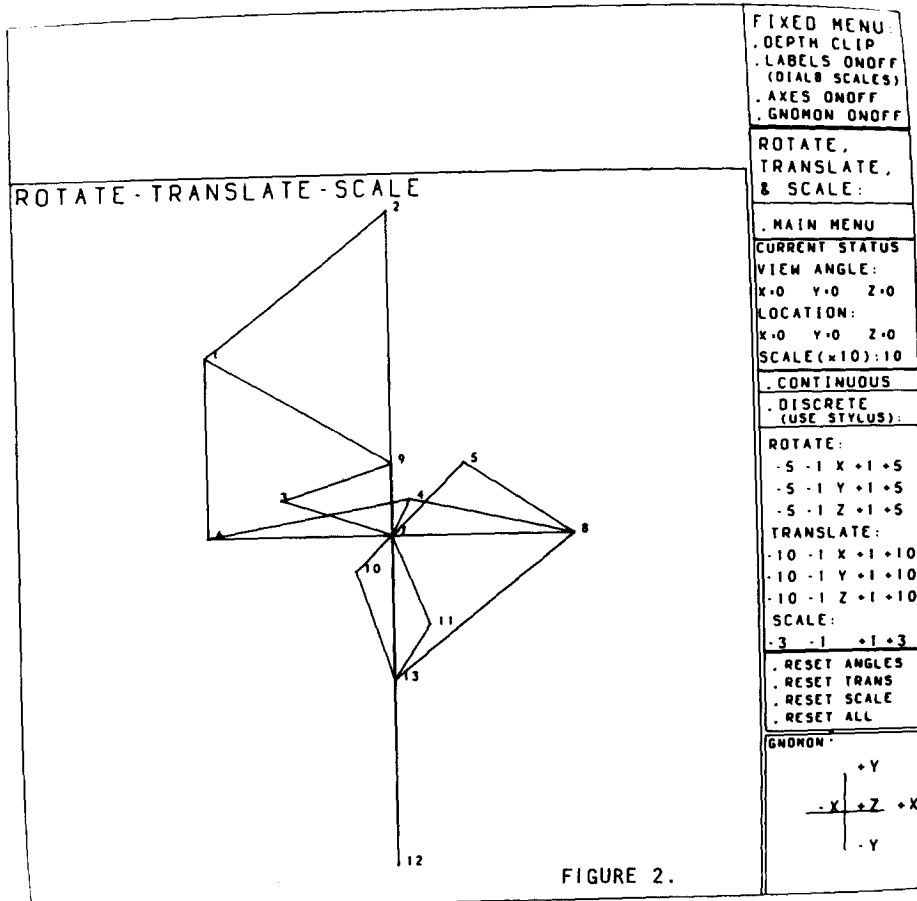
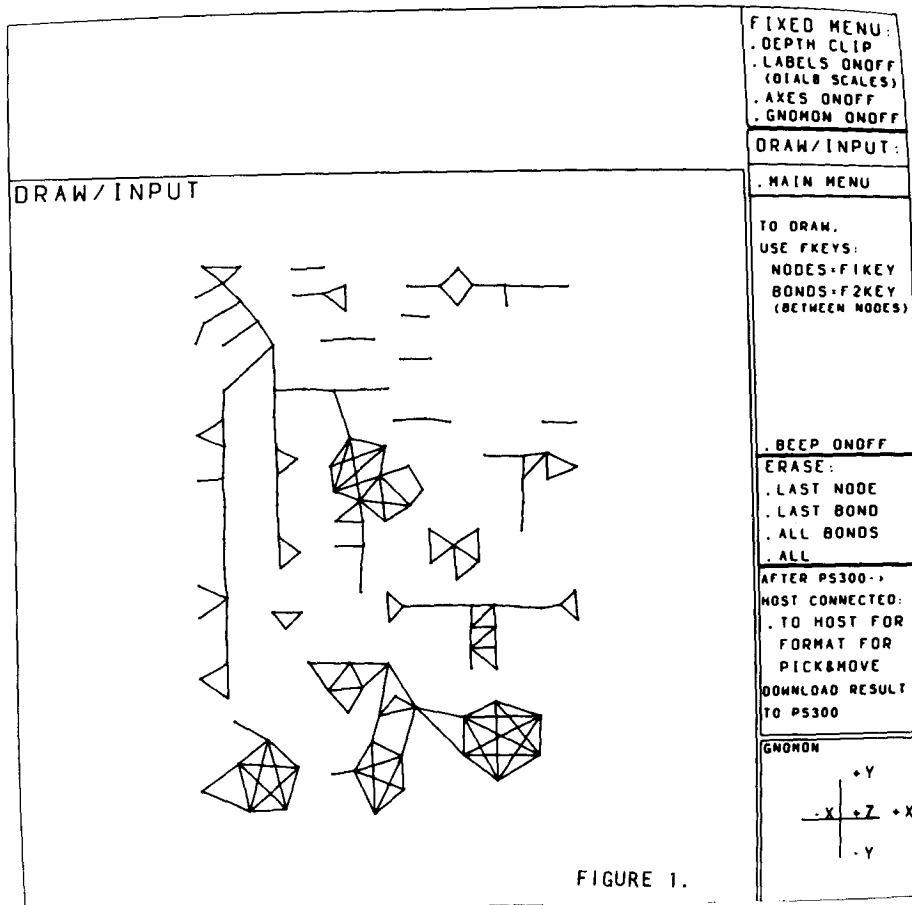
The author would like to thank Lin Freeman and Steve Seidman for very helpful comments in earlier discussions.

