A PERSONAL HISTORY OF RESEARCH ON THE
DIFFUSION OF INNOVATIONS

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P. D. Converse Symposium Paper #7

College of Commerce and Business Administration
University of Illinois at Urbana-Champaign
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by

Everett M. Rogers
Professor of Journalism and of Population Planning
University of Michigan

Paper presented at the Ninth Paul D. Converse Marketing Symposium
I take this special opportunity to look back over the past 21 years of my personal experience with the field of social scientific research usually referred to as the "diffusion of innovations," in order to trace the natural history of this invisible college since its "revolutionary paradigm" in the late 1930's. My purpose is not just to present a personalized natural history of the diffusion field, but also (1) to illustrate, with this one case study, some general observations about patterns in the growth of science, (2) to describe some deficiencies of modern-day social scientific inquiry that I feel need to be corrected in the near future, and (3) to trace the interrelationships of diffusion research with the field of marketing, as is particularly appropriate on this occasion of the Ninth Paul D. Converse Marketing Symposium.

I begin with a general picture of the diffusion field, and then get more personal in a following section, closing with a focus on marketing research on diffusion, and on the shortcomings of diffusion research.

THE RISE OF DIFFUSION RESEARCH

A misconception that I have occasionally encountered on the part of contemporary students is that the field of research on the diffusion of innovations began with the publication of my book, Diffusion of Innovations, in 1962. I think this book did help publicize this field's current name, and undoubtedly my book promoted the view of diffusion as a general process,
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independent (1) of the particular type of innovation studied, or (2) of the academic discipline represented by the researcher conducting a diffusion study. But 405 diffusion publications, including one synthetic book, had already appeared prior to my book's publication in 1962.

Although the roots of the diffusion field can be traced (1) to the German-Austrian and British anthropological schools of diffusionism, and (2) to the French sociologist Gabriel Tarde (1903), the revolutionary paradigm for diffusion research occurred in the early 1940's when two sociologists at Iowa State University, Bryce Ryan and Neal Gross (1943), published their seminal study of the diffusion of hybrid seed corn among Iowa farmers.

Revolutionary Paradigms and Invisible Colleges

Any given field of scientific research begins with a major breakthrough or reconceptualization which provides a new way of looking at some phenomenon (Kuhn, 1962). This revolutionary paradigm typically sets off a furious amount of intellectual effort as promising young scientists are attracted to the field, either to advance the new conceptualization with their research or to disprove certain of its aspects. Gradually, a scientific consensus about the field is developed, and, perhaps after several generations of academic scholars, the "invisible college" (composed of researchers on a common topic who are linked by communication ties) declines in scientific interest as fewer findings of an exciting nature are turned up. These are the usual stages in the normal growth of science, Kuhn (1962) claims.
Research on the diffusion of innovations followed these rise-and-fall stages rather closely, although the final stage of decline has not yet begun (Crane, 1972). The hybrid corn study set forth a new approach to the study of communication and change, that was soon followed up by an increasing number of scholars in a wide variety of scientific fields. Within 10 years (by 1952), over 100 diffusion researches were completed; during the next decade (by 1962), another 450; and by the end of 1974, another 1,250. Today there are over 2,700 publications about the diffusion of innovations, including about 1,800 empirical research reports and 900 other writings (Rogers and Thomas, 1975). The amount of scientific activity in investigating the diffusion of innovations increased at an exponential rate (doubling almost every two years) since the revolutionary paradigm appeared 32 years ago, as Kuhn's (1962) theory of the growth of science would predict.

The Hybrid Corn Study

The main elements in the "classical model" of the diffusion of new ideas that emerged are (1) the innovation, defined as an idea, practice, or object perceived as new by an individual or other relevant unit of adoption, (2) which is communicated through certain channels (3) over time (4) among the members of a social system. The Ryan and Gross (1943) study focused on hybrid corn, one of the most important innovations in Midwestern agriculture. Data were gathered by personal interviews with all the Iowa farmers in two communities. The rate of adoption of the agricultural innovation followed an S-shaped, normal curve when plotted on a cumulative basis over time. The first farmers to adopt (the innovators) were more cosmopolite (indicated by traveling more
null
frequently to Des Moines) and of higher socio-economic status, than later adopters. The typical Iowa farmer first heard about the innovation from a seed corn salesman, but interpersonal communication with peers was the most frequent channel leading to persuasion. The innovation-process from awareness-knowledge to final adoption averaged about nine years, indicating that considerable time was required for adoption to occur.

