

Oral Comments on Garvey & Griffith's "Informal
Channels of Communication in the Behavioral Sciences,"
Response July 29, 1965

ALFRED DE GRAZIA

Editor, American Behavioral Scientist

I have been asked to give informal comments on an informal communication, about informal communications. I must say that this is the most significant research that I've seen in the field of scientific communications. I have felt for several years that it would be, and I've watched with some concern and anxiety the progress of the psychologists in leading the way in this area. It is unfortunate that other kinds of social scientists have not been so forward.

The present concern is part of a much larger problem of scientific communication that stretches off into the natural sciences. These sciences are perhaps worse off. They may have better superficial retrieval devices but they are also very much burdened with hierarchies, establishments, and various limitations of a structural sort that the social sciences avoid simply because nobody has figured out how to control social scientists as well as the natural scientists control their own through such means as their more strict vocabulary and precise hypotheses.

So I wish to make quite clear my wholehearted support of the research and my anticipation of future volumes, which I shall read with all the abandon of a new James Bond thriller.

I might say that I wasn't too happy with the distinction that was made between the *formal* and *informal*. I thought it was a fairly *formal* distinction. I would have preferred a distinction made in terms of sanctions; that is, where rewards or punishments for a communication are great enough, it becomes a *formal* communication. Or where people subjectively perceive that they are doing something important, or more important, then it becomes a formal communication. Perhaps a more subjective definition like that could be added to the present, structured definition.

I wonder, too, whether there might not be some hidden values in the rather conventional acceptance of the term "functionalism," which causes so much trouble in social linguistics. I believe in functional analysis and all that goes with it. However, very often, there are *presumed* motives and goals included in what purports to be an *objective* functional, or neutral functional analysis, and then other kinds of functions are left out because they "should not" be mentioned. For example, in the field of political parties where I have been writing recently, I

discovered what I thought to be a great new thing, namely that political parties really exist because they allow everyone to be concurrently ignorant and intelligent. You can know absolutely nothing about what is going on in politics if you simply say that you are a Democrat or a Republican, and stop there. But this use of the party pervades the legislative halls too, and time after time we discover that a great deal of the voting done in the name of the party is not really party voting because it has nothing to do with issues. People want to justify their actions and so the old cliché of "I'm a Democrat so I voted the Democratic ticket" is used.

The problem of maintaining objectivity in an analysis of function is a difficult one. For example, we could talk about the payoffs of informal communications in much more specific terms. Where do people formally and informally communicate? We discover that the informal is a more pleasant ambience.

On the other side the formal communication is usually prepared under, you might say, *negative* circumstances, with children under foot, grass to mow, in a library stack, etc. Now if you start weighing the respective human values of these two ambiances of functional analysis, then you discover that the time may be approaching when all contributions will be informal, because the surrounding factors are so pleasant in the informal communication.

And this condition will be especially severe when we realize that "nobody reads anybody," that the average number of readers of articles in important psychological journals seem to be about a hundred persons (that's first time readers, perhaps fifty years from now others will read more).

Then we also have a shadow system whereby people buy reprints and foist these upon their friends and hope for some sort of response, thus assuring themselves that there is somebody, somewhere reading.

But that's a very costly system and our authors here are correct to stress the high cost of the informal system. We are paying very heavily for the inadequacies of the formal system.

And we could, as I have pointed out, pay even more highly for the informal system if it spreads more completely in chaotic fashion.

I think we might also have brought out other positive functions of the informal system; perhaps they were implied, I wouldn't say they were not. In some cases there occurs a natural superiority of the informal product. Men say things in their right minds that they wouldn't say when writing for publication. I guess you can put it the other way around, too.

Also, who has the "right mind," the author as he does the first draft? Yes and no. Certainly for heuristic purposes, for teaching others,

there is nothing so useful as a publication that has all the weaknesses of the author in it. By the time you come around to putting out your formal publication, you've seen to it that you are not vulnerable. But also not so bright, perhaps. And the student learns by errors. Nothing is so informative as the errors of a great man.

Of course, there is also the incentive, and you might speak again of the dysfunctions of the informal system, the problem of sloppiness, of grinding out everything that one wishes to communicate.

But you know that the proliferation of the number of magazines in this world is such that it is possible ultimately to publish anything. I could give you a good map of how to publish anything. It could ultimately lead to Pakistan; but it will have the kudos of an editorial board and a linotype machine in back of it.

This mania to publish leads to stern reproofs. For example, Dr. Harold Urey told me in a letter not too long ago that he longed for the old days when a few competent men were in charge of the journals and people really had to go through the hoops in order to have their works published. He said that now every second or third rate government scientist or professor has a mimeograph machine in the back room and turns out anything he pleases.

That is Urey's belief. I emphatically disagree with him.

I wonder, also, whether there will be some kind of accordion-effect developing. Perhaps these gatherings will continue on and on until the world is full of informal communications. But then, if we engage in miracles of retrieval, and push everything else out of the library and just put in retrieval machinery and bibliographies, we'll find that interest in gatherings would stop and informal communications cease.

And will we not also find that this will engender reaction on the part of the elite in the profession. Will they not try to put the damper on informal communication because they discover people will not patronize the shops that the elite have set up in the professional journals and associations. I think we can expect quite a battle here in the form of academic struggles.

As our authors imply, the scientific periodical is theoretically dead. Still it refuses to die. But some people will begin the funeral oration and set off a counter-revolution at some point. At that point we may expect to find sentiments such as those of Dr. Urey, that the professions through their organizations, should control the flow of publications. The rationale will be that the public should not be subjected to the annoyance, the misinformation, and the lack of scientific standards that pervade the hundreds of thousands of informal communications.

We have said nothing about books. Books present a grave problem

of retrieval too. How many titles a year are relevant? Perhaps 20,000 *per annum* at the most are serious books, distributed in prominent languages. Multiply that figure for a generation and you find an astonishing number of titles, at least the equivalent of all the titles that have ever been printed, and these are to be read by people, who if they read like mad for 45 years, will only read a couple of thousand books in their lifetime. Not to mention the articles they must read. So that if we put the articles in with the books, I think we arrive at rather startling statistics on the production of books and articles. The number of books and articles which any generation of scholars can read is small indeed when compared to the mass of reading matter available.

Again we confront this "yin and yan" problem of the elite principle versus the democratic principle. Someone has to pluck up his courage to draw distinctions between material which should be retrieved, material which should be referred to and read, and material which is of little importance and can be ignored.

I am impressed also with the resemblance of this field of research despite its formidable subject-matter, to market research. And we have here very traditional problems in market analysis: to find out what it is that people are consuming, and why they are consuming it, in what forms they would like their materials packaged, where they would like to buy them, and how much you can charge for them.

I've noticed a tendency, carried over from our old puristic days, to forget about these very practical matters and to think that you can somehow study the whole problem of scientific communication as pure theory. But the problems we are talking about are the problems of the gathering, production, dissemination of information, and it is a commodity, or can be construed as such. It may be the most wonderful commodity in the world, and one that we think is much better than toothpaste to concern ourselves with. But we may also find that the many lessons of toothpaste merchandising and marketing are of some use in the fields of scientific communication.

Well, we have had predicted here in the field of scientific communication a crisis within five years. Perhaps that will come true. The first crisis is a conventional crisis, though. If we draw an analogy between the changes in the field of information and the changes which took place in Russia early in this century, we would call this crisis the "Kerensky revolution." Grave inroads will be made in the old system of retrieval; rather simple retrieval systems will be developed; but authors will go on writing as they always have. We shall find better ways of getting a work out quickly and into the right hands.

All of these developments will occur in the "Kerensky phase," and

we may look back with nostalgia at this period of revolution, even though we are Czarists at heart, because the phase after that will be really destructive. That will be the Bolshevik phase in which we can expect a real revolution at the grass roots of communication. There will be changes in the whole manner of writing. Because when you have millions of items to distribute, the problem of redundancy becomes terrible and there must be some steps taken to see that authors do not duplicate each other's work but conduct original inquiries and record their conclusions. The manner of writing will have to be adjusted to the manner of retrieval, thus necessitating adjustments in the design of the computer.

The exigencies of the machine are already affecting scientific vocabularies in the incipient phases of information retrieval and computerized indexing. This in turn will affect the habits of scholars, their manner of writing, and the recognition of scholarship. Ultimately we must cope with the problem of formulating proposals in such a way as to guarantee their retrieval.

There will also occur a final problem: that of the nameless author. Within a generation or so we will be unable to guarantee an author either traditional or conventional identification for his work. The work will either be in a useable form and contribute to its author's anonymity or it will bear his name and make the knowledge of the name an essential factor in retrieval. In the latter case, the work could face little use and in a sense it would achieve its own anonymity.

Undoubtedly we shall survive these successive revolutions in science. They certainly are not as ominous as some of the other transformations which threaten us.