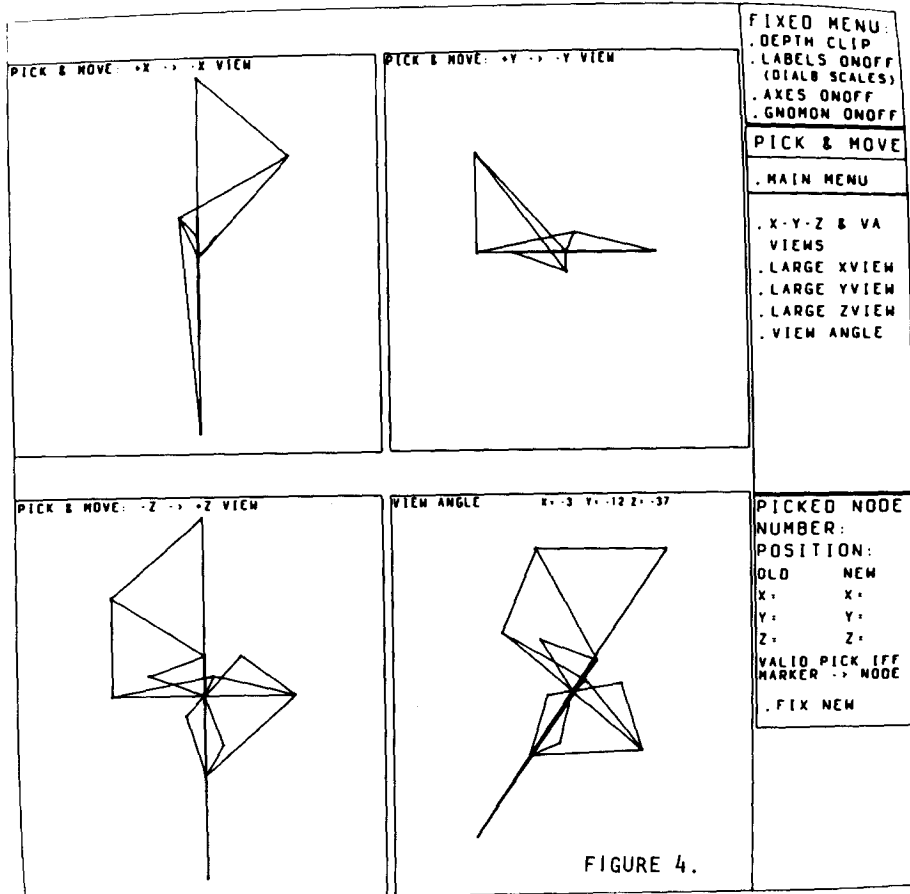
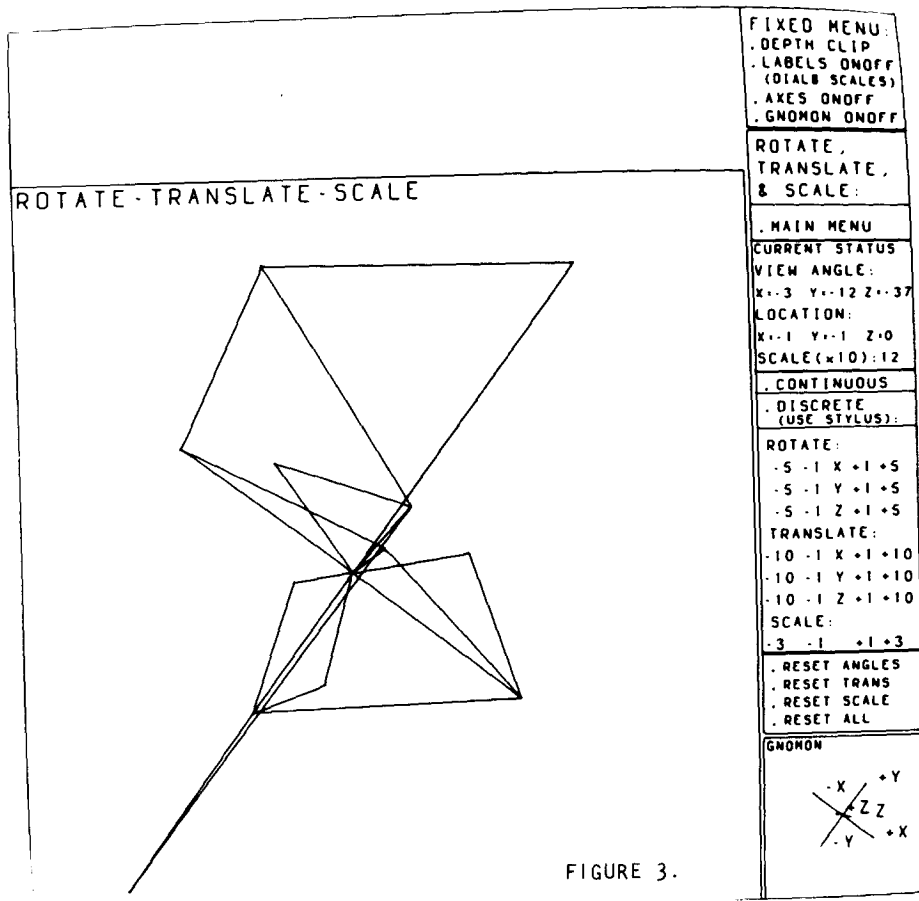
#### Reference

- Coleman, J.S. 1961. The Adolescent Society: The Social Life of the Teenager and its Impact on Education. New York: Free Press.
- Foley, J.D. and A. Van Dam. 1982. Fundamentals of Interactive Computer Graphics. Reading, MA: Addison-Wesley.
- Klov Dahl, A.S. 1981. "A note on Images of Networks". Social Networks 3:197-214.
- Newman, W.N. and R.F. Sproul. 1979. Principles of Interactive Computer Graphics. New York, NY: McGraw-Hill.
- Sutherland, I.E. 1963. "Sketchpad: A Man-Machine Graphical Communication System". AFIPS, SJCC Conf. Proc. 23:329-346.

NOTE: In the Figures that follow black and white have been reversed to enhance viewability. (The monochrome version of the PS 300 draws white lines on a dark background.)







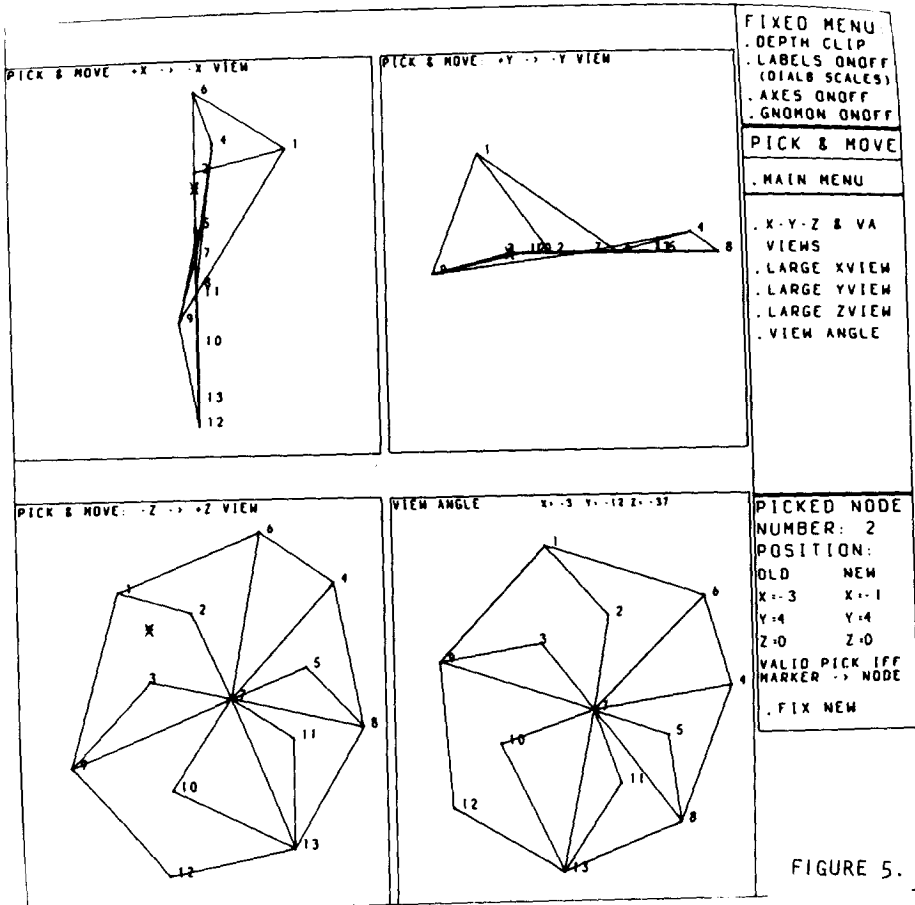


FIGURE 5.