The Ryan and Gross (1943) study was so influential in affecting later studies on diffusion that Crane's (1972, p. 74) analysis (of rural sociologists investigating diffusion) found the hybrid corn study was responsible for 15 of the 18 most widely-used "innovations" (defined as the first use of a dependent or independent variable in a research publication in the diffusion field), and accounted for 21 percent of the total of 201 innovations! "A significant proportion of the innovative work in the area had already been done by the time the field began to acquire a significant number of new members" (Crane, 1972, p. 67).

In short, most of the 32 years and 2,699 publications dealing with the diffusion of innovations have only followed up on the original leads provided by the hybrid corn study, exploring them in greater empirical detail but seldom plowing really new ground.

For example, Ryan and Gross (1943) set forth these leads, which were to become popular in later diffusion studies:

1. The "S"-shape of the rate of adoption of an innovation over time.
2. The characteristics of the various adopter categories.
3. The relative importance of different communication channels/sources at various stages in the innovation-decision process.
One of the deficiencies of the hybrid corn study was its lack of attention to opinion leadership patterns in the interpersonal diffusion of the innovation within the two Iowa communities that were studied, although radiometric data about diffusion would have been easy to gather, as all the farmers in the two systems were interviewed (Katz, 1966). Only in much later diffusion studies did scholars begin to give proper attention to interpersonal communication flows, and such diffusion networks have still not received their just due by diffusion students (a point to which we shall return).

Positive and Negative Consequences of a Dominant Paradigm

Why are scientists in any particular research community so influenced by ruling paradigms? Undoubtedly one reason may lie in the high degree of uncertainty experienced by a scientist in approaching a research problem. The scientist must decide exactly which aspect of the problem he will investigate, through what methods of data-gathering and analysis, in order to test which concepts, etc. A dominant paradigm provides specific answers and guidelines to each of these questions that are involved in every research study, and there is a natural tendency for scientists to follow the paradigm closely (Kaplan, 1964).

One advantage of the fact that most fields of scientific research are organized as an invisible college around an intellectual paradigm is that such scientific concensus about what is important, and what is not, and the close informal communication among scientists, lends a great deal of stability and standardization to the field. For instance, comparisons across the 2,700 diffusion publications is greatly facilitated by the fact that they share a common paradigm (especially since the late 1960's, when Rogers with Shoemaker [1971, p. 47] concluded that diffusion research was emerging as "a single,
integrated body of concepts and generalizations.

The fact that many diffusion studies look a great deal alike facilitates the synthesis of diffusion research findings (as I well know), and allows the results to be added up in a cumulative and orderly manner. This process of slowly inching forward by a scientific field is made possible by the stability of the invisible college, which in turn rests on the social structure of the informal communication patterns among the scientists.

For example, Crane's (1972) network analysis of the cliques among diffusion scholars in the rural sociology research tradition in the mid-1960's is depicted in Figure 1. Two main cliques, one of 27 members and one of 32 members, dominate this invisible college. Each clique is headed by an "opinion leader", often this individual was relatively early in engaging in diffusion research and his followers are former students whose theses he directed. These opinion leaders tend to be the "high producers" of scientific literature in the diffusion field. Opinion leadership is very concentrated in the invisible college; 6 percent of the 203 scientists received 58 percent of all the sociometric choices made indicating who informally influenced their research (Crane, 1972, p. 50).

The high degree of informal social structure in the communication network for the invisible college of diffusion researchers helps provide stability, continuity, and incremental growth in scientific advance.

The standardization of research approaches around the classical diffusion model has also greatly limited the contribution of diffusion research (1) to more effective social programs, and (2) to furthering the scientific understanding of communication and human behavior change. Presumably this indictment is what
Figure 1. The Interpersonal Communication Network of the Invisible College of Rural Sociologists Studying Diffusion in the Mid-1960's.

Note: Direct communication is shown by solid lines, and indirect communication (indicating that any member of a clique was linked to an individual in another clique through someone else) by broken lines. The number identifying each clique is the number of members (a letter is used to distinguish between cliques of the same size), and a "1" indicates an isolate.

