# A Tribute to the Second Sigma

By David Wallace

These pages are graced by David Wallace's presidential address. He delivered it to the members of the American Association for Public Opinion Research and their guests at the Fourteenth Annual Conference on May 16, 1959, in the Sagamore Hotel at Bolton Landing, Lake George, New York. The Proceedings of the Conference over which he presided appear elsewhere in this issue.

David Wallace served as President of the American Association for Public Opinion Research during the 1958-1959 term. He is currently interrupting his research career for a year of study at Columbia University and lecturing at the New School of Social Research. He was formerly on the staff of the M-E-L Division of the Ford Motor Company.



This is AAPOR's fourteenth annual meeting. And as one of the rapidly withering band of true souls who have been in attendance at each and all, perhaps I may be permitted a bit of reminiscing.

You know—there was no such infliction as the presidential address in AAPOR during our first four years. Then, in 1950, Paul Lazarsfeld, our president that year, occupied the podium with Elmo Roper in one of those brotherhood-of-academics-and-commercials sessions that we do so well. In his allotted time, Paul made some insightful but informal remarks on the obligations of the pollster to the historian. In due course, these came to be "expanded," as the expression goes, into a scholarly piece published in the Winter issue of POQ. (I forget in which season the "Winter" issue actually appeared that year.) It was labeled a "Presidential Address." And thus the institution was born. Tonight I shall try to preserve its rich heritage of rolling rhetoric—though in charity, I beg you to remember that the institution has preceded the incumbent, and not vice versa.

It seems back in the dark ages that our first gathering was held, in, of all implausible places, Central City, Colorado, once a ghost mining town, 26 miles outside of Denver. The idea of such a meeting was promoted by a visionary of whom, unfortunately, too few of us today have even heard. His name was Harry Field. Harry had been an advertising account executive with Young & Rubicam and, through illness in his family, had emigrated to Denver. While at Y & R, Harry had been intrigued with the research George Gallup was doing there. So, with time on his hands in the West, Harry founded the National Opinion Research Center at the University of Denver. That was in 1941, five years before the Central City meeting.

Perhaps prophetically, AAPOR—this interminable name was not to be hung upon us until the following year at Williamstown, Massachusetts—was not founded without a touch of skullduggery. Denver, as many of you may have heard, is a long way off, and there was an honest question as to how many rational persons might travel 2,000 miles to a meeting of a yet-unborn association. In the spring of 1946, then, Harry sent a "feeler" invitation to some eighty individuals in the field—and particularly to the Big Three of election polling, Messrs. Roper, Gallup, and Crossley. Through some vagueness of wording, the letters to each of these luminaries contained the strong implication that the other two were highly enthusiastic, if not that they had already made plans to come. I don't say this was an outright lie, because Harry was a man of principles as well as of persuasion. Let's just say it wasn't too clear. But when one of the parties in this squeeze play did accept—I think the first was Gallup—immediate and crystal-clear telegrams were rushed to the remaining two. In this way, Field bagged his big-name stars, and, overnight, firm announcements were sent to one hundred additional persons—practically everyone who had even a relative in the survey field. It was thus that your speaker was included.

Associated in various ways with NORC at that time were Clyde Hart, Don Calahan, and Ann Scheutz, among others in the West. In the "Eastern Office" were Paul Sheatsley, Herb Hyman, and Herb Stember. Uptown in Denver's commercial world was a NORC alumnus and now-entrepreneur named Bill McPhee. With these as a sort of nucleus, seventy-three hardy pioneers assembled at Central City, of whom certainly no more than one-fourth were on the payroll, in one way or another, of NORC. Never have we had so large, or so cohesive, a "local arrangements" staff.

Central City is memorable in many ways. Our meeting place was the Teller House, best-known to the outside world as the authentic site of the Face on the Barroom Floor. The Face was still there, somewhat refurbished, to be sure, and now in oil color. It was tastefully floodlit and protected from irreverent scuffing by a table nailed to the floor. Next door was the Opera House, at which a road company of the Metropolitan Opera is engaged each year for a two-week music festival. It so happened that the yet-unnamed AAPOR overlapped the Metropolitan not only by one day in time, but in competition for the somewhat limited and picturesque facilities of the Teller House. The overlapping in time produced easily the most expert singing of old songs at 3 o'clock in the morning of any AAPOR conference. But the overlapping in use of the so-called "facilities" of the Teller House holds more personal poignance for your speaker.