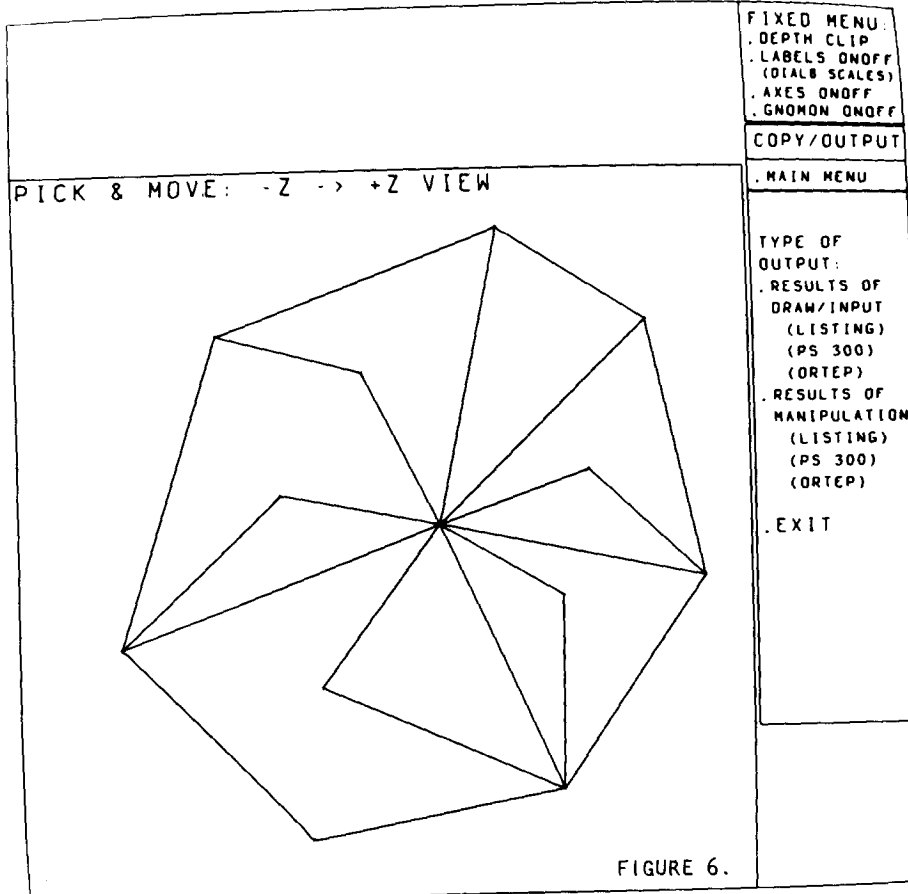


FIGURE 6.

## **STRUCTURE**

**Sociometric Indices, Cliques, Structural Equivalence, Density Tables,  
Contagion, Structural Autonomy, and Equilibria in Multiple Network Systems**

March 1986, Version 3.0

For mainframes to micros, the new version of STRUCTURE is a general purpose program implementing basic principles of network analysis for teaching and empirical research. Version 3 refines and extends the 1976 and 1982 versions of the program. Control codes executing Version 3 are merely additions to the codes that ran Version 2 so files set up to run under Version 2 will also run under Version 3. You receive the program in three forms: (a) **STRUC.FOR** is an ASCII file containing program source code written in rudimentary FORTRAN77 with extensive comment cards to analyze systems of up to 150 actors in 9 networks. The code is self-contained, making no external calls for eigenvector calculations, and ready to send by modem to your mainframe for compilation and execution. (b) **STRUC.EXE** is an executable file ready to provide all features of the mainframe program on an IBM microcomputer running MS-DOS 2.0 or higher with 256K or more of available RAM. Apart from its runtime messages and interactive front end (merely type "struc" and then answer questions to define files or ports for program input, printout, and card image data output), STRUC.EXE is compiled directly from the mainframe source code configured to describe up to 50 actors in 4 networks. (c) **BSTRUC.EXE** is another compilation ready to run under MS-DOS 2.0 or higher. This is identical to STRUC.EXE except that it is bigger, describing up to 125 actors in 2 networks and requiring 520K of available RAM. The choice of implementations is yours. Compiling STRUC.FOR on a mainframe yields virtually instantaneous results on even the largest analysis, but involves the inconvenience of modems and charges for computer time. STRUC.EXE and BSTRUC.EXE can be used with the word and data processing convenience of a microcomputer, but leave you staring at the screen for a while. For example, an analysis of the network among 32 persons -- which involves reading in the data, searching for indirect connections of up to 7 links, scaling relations, computing distances, cluster analyzing to detect structurally equivalent individuals, generating several network indices describing the form of relations in which each person is involved, and printing all of this to RAM drive (RUN7 in the example data sets) -- requires a little over five minutes to complete on an IBM PC without an 8087 chip, about three minutes on an IBM PC with an 8087 chip, and just over a minute on an enhanced IBM AT.

### **Program Output**

The program provides a rich assortment of models. **RELATIONS** -- Square tables of any data can be analyzed as networks or raw and scaled path distances can be derived from sociometric choice data. **NETWORK INDICES** -- Various network indices are available to describe the form of relations in which each actor is involved (including size, density, multiplexity, range, connectivity, aggregate prominence, reflected prominence, primary form, and secondary form). **CLIQUES** and **STRUCTURAL EQUIVALENCE** -- Hierarchical cluster analyses of distances based on raw relation patterns, transformed relation patterns, or dyadic interaction are available to identify cliques or positions jointly occupied by structurally equivalent actors. **SUBGROUP TESTING** -- Standard deviations, correlations, and principal components indicate the structural equivalence of actors hypothesized to jointly occupy a

## STRUCTURE

position. Correlations between position and actor are also provided, indicating the extent to which each occupant is an ideal indicator of the position. **DENSITY TABLES** -- Multiple network density tables are available to describe structure at the level of network subgroups. Dichotomizing the densities into binary data transforms the density tables to image matrices in a blockmodel of the system. Tables are reported with subgroup and person specific indicators of structural equivalence. **SOCIAL CONTAGION** -- Normative response data are available under principles of cohesion and structural equivalence. Given observed response variables, normative responses expected of each actor are derived with a detailed breakdown of each alter's contribution to the expected normative response. Missing data on the observed response variables can be imputed from the available data and network structure and checks on degree of imputation are output. **STRUCTURAL AUTONOMY** -- Detailed analysis of each actor's ego network within the larger system is available, describing the aggregate constraint on entrepreneurial opportunities available to the individual with a breakdown of the extent to which specific relationships limit those opportunities. **EQUILIBRIA** -- The equilibrium structure of observed networks can be generated under alternative exchange and system stratification assumptions determining the power of individuals across multiple networks. Network indices and subgroups are output to describe equilibrium network structure.

In addition to printed output, card images of the network data and indices generated in an analysis can be written to an output file. This output can be merged with other data so that general purpose data analysis packages can be used to study connections between network structure and nonnetwork data on attitudes, attributes, and behaviors.

### Program Manual Contents

The program operates from a batch file containing a parameter card and data for each of up to 99 separate analyses in a single run. Each class of models provided by the program is described in a chapter of the manual:

<b>Chapter 1. Running the Program</b> .....	1
An Initial Example Run	
Analysis card	
Parameter card	
Title card	
Identification data	
Network data	
<b>Chapter 2. Relations and Network Indices</b> .....	7
Measures of relationship	
Measures of network structure	
Relation codes on the parameter card	
Writing results to an output file	
<b>Chapter 3. Network Subgroups</b> .....	17
Interpreting the cluster analyses	
Detecting cliques	
Detecting structural equivalence	
Subgroup codes on the parameter card	
Writing results to an output file	

## STRUCTURE

<b>Chapter 4. Density Tables and Blockmodels</b> . . . . .	<b>27</b>
Subgroups and summary network structure	
Structural equivalence indicators	
Defining a density table run	
<b>Chapter 5. Social Contagion</b> . . . . .	<b>33</b>
Normative response under cohesion and structural equivalence	
Coordination among alters	
Missing data	
Defining a social contagion run	
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<b>Chapter 6. Structural Autonomy</b> . . . . .	<b>43</b>
Network constraint	
Hierarchy and strain in the distribution of constraint	
Defining a structural autonomy run	
Writing results to an output file	
<b>Chapter 7. Equilibrium Network Structure</b> . . . . .	<b>53</b>
The basic model	
Social structure and substitutability in exchange	
Exchange within and across networks	
Defining an equilibrium run	
<b>Appendix (Technical Notes)</b> . . . . .	<b>61</b>
<b>References</b> . . . . .	<b>67</b>
<b>Listing of Example Data</b> . . . . .	<b>71</b>

### Obtaining the Program

A copy of the program can be obtained by requesting Technical Report #TR2 from Ms. Sally Otis at the Center for the Social Sciences, Columbia University, New York, NY 10027 (212-280-3093). Accompany program requests with a check for \$25.00 made out to the "Research Program in Structural Analysis" to help pay for mailing and duplication costs.