*Source: Crane (1972, p. 138).
the dean of one U.S. school of communication had in mind when he characterized the diffusion field as "A mile wide and an inch deep." Radical innovation in the scientific study of the diffusion of innovations has been relatively rare since the early 1960's, and I doubt this lack of ingenious creativity is due to the exhaustion of all the potential new alternatives. Some of the recent intellectual advances in the diffusion field, in fact, seem to have been made by researchers who were not very widely read in the diffusion literature, or at least who had not earned their doctorates as specialists in diffusion research. An illustration is the recent work on the topic of "the strength of weak ties," a new and promising research departure in the diffusion field (Liu and Duff, 1972; Granovetter, 1973). In this case, two sets of authors independently discovered this diffusion strength of weak sociometric ties, and published articles with identical names within a few months of each other in 1972-3, although approaching the issue in somewhat different ways. Professors Liu, Duff, and Granovetter were acquainted with the diffusion literature, but they had not previously published on this topic, and their articles show a relatively fresh approach to analyzing diffusion networks. Perhaps this relative newness in working with the classical diffusion model was one requisite for the originality of their contribution.

Another illustration of the stultifying effect of the dominant paradigm of diffusion is provided by the rise of "KAP surveys" which were conducted in developing countries since the early 1960's. KAP studies are sample surveys of knowledge (K), attitudes (A), and practice (P) (that is, adoption) of family planning innovations. K, A, and P are the logical dependent variables in evaluations of family planning communication campaigns, and as national family planning programs arose after 1960 in many developing nations (especially in Asia)
to cope with the over-population problem, KAP-type diffusion researches blossomed on all sides. Over 500 such KAP surveys were conducted in 72 nations by 1973 (Rogers, 1972, p. 377); India alone represented the locale for over half of these investigations.

With the exception of the Taichung experiment in Taiwan (Freedman and Takeshita, 1969), the intellectual contribution of these KAP surveys "to scientific understanding of human behavior change has been dismal" (Rogers, 1973, p. 378). The dependent variables of K, A, and P are inadequate (in that continuation rates for family planning adopters should also be measured), the independent variables are hackneyed and are generally ineffective in explaining much variance in the dependent variables, and the research methods used for data-gathering and measurement are questionable (Hauser, 1967). One reason for these problems lies in the intimate communication among the KAP researchers, which led to too-close copying, and too-early standardization, of the diffusion research approaches in the sub-field of family planning.

Further understanding of the invisible college of diffusion researchers is provided by its history, as I have known it. I cannot claim that the "life and times" of the diffusion field as described in the following account is anything more than one individual's rememberances, perhaps made more accurately recallable than would otherwise be so by the fact that the details are based on, and largely supported by, the written artifacts of the diffusion scholars (that is, their publications) over the past 21 years. In the account that follows I stress the period from 1954, when I became interested in research on the diffusion of farm innovations, until 1962, when Diffusion of Innovations appeared.
MY PERSONALIZED HISTORY OF DIFFUSION RESEARCH

My original interest in diffusion grew out of my background as an Iowa farm boy, and high school and college training in technical agriculture. Especially during the summers of my four undergraduate years at Iowa State University, when I worked on the home farm, I was frustrated by the unwillingness of my father and most of his neighbors to quickly adopt new weed sprays, farm equipment, and other innovations. Yet one neighbor, a local laughing-stock, was a super-innovator with the latest in equipment, seeds, and agricultural chemicals. He was ridiculed for his deviance from the neighborhood norm, and it was constantly expected that he would go into bankruptcy because of his rash innovativeness. Gradually, I became aware of the fact that this innovator was actually following closely the recommendations of agricultural specialists at Iowa State University.

Iowa State University: 1954-1957

Out of these years grew some limited intuitive understandings about agricultural diffusion, and a strong desire to learn more about why "most farmers did not adopt innovations." In 1954, I returned from military service to graduate work at Iowa State, and was fortunate to know Professor George M. Beal in Sociology. During my first term as a graduate student in Ames, I enrolled in his seminar on the diffusion of agricultural innovations (or "practices", as they were called then). We reviewed the work of Eugene Wilkening at the University of Wisconsin and Herbert Lionberger at the University of Missouri, and of course the Ryan and Gross hybrid corn study, completed by these sociologists 15 years before. During this period, after the departure of Ryan and
Gross, little further work on diffusion had occurred at Iowa State, but Professor Beal was involved in planning a diffusion research project in a nearby farm community, Collins, Iowa. Our seminar served as an intellectual springboard for the research project, and before the end of the term I was working for Beal on the study, pretesting the interview schedule. During the following summer, I helped interview the 150 farmers in the Collins community, and their wives, and eventually analyzed some of these data in my Ph.D. dissertation.

So during my first months in graduate work, I had become an "instant" diffusion researcher. I remember being tremendously motivated in my study and field research. Ames was an exciting place for a young diffusion scholar. A series of recent agricultural scientific break-throughs had spawned farm innovations: 2,4-D weed spray, antibiotic feeds for hogs, diethelstylbestrol for cattle. Some of the inventors and developers of these innovations were agriculture faculty at Iowa State. The Iowa Extension Service, also headquartered in Ames, was diffusing these new ideas to farmers, who seemed to be adopting them. There was a lot of agricultural innovation to study, and Iowa, being one of the most prosperous farm states, seemed like a good place to conduct such diffusion research.

Through the efforts of Professor Beal, and his colleague Joe Bohlen, the classical diffusion model was starting to come together in a synthetic way, based heavily on the work of Ryan/Gross, Wilkening, and Lionberger. Beal and Bohlen worked out a highly visualized presentation about diffusion for the annual meeting of all the Iowa extension workers in 1954, which wowed them. Soon the synthesis was available in print form (North Central Rural Sociology Subcommittee for the Study of Diffusion of Farm Practices, 1955), and the Beal/Bohlen team
was becoming famous for their flannel board presentation on diffusion, appearing almost weekly before extension workers, and agency people, and at various conventions. Unbelievably, their bulletin sold over 200,000 copies within a few years. Suddenly, agricultural diffusion research had become popular, and several projects were funded by state agricultural universities.

Our research in the Collins community provided the data base to test out certain aspects of the diffusion model that was being assembled. For instance, we were able to study the stages in the innovation-decision process (Beal and others, 1957), work out a method for adopter categorization (Rogers, 1958), and trace out the sociometric opinion leadership patterns among the 150 farmers, in a way that Ryan and Gross had not done in the hybrid corn study.

The Ryan and Gross (1943) study had a very strong impact on our thinking at Ames in the mid-1950's; I still have a well-thumbed reprint of their article, which I suppose I must have read at least 80 times during my three years at Iowa State. But I was also beginning to look elsewhere for leads on diffusion, first to educational diffusion research by Professor Paul Mort and others at Columbia University Teachers College (I stumbled upon this work while leafing through an educational journal in the waiting room of a professor's office), and then to the medical drug diffusion study conducted by James Coleman, Elihu Katz and Herbert Menzel at the Bureau of Applied Social Research (BASR) at Columbia University. In 1956 I obtained a small grant from a foundation, which entailed my participation at a conference in New York at which the BASR researchers were also represented.

The agricultural, educational, and medical drug diffusion researches led to strikingly similar findings: S-shaped curves, opinion leaders, innovators. I became convinced that a general diffusion process occurred for many
types of innovations, a theme later developed in my 1962 book.

In 1957 I successfully defended my Ph.D. dissertation, a multiple correlation analysis of agricultural innovativeness among the 150 farmers in Collins (with the disappointing achievement of explaining only 17 per cent of the variance in the dependent variable), and accepted a position in rural sociology at Ohio State University.

Ohio State University: 1957-63

One of the limitations of agricultural diffusion studies prior to 1957 was the inability to generalize the results to broader populations than just the single community that was studied. So I launched an agricultural diffusion project upon arrival in Columbus that entailed gathering interview data from a randomly selected statewide sample of farmers.* Among my duties at Ohio State, I also taught a course on the diffusion of innovations (one of only about five in the U.S. at the time), which helped me develop the basic framework for Diffusion of Innovations, published in 1962.

I had submitted an outline for this proposed volume to five or six publishers, and all rejected it but Free Press. Their editor could somehow see the wisdom of publishing a book on what then was perceived by most sociologist as an applied and low-prestige topic, with very little by way of a guaranteed market (there were probably less than 100 students per year enrolled in university courses on diffusion).

The first printing was only 2,000 copies, I believe, and my first year's royalty check was for sales of only 350 copies. I never saw an ad for the book or any other promotion by the publisher (perhaps this explains why they could afford to publish it). But, surprisingly, in the second year of publication,
the book's first printing was exhausted, and its sales have followed a beautiful S-shaped curve since (until further printings were stopped in 1971, when the revised edition appeared).

I won't bore you with my further academic wanderings through a Fulbright lectureship at the National University of Colombia in Bogotá, nine years at Michigan State University, and now two years at the University of Michigan. My present research centers around how to put social structure into the diffusion process through (1) network analysis (more on this later), and (2) studying innovation in organizations, which are highly structured systems.

Hopefully this personal history of diffusion tells something about the growth and changes in the field, as well as my personal drifting through it in the past two decades.

MARKETING RESEARCH ON DIFFUSION

The marketing tradition of diffusion research has come on strong since the early 1960's. About 8 percent of the 1,800 empirical publications on diffusion were authored by researchers associated with the field of marketing. Many marketing texts these days have a chapter on diffusion, or at least give considerable coverage to such topics as the innovation-decision process, adopter categories, opinion leadership, and the S-shaped diffusion curve.

Marketing managers of firms have long been concerned with how to launch new products more efficiently. One reason for this interest is the high failure rate of new consumer products, estimated at 92 percent of the approximately 6,000 new consumer items introduced each year (Conner, 1964).
The adoption of most innovations involves sale of a new product, of course, and it was easy for commercial firms to conceive of their new products as innovations, and to adopt the theoretical and methodological framework of diffusion research to marketing problems. The oral/visual presentation on diffusion by Professors Beal and Bohlen to advertising agencies helped create commercial interest in the diffusion model. University faculty members in graduate schools of business led the marketing discipline into diffusion research (Zaltman, 1964), to be followed soon after by marketing researchers in the employ of commercial firms. Unfortunately, a large proportion of these research reports lie only in the secret files of the sponsoring companies because of competitive threat, and so they are unavailable to attempts at academic synthesis and the progress of scientific understanding of the diffusion process.

Field Experiments on Diffusion

Many of the diffusion researches in the marketing field were either conducted by the commercial manufacturers of the new product, or by university professors with the sponsorship, or at least the cooperation, of the manufacturers. One advantage of this close relationship was that the diffusion researchers in the field of marketing often had some degree of control over the diffusion strategies that were used to promote the new products. This is a particularly important ingredient in the conduct of field experiments on diffusion. In other fields than marketing, diffusion scholars have seldom been able to manipulate the "treatment" variables, and so it has been impossible to conduct field experiments.
Perhaps a somewhat typical illustration of the field experimental approach by marketing researchers is provided by Arndt's (1967) study of the diffusion of a new food product. A letter about this innovation, enclosing a coupon allowing its purchase at one-third price, was sent to 495 housewives living in a married student apartment complex at one university. Personal interviews were carried out with these consumers 16 days after launch of the diffusion campaign. Arndt found that interpersonal peer communication about the new product frequently led to its initial purchase. Housewives who perceived the innovation as risky were more likely to seek the advice of their neighborhood opinion leaders about it. Naturally, this type of field experiment allowed determination of the impact of the reduced-price sample offer; the measure of impact was the rate of adoption (that is, purchase) of the new food product.

Pro-Source Orientation

The diffusion research tradition of marketing has displayed an especially strong bias toward producing research results of use to the innovation's source (that is, the manufacturer of the new product), rather than to the consumers. This pro-innovation and pro-source orientation is also characteristic of other branches of diffusion research, but somewhat less so than in the field of marketing.

One cannot help but wonder how the research approach (and the understandings that were obtained) might have been different had the Ryan and Gross (1943) hybrid corn study been sponsored by the Iowa Farm Bureau Federation, rather than by the Iowa Agricultural Extension Service, and had the Coleman and others (1966) investigation of a new medical drug been conducted
under the auspices of the American Medical Association, rather than Pfizer Drug Company. Perhaps "diffusion" research would have been called something like "innovation-seeking" or the "evaluation of innovations" had the receivers been in control (Rogers with Shoemaker, 1971, p. 79).

The source-bias in marketing research on diffusion is especially surprising as this scientific specialty is often called "consumer research" in graduate schools of marketing, and it is often inspired by the "marketing concept", an approach that puts the customer in control of the marketing process, at least in principle (Kotler and Zaltman, 1971). Often, in diffusion researches following the marketing concept, the customer has been studied, but usually to the main direct advantage of the seller of the new product or service, and only indirectly, if at all, to the advantage of the consumer.

Consumers have often benefited from the diffusion researches in which they were respondents, even though such studies were usually commissioned by the selling agencies, if the consumers' needs are met by the new products that emerge from such diffusion researches. These investigations can put the consumer in the driver's seat regarding new products, especially through a variant of diffusion inquiry called "acceptability research," in which the consumers' desires are determined and then a new product is designed to meet these previously unmet needs. Acceptability research began at the hands of marketing researchers,* and is now also followed in wider contexts. For example, the World Health Organization is currently involved in a research program in which the desired qualities of contraceptives are determined for the fertile

*And on the part of Flicgel and Kivlin (1966).
audience in Latin America, Africa, and Asia so as to guide WHO bio-medical researchers in the invention and development of future methods of family planning. This acceptability approach puts the potential consuming couples in the position, via survey researches, of helping design more acceptable contraceptives.

Nevertheless, certain basic consumer-oriented research questions have not been asked in diffusion research, such as: How can the consumer be protected against the influence of advertising (or other promotional) messages? What information does the consumer need to know in order to make intelligent innovation-decisions?

Marketing Strategies in Diffusion

Not only did the classical diffusion model intellectually infect the marketing field, especially in the 1960's, but, in more recent years, this academic borrowing has flowed in the opposite direction, as we have just illustrated in the case of acceptability research in designing innovations.

A second major contribution of marketing to the diffusion field is strategic thinking. Diffusion research has always been a very applied type of social science research (which does not deny that it can also be very theoretical), and one reason why there are so many diffusion studies is because somebody was willing to pay for them, usually on the assumption that the results of such researches would be practically useful.

And indeed many diffusion research results have been used by change agencies. The sensitizing concepts of opinion leadership, innovators, and stages in the innovation-decision process are today a part of the everyday thinking of change agents in most parts of the world.
But exact specification of how to manage diffusion activities so as to achieve maximum desired results has been lacking until recently, when attention began to focus on diffusion strategies, defined as plans or designs for changing human behavior on a large-scale basis through the transfer of new ideas (Rogers, 1973, p. 28). Essentially, a strategy is a unit of communication management.

The emphasis on strategies as bite-sized, useful nuggets of behavioral science know-how came to the diffusion field from marketing; articles by Kotler and Levy (1969) and by Kotler and Zaltman (1971) were especially influential in broadening the scope of marketing strategies from just the selling of toothpaste to social marketing concerns with all types of innovations. Formulation of diffusion strategies has probably received the strongest emphasis to date for family planning innovations (Roberto, 1972; Rogers, 1973), but such marketing-type strategies as audience segmentation and product differentiation are equally applicable to a wide range of other innovations.

I expect we shall see a much wider swing to strategic thinking about the diffusion of innovations in the immediate future, and in fact a widescope diffusion of marketing concepts to many types of social science and of social programs, if the perceived acceptability problems of "marketing" can only be overcome.

TOWARD OVERCOMING THE SHORTCOMINGS OF DIFFUSION RESEARCH

When we look back over the yesterday of the diffusion field, we see a rapidly-growing invisible college that today represents one of the largest sets of social science research, commanding considerable research resources, and with
a high degree of orderly progress toward scientific goals, even if the early
days of creative originality are long gone. In short, diffusion research has
become established, if not an Establishment.

But I am far from sanguine about diffusion research today, and, in fact,
about the broader nature of social scientific research of which it is part.
I see three main shortcomings which need to be overcome:

1. Lack of a process orientation.
2. A pro-innovation bias.
3. A psychological orientation that shortchanges structure.

Lack of a Process Orientation

Certainly diffusion is a process. But the research designs and measure-
ments of diffusion almost never adequately allow analysis of the over-time aspects
of diffusion (that would be necessary to adequately explore process). Very few
researches include data at more than one observation point, and almost none at
more than two such points in time. So almost all diffusion research is unable to
trace the change in a variable over "real" time; it deals only with the present-
tense of behavior. Diffusion thus becomes, in the actuality of research, an
artificially-halted snapshot.

Why has diffusion research not dealt more adequately with the change-
over-time aspects of process?

1. We lack concepts and propositions which reflect a process-
orientation.
2. Time-series data are expensive to gather, unless one depends
on respondent recall, a procedure that is often less than satisfactory.
3. Repeated data-gathering over time leads to problems of respondent sensitization (unless one uses unobtrusive and non-reactive measurement methods).

4. Diffusion researchers are often pressured by research sponsors, doctoral requirements, and other logistic forces to produce immediate results; this is a strong discouragement to over-time research designs.

Most diffusion research designs consist of correlational analyses of cross-sectional data gathered in one-shot surveys of the respondents, thus exactly following the method pioneered by Ryan and Gross (1943). By 1968 (the last time a tabulation was made of the methodologies used in diffusion studies), only 65 of the then 1,084 empirical diffusion publications, (about 6 percent), reported results from field experiments. Even allowing for the 67 diffusion publications (another 6 percent) that reported longitudinal panel studies at two or more points in time, the vast majority (about 88 percent) of all diffusion researches are one-shot surveys allowing only cross-sectional data-analysis. Such research designs cannot tell us very much about the process of diffusion over time, other than what can be reconstructed from recall data.

Future diffusion research ought to make much greater use of field experiments and longitudinal panel studies which, by their research designs, are able to take "moving pictures" of the diffusion process.

The Pro-Innovation Bias

Most diffusion research has an inherent pro-change bias in that it assumes the innovations studied are "good" and should be adopted by everyone. Undoubtedly, hybrid corn was profitable for each of the Iowa
farmers in the Ryan and Gross (1943) study, but most other innovations that have been studied do not have this high degree of relative advantage. Many individuals, for their own good, should not adopt them.

The pro-innovation bias, coupled with the unfortunate and overwhelming dependence on survey research designs, means that diffusion research has mostly studied "what is", rather than "what could be" about diffusion processes. So method has followed the assumption that innovation is good, that the present process of diffusion is satisfactory and needs only minor tune-up, rather than a major overhaul. Röling and others (1974) have heavily scored diffusion research on this count, arguing that it has often led to increased inequity in the distribution of socio-economic benefits of innovation; field experimental designs are needed to test alternatives to current practice, rather than the replication of more surveys of "what is". When diffusion research is almost all of the latter type, an implication is thus given that present methods of diffusion are largely appropriate, even when the researches rather consistently find that the diffusion of an innovation causes a wider "communication effects gap" between the more-advantaged, and the less-advantaged, segments of the audience. Since the important article by Tichenor and others (1970) focusing scientific attention on the distribution of diffusion effects, concern and inquiry about the growing gaps caused by the diffusion of innovations has been evinced by several authors (Katzman, 1974; Donohue and others, 1975; Rogers, 1974).

THE PSYCHOLOGICAL BIAS

The Individual as the Unit of Analysis

The overwhelming focus on the individual as the unit of analysis
in diffusion research (while largely ignoring the importance of communication relationships between sources and receivers) is often due to the assumption that if the individual is the unit of response, he must consequently be the unit of analysis (Coleman, 1958). Only recently has the main focus in diffusion research on the individual as the unit of analysis shifted to the dyad, clique, network, or system of individuals; to the communication relationships between individuals, rather than on the individuals themselves. Encouraging attempts to overcome the psychological bias in diffusion research are provided by network analysis, and by the open systems approach (Rogers and Agarwala-Rogers, in press).

These conceptual-methodological approaches suggest that even when the individual is the unit of response, the communication relationship (even thought it can't "speak") can be the unit of analysis via some type of sociometric measurement. Sampling and data-analysis procedures for relational analysis are being worked out (Rogers and Bhowmik, 1971), but we still lack relational concepts, and theories linking these concepts. Until diffusion scholars begin to think in relational terms, there will not be much relational analysis.

Person-Blame

One reason for the artificially "de-structured" psychological bias in diffusion research is the acceptance of a person-blame causal-attribution definition of the social problems that are studied: Individual-blame is the tendency to hold an individual responsible for his problems. Obviously, what is done about a social problem, including research, depends upon how it is
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defined. Many illustrations of individual-blame can be cited in behavioral research. Caplan and Nelson (1973) find a high degree of individual-blame in psychological research on such social problems as highway safety and race relations. They ask: "Why do we constantly study the poor rather than the nonpoor in order to understand the origins of poverty?"

Diffusion research was originally (and for many years) as guilty as other types of behavioral research in following an individual-blame approach: "We note an assumption in diffusion writings that the rate of adoption should be speeded up, that the innovation should be adopted by receivers, etc. [This is a consequence of the pro-innovation bias of diffusion research]. Seldom is it implied in diffusion documents that the source or the channels may be at fault for not providing more adequate information, for promoting inadequate or inappropriate innovations, etc." (Rogers with Shoemaker, 1971, p. 79).

Restoring Social Structure to Diffusion Research

The re-focusing of diffusion researches really began with the drug study among medical doctors by Coleman and others (1966). Especially thereafter it became a common procedure for diffusion scholars to ask their respondents sociometric questions of the general form: "From whom in this system did you obtain information that led you to adopt this innovation?" The sociometric dyad represented by each answer to this question could consequently by punched on an IBM card (including data on the characteristics of the seeker and the sought), which then became the unit of analysis.
The relational data thus obtained was utilized to provide deeper insight into the role of opinion leaders in the two-step flow of communication, a conceptualization that was originated prior to most diffusion research by Lazarsfeld and others (1944). The two-step flow hypothesis turned out mainly to be a gross over-simplification (as the flow of communication may actually have any number of steps) later research showed, but the concept of opinion leadership has much theoretical and practical utility. Diffusion researches were able to advance understandings of opinion leadership because of their unique capacity to focus on the flow of innovations, new messages (to the receiver) that seem to leave deeper (and hence more recallable) scratches on men's minds. But the real advance in understanding interpersonal diffusion, including opinion leadership, had to await the use of network analysis.

Network Analysis of Diffusion

Network analysis is a method of research for identifying the communication structure of a system, in which sociometric data about communication flows or patterns are analyzed by using interpersonal relationships as the units of analysis (Rogers and Rogers-Agarwala, in press). This tool promises to capitalize on the unique ability of diffusion inquiry to reconstruct specific message flows in a system, and then to overlay the social structure of the system on these flows. The innovation's diffusion brings life to the otherwise static nature of the structural variables; network analysis permits understanding the social structure as it channels the process of diffusion. About the only other place in communication research where network analysis has been used to restore social structure to the communication process is in a few recent investigations of organizational communication.
An illustration of a network analysis of the interpersonal diffusion of family planning in two Korean villages is shown in Figures 2 and 3 (Rogers and others, 1975). Village A (shown in Figure 2) has twice the rate of adoption, and half the level of discontinuance, of Village B, although they are of the same size (N=39), and an identical diffusion campaign was launched at the same time (1964) in both villages. The differences seem due (1) to the nature of the informal cliques, liaison and bridges in the two networks, and (2) to the opinion leaders’ behavior (most of the main leaders in Village B have discontinued the innovation).

The Strength of Weak Ties

Out of the network analyses of interpersonal diffusion grew a research issue that came to be called "the strength of weak ties" (Liu and Duff, 1972; Granovetter, 1973), a topic we mentioned previously. The proposition summarizing this research is that: The informational strength of dyadic communication relationships is inversely related to the degree of homophily (and the strength of the attraction) between the source and the receiver. Or in other words, an innovation is diffused to a larger number of individuals, and traverses a greater social distance, when passed through weak ties rather than strong (Granovetter, 1973).

Each individual operates in his/her particular communication environment for any given topic, consisting of a number of friends and acquaintances with whom the topic is discussed most frequently. These friends are usually (1) highly homophilous (or similar) with the individual, and with each other, and (2) most of the individual's friends are friends of each
Figure 2. Adoption of Family Planning in Village A in 1973.

Figure 3. Adoption of Family Planning in Village B in 1973.
other, thus constituting an "interlocking network" (Laumann, 1973; Rogers, 1973). This homophily and close attraction facilitates effective communication, but it acts as a barrier to prevent new ideas from entering the network. So there is not much informational strength in the interlocking network; needed are some heterophilous ties into the network to give it more openness. These "weak ties" enable innovations to flow from clique to clique via liaisons and bridges.

Network analysis of the diffusion of the IUD in the Philippines demonstrated this strength of weak ties: The innovation spread most easily within interlocking cliques, among housewives of very similar social status (Liu and Duff, 1972). But heterophilous flows were necessary to link these cliques; usually these "weak ties" connected two women who were not close friends, and allowed the IUD to travel from a higher-status to a somewhat lower-status housewife. So at least occasional heterophilous dyadic communication in a network was a structural prerequisite for effective diffusion.

The case of network analysis of the strength of weak ties illustrates an important recent trend in diffusion research: The concepts used in this analysis are relational constructs. Perhaps we are seeing the real beginning of relational thinking in communication research.

CONCLUSIONS

Our quick tour of the past 32 years of diffusion research provides a case illustration of the growth of an invisible college. The acceptance of a revolutionary paradigm by scholars in a field enables them to cope with
uncertainty and information overload, through the simplification of reality that the paradigm represents. It also imposes and standardizes a set of assumptions and conceptual biases that, once begun, are difficult to recognize and overcome.

In my opinion the research designs, concepts, and measurement in research on the diffusion of innovations ought to be continually questioned by scholars in this field, and alternatives considered. Then indeed we might see some real innovation again in research on the diffusion of innovations.
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