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# PRESENT AT THE BEGINNING:

## SOME PERSONAL NOTES ON OB'S EARLY DAYS AND LATER*

George Strauss

This isn't going to be a conventional autobiography. The great events of my life, my marriage and family, are hardly of interest to the wider world. The bulk of this chapter consists of some highly personal views of how what later became Organizational Behavior looked to me in the late 1940s and early 1950s, when I was a graduate student at MIT. I will then use my remaining space for a quick sketch of the next 40 years of both the field and my career.

I work in what we call at Berkeley OBIR (Organizational Behavior and Industrial Relations) and much of my work has been at the intersection of these two fields. Of the two fields my greatest emphasis has been on IR. My relationship to OB has been more of an observer than a participant -- and it is an observer that much of this is written.

## HOW I GOT INTO OBIR

Not long ago, Frank Schmidt of the University of Iowa asked me, "What brought you to Industrial Relations? Like most IR people, was there something special in your background?" Well, yes and no. My father was a chemical engineer whose interests included science, natural history, his garden, and much else (I used to think he knew everything), but not politics. My mother had been a nurse in the Belgian Army during World War I. The Army had sent her to this country for advanced training. Instead she met and married my father. Her interests were cleanliness, order and good cooking. Among her highest compliments was, "My, he has a good appetite."

Maybe industrial relations was in my genes: my Belgian grandfather had been a longshore boss and an active Socialist. He had sided with the union in a bitter strike at the turn of the century. When the union lost he was blacklisted and had to change his occupation. He died young, leaving behind seven children under 18, and for some time after my mother's family lived in real poverty. My American grandfather was a lawyer and later a judge, the first Democrat elected from Wilkes-Barre, Pennsylvania in decades. As a lawyer he represented the United Mine Workers and one summer spoke on the Chautauqua lecture circuit, presenting labor's case.

I grew up in Staten Island, New York, during the Depression. While I was never poor or hungry myself, it was drummed into me that many people were poor and hungry (especially Belgians during the war) and therefore I should always eat everything on my plate, a lesson I haven't since forgotten. So I ate well, but felt a bit guilty in doing so. The sight of people begging and selling apples in the street greatly bothered me. What right did I have to eat so regularly?

In the fifth grade I read The New Russian Primer, which contrasted the joys of a planned society with the miseries of capitalism in a depression. For a year I

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* For Arthur Bedeian, ed. Management Laureates: A Collection of Autobiographical Essays. JAI Press.

was a communist. But in the sixth grade I read Bellamy's Looking Backward (1887), and so became a Fabian Socialist.

Obviously I was a rather strange kid. Beginning at age 10 I began to read the New York Times religiously, including the full texts of major New Deal legislation and Supreme Court decisions. The progress of the labor movement especially interested me. I remember my great thrill when General Motors signed its first agreement with the UAW. (That was a private thrill. My public thrill was when the Yankees won three series in a row.)

I went to a private school (the same one that Fritz Roethlisberger had attended some 24 years earlier). In the fifth grade while studying the Middle Ages, we made illustrated parchments in bright-colored inks. For their messages my classmates generally took conventional pieties, such as "Love Your Mother" or the Golden Rule. Mine was taken from Section 7A of the National Industrial Recovery Act: "Employees shall have the right to organize and bargain collectively...". My illustrations showed a breadline transformed into a picket line.

By high school I became more conventional. I had played football all along and finally made the varsity team (in my small school that was easy). My senior paper was not about politics at all, but was heavily influenced by the Lynds' Middletown (1929). Based on old newspaper files, I tried to place the early development of my school in the social history of Staten Island's 1880s. I ascribed its founding to the social-climbing efforts of German immigrants seeking to overcome any taints caused by their association with the Island's then flourishing beer industry. A great idea, but I had little evidence. In any case I was downgraded for my great nemesis, bad spelling.

For college I went to Swarthmore (in 1940), majoring in economics and political science (both "relevant" courses) though philosophy and history were attractive diversions. I took introductory psychology, hoping it would be loaded with Freud and sex. Instead it was concerned with vision and perception. How dull! I dropped the course.

Swarthmore was a place for someone with political interests. Quickly I became active in the Swarthmore Student Union (SSU), a non-Communist liberal group which, earlier, under the leadership of Molly Yard (now head of the National Organization of Women), had split with the Communist-led American Student Union over Russia's 1939 invasion of Finland. ASU had supported Russia. The Swarthmore group opposed both Hitler and Stalin, and much of our effort was spent fighting Communists. Strange as it may seem to student activists today, SSU strongly supported the allied war effort, the draft, and a larger military budget. Domestically we supported all the standard liberal causes of the time, including unions. As to campus affairs, we were against fraternities and in favor of admitting blacks (something which Swarthmore, a Quaker college, was quite late in doing). Eleanor Roosevelt was one of our heroes and indeed I introduced her at a SSU-sponsored meeting and sat (tongue-tied) next to her at dinner. In my junior year I was elected SSU chair.

Some of my limited free time I spent helping to put out the local labor weekly, sometimes working all night. Here I met local labor leaders in the flesh. Then in spring 1942, with the U.S. already at war, one of my political science professors, Vernon O'Rourke, decided to run for Congress against a diehard reactionary Republican (among his sins was voting against the draft). I was O'Rourke's

campus campaign manager. We ran quite a campaign: bands of students pushed doorbells daily and a quarter of the entire student body took election day off to round up the votes. We lost our heavily Republican district but lead the rest of the ticket by a large margin and did better than FDR two years earlier. One thing I learned from my political activism (by contrast with that learned by the activists of the 1960s) was the value of compromise, bargaining, and smoothing feelings -- not that I ever performed these arts well.

By now the war was beginning to close in. I entered the Army in February 1943, being assigned to a Miami Beach hotel for basic training. My military career was not particularly distinguished. In fact, for me the war was personally a bore. I left the U.S. only once, on a B-29 test flight to Bermuda to determine whether we GIs preferred normal airline food to K rations (guess what we decided?). Still I learned a good deal about a bureaucratic -- very bureaucratic -- organization. I picked up enough "war stories" to carry me through many lectures later on. I will abuse you with two samples.

War Story No. 1. One of my first days at Miami Beach I volunteered for KP. Let's get the war over in a hurry, I decided, so I can get back to school. Later I could make a point in class here about motivation, public goods, etc., but what I really learned was that some people (like my sergeant) become sadistic when given a little power. After 18 continuous hours of largely make-work I never volunteered for anything in the Army again. From then on I became adept at what F.W. Taylor aptly called "soldiering."

War Story No 2. A year later I had picked up a little power myself. I was sergeant, working at LaGuardia Field, NY (living at home, taking advantage of the USO's ample supply of free tickets for New York plays) and assigned to the Office of Flying Safety. Our job was to monitor military flights in an area from roughly Washington to Boston. If weather turned bad we were to reroute these planes to safety. One hot summer Sunday afternoon I was working alone when thunderstorms broke out through southern New Jersey. Immediately (I loved the authority) I ordered all northbound planes from Washington to land at Philadelphia. Technically I did this in the name of Hap Arnold, Commanding General, Army Air Force, since the Office of Flying Safety was part of the General's staff. Unbeknownst to me, one of the planes which landed was piloted by Hap himself, flying under his copilot's name. As a Command Pilot, Hap had the right to ignore my order. As a sensible pilot, he landed. Here were all sorts of lessons here about roles, staff-line relations, and authority (French and Raven, take note).

Several hundred stories later the war was over. I was discharged at San Pedro, California, (I'd arranged this with great effort so as to collect the maximum travel pay home). Taking advantage of my uniform I hitch-hiked across country (great stories there) and returned for my senior year to Swarthmore. Then followed glorious days! The world turned from black and white to technicolor. Wonderful people to talk to. Interesting problems to grapple with. Once again O'Rourke ran for Congress and I coordinated the campus end (this time much more efficiently). Once again we mobilized most of the students. Once again we lost, but again greatly lead the rest of our ticket.

With graduation looming I had to face the real world once more. For a while I played with entering the union movement. But one of my most respected role models (the chief steward at Westinghouse) threw cold water on my cockiness.

"George, my advice is you get a job in some plant (just keep your degree quiet)" he said. "If you are as good as you think you are, in a few years you will be elected shop steward. Then a few years later, if you handle your politics right, you'll be chief steward. Then, maybe they will take you onto the international staff. But don't count on it." After three years in the Army, this didn't seem promising. Instead I decided to go to graduate school, picking MIT over Harvard and Berkeley. My choice of MIT was based on Paul Samuelson's stellar performance as an oral examiner in the Swarthmore honors system and his cordial, personal letter admitting me, as contrasted with Harvard and Berkeley's cold form letters.

Vaguely I planned to study industrial relations, hoping somehow this might make some contribution to the union movement. In this I was not alone. The postwar wave of strikes had made industrial relations the country's number-one social problem. I was part of a large cohort of returning veterans who entered graduate study of industrial relations, courtesy of the GI Bill. In 1947 more members of the American Economic Association listed labor as their major field of interest than any other field. The professors who dated their Ph.D.s from this zesty era constituted the core of most academic industrial relations groups until recently. We saw collective bargaining as a realistic means to give the working man a new sense of dignity, to correct inequities in the distribution of income, and to improve society. The important point was that industrial relations at the time was both intellectually challenging and socially relevant. (Strauss and Feuille, 1978).

## CAMBRIDGE

I was lucky to arrive in Cambridge at a critical time in OB's development. Cambridge offered a real intellectual smorgasbord. At MIT I was in the Department of Economics and Social Sciences which included both Psychology and Industrial Relations. There was similar disciplinary flexibility at Harvard where the Department of Social Relations combined Anthropology, Sociology, and Social Psychology. At MIT we graduate students were divided into As (economists), Bs (industrial relations) and Cs (psychologists). Among my fellow Bs were George Shultz, later to become a great Secretary of State, of Labor and of the Treasury, and two long-term friends, Leonard Sayles, my writing collaborator, and Ralph Bergmann, whose career has included the United Rubber Workers, the International Labor Organization, and Fresno State University.

Regardless of designation we were broadly trained. Our comprehensive exams covered three major fields (mine were economic theory, industrial relations, and human relations) and three minor fields (mine: personnel, labor law, and group dynamics). Additionally, we needed an outside department. I chose Industrial Administration (later the Sloan School), then better known as Course XV. On top of this, MIT students were allowed to take courses at Harvard, and I took advantage of the opportunity. Finally, being young and full of energy, I did a fair amount of relevant reading on my own. In short, my training was like the old Missouri River, both broad and shallow.

Our training was also very flexible. Because the field was so new, at least in Human Relations, our professors had little advantage over us. Everywhere we looked was terra incognita. As a consequence, there was a great sense of collegiality among the students and (from our point of view) between students and faculty. My dissertation chair was Charles Myers, a labor economist, was very supportive but he did little to influence my approach or choice of topics.

Though the main emphasis of this story is on what later became OB, let me first say a few words about some of my other interests and classes.

Economics.

Prior to MIT the thought hadn't entered my mind that Economics and Industrial Relations might be separate fields (a lesson which some of my colleagues still haven't learned). Economics was formally my primary field and Economic Theory, with Paul Samuelson as instructor, was my key first seminar. Six weeks into his seminar, Paul handed me a reprint of a recently published paper. There was a flaw here, he said. I should examine it carefully. Next week we might discuss it in his office and perhaps I would like to write a reply. I wrestled with the paper all week. A theoretical piece, it dealt with selling costs and imperfect competition. Like most works of this sort its assumptions were simplistic and unrealistic. Aside from these major faults I could find nothing wrong with the argument. Sheepishly, at the appointed time, I reported my failure. Kindly and patiently, Paul pointed out the flaw. "How trivial," I thought to myself, "Who cares?" This was a critical turning point in my career. I was not to be an economist. What a relief!

Industrial Relations.

Industrial Relations was another matter. Three topics were of especial interest: first, labor history, (a lifelong interest - I devoured much history, besides our assigned reading); second, the "causes of industrial peace." The National Planning Association series by this name were just being published, and these gave hope that, through effective communications, appropriate organization and, above all, trust, peaceful labor relations could be obtained. The values and solutions implied were similar to those being advanced by Human Relations.

Finally, as a budding IR man, I needed to know labor law. So, after one labor law course at MIT, I took the basic labor law course at the Harvard Law School with Archie Cox (later Watergate prosecutor) and then a year-long seminar with Cox and John Dunlop (later Harvard Dean and Secretary of Labor). By contrast with the social science professors I had known, Law School professors were viewed by their students as God-like. So when Cox had his secretary invite me to his "chambers" my fellow students were much impressed. And when he asked me to give a paper (based on my seminar work) on union democracy to the spring meeting of the newly established Industrial Relations Research Association, I was on top of the world.

Another seminar was with Joe Scanlon. Joe, an ex-boxer, steelworker and union official, was the author of the Scanlon Plan, perhaps still the best devised plan for combining worker participation with financial incentives. In Joe's seminar I learned a bit about how to role-play collective bargaining. Yet, to my disappointment, Joe and I didn't strike it off well together. Possibly I was insufficiently sensitive to the fact that critical questions about his plan were unwelcome.

Time and Motion Study

For my unwilling minor in industrial administration I took cost accounting, controllership and time and motion study. Only time and motion study was of note.

For those of us interested in human relations, F. W. Taylor was the devil incarnate. The same building which housed the MIT Economic Department also contained a bastion of pure Taylorism. I decided to beard the devil in his den and to take a course in time and motion study. It beautifully confirmed my worst expectations. Probably it was much the same course which Roethlisberger had taken 25 years earlier, a course he later called "pure, unadulterated nonsense." (1977, p. 21). As did Roethlisberger, "I took great delight in collecting ..'horror stories'" (p. 21) about how bad it was.

We learned how to hold the stop-watch and the clipboard, how to find "cooperative workers", how to use "therbligs" (Gilbreath spelled more or less backwards), and above all "normalizing," how to tell whether a worker was working at, below, or above the mystic "normal" pace. We learned the latter by watching movies of women packing Necco Wafers at various speeds and guessing how fast they were working.

Quickly I learned that though Scientific Management pretended to be scientific, in fact it was highly subjective. Even today a typical union contract provides that time studies shall "be based on the time required by a qualified normal employee working at a normal pace under normal conditions, using the proper method with normal material at normal machine speeds." Later on, when I was teaching time study to unionists, in the late 1950s, I insisted that they never touch a stop-watch. That would get them into management's trap. Instead they should challenge management on every application of "normal".

For our term papers we were divided into teams of four, with instructions to find a real job, time it, simplify it, and then retime it. My three engineering colleagues quickly gained entry to a company and started timing a presumably typical job. While they were doing their calculations I talked to the operator involved. Using my newly acquired interviewing skills I learned that he worked the standard way only when the boss was around. Actually he had developed a little gadget, which allowed him to work twice as fast.

Back at MIT, my colleagues did their little bit, analyzing and simplifying the job, eliminating some motions, transferring work from one hand to another, and finally cutting the allowed time by one quarter. We wrote our report. Then I added an appendix, based on my interview. We all got As. Fine, but my stupid colleagues handed the company a copy of our report, including my interview!

So my Taylorist course confirmed my prejudices. Later, as I read more about the intellectual milieu in which he worked and his quirky, convoluted personal life (Kakar, 19??), I developed a real interest in the old boy. The juicy details of his life have enlivened many of my lectures. Certainly there are some damning quotes in his work which can be easily taken out of context. Yet today I think he and Max Weber (the two were often linked together) had a very, very bad press. Taylor was among the first to stress and study motivation from a managerial point of view; he believed in rules (which protect workers as well as management) and so helped defang the arbitrary supervisor; further, he assumed that management could

be studied -- that it was a science rather than art. Indeed it might be argued that he was the father of OB. But it took years for this revisionist point of view to gain a hearing. At the time Taylor represented everything that human relationists felt wrong.

Having disposed of the anti-Christ, let me turn to the true religion, Human Relations.

Hawthorne and Harvard-style Human Relations.

Much of what I knew as human relations could be traced back to a network which developed at Harvard in the 1930's, where Lawrence Henderson (a biologist) and Elton Mayo (a medical school drop-out) played major roles, influencing not just Roethlisberger and Hawthorne, but Lloyd Warner, William F. Whyte, and Chester Barnard, all of whom were at Harvard during this period.

By the time I reached Cambridge only Fritz Roethlisberger and George Homans were left. Although I never had classes with either I was greatly influenced by the Hawthorne experiments in which both had participated. I read Management and the Worker (1939) with great care and some of Mayo's case studies. On the other hand, his more philosophical work turned me off as quite elitist -- and still does.

Hawthorne's values were the antithesis of Taylorism. Hawthorne dominated the human relations field of the 1940s. Its lessons, as I saw them were: (1) the significance of social needs; (2) the power of group standards; and (3) the difference between the Relay Assembly Test Room, where one form of supervision led to a group decision to increase productivity, and the Bank Wiring Observation Room, where another form of supervision led to output control.

Further, Hawthorne pioneered in the methodology of organizational research. The Relay Assembly Room involved a field experiment, the Bank Wiring Room applied what we now call ethnography, the observation of on-going behavior. Hawthorne's final stage illustrated the value of non-directive listening as both a research technique and a tool for sound management. (Carl Rogers discovered the non-directive technique at much the same time as Roethlisberger; Rogers, however, used it primarily as a form of therapy; Roethlisberger used it initially for research). The methodology of the Relay Assembly Room experiment was grievously flawed, but this is irrelevant. Hawthorne gave us a new way to think about work life. Eventually it gave rise to more carefully designed research.

Except perhaps for Mayo's rather fuzzy philosophizing, Hawthorne never contributed to theory in the way that Barnard's contemporaneously published work did. This may have been related to the Harvard case study tradition. In teaching cases the instructor asks questions, for example, as to the protagonists' motivations and the possible implications of various courses of action. The purpose is for the student to learn as much as possible about the individual situation. But other than asking questions and suggesting a broad range of possible relevant factors, the case study approach is short on analytical tools for resolving problems. It is primarily inductive rather than deductive.

Counseling and non-directive listening, as practised by Roethlisberger, was in some ways merely an application of the case method. Indeed the non-directive

listener was expected to behave somewhat like the case-method instructor. Clients were expected to think through their problems and develop their own solutions.

Roethlisberger taught one of the earliest "Human Relations" courses, in 1948, and perhaps the first seminar to be called "Organizational Behavior", in 1957. Arguably, therefore, Harvard and Roethlisberger were OB's founders. Nevertheless, despite the significant contributions of such luminaries as Paul Lawrence and Jay Lorsch, the field's cutting edge went elsewhere. The Harvard Business School environment was a difficult one in which to go beyond case collecting.

Applied Anthropology.

Hawthorne's methodology and analytic framework borrowed heavily from an amazing variety of sources, ranging from philosophy and physiology to Janet, Pareto, and Freud. Among the more significant contributions were made by anthropologists, indirectly by Durkheim and directly by Warner, a former student of Malinowski.

Aside from the Hawthorne group there were a number of key people who were interested in applying anthropological concepts and methodologies (primarily through direct observation) to contemporary society. Many were formally trained as sociologists and were allied with the then-vibrant Chicago school of urban sociologists. Many called themselves applied anthropologists and belonged to the newly formed Society for Applied Anthropology.

The work of applied anthropologists influenced my thinking greatly. The Lynds' Middletown and Warner's Yankee City series, for example, taught sensitivity to status, status symbols, social (group) standards, and ethnic differences. For a while I almost automatically tried to classify people and communities by whether they were Lower-Middle, for example, or Upper-Lower. Bill Whyte's two major studies, Street Corner Society (1943) and Human Relations in the Restaurant Industry (1948), built upon this foundation. His study of status among vegetable preparers had quite an impact on me: the first thing I looked for in a factory was its status ladder. Doc, in Street Corner, defined for me what leadership was all about. Only recently did Bob House make me realize that there is another aspect of leadership, charisma. (Doc had charisma, too!)

The Restaurant Industry made another important point. The objective conditions of the job could have a major impact on attitudes and behavior. These included what we would now call job characteristics (such as Task Significance), work flow, and opportunities for communications. These insights were reinforced by the work of psychologists associated with the British Industrial Health Research Board's pathfinding experiments (e.g., Wyatt and Langdon, 1937). Pat Smith's early work (1953) was also quite relevant. Together these studies laid the basis for modern Job Design. Later research (e.g., Hackman & Lawler, 19 ) did little more than confirm their findings.

In my early days I attended the meetings of the Society for Applied Anthropology fairly regularly. I also wrote two articles for its journal, now called Human Organization. The Society's best known figure was Margaret Meade, who reminded me much of a clucking mother hen, watching us, her brood. I was delighted when she showed some interest in my dissertation. For quite a while I

listed my field on questionnaires as "anthropology." What pretension! But I certainly wasn't an economist.

Group Dynamics

On the other side of the MIT campus from Economics, in a war-temporary building, was the Research Center for Group Dynamics (RCGD), sadly bereft of its founder, Kurt Lewin who died the day I arrived in Cambridge, But his spirit lived on. The RCGD, with its degree in Group Psychology, was closely linked with that of the Tavistock Institute in London and together they were in the forefront of what was then known as group dynamics.

Despite the physical distance between RCGD and my own group, I knew quite a lot of the RCGD people socially, though our groups were somewhat rivals. Once we had a touch football match: George Shultz was our quarterback and Hal Leavitt our star pass receiver. For the Dynamos Hal Kelley received the passes. Was John Thibault their quarterback? Who won? Here are critical social data I forget.

The relationship between our two groups was relatively short-lived, since RCGD had difficulty in getting permanent MIT funding and moved to Michigan sometime in 1948 or 1949. Nevertheless, the influence of RCGD on my thinking persisted.

A major distinction between group dynamics and Harvard-style human relations, was that group dynamics people were attached to an established field, psychology, and theories were important to them. Yet theories and research were expected to be more directly applicable than they are today. As Lewin put it, "There is nothing so practical as a good theory." But theory could be learned through practice, he said. "The best way to understand an organization is to change it." In short, for Lewin "action research" required that social theory and social action be closely integrated.

Group dynamics, defined broadly, made many important contributions to our understanding of leadership, democracy, group process, and resistance to change. Further it gave rise to T-groups and eventually OD. T-groups in those days were somewhat differently designed than those of later years. As we used them at MIT, there was a formally assigned role of process observer, a role which was rotated among members of the group. Early T-groups concentrated on group functioning rather than individuals. They were designed to train better committee members and chairpersons. Only later did the possibility emerge that they could be used to make not just better conference leaders, but better managers and better people as well. Eventually, it became "an all but religious exercise" (Back, 1972; cf. Strauss, 1976).

Group dynamics had two important "laboratories." First there were summer programs offered at the Gould Academy, in Bethel Maine. Here T-groups took their classic form. The second was the Harwood Manufacturing Company, an Appalachian pajama plant, whose president would sometimes sign his letters, Alfred J. Marrow, President-Psychologist. Harwood was the site of many early field experiments (especially on group decision) including some conducted by Lewin, Gordon Allport, Jack French, and Alex Bavelas (who served for a while as personnel director). Some years later a different Harwood plant was the site of one of the most thorough studies of organizational change so far: Marrow, David Bowers, and Stan Seashores's Management by Participation (1967).

Harwood was unionized. According to Bill Gomberg, then a union official but later a Wharton professor, Marrow treated collective bargaining negotiations as a form of psychotherapy. As Gomberg told the story, Marrow once met him at the railroad station in the midst of negotiations. "The process is proceeding well," Marrow was alleged to have told him. "A great deal of hostility has been expressed and we are now going through cathexis. At this stage you will be very useful, Dr. Gomberg." To which Bill said he replied, "[Obscenity] to you, you management bastard. We can't eat that crap. Give us 30 cents an hour!" Later I asked Chick Chaikin, the union's national president, what was the secret of Harwood's success. Not Alfred's psychology, Chick replied, but his brother's skill as production whiz.

I visited Bethel only for a day and I had only one class at the RCGD . This class, with Jack French, dealt with democracy, always a hot topic for me. But rather than read about the democratic theory, the class rules were that we would discuss the idea, issue by issue, and develop principles through consensus. This was to be a democratic class. After three or four sessions we got around to defining democracy. My group-dynamic classmates insisted it involved everyone going along. My opinion was that all democracy required was majority vote and civil rights -- but if I were outvoted, I'd be go along, at least for the purpose of this class. No, my classmates insisted, voting would be unfair to those outvoted. We would have to reach an agreement. Obstinately I held to my position. After an hour of stalemate French "autocratically" changed the format of the class to require some <u>real</u> work. Feeling hostility, I dropped out. A bit later -- in the football game, I mentioned earlier, one of the Dynamos (whose name I still won't mention) clipped me badly, preventing an intercept. Given my Freudian interest, I was sure it was no mistake.

Aside from this unsuccessful RCGD class. I took Freed Bales's seminar at Harvard, with much observation of groups through one-way glass. Here I with struggled learning how to code human interactions in terms of his twelve "interaction process categories" (e.g., "shows solidarity", "asks opinions" and the like). Despite my own inability to code interactions as fast as they occurred, this seemed a powerful instrument. It has dropped out of the literature with which I'm familiar but survives as a consultant training technique called SYMLOG.

<u>Douglas McGregor and Human Relations</u>

Though group dynamics and applied anthropology provided the major conceptual references for our thinking, my major organizational identification was with Industrial Relations (already discussed) and Human Relations. The Human Relations group within MIT Economics consisted of Irving Knickerbocker, Alex Bavelas, Mason Haire, and Douglas McGregor. I sat through Alex's undergraduate class, picking up a number of wonderful stories and cases based on his experience. One of these I published under our two names (Bavelas and Strauss, 1955) since Alex was notorious for his reluctance to publish on his own (Lewin had earlier performed a similar service for one of Alex's cases, Lewin, 1948).

Perhaps my most important course was with Doug McGregor. Tall, red-headed, with a little mustache, Doug looked the archetypical psychiatrist. Talking slowly, he made magnificent use of his pipe. (One of his best sessions was on "pipe work." When things got tough, he said, the trick was to let your pipe go out. By the time you have it lit again the manager you were working with will have solved his problem by himself).

Doug's seminar gave us a chance to apply and integrate the various concepts we had learned elsewhere. But at first the seminar was quite disappointing. Doug was a nice guy and the seminar (Ec 95), titled Human Relations, was pretty central to our interests, but the stuff Doug was lecturing about most of us had read on our own (one could read the field's entire literature rather quickly). So, over coffee, some of us dissidents started planning the kind of course we would like Doug to teach. Finally three of us confronted him. "Would it possible for him to cover some of the items on our agenda?" we asked. Doug smiled, puffed on his pipe for a while, and then said, "I was worried my experiment was going to fail and that you would never take over. Great. Let's do what you want."

The course we wanted (and later Doug confided he wanted) was concerned primarily with introducing change, influencing managers to confer and delegate, and encouraging subordinates to accept responsibility. In the usual seminar we would discuss a problem, talk through the various potential forms of resistance to change, suggest alternate change strategies, and then role-play them. Typically one student would play the role of a foreman, another the role of a worker, etc. Then we would discuss how we, as personnel people, would induce the foreman to implement our preferred strategy (often through non-directive counseling). Meanwhile one or two students served as "process observers" and would report ("feed back") periodically on what they saw happening to the group.

Sometimes we would end the seminar with fifteen minutes of "process." At times, we students (and significant others) would meet for a pot luck dinner and further "process", sometimes with a process-observer to provide feedback on our discussion of process. Process, the dynamics of interpersonal relations, was our greatest interest. (Consistent with this approach, my Ph.D. orals involved roleplaying: Mason Haire played a recalcitrant foreman, whom, I, as personnel director, had to straighten out).

In short, the emphasis in Doug's course was on becoming change-agents, changing our own behavior and that of others. He trained us to be non-directive therapists on the assumption that we would either become personnel directors or at least should know how to behave as personnel directors. In this role we should stick to the "sanitary". Personality or childhood problems were none of our business: we should stick to the "here and now", the manifest rather than the latent. On the other hand, we should be aware of the possibility that others would treat us as father figures and would transfer to us the feelings they once had for their real fathers. Knowing this possibility, we should know how to avoid it. Further we should confront our own impulses to play father, a warning one of my colleagues failed to take when he became carried away in an attempt to help an emotionally disturbed student.

There was some correspondence between Doug's emphasis on training us, in a doctoral program, to be good personnel people, and the early stress in the Harvard's doctoral program on how to be a good manager (Roethlisberger, 1977). To the extent that thought was given to the matter, it was assumed that to teach managers we had to be good managers ourselves (and that research was secondary). Significantly, a majority of my classmates spent at least part of their careers with companies (or, in one case, Ralph Bergmann, a union) and many, such as Shultz and Bergmann, moved back and forth between academic and non-academic pursuits.

Though today McGregor is best known for Theory X and Theory Y, these concepts hadn't been developed yet. Instead he talked about augmentative and reductive relationships. The augmentative relationship increased satisfaction and so was akin to Theory Y. Reduction led to frustration and this to aggression and other dysfunctional behavior (my McGregor notes are full of references to Dollard and Doob, 1947).

Personnel.

We also had a course called "Personnel", taught by Paul Pigors. For reading we were assigned Barnard as well as variety of traditional management texts, such as Alvin Brown. I found them all quite boring. Barnard was hard going and his subtleties escaped me. After Hawthorne, almost everything Barnard said seemed trite, for example, the importance of subordinates' consent. Had he provided case examples his propositions might have made greater impact.

Of greater interest we were assigned two field work exercises. One required us to be a change agent and my approach was not particularly non-directive. I was assigned a section of Somerville with instructions to recruit a core of volunteers to raise money for the Community Chest. After some initial setbacks I approached the parish priest, who also proved uncooperative. I resigned myself to collecting the money myself, on a door-to-door basis. Fortunately, at a MIT lunch I met the archbishop's secretary. I told him my problem. The next day I got a call from an irate priest. "You squealed on me. You got me in trouble with the archbishop. How much do you want?" "Two thousand dollars will do us fine" I replied. ($500 had been the most ever raised in that section.) Two weeks later he delivered. Note: in a pinch I abandoned Theory Y for bargaining clout.

Even today, when the emphasis is on theory and research, doctoral candidates might gain from practical experiences such as ours.

Wage Administration.

Among my more interesting courses was a Saturday morning seminar with Bob Livernash, then with the New Hampshire state personnel department and later the Harvard Business School. Bob was in the midst of a job evaluation program. He regaled us with stories about the politics and intergroup conflicts this seeming mechanical activity inevitably involved. From Bob I learned that personnel work is 20 percent technique and 80 percent implementation, a point which modern human resource texts seem to have forgotten. Bob was also a useful antidote to McGregor: as Bob's stories illustrated, personnel work requires more than being a nice guy and listening non- directively; it requires heavy doses of backroom politics.

Further, Bob's approach (and that of Clark Kerr in work written contemporaneously) foreshadowed recent work of Jeff Pfeffer and Jim Barron (as Jeff generously acknowledges). Parenthetically, for people of my generation, very little of recent work on personnel issues by sociologists or efficiency wage theorists is at all novel. We knew it all along from the studies of the 1940s. The difference is that today's conclusions are based on quantitative analysis of massive data sets rather than on case studies, and propositions are stated more rigorously and tied to fundamental theories rather than treated as problems to be solved by practitioners.

## THE FIELD IN THE EARLY 1950s

As the names of the MIT and Harvard departments indicate, the immediate post-war period was one of great disciplinary fluidity. Theory was de-emphasized. Instead the emphasis was on integrating and applying the social sciences. This period saw the establishment of the Society for the Psychological Study of Social Issues, the Society for Applied Anthropology, and the Industrial Relations Research Association, all of which I joined. All three were much concerned with the application of the social sciences and the first two published journals (Applied Anthropology, later Human Organization, and the Journal of Social Issues). At the same time, Human Relations, whose motto was "toward the integration of the social sciences", was started at Tavistock in Britain.

Human Relations normative and optimistic. Lewin, Roethlisberger, and McGregor were missionaries, and so were we students. In our rose-colored view, the social sciences could and should be used to improve society. Soon we would have the answer to racial prejudice (Lewin's great concern), labor-management problems, economic fluctuations, and possibly even war (though I was personally far from optimistic as to the possibility of avoiding atomic destruction).

Though we were firmly convinced that most problems could be resolved through research, our research tools were fairly limited. The most important were ethnographic case studies, particularly those based on participant or non-participant direct observation. We also made use of interviews (preferably non-directive), field experiments (such as the Relay Assembly room), and laboratory experiments (as used in group dynamics). Individual attitudes received little attention, however. Homans called them merely "sentiments". We had no training in statistics.

Consistent with our methodology our primary research focus was on groups, especially on such issues as norms, group standards, status and sociometric patterns. For us the chief lesson to be learned from Hawthorne and similar research was the repressive effect of formal organization and the liberating aspects of informal organization. The trick to successful management, as we saw it, was to enlist the services of informal leaders and to involve the group in solving common problems through group decision. The effective supervisor would act as an informal leader.

Externally how groups would develop would be affected by technology. Internally their operations could be improved through an understanding of group dynamics. In this way both anthropology and group dynamics could be integrated into human relations.

Our emphasis on the group level helped blind us to other issues. We were familiar with Abe Maslow's needs hierarchy and Henry Murray's concept of need achievement. Further, McGregor taught us what later became Theories X and Y. Nevertheless, human relations of the 1950s, compared with the OB of the 1960s, paid little attention to individual needs or motivation. Similarly, though McGregor and Myers were much interested in the staff role, there was otherwise little concern for the larger formal organization. Certainly ours was a closed theory approach.

Despite the seeming breadth of my training, Homans The Human Group (1950) was as close as I got to theory. Perhaps the reason I liked it was that theory was closely linked to cases; indeed it seemed to flow out of the cases, which was the

way I (and presumably many of my colleagues) thought. As Whyte put it, "I did not develop [my] ideas by any logical process. They dawned on me out of what I was seeing, hearing, doing, -- and feeling. They grew out of an effort to organize a confusing welter of experience" (1955, p. 357). In other words our thinking was inductive. We went from case to theory, (and not too much theory please) rather than the other way around.

## My Life in the 1950s

Now to turn back to me, personally. Obviously I did more during this period than just attend class and read books. I enjoyed the many opportunities available in the Boston area and New England. Summers I took jobs for work experience (MIT was big on this) and also to earn a little money. One summer I worked in a unionized milk bottling plant and as a nonunion construction laborer. A year later I was a unit supervisor in camp for mostly delinquent kids, supervising counselors who themselves were at least partially delinquent. As a supervisor I found that non-directive listening went a long way, but after that I was in trouble. Still another summer I was a Field Examiner for the National Labor Relations Board, learning the grimy side of labor relations.

At the NLRB I learned a cruel lesson. I was assigned an elderly secretary. Being a student of McGregor's I assumed she was bored with her job (certainly she griped enough) and that the sure answer was participation and job enrichment. So I asked her advice (based on her vast experience, as I told her) with regards to every letter and was soon delegating responsibilities to her to draft letters herself. Not long after, she blew up "Look, you're a [Grade] 7, I'm a 2. You're paid to draft these letters. I'm only paid to type up what you say. If you don't quit it, I'm going to the union." (Later, when I told Ren Likert this story, he told me "George, you pushed her too fast.")

### Teaching.

In my last three school years I worked as a Teaching Assistant. I taught the works, Introductory Economics (Ec 11), Industrial Relations (Ec 60), and Social Psychology (Ec 70). As TAs in Ec 60 and 70 we set our own syllabi (except for a common basic text), did all the teaching, and graded our own exams. My Industrial Relations was mostly human relations, with much role playing. The one semester I taught Psychology we all (Hal Leavitt was one of us) decided that we would teach via a T-group, perhaps the first time a T-group was taught for credit. This worked for a while, until one by one the student sections revolted. "We are tired of this democratic nonsense," they said. "The instructor should be dictator." My section was the last to revolt, perhaps because I was such a great T-group leader or perhaps because they knew I would make a lousy lecturer.

### Dissertation.

By 1949, having passed my comprehensive exams (which included roleplaying), I began thinking about my dissertation. My first thought was to study several Scanlon Plan companies. Joe Scanlon showed little interest, understandably perhaps, given the nasty questions I had asked in his seminar. So I picked was human relations in unions, especially local unions. I was influenced especially by Homans's just published Human Group (1950), as well as my experience with the Labor Board. I began to think of the union as a network of informal groups, much

as Whyte saw Cornerville. I was interested in how leaders formed within each group, how various groups and their leaders related to each other, and how all this was impacted by the