You will be sent a manual, reference sheet, and microcomputer diskettes containing the program source code STRUC.FOR, executable files STRUC.EXE and BSTRUC.EXE, input data files defining 26 example analyses, and program printout from the example analyses. The example data are by and large taken from illustrative and substantive analyses in *Toward a Structural Theory of Action*, Burt, 1982, and *Applied Network Analysis*, Burt and Minor, 1983. In addition to these books, Knoke and Kuklinski's (1982) *Network Analysis* on practical issues in carrying out a network analysis is a handy reference for interpreting the network analysis output. The source code, example data, and printout are ASCII files on the microcomputer diskettes. The example data are in files with .DAT extensions and the printouts are in files with .PRT extensions. For convenient reference, the example data are also listed at the end of the program manual. Unless otherwise specified, the diskettes are in DOS 360K format for an IBM microcomputer. Formats for a variety of CP/M machines are also available, but the STRUC.EXE and BSTRUC.EXE files will only run under MS-DOS 2.0 or higher.

## Note -- Easy Access to Two Important Network Data Sets

### The Medical Innovation Network Data

This data set has been prepared to facilitate the efforts of colleagues interested in taking advantage of the network data obtained in the Coleman, Katz and Menzel's (1966) classic sociological study, *Medical Innovation*. The codebook describes the variables in two microcomputer diskette files; SOURCE.DAT and ANALYSIS.DAT. Unless otherwise specified, the diskette is in DOS 360K format for an IBM microcomputer. Formats for a variety of CP/M machines are also available. The diskette and codebook are available by requesting Technical Report #TR3 from the Center for the Social Sciences, Columbia University, NY NY 10027 (212-280-3093). Accompany your request with a \$10 check made out to the "Research Program in Structural Analysis" to help pay for mailing and duplication costs.

#### SOURCE.DAT

The data in SOURCE.DAT have been selected from multiple card decks of the original data reported in *Medical Innovation*. They have been selected to represent the importance and diversity of evidence underlying general conclusions advanced in the study. The recorded data have been checked by comparing cells and marginals in crosstabulations of these data against corresponding frequencies in the original codebooks and the published report, *Medical Innovation*. Some discrepancies between data, codebook frequencies and published report were found, but they were very rare and relatively minor. The most serious involved partnerships. One more doctor in Galesburg and two fewer in Quincy operated practices sharing their offices or waiting rooms with other doctors than was reported on pages 73-74 of *Medical Innovation*.\*

Five classes of data are reported: respondent identification, adoption data, sociometric choice data, data on the respondent's contact with the medical profession, and data on his medical practice. A great deal more information is available than is reported here or in *Medical Innovation*. Scholars interested in obtaining additional data are referred to the Roper Center at the University of Connecticut. The original nineteen data decks and associated materials are archived there along with data obtained in several other studies conducted through the Bureau of Applied Social Research at Columbia University. The original respondent identification codes are reported here to facilitate merging these data with additional data. The study is referenced in the original materials as the "Doctor's Innovation Study" and the "FOSI Study" (for Flow of Scientific Information in the Medical Profession). The sociometric choices reported here are taken from the basic sociometric deck in Series B of the original materials. The remaining data are selections from card decks in Series A as indicated in the codebook below.

Variables are referenced in the codebook below by the column(s) in which they occur, the codes they contain, the question wording eliciting the codes, the column and card deck in the original materials from which they have been obtained, and the relative frequencies with which each code occurs among doctors in the prescription sample versus all 216 interviewed doctors. Frequencies are given in parentheses to the right of each code number and are

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\*Several graduate students assisted in this task, but Rumi Kato Price deserves a special note of appreciation for her final, painstaking review of the data at the University of California, Berkeley's Survey Research Center.

### Easy Access to Two Important Network Data Sets

separated by a slash: (prescription sample/total sample). One card is listed for each of 246 potential respondents; 125 respondents in the prescription sample, 91 respondents not in the prescription sample, 12 informants, and 18 nonrespondents who were the object of sociometric choices in at least one network from two or more respondents (see column two in the codebook).

#### ANALYSIS.DAT

The additional data in ANALYSIS.DAT are variables created for the reanalysis of the *Medical Innovation* data for evidence of social contagion (reported in Center Preprint #P106, "Contagion and Innovation, Cohesion Versus Structural Equivalence," a copy of which is appended to this codebook). Four card images are reported for each of the 130 general practitioners, internists and pediatricians studied as potential adopters in *Medical Innovation*. The first card image is merely a duplication of the physician's data in SOURCE.DAT. The second lists a variety of contagion variables keyed to the text of "Contagion and Innovation" for explanation. The third card image lists data on the spread of adoptions among the physician's structural equivalence alters over time and the fourth lists the same data for his cohesion alters. These data are provided because of their general analytical interest and the considerable software required to produce them from the raw data.

### The 1985 General Social Survey Network Data

This data set has been prepared to facilitate the efforts of colleagues interested in taking advantage of the network data obtained in the 1985 General Social Survey. Most of the codebook pages are taken from the 1985 GSS Codebook. They define the variables in the microcomputer diskette file NETWORK.DAT containing the network data on all 1534 respondents to the 1985 GSS and some background data on each respondent selected from the complete 1985 GSS data set (see the FORTRAN code used to read the raw data tape, appended here and contained in the diskette file NETWORK.FOR). Unless otherwise specified, the diskette is in DOS 360K format for an IBM microcomputer. Formats for a variety of CP/M machines are also available. The diskette and codebook are available by requesting Technical Report #TR1 from the Center for the Social Sciences, Columbia University, NY NY 10027 (212-280-3093). Accompany your request with a \$10 check made out to the "Research Program in Structural Analysis" to help pay for mailing and duplication costs.

The complete 1985 GSS data set is available on computer tape through the Inter-University Consortium for Political and Social Research. Persons whose institutions are not members of the Consortium can obtain the data and codebooks from the Roper Public Opinion Research Center (Box U-164R, University of Connecticut, Storrs, CT 06268). Further details on the GSS can be obtained by writing to Dr. Tom Smith, National Opinion Research Center, University of Chicago, Chicago, IL 60637, or by telephoning Dr. Smith at 312-962-1200.

Further details about the 1985 GSS network items can be obtained from the following technical reports available from Dr. Smith at NORC or from the Reprint Librarian at the Center for the Social Sciences:

Burt, R. S. (1984) "Network items and the General Social Survey," with Addendum giving the final form of the items adopted for the 1985 GSS, 52 pages. (GSS Report #52; Center Reprint #R43, \$6; published in *Social Networks* 6:293-339)



## Easy Access to Two Important Network Data Sets

This is an argument for obtaining network data in the General Social Survey (GSS). The proposal requires a discussion of how and why at least minimal network data ought to be obtained in a probability sample of attitudes and behaviors. I begin with general concerns; briefly describing the proposal, available experience with the proposed items in large probability samples, how the proposed items are different from existing GSS items, kinds of variables that the proposed items would generate, and kinds of research questions that could be addressed if the proposed items were included in the GSS. I then address comparatively focused questions likely to arise in deliberations over the proposal; explaining how much interview time the proposed items are expected to require, why one rather than multiple name generators are proposed, why recording five alters is proposed, why intimacy is proposed as the name generator criterion content, why a short form is proposed for obtaining formal data, how priorities among name interpreter attribute items were estimated, how the proposed items elicit data on the strength and content of relationships, and how the proposed data might be coded for easy access by GSS users.

Marsden, P. V. (1985) "The discussion networks of the American population," 28 pages. (GSS Report #59)

Results are reported on the size, density, heterogeneity, and kin/nonkin composition of interpersonal environments involving the discussion of "important matters." Data were obtained in the 1985 General Social Survey. These are the first survey data representative of the American population. The personal networks they describe are small, relatively dense, homogeneous by comparison with the sample of respondents, and centered on kin. Bivariate examination of differences by age, education, race ethnicity, and sex suggests that network range is greatest among the young and the highly educated. Few sex differences in network structure are found.

Burt, R. S. (1985) "A note on sociometric order in the General Social Survey network data," 30 pages. (GSS Report #60; Center Preprint #P99, \$3; to appear in *Social Networks* 8)

The people identified as important discussion partners in the GSS network data were cited in order of strength of relationship with respondent; the first cited person having the strongest relation, the second having the next strongest, and so on. On average, the third citation is a turning point. There is a steep, linear decline in relationship strength across the first three people cited as discussion partners and a slower, but continuing decline, across the fourth and fifth people cited. Order effects on closeness and contact frequency are described in the context of network size and relation content. There is a kinship bias only in deciding who to name first; spouses tended to be the first discussion partner cited and other kin tended not to be. There is a sex homophily bias across all respondents -- people of one's own sex were cited as discussion partners before members of the opposite sex -- but it emerged differently for men and women. Women, especially married women, expressed sex bias in the people with whom they spent time while men expressed sex bias in the people with whom they felt close. Men claimed closer relations with women than men but in fact listed their important discussion partners in descending order of closeness and began the list with the names of other men. Finally, there is evidence of a co-worker bias in discussion relations beyond the family; respondents tended to mention co-workers as daily contacts but late in their list of important discussion partners. With the exception of the spouse bias, all evidence of content bias is markedly weaker than the consistent tendency for respondents to list discussion relations in descending order of closeness and contact frequency.

Burt, R. S. (1985) "A note on kinds of relations in American discussion networks," 25 pages. (GSS Report #61; Center Preprint #P100, \$3)

Diverse kinds of relations are mixed together in the 1985 General Social Survey data on American discussion networks. In the interest of social structural hypotheses being correctly specified when studied with the data, we distinguish kinds of GSS discussion relations by form, content, and respondent. The formal strength of a discussion relation varies with the order in which it was cited and its linkage to other discussion relations. Three dimensions of content distinguish kinds of discussion relations; kinship versus work, close versus casual, and frequent versus infrequent. These contents are mixed together in different ways by respondents differing in socioeconomic status, age, race, marital status and urbanism.

Burt, R. S. and M. G. Guilarte (1985) "A note on scaling the General Social Survey network item response categories," 12 pages. (GSS Report #62; Center Preprint #P101, \$3, to appear in *Social Networks* 8)

The idea of structural balance is used to suggest quantitative intervals between relationship strength response categories in the GSS network data. In contrast to an assumption of equal intervals between the categories of relationship strength, the intervals appear quite unequal. Relations with discussion partners "less close" to their respondent than other cited discussion

### Easy Access to Two Important Network Data Sets,

partners are about .7 the strength of relations with "especially close" discussion partners. The middle category of relations between discussion partners appear to be little more than acquaintance relations; about .2 of the distance from people who are "total strangers" to people who are "especially close."

Burt, R. S. (1985) "A note on the General Social Survey's ersatz network density item," 12 pages. (GSS Report #63; Center Preprint #P102, \$3)

Data obtained with the GSS ersatz network density item are compared to density data obtained with the more traditional, more costly, GSS sociometric network items. The inexpensive ersatz density data are not independent of network density, but they are almost completely unreliable. The full range of possible densities occurs at each level of ersatz density and only 1% to 2% of variation in network density can be described with ersatz density. Hypotheses operationalized with the ersatz density variable specified as a predictor will be biased toward the null hypothesis. Given this GSS experiment, the reliability of conclusions from studies replacing sociometric network items with inexpensive items purporting to measure network structure directly should be viewed with suspicion.

Burt, R. S. (1985) "A note on missing network data in the General Social Survey," 13 pages. (GSS Report #64; Center Preprint #P103, \$3)

There is very little network data missing on respondents to the 1985 General Social Survey. Missing data on relations between discussion partners pose the greatest problem but the unknown relations are strongly associated with relations known to be weak. The association between missing and weak relations remains strong after controls for the number, strength, and nature of a respondent's discussion relations. Moreover, the association remains strong across different kinds of respondents despite significant tendencies for certain kinds of respondents to have provided incomplete network data. The implication is that the missing network data can be replaced with quantitative data indicating a weak relation.

8. Burt, R. S. (1986) "Strangers, friends, and happiness," 19 pages. (Center Preprint #P110, \$4)

Using network data obtained in the 1985 General Social Survey, expressions of happiness are shown in this brief note to increase with the size of a person's discussion network and decrease with the density of strangers in the network. The density of especially close relations in the network has no direct effect on happiness. The network size and stranger density effects remain strong even after respondent differences in socioeconomic status, age, sex, race, and domestic situation are held constant. In sum, two conclusions: (a) Repeating research results with more narrowly defined populations, the size and structure of the informal discussion network surrounding a person has a significant effect on his or her expressions of happiness. (b) This effect is driven less by especially close relations among the respondent's contacts than by strangers among those contacts. It is the negative impact of missing relations rather than the positive impact of close relations that determines expressions of happiness.

SYLLABUS FOR ANSC 645: COMMUNICATION NETWORKS  
RONALD E. RICE and EVERETT M. ROGERS  
ANNENBERG SCHOOL OF COMMUNICATIONS, UNIVERSITY OF SOUTHERN CALIFORNIA, LOS ANGELES, CA 90089

This doctoral-level course is intended to introduce the analytical and conceptual perspective known as network analysis. The course particularly emphasizes aspects related to communication patterns, processes, content, settings, influences and impacts.

Required Texts:

Burt, R. and Minor, M. (eds.) Applied Network Analysis. Beverly Hills, CA: Sage, 1983. (ANA)  
Knoke, D. and Kuklinski, J. Network Analysis. Beverly Hills, CA: Sage, 1982. (NA)  
Rogers, E.M. and Kincaid, D.L. Communication Networks: Toward a New Paradigm for Research. NY: Free Press, 1981. (CN)

Introduction to the Network Perspective

Thurman, B. "In the Office: Networks and Coalitions." Social Networks, 1979, 2, 47-63.  
Scherer, J. "The Functions of Social Networks: An Exercise in Terse Conclusions." Connections, 1983, VI, 3, 22-31.  
"The Convergence Model of Communication and Network Analysis." pp. 31-78 in CN.  
"Introduction." "Basic Concepts." pp. 7-21 in NA.  
"Summing Up." pp. 325-345 in CN.

Basic Concepts and Models of Network Analysis

Pool, I. deSola and Kochen, M. "Contacts and Influence." Social Networks, 1978, 1, 5-51.  
"Communication Network Analysis." pp. 79-96 in CN.  
Burt, R. "Models of Network Structure." Annual Review of Sociology, 1980, 6, 79-89. (pp 90 - 141, later)  
Wellman, B. "Network Analysis: Some Basic Principles." in Collins, R. (ed.) Sociological Theory 1983. SF: Jossey-Bass, 1983, 155-200.

Data Collection

"Data Collection." pp. 22-34 in NA.  
"Measuring Communication Network Links." "Levels of Analysis." pp. 96-142 in CN.  
Laumann, E.; Marsden, P. and Prensky, D. "The Boundary Specification Problem in Network Analysis." pp. 18-34 in ANA.  
Burt, R. "Network Items and the General Social Survey." Social Networks, 1984, 6, 293-339.

We will develop a very short questionnaire for the class to administer to itself! This questionnaire will include items measuring two kinds of links. The questionnaire will also include a portion of the General Social Survey as described by Burt. The questionnaire will be printed up and delivered to you in a few days. Please return the questionnaire the following day.

Measures, Indices, Roles

"Measures of Proximity." pp. 147-156 in CN.  
Freeman, L. "Centrality in Social Networks: Conceptual Clarification." Social Networks, 1979, 1, 215-239.  
Freeman, L.; Roeder, D. and Mulholland, R. "Centrality in Social Networks: Experimental Results." Social Networks, 1980, 2, 119-141.  
Burt, R. "Range." in ANA.  
Knoke, D. and Burt, R. "Prominence." in ANA.  
Schwartz, D. and Jacobson, E. "Organizational Communication Network Analysis: The Liaison Communication Role." Organizational Behavior and Human Performance, 1977, 18, 158-174.  
Friedkin, N. "The Development of Structure in Random Networks: An Analysis of the Effects of Increasing Network Density on Five Measures of Structure." Social Networks, 1981, 3, 1, 41-52.

Assignment 1.

Using the matrix of network links handed out in this class, calculate for the first two members the following: some version of centrality, range, integration;  
for the network as a whole, calculate: system density and structure.  
Write down the formula you used and its source. Be prepared to discuss the implication of the particular formula you used. This assignment is due next week.

## An Overview of Network Analysis Methods

### Assignment #1 Due.

Rice, R.E. and Richards, W., Jr. "An Overview of Communication Network Analysis Programs and Methods." in Dervin, B. and Voigt, M. (eds.) *Progress in Communication Sciences*, Vol. 6. NJ: Ablex, 1985, 105-165.

"Methods and Models." pp. 35-86 in NA.

pp. 157 - 215 in CN. Compares 5 methods of network analysis.

pp. 90 - 141 of the Burt article ("Models of Network Structure")

## Respondent Accuracy, Perceptions of Social Structure

Bernard, H.; Killworth, P.; Kronenfeld, D. and Sailer, L. "The Problem of Informant Accuracy: The Validity of Retrospective Data." *Annual Review of Anthropology*, 1984, 13, 495-517.

Bernard, H.; Killworth, P. and Sailer, L. "Informant Accuracy in Social Network Data V." *Social Science Research*, 1982, 11, 30-66.

Richards, W. Jr. "Data, Models and Assumptions in Network Analysis." in McPhee, R. and Tompkins, P. (eds.) *Organizational Communication: Traditional Themes and New Directions*. Beverly Hills, CA: Sage, 1985, 109-128.

Burt, R. "A Note on Inferences Concerning Network Subgroups." pp. 283-301 in ANA.

Killworth, P.; Bernard, H. and McCarty, C. "Measuring Patterns of Acquaintanceship." *Current Anthropology*, 1984, 25, 4, 381-397.

### Assignment 2.

At the Sunbelt conference, attend a presentation of your choice, make sure that you get a copy of the conference paper (you should track this down from the presenter before the presentation), interview the author/presenter, and prepare a 8-page report on the paper. Your report should include: (a) a summary of the theoretical and methodological approach to the problem, (b) a critique of the paper, and (c) an indication of the aspects of the paper that the author/presenter feels are important, troublesome or useful.

## Data Collection: Designs and Decisions

### Assignment #2 Due.

Today, we'll focus in on problems of collecting, measuring and handling network data. Such problems in two papers will be summarized and discussed.

Rogers, E.M. and Lievrouw, L. "Triangulation as a Research Strategy in the Study of a Biomedical Invisible College." Presented to Sunbelt Networks Conference, Feb, 1986.

Rice, R.E. and Love, G. "Electronic Emotion: Socio-Emotional Content in a Computer-Mediated Computer Network." Presented to Sunbelt Networks Conference, Feb, 1986.

### Assignment 3.

The goal for Assignment 3 is to for you to decide how you would solve one of the problems mentioned in the discussions of either of the two papers. You might decide to write about how you would have collected or managed the data in such a way as to have avoided some of the problems discussed. Or, you might want to point out other problems that were not discussed, which you identified and for which you have suggestions. The paper should be no longer than five pages.

## Diffusion of Innovations via Networks

### Assignment #3 is Due.

Granovetter, M. "The Strength of Weak Ties: A Network Theory Revisited." in Marsden, P. and Lin, N. (eds.) *Social Structure and Network Analysis*. Beverly Hills, CA: Sage, 1982, 105-130.

Friedkin, N. "Information flow Through Strong and Weak Ties in Intra-Organizational Networks." *Social Networks*, 1982, 3, 4, 273-286.

Burt, R. "Innovation as a Structural Interest: Rethinking the Impact of Network Position on Innovation Adoption." *Social Networks*, 1980, 2, 4, 327-356.

Reingen, P.; Foster, B.; Brown, J. and Seidman, S. "Brand Congruence in Interpersonal Relations: A Social Network Analysis." *Journal of Consumer Research*, 1984, 11, 771-783.

Tushman, M. "Special Boundary Roles in the Innovation Process." *Administrative Science Quarterly*, 1977, December, 22, 587-603.

#### Assignment 4.

Your final project requires you to read a classic article in network analysis, and, using any other network analysis routine, re-analyzing the data using a different network analysis method. The goal of the re-analysis is to (a) challenge the theoretical motivations for using the original method, (b) challenge the substantive findings of the original paper by providing alternative interpretations from your analyses, (c) extend or offer different analytical insights by using a different method, and/or (d) become more familiar with two network methods. So, theory and method must be well-matched in your actual analysis and in your critique of the original paper.

The first problem, of course, is choosing a problem and a method. Included in your reading packet at this stage is: UCINET. "A Growing Set of Programs and Data Sets for Social Network Analysis." by the Program in Social Networks Analysis at the University of California, Irvine. It includes documentation on 14 network analysis programs, and 8 classic network data sets. Other network approaches, such as factor analysis, multi-dimensional scaling, or NEGOPY, are accessible through the University Computing Services on the IBM computer.

You should know that these references are the original sources of the data, but not always the best reference for analyses of the data. Therefore, an implicit portion of this assignment is to track down other, related articles stemming from this original article, which would be better sources to use. We will talk about this aspect of assignments 4, 5 and 6, but you should be prepared to use the Social Sciences Citation Index to track down subsequent articles which cite these original articles.

1. Bernard, H. and Killworth, P. "Informant Accuracy in Social Network Data II." Human Communication Research, 1977, 4, 3-18.

or

Killworth, P. and Bernard, H. "Informant Accuracy in Social Network Data III." Social Networks, 1979/80, 2, 19-46.

See their other Accuracy studies referenced in the Accuracy V article listed in February 13.

2. Davis, A., et al. Deep South. Chicago: University of Chicago Press, 1941. Note that Patrick Doreian has re-analysed these data using Q-Analysis, published in Social Networks.

3. Nordlie, P.G. "A Longitudinal Study of Interpersonal Attraction in a Natural Group Setting." This was written up elsewhere by Newcomb.

4. Roethlisberger, F. and Dickson, W. Management and the Worker. Cambridge: Harvard U. Press, 1939. This was reanalyzed in one of the Breiger, Boorman and Arabia articles which illustrated blockmodelling.

5. Sampson, S. Crisis in a Cloister. Unpublished Ph.D. Dissertation. Ithaca, NY: Cornell University, 1969. Ann Arbor, MI: University Microfilms, No. 69-5775. (This dataset is well described in the Rice and Richards chapter, which also includes references to several other analyses of the Sampson data. However, because of the extensive analysis by Rice and Richards, you'd have to come up with something pretty creative.)

6. Thurman, B. "In the Office: Networks and Coalitions." Social Networks, 1979, 2, 47-63.

#### Individual and Group Networks

Assignment #4 is due.

"Network Variables in Explaining Individual Behavior." pp. 220-225 in CN.

"Communication Networks in Explaining Group and System Performance." pp. 256-296 in CN.

"Whom Do Networks Link?" pp. 297-324 in CN.

Shaw, M. "Communication Networks." and "Communication Networks Fourteen Years Later." in Berkowitz, L. (ed.) Group Processes. NY: Academic Press, 1978, 313-349, 351-361.

#### Assignment 5.

This assignment asks you to be much more specific about what topics in your chosen original network paper provide a good opportunity for re-analysis. What alternative perspectives might be useful? How might the data be re-grouped or re-analysed? What assumptions do the original author make that were not fully explored or which can be questioned by re-analysis? What additional insights might you find from using a different network approach? For this

assignment, then, be specific as to the theoretical and methodological motivations you have in re-analysing the original paper. Outline your arguments. Summarize and critique the paper. Choose and justify which approach you will now take for your final project.

#### Intra-Organizational Networks

Assignment #5 is due.

Lincoln, J. and Miller, J. "Work and Friendship Ties in Organizations: A Comparative Analysis of Relational Networks." *Administrative Science Quarterly*, 1979, 24, 181-199.

Tichy, N. "Networks in Organizations." in Nystrom, P. and Starbuck, W. (eds.) *Handbook of Organizational Design*, Vol. 2. Oxford: Oxford University Press, 1981, 225-249.

Monge, P.; Edwards, J. and Kirste, K. "The Determinants of Communication and Communication Structure in Large Organizations: A Review of Research." in Ruben, B. (ed.) *Communication Yearbook*, Vol. 2, 1978, 311-331.

Fombrun, C. "Strategies for Network Research in Organizations." *Academy of Management Review*, 1982, 7, 280-291.

#### Inter-Organizational Networks

Aldrich, H. and Whetten, D. "Organization-sets, Action-sets, and Networks: Making the Most of Simplicity." in Nystrom, P. and Starbuck, W. (eds.) *Handbook of Organizational Design*, Vol. 1. Oxford: Oxford University Press, 1981, 385-408.

Fennema, M. and Schifj, H. "Analysing Interlocking Directorates: Theory and Methods." *Social Networks*, 1979, 1, 4, 297-332.

Lincoln, J.R. "Intra- (and Inter-) Organizational Networks." *Research in the Sociology of Organizations*, 1982, 1, 1-38.

Eisenberg, E.; Farace, R.; Monge, P.; Bettinghaus, E.; Kurchner-Hawkins, R.; Miller, K. and Rothman, L. "Communication Linkages in Interorganizational Systems: Review and Synthesis." in Dervin, B. and Voigt, M. (eds.) *Progress in Communication Sciences*, Vol. 6. Norwood, NJ: Ablex, 1985, 231-262.

Reeves, B. and Borgman, C. "A Bibliometric Evaluation of Core Journals in Communication Research: Networks of Communication Publications." *Human Communication Research*, 1983, 10, 1, 119-136.

#### Network Emergence, Stability, Dynamics

Monge, P. and Eisenberg, E. "Emergent Networks." in Porter, L.; putnam, L.; Roberts, K. and Jablin, F. (eds.) *Handbook of Organizational Communication*. Beverly Hills, CA: Sage, 1986. in preparation.

Hammer, M. "Predictability of Social Connections Over Time." *Social Networks*, 1980, 2, 165-180.

Monge, P.; Rothman, L.; Eisenberg, E.; Miller, K. and Kirste, K. "The Dynamics of Organizational Proximity." *Management Science*, 1985, 31, 9 1129-1141.

Barnett, G. and Rice, R.E. "Longitudinal Non-Euclidean Networks: Applying Galileo." *Social Networks*, 1986.

Rice, R. "Resources for Longitudinal Network Analysis." *Connections*, 1981, 4, 2, 10-22.

#### Analyses of Computer-Based Communication Networks

Freeman, L. "The Impact of Computer-Based Communication on the Social Structure of an Emerging Scientific Speciality." *Social Networks*, 1984, 6, 3, 201-222.

Freeman, L. "Q-Analysis and the Structure of Friendship Networks." *International Journal of Man-Machine Studies*, 1980, 12, 367-378.

Rice, R.E. "Communication Networking in Computer-Conferencing Systems: A Longitudinal Study of Group Roles and System Structure." in Burgoon, M. (ed.) *Communication Yearbook*, Vol. 6., 1982, 925-944.

Danowski, J. "Computer-Mediated Communication: A Network Analysis Using a CBBS Conference." in Burgoon, M. (ed.) *Communication Yearbook*, Vol. 6. Beverly Hills, CA: Sage, 1982, 905-924.

Danowski, J. and Edison-Swift, P. "Crisis Effects on Intraorganizational Computer-Based Communication Networks." *Communication Research*, 1985, 12, 2, 251-270.

#### Final Project Due.

While presentations of the final reports will have probably started one or two weeks ago, the actual, polished, final project is due on this date -- late papers are not accepted. Be sure to include a one-page summary of your report: the reference and nature of the original article, the data and method it used, the theoretical/methodological problem which motivated the analysis,

the alternative method or approach you used, the different/opposing/additional results you obtained, the subsequent consequences for the original article, and implications for network research. The final report must be no longer than 25 pages, including all references, tables and figures. Be sure to include complete references for all materials you used, particularly the original article(s) you critiqued.

NETWORK NOTEBOOK (cont'd from page 46)

Editors: LLOYD BONFIELD (Law, Tulane, New Orleans) & RICHARD WALL (Cambridge [UK] Group for the Hist. of Pop. & Soc. Struc.). Editorial board includes Barbara Laslett (Soc, Minn), Peter Laslett (Camb. Group), Louise Tilly (New Schl. for Soc Res.) & E.A. Wrigley (LSE). Subs (3x/yr) US\$29 to Cambridge U Pr, 32 E 57 St, New York NY 10022.

EUROPEAN JOURNAL OF COMMUNICATION is a quarterly interested in comparative communication analysis & research, & in comparisons between Europe & other parts of the world. Vol. 1 includes Jan van Cuilenburg, J. Kleinnijenhuis & J A de Gidder, "A theory of evaluative discourse: towards a graph theory of journalistic texts" & Elihu Katz, "The export of meaning: cross cultural readings of 'Dallas'". Eds: Jay Blumler (TV Res, Leeds), Denis McQuail (Amsterdam) & K. E. Rosengren (Goteborg). Subs: US\$33[#22], Sage Pubs, Box 11017, Beverly Hills CA 90213.

INTERNATIONAL JOURNAL OF SOCIOLOGY & SOCIAL POLICY is a 5-yr old quarterly, with an emphasis on review essays, comparative analyses & social thought. Ed: Barrie Pettman (Int'l Mngemt Ctr, Buckingham, Eng.). Subs: US\$150, MCB University Press, 62 Toller Lane, Bradford, Eng. BD8 9BY.

INTERNATIONAL SOCIOLOGY is the Int'l Soc Assoc's new quarterly, especially for papers with international, cross-cultural & comparative interest. Early issues include papers by Immanuel Wallerstein & Johann Galtung. Ed: Martin Albrow, University C., PO Box 78, Cardiff CF1 1XL, Wales, UK (tel: 0222-44211). Subs, US\$25, University College Cardiff Press, same address.

REVUE INTERNATIONALE DE SOCIOLOGIE is an old journal (1895) undergoing rejuvenation. Each issue (3x/yr.) will principally be devoted to a special topic, with international contributors. [INSNA has been asked to contribute--anyone interested in editing?] Info from Mino Vianello, Interdisciplinary Group for Social Research, Univ of Rome 'La Sapienza', Via Brennero 36, 00141 Roma. Tel: 8928524.

GENDER & SOCIETY is the new "feminist perspective", interdisciplinary journal of (US) Sociologists for Women in Society. It emphasizes theory & research focusing on "the social structural study of gender as a basic principle of the social order & as a primary social category". Ed: Judith Lorbe, Soc, Grad Ctr, CUNY, 33 W 42 St, New York NY 10036.

SOCIOLOGY OF HEALTH & ILLNESS has shifted to a quarterly, a new publishers & will have "greater emphasis on N American issues". Ed: Anne Murcott. Subs: £16.50[US\$32].

JOURNAL OF AGING STUDIES is an international, interdisciplinary quarterly wanting critical, theoretical & empirical papers. Ed: Jaber Gubrium, Soc & Cultural Studies, Marquette, Milwaukee WI 53233.

JOURNAL OF INTERPERSONAL VIOLENCE is a quarterly devoted to the study of victims & perpetrators. Ed: John Conte, Schl of Soc Service Admin, U of Chicago, 969 E 60 St, Chicago IL 60637.

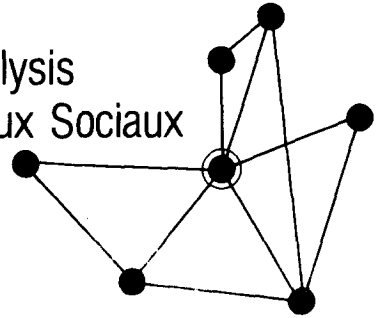
NEIGHBORHOOD CARETAKER: A JOURNAL OF NEIGHBORHOOD HEALTH SCIENCES is a 4-page monthly newsletter presenting short teasers that summarize the ideas or facts from a longer article available on request. It focuses on neighborhood health, including housing, the care of the elderly & frail, & alcoholism. Info, 3038 Fall Creek Pkwy N, Indianapolis IN 46205. Sub: \$25.

JOURNAL OF AGING & JUDAISM examines issues facing the Jewish community in its delivery of services to the aged, e.g. aged Holocaust survivors, rabbinic attitudes toward aging, interfaith approaches toward service delivery. Ed: Kerry Olitzky, Hebrew Union C., 1 W 4 St, New York NY 10012. Subs, \$19, Human Sciences Pr, 72 5th Ave, New York NY 10011.

INTERNATIONAL PEACE RESEARCH NEWSLETTER is edited by INSNA member Elise Boulding, for the Int'l Peace Research Assoc. Like INSNA its HQ functions as a node, with correspondence from people searching for a place to study & scholars hoping to make contact with others working on the same issues. It facilitates the connections of smaller, issue-specific nets of scholars. Subs, \$15, IPRA, Mershon Ctr, Ohio St, 199 W 10th Ave, Columbus OH 43201. Tel: 614-422-1681. [Source: NETWORKING NEWSLETTER].

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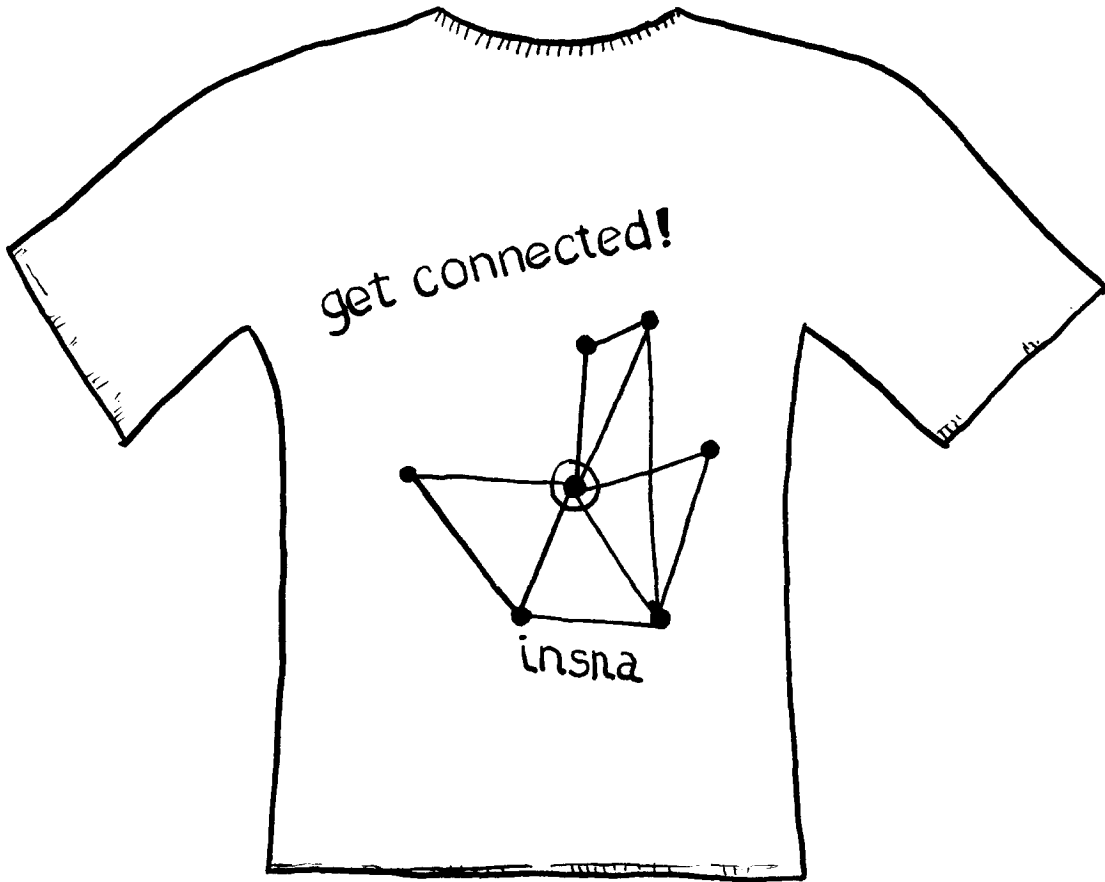
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