The Teller House at that moment boasted but one bathroom per floor of a dozen or more rooms. Upon retiring late, your speaker decided to play the bathroom deal smart. He set his alarm for 6 A.M. and bounded out of bed after three refreshing hours of sleep. In slippers and robe, towel over one arm, he groped his way down the dim, narrow hallway in the direction of the said facilities. At the end, he took the required sharp turn left, and found himself—seventh in line! The Metropolitan was leaving early that morning.

Those were exciting days in the opinion business. Efforts have been made to describe just when it first began. In his book, *Modern Public Opinion,* William Albig picks 1930 as perhaps the start of any serious flow of articles on opinion theory and survey methodology. Fred Stephan relates that the earliest academic articles ran through 1928 into the early 1930's. Arch Crossley wrote a few years back, "the coming of age of public opinion research as we know it today can be placed at about 20 years ago."

But no matter. Or, as it has been phrased recently, "not to beat an old bag of bones around,"[1] the fact is that during the mid- and late 1930's there was lots of activity in the field. A few years earlier, George Gallup had run up a dissertation at Iowa on the readership of the *Des Moines Register and Tribune*. Doctorate in hand, he became an Instructor of Journalism at Northwestern, where he was in 1932 when Raymond Rubicam snatched him from the ranks of the ill-paid to set up a research shop at Y & R. The American Institute of Public Opinion, a subsequent enthusiasm, was launched in 1935.

During approximately the same years, another chap in the Midwest had set his heart on becoming a diplomat and so had attended Edinburgh, among other places. There, in the shadows of Adam Smith, he studied Moral Philosophy and Economics. He was even then a devout Democrat, and that was diplomacy's loss. A sudden Republican shift eliminated his sponsor, and so he went into the jewelry business instead. Some time later, after seven years of striving valiantly to be the Tiffany's of Preston, Iowa, young Elmo Roper was $29,800 in the red. To bail out, he took a salaried job with Seth Thomas Clocks and in due course became solvent as well as National Assistant Sales Manager. Later, he was engaged by the Orange Blossom Engagement Ring Corporation to look into their slumping sales and profit. He must have done a spectacular job, for in 1932—again, in the depression—"Orange Blossom" was riding high again. This caused talk, and at a luncheon with Richardson Wood, then of the J. Walter Thompson advertising agency, and Paul Cherington, an independent consultant and teacher, Roper heard for the first time that he had been doing "market research." The three shortly went into business as Cherington, Roper, and Wood. Two years later, they approached *Fortune* with the novel notion that a continuing survey of public opinion might make a worth-while editorial feature. The Fortune Survey was born in July 1935.

In New Jersey, meanwhile, a Princeton lad was checking radio commercials that the Frank Seaman advertising agency had bought. It was not unknown then—or even today—for an out-of-the-way station to sell time to a national advertiser and then substitute a local plug instead. From this start, Arch Crossley hatched the brainstorm that it might be possible to measure

[1] Senator Dirksen's malapropism on the occasion of the Senate's confirmation of Mrs. Clare Luce as Ambassador to Brazil, May 1959.

the size of the audience of a radio station or program. Crossley, Inc., launched the Cooperative Analysis of Broadcasting in 1930, for which it received the Harvard Award in 1931. A succession of other ideas and other awards followed. Perhaps the most intriguing was the self-validating "Garbage Pail" index of consumer consumption. Special containers were supplied to a sample of people in which they deposited the various cans, wrappers, bottles, and whatever they had emptied during the week. (It is rumored that tabulation was done in an odorproof room.) Then, in 1935, with both Roper and Gallup planning to poll the man-in-the-street prior to the forthcoming presidential election, King Features asked Crossley to do a similar job for them.

At about the same time, a boat pulled in from Vienna and, amid clouds of cigar smoke and excited accents, a man named Lazarsfeld stepped off. Paul had been pioneering with psychology in market research in Vienna, and the Rockefeller Foundation thought that a two-year fellowship in the United States might broaden such a man. It was an aid-to-undeveloped-areas kind of thing. Well, the United States has not been the same since. After the fellowship ran out, Paul found he liked it here and wangled a bread-and-butter job at the "University of Newark." Then radio started to boom and the Rockefeller Foundation, again, became interested in its effect on the national mind. As director of a first project to this end, they picked young Lazarsfeld and gave him two assistants named Cantril and Stanton. The former was to serve as a sort of cultural link with the Princeton milieu, where the Office of Radio Research would be located, and the latter was a bridge—and translator—to the commercial radio world. Young Stanton was in research at CBS at the time, so his services wouldn't be too greatly missed. In due course, the project was shifted to Columbia and became the Bureau of Applied Social Research, which shortly would be conducting the first panel study of voting behavior in Sandusky, Ohio. (Tests of the idea had been done from Princeton in the 1938 New Jersey gubernatorial election.) Then a post opened in the faculty at Columbia and Paul became an academic.

Another first in these years was *Life's* idea of measuring the total *readership*—as opposed to paid circulation—of mass magazines, under the direction of a body called the Magazine Audience Group. Neil DuBois was Research Director of *Life*. Among the members of MAG were Gallup, Roper, Crossley, Lazarsfeld, Ray Franzen, and Sam Wilks.

In any rumination on the early days of our business, a grateful spot must be reserved for the circulation department of the *Literary Digest*. As a promotion gimmick, the *Digest* had mailed millions of "straw ballots" to telephone and automobile owners before the 1928 and 1932 presidential elections. They had been correct in predicting both outcomes. Flushed with confidence, the *Digest* in 1936 loudly ballyhooed a Landon victory—while the *nouveaux* Gallups, Ropers, Crossleys, all using relatively tiny quota samples of only a few thousand cases, demurred for Roosevelt. The success of "scientific polling" could hardly have been more spectacular. To the man on the street—and to a host of new clients—opinion research was born!

Things moved quickly after that. Shortly, a major upheaval in methodology came from Washington, where Morris Hansen, Steve Stock, Les Frankel, Fred Stephan, and others crusaded for the "area," or "probability," method of sampling. Then came the war, and it seemed that all social science was absorbed bodily by Washington. In the OSS, OWI, OPA, the I & E Division of the Army, the Department of Agriculture, huge new developments in survey techniques, ideas, and theory formation were made by groups working around Sam Stouffer, Rensis Likert, Angus Campbell, Louis Guttman, Carl Hovland, Kurt Lewin, and a long list of others. Anyone who wasn't living in Washington—or commuting back and forth as a "consultant"—simply wasn't in research at that time. The activities of just one of these groups, the I & E Division, are documented in the classic four volumes of *The American Soldier*.

This, then, was the electric atmosphere of communication and public opinion research as Harry Field gathered in Central City on July 29, 1946, such Washington alumni and civilian practitioners as could raise the plane fare.[2]

Compared with those vibrant days, the present seems strangely quiet.

Why the lack of noise these days? Or could I be wrong? Are there just as many exciting things going on, and is the trouble simply that we have become accustomed to them? Or have we perhaps grown up and graduated to less boisterous and more genteel matters? I'm not sure, but I do know that I am not alone in this disquiet.

Last year, at the Edgewater Beach meeting, Barney Berelson reported on the "State of Communication Research," now extended as the keystone of a four-way symposium in the spring issue of POQ. Barney argued then, and now, that the "state (of communication research) is withering away." He pointed to the four major approaches during the past twenty-five years: the political approach of Lasswell in the early thirties, the sample survey approach of Lazarsfeld in the late thirties, the small-groups approach of Lewin in the late thirties, and the experimental approach of Hovland in the early forties.

But all these were *part* of the excitement of fifteen and more years ago; all were *prior* to the Central City meeting.

Then Barney continued: "After reviewing the development and the characteristics of the four major 'schools,' I conclude that the innovations of fifteen to twenty-five years ago have been wearing out, and that no new ideas of comparable scope are appearing to take their place. . . . Communica-

[2] Harry Field was killed in an airplane crash in Europe in 1947.

tions research has had a distinguished past, but the question remains as to what its future will be."[3]

These movements, of course, were primarily in methodology. What about our theory? David Krech, a long-time needler of our activity, wrote some years ago: "Although these social psychologists attempt to measure beliefs, attitudes, judgments, and opinions, neither the commercial polling organizations nor the academic research groups have shown any significant tendency to concern themselves with . . . the theory of that which is being measured. . . . There is a tremendous amount of busy-work, of measuring and adding and percentaging . . . they know not what."[4]

Then, William Albig, again, reviewing "Two Decades of Opinion Study: 1936-1956" for the twentieth-anniversary issue of POQ, notes with appreciation an "enormous production," "conducted with enormous gusto,"—but— "In spite of this . . . I am not encouraged when I review what I have learned of meaningful theoretical significance about communications . . . and . . . about the theory of public opinion."[5]

In the same issue, scarcely fifteen pages later, Harold Lasswell views "The Impact of Public Opinion Research on Our Society." And here, again is the indictment: "At the level of fundamental theory nothing has been added"[6] —even since the classical writings of the nineteenth century!

And while Harry Alpert was more sanguine in his address on "Public Opinion Research as Science," at Buck Hill Falls, he nonetheless observed, "If we are to pursue the scientific study of public opinion research" . . . we must "concentrate more intensively . . . on the development of conceptual frameworks capable of organizing into meaningful classes and categories the over-abundant amounts of raw empirical materials which are being rapidly amassed from numerous sources."[7]

Here, again, I believe, is a feeling that not much new theory is coming into evidence. And yet this is odd, because Clyde Hart recalls that in 1954 our Committee on Research Development polled the membership of AAPOR on the aspects of our business they considered most in need of further research. "Replies from ninety members put theory and problem statement at the top of the list."[8]

[3] Bernard Berelson, "The Present State of Communication Research," *Public Opinion Quarterly*, Vol. 22, 1958, p. 178, and expanded in "The State of Communication Research," *Public Opinion Quarterly*, Vol. 23, 1959, pp. 1ff.

[4] David Krech, in Harry Helson, editor, *Theoretical Foundations of Psychology*, Princeton, N.J., Van Nostrand, 1951, p. 659.

[5] William Albig, "Two Decades of Opinion Study: 1936-1956," *Public Opinion Quarterly*, Vol. 21, 1957, p. 16.

[6] Harold Lasswell, "The Impact of Public Opinion Research on Our Society," *Public Opinion Quarterly*, Vol. 21, 1957, p. 33.

[7] Harry Alpert, "Public Opinion Research as Science, *Public Opinion Quarterly*, Vol. 20, 1956, p. 498.

[8] Clyde Hart and Don Cahalan, "The Development of AAPOR," *Public Opinion Quarterly*, Vol. 21, 1957, p. 171.

Well, now, what *is* the answer? Lord knows, there is no lack of "busy-ness" in the field. There are more people than ever, more budgets than ever, more projects, more data. In posing this question to others, I frequently get the response that it was easier to have novel ideas in the early days; that so little was known almost any good idea was a major contribution. The inference seems to be that we are exhausting our problems, that we're running into sort of a Hovland "ceiling effect," in which the higher you go, the less space remains for potential expansion, and therefore that comparable input results in ever-decreasing increments of progress on the output side. But this line of reasoning assumes there is an absolute and inviolate top—like the 100 per cent mark on a percentage scale. Such a notion also begs the main point at issue, for the charges are not that our theory building has slowed, but that it seems not even to have started.

On the exhaustion of knowledge, your speaker is reminded that when he was a promising young premedical student some years ago, the chemistry and physics in which he majored were regarded as "mature" sciences. There was not much more to be known about them. The structure of the molecule was such-and-such; its composition was clear and precise, consisting of a number of definite things called "atoms" (the word "atom," incidentally, is from the Greek "atomos," meaning indivisible) which milled around in a nice set order. Chemistry and physics were cut-and-dried (by his instructors anyway).

But today, not only the atom, but its nucleus of protons, neutrons, electrons, and mesons are vast mysteries. A recent issue of *Scientific American* mentions that no fewer than 208 "particles" are now identified in the ***nucleus*** alone of the lead atom, and that the pattern of this arrangement is sheer conjecture.[9] I don't dare to speak now of chemistry, except to mention that the last I heard it was no longer being regarded as a science in itself, but merely as a branch of physics. Ah, for the simple days when facts were known!

In another area of communications in which some of us work there is an even more startling lack of knowledge—and even of seeming concern about that lack. I speak now of the nature of advertising; what it is and how it works. Last year, several *billions* of dollars were spent for advertising in just the major media of newspapers, magazines, television, and radio. Yet to my knowledge, at least, there is nowhere in this field a single comprehensive statement as to the theory, or nature of the dynamic, of advertising. This is all the more surprising because the authorities in this field are acting as experts in advising business how to communicate its messages to people. Martin Mayer in *Madison Avenue, USA*, was struck by this same phenomenon after ten months of interviewing some of the biggest figures in advertising. In fact, Mayer felt called upon to volunteer his own hypothesis in a final chapter entitled "A Premise for a Theory and Some Modest Pro-

[9] R. E. Periels, "The Atomic Nucleus," *Scientific American*, January 1959.

posals." But even here, all that Mayer could suggest is that advertising gives "added value" to a product.

For what it is worth, your speaker has a private suspicion that one of the major forces in advertising may be more sociological in nature. Rather than being a direct one-to-one relationship between seller and buyer, it may just be that mass advertising gives the prospect an assurance that *other* persons in his social environment are being informed of the virtues of the product at the same time he is; and thus, should he buy, his act will be supported by their appreciation that he has made a wise move. Why buy a Schultz refrigerator instead of a GE—even though they may be identical—if you have to explain your choice to everyone who may drop by for a beer? Better buy GE in the first place, and have your judgment pre-accepted. But, as I say, this is private conjecture—and, to the best of my belief, not generally recognized by the sages of Madison Avenue.

Why are so few new ideas coming out of opinion research? Why so few controversies? So little noise? Certainly, there is no lack of behavior still to be explored and explained. No lack of people working. What has happened since the early boisterous days of Central City and beyond?

To find an answer, we might pursue the best traditions of Durkheim and Weber. That is, to remove ourselves for a moment from the trivia and the thousand-and-one distractions on the inside, and take a long, detached look at what I hear called the "external data," or the "empirical manifestations." Perhaps from this vantage point we can deduce something of what would have to go on inside in order to produce the outside effect.

Foremost among the external data is one of the phenomena we already have noted; namely, that whereas during the prewar and war years there was a lot of excitement and novelty in our field, there appears to be little comparable inventiveness since. In fact, about the only hubbub I can think of recently came from a few flurries over the F-scale, subliminal advertising, and the appropriativeness of significance tests in social research. Other external data, of course, are the economic and political milieux in which we have lived since the war. The former has been downright prosperous: even after last year's recession, GNP today is more than twice that of fifteen years ago. Among political issues recently, only desegregation seems to have made any deep impact; even Sputnik I now appears to be largely forgotten, and Berlin and "strontium fall-out" pack no real popular wallop. In contrast, there were plenty of economic and ideological issues in the thirties and forties. We might note also another, although not necessarily equal, development: the fact that there has been a generally very comfortable flow of funds for research from prospering governmental, philanthropic, and commercial sources.

This is far from a comprehensive, or even an adequate, summary of the exterior, but it already suggests a relationship.

Could it be that some tiny part of our lack of invention is due, whether we admit it or not, to the fact that *we are pretty content with things as they are*—that *we simply do not want any upsetting new ideas or theories?*

There are other factors, certainly. One is that after the first big break into any new field there invariably follows a period of fractionation; the chasing down of exceptions to the generalized theory, the resolving of "special cases." This makes a low hum instead of a roar. A second factor is more unique with us. The early "opinion research" was in the open public area of political behavior. Since then, a number of more private and specialized—and to practitioners, perhaps more intriguing—interests have grown up in the side areas of client-therapist relations in psychiatry, medicine, religion, mental health and so on. These, admittedly, have diverted a lot of attention from the mainstream of political and consumer opinions. But their presence does not eliminate what I would like to focus on here—the raw fact that *we may not want any controversial new theory; that we may be pretty content with the way things are.*

It is much more than a saying that you get pretty much what you want in this world. It is almost an axiom, in fact. For, having once determined what it is we want, it is all but impossible to keep our thoughts, perceptions, and actions from following in this direction. It is the mechanism of the self-fulfilling prophecy, in which a person himself largely brings about what he wishes for. As Walter Lippmann has observed in the matter of stereotypes, "When a system of stereotypes is well fixed, our attention is called to those facts which support it, and is diverted from those which contradict."[10] It is selective perception, congruence with congenial attitudes, sensitivity, or whatever you wish.

Let's face it. A really new idea or theory in any field is upsetting, *per se.* It says in effect that what we believed in and knew—and perhaps advised others on—only the day before is now wrong. Overnight, we have become nonexperts. This is unpleasant.

So the new must always be challenged, resisted—beaten to the ground if possible. For, in the short term, only a very few can benefit in its being—while armies must yield pride and vested position by its acceptance.

In a field such as ours which is so new, ephemeral, complex, and uncharted—sharing the birthright of all social sciences—eminence is but the more precarious. When even what we can regard as our "facts" is unclear, there almost *has to be* a tendency to cling too tenaciously to that which seems solid, and thus to resist any enlargement of the areas of doubt.

So I wonder—could it be that somehow, in these immature but easy times, we have lost any incentive to stick out our necks? That with more persons in the field who have some speaking knowledge about it, failure is felt to be more readily detected and turned into ridicule? Have the stakes become so

[10] Walter P. Lippmann, *Public Opinion,* New York, Macmillan, 1922, p. 119.

high in terms of reputations and livelihoods that it just doesn't pay to take risks anymore? In short, are we playing it too safe to come up with any invention?

Some time ago, I had what seems to be the naïve notion that one thing AAPOR might properly do to advance our field was to encourage the release and publication, after a decent time, of the many, many private studies that are now entombed in files. In feeling out the matter with the research head of one of the more active advertising agencies, I found no great reluctance under the heading of the "confidential" nature of the studies; he thought that many clients would agree to release. But a stumbling block was the agency itself. As he explained it, what with pressures of time and client requirements, the agency was not anxious to lay its work open to professional scrutiny. I think this is what I am talking about.

If this is so, we may just as well save the energy that has been spent in working up tears and beating our breasts about the lack of new ideas and theory. *We're just plain not going to have any.* For, throughout history, few real advances have been made by men playing it safe—and, quite frequently, the great discoveries have been made by odd-balls and deviants.

Which brings me to the somewhat cryptic—and admittedly inexact—title of this essay, "A Tribute to the Second Sigma."

What I have in mind is the classic representation of the normal curve, in which two-thirds or so of the subjects are huddled together in the middle. This, I have been told, is the First Sigma. A little further out on both sides is the Second Sigma, containing approximately one-fourth of the subjects, plus and minus. And still further out are the Third, Fourth, and whatever-you-want-to-make-it, Sigmas, containing the remaining 5 per cent of the cases. These are the real weirdies. Now there are faults in this analogy, but it will serve the purpose. And although one-fourth of mankind is more than I wish to pay tribute to in this address, you will see in a minute why I don't want to invoke the weirdies. My tribute, then, is on the generous side.

History abounds with examples of reluctantly accepted great ideas. In the physical sciences, alone, it would have to include those of Copernicus, Galileo, Newton, Darwin, Father Mendel, Albert Einstein, and a host of others. None were in what you might call the accepted ideology of their time; few were easily or immediately acclaimed.[11] Just one small and little-publicized example in the case of Darwin. You are familiar, of course, with the spectacular simultaneity with which Darwin and Wallace arrived at the theory of natural selection, and the dramatic co-reading of their papers before the Linnaean Society in London in 1858. But in his report on the year's activities, Professor Thomas Bell, the society's then-president, saw fit to regret that 1858

[11] Copernicus hugged his secret of the heliocentric nature of the universe for thirty-six years because he didn't want to be ridiculed by his academic peers. Mendel's discovery of the laws of heredity lay unnoticed for thirtv-odd years.

had "not been marked by any of those striking discoveries which at once revolutionize, so to speak, the department of science on which they bear."[12]

On a more modest scale, we have the recent example of the "Crazy Greek" who, with conspicuously little formal erudition, anticipated some of our best physicists at Berkeley and Brookhaven with his so-called "strong focusing" principle of trapping fast-moving electrons within a strong magnetic field—and then how this notion led to Project Argus—the exploding of atomic missiles 300 miles in space. The story of Mr. Christofilos' repeated rejection before he was found to be right and the abruptness with which he was then put to work at Brookhaven must certainly be entered among the legends of belated recognition of maverick genius.

As Dean Schilling, of Pennsylvania State University, says, "science is a typically human enterprise with the . . . weaknesses and strengths these usually possess. . . . Here ideas are tentative and impermanent. . . . More often than not they are audacious guesses or vague hunches that rarely conform to established patterns of thought. Often they are thoroughly unorthodox and what many people would even regard as 'unscientific.' "[13]

In the era of excitement in our own field twenty-odd years ago, certainly the Gallups, Ropers, Crossleys, Lasswells, Lazarsfelds, and others were not dedicated to the *status quo*. They were trying out new ideas and hunches—daring to bet they would be right more often than wrong. In any distributional arrangement of accepted procedures of those days, these men would not be found in the First Sigma, the two-thirds of their contemporaries who were playing it safe by holding to the tried and true. No, indeed, they chose their futures in the second, or third—or further—sigmas. And by upsetting the accepted, they led the way to our progress. This was not done by pussy-footing, or inching along slightly ahead of the prevailing pace. Their steps were giant steps—and risky ones at times.

Before leaving evolutionary theory, there is a debt we may owe to the men of the Second Sigma—those who have had the daring, the brashness, the vision, the genius to think and act a little bit differently from the prevailing mass. The debt is for our very existence. For many years, Darwin pondered why some species had disappeared from the face of the earth, while others had survived. At long last he hit upon the Principle of Divergence. In his words, "The more diversified the descendants from any one species become in structure, constitution and habits, by so much will they be better able to seize upon many and widely diversified places in the policy of nature, and so be enabled to increase in numbers."[14] In short, to paraphrase a familiar expression in academia, "Diversify or Die!"

A few moments back, I raised the speculation that perhaps the environ-

[12] *New York Times*, July 2, 1958.

[13] H. K. Schilling, "A Human Enterprise," *Science*, Vol. 127, 1958, p. 1324.

[14] Charles Darwin, *Origin of Species*, Mentor, p. 112.

ment of recent years has in some subtle way increased the risks to professional reputations of coming out with a new idea which may be found faulty, and that this has inhibited any real invention, even though we bemoan its lack. A recent issue of the *Sociological Review* serves as an example of what I think I mean.[15] This is the issue devoted to "deviant behavior"—itself pertinent to the topic in hand. It contains articles on both the plus and the minus sides of the case.

On the plus side, we find the two opening articles by Robert Dubin and Richard Cloward. Both offer considered and important extensions of Bob Merton's typology of deviant behavior, as published in his chapter on "Social Structure and Anomie."[16] The typology includes four modes of adaptation: innovation, ritualism, retreatism, and rebellion. Dubin's paper extends Merton's four distinctions to fourteen in order to differentiate between the concepts of behavior and of value. Cloward, working in the field of criminology, extends Merton's early ideas to accommodate the idea of "illegitimate" as well as legitimate means to approach a given goal. Both papers are scholarly and responsible, worthy contributions to the theory of anomie.

Following these is Merton's "Comment" (of some twenty-four columns!) from which I would like to quote because it demonstrates an attitude worthy of the highest commendation, and which is better stated by Merton than by me. Merton openly welcomes these contributions, saying, "Both papers, in my opinion, move toward a more adequate sociological theory of deviant behavior. In so doing they exemplify one way in which a theory develops through successive approximations. A set of ideas serves, for a time, as a more or less useful guide for investigation of a set of problems. An inquiry proceeds along these lines, it uncovers a gap in the theory; the set of ideas is found to be not discriminating enough to deal with aspects of the phenomena to which it should apply in principle. . . . Not the least merit of contributions such as theirs is that they keep us from behaving like sociological barnacles clinging desperately to theories we have learned in our youth, or which we may have helped develop at any age."

Then Merton, finding these papers heuristic to his own thought, proceeds to extend the extensions, and to point out additional ideas not developed by the authors. In all, a highly statesmanlike exhibition of mutual advancement of a theory.

But then—a few pages later—we come to another article. This is the "critique" of Albert Cohen's fascinating theory of deviant behavior.[17] This is not the place to deal substantively with Cohen's ideas, except to say that

[15] *American Sociological Review*, April 1959.

[16] Robert K. Merton, *Social Theory and Social Structure*, Glencoe, Ill., Free Press, 1949, Chap. 4.

[17] Albert Cohen, *Delinquent Boys: The Culture of the Gang*, Glencoe, Ill., Free Press, 1955.

he sees "working-class boys," to use a euphemism, being brought up in an environment where parental education may be grade school or less; where the prevailing literature is the comics; where the intellectual outlook is bounded by the batting averages of the Dodgers and who-looks-good-in-the-third-race at Jamaica. As these boys grow, they are plunged into a school and social system tailored by, and to, middle-class mores and the Protestant ethic. The result is divided values, bewilderment, frustration—and ultimate revolt against the system that brought them into the dilemma. Cohen's thesis is novel, scholarly, and has been extraordinarily well received.

In the article under discussion—which I emphatically place on the negative side of this contrast—the authors go to extravagant lengths to pick apart Cohen's ideas, limb from limb. Here, again, a quote serves best: "We contend that: 1) Cohen does not present adequate support, either in theory or in fact, for his explanation of the delinquent subculture; 2) the methodological basis of the theory renders it inherently untestable; 3) the theory is ambiguous concerning the relation between the emergence of the subculture and its maintenance; and 4) the theory should include an explanation of the persistence of the subculture if it is to meet an adequate test." In all, a picky, contentious, myopic sort of thing which, after two charitable readings, contributed little to this student, at least.

As I say, these two examples taken together; the positive, constructive contributions of Dubin, Cloward, and Merton on the one hand, and the negative, witch-hunting, linguistic contortions of what shall be nameless authors on the other, are a dramatic expression of what I am trying to get at. Granted there is a place, and, if you will, a necessity, for searching examination of whatever new theory or procedure is proposed. But, somehow, I do not find the two efforts of equal stature.

In his "Comment," Merton notes that a "central goal of science is the advancement of knowledge" and that, "indeed, the institution of science actually calls for variants that will better meet the goals of new knowledge." Then he goes to some lengths to distinguish between the good word "variant" and the blacksheep "deviant," the former being variability of approach *within* the system or, as he puts it, the "institutional goal," while the latter is rejection of, or departure from, the system.

Now Merton knows more of these sociological words than I, but it seems to me that we are talking along pretty much the same line. Here, at least. In my own little paradigm—see how you catch on to the lingo?—I have attempted to do something of the same by dealing with a single dimension: a normal distribution along a common baseline of contributions to public opinion theory and measurement. My eulogy is to those who possess the gumption to contribute some variety to the cause, without necessarily calling for the grotesque, the absurd (but who knows?) of the third, fourth, and further-out sigmas. As admitted earlier, a kind of soap-scrubbed liberalism.

To advance—or, in this evolving world, even to keep from falling behind—requires ideas which are in some way different from those now in vogue. A new idea affronts current notions—it wouldn't be a new idea if it didn't. It is the price of progress that there must always be difference—some noise—in the system. All creativity is essentially a departure from agreed-upon ways of looking at things; and to hold blindly that the "proven," the accepted, are the only legitimate ways to deal with a problem is to contribute to an atmosphere of hostility toward the creativity on which our progress depends.

The British scientist, C. D. Darlington, put the matter more strongly in his famed 1948 Conway Memorial lecture. He spoke of scientific discovery as "often carelessly looked upon as the creation of some new knowledge which can be added to the great body of old knowledge. This is true of the strictly trivial discoveries. . . . But the fundamental discoveries on which scientific advance ultimately depends . . . always entail the destruction or disintegration of old knowledge (i.e., the accepted way) before the new can be created."[18]

Now, here's the pay-off. Obviously, we can't pass a resolution that starting tomorrow at 12:01 A.M. everyone here must start trying to be a genius. The talents of genius, for true invention, are given to but few of us. Nonetheless, there is a way within the reach of us all in which we can create creativity.

*The way is simply to permit it!*

Each of us can contribute importantly to the birth of new ideas and formulations by providing an atmosphere of encouragement to them. Instead of trying to tear down every variation that comes along, and thereby to inhibit the next, and perhaps more successful, try, we can adopt an "institutional goal" of thinking ever in terms of the advancement of our knowledge—and being hospitable to the variants that necessarily are involved in it. In short, we can adopt an open-door policy to invention.

One answer, of course, to the seeming disparity between the older days and now is that we didn't know then how vast and complex is the nature of opinion formation and measurement. We didn't know, so like a brash youngster we simply barged in and did something. Today, having seen a bit more and being more mature, we may be awed and constrained, afraid of looking foolish should we fail. If so, I would suggest we forget our present sophomoric pretentions and start barging again.

For knowledge of the total range of human behavior is inexhaustible and the ultimate is unattainable. We are never going to be smart in any absolute sense, so we might as well get going with what we have.

In closing, I am reminded of a remark I heard a year or so ago in Dearborn. I was lunching with several top men in the automobile business. (It was one of the few times, incidentally, when the topic got off automobiles.)

[18] *The Conflict of Society and Science*, London, C. A. Watts Co., Ltd., 1948.

The first satellites had just been tossed up and we were talking of perigees and apogees and the new cosmos since Sputnik I. One chap, thinking, I guess, of the telephone, the automobile, radio, television, electronics, and such, broke in with, "Do you think there will ever again be a fifty-year period in which so much is discovered?"

I was appalled at this perspective. Sure, today we can split atoms, create nuclear fission, throw spitballs beyond the moon. But we can regain a little humbleness by reminding ourselves that man has yet to make a blade of grass. He has yet to create a single living cell. He does not know what the common cold is, or how his own digestive process works. He has only the vaguest notion of the cortex of the human brain—and is importantly prevented from knowing because of a self-imposed ethic. He does not know the nature of light by which he sees—whether it is composed of waves or, again, of particles. And certainly he knows nothing of the processes through which he gets all his erudition—the processes of learning, thinking, communicating, and having opinions on these matters.

We have exhausted all knowledge? One final plea for perspective. Man can trace his history back perhaps 5,000 years. Our present calendar focuses on 2,000 of those years, beginning at A.D. 1. If we let the *thickness* of this index card represent the total of those 2,000 years, then the span of man's time on earth is about 4 feet in length, or some 5,000 times the length of Christian history. The distance back to the first primitive living cell, the origin of life, is then 200 feet; and the age of this minor, pint-sized planet which we call earth reaches out one-third of a mile.

On this same scale, the total span of what we call "opinion research" is some *one ten-thousandths of 1 inch!*

There is a long way to go in the advancement of our knowledge. We desperately need the ideas and help of the men in the Second Sigma. Let us encourage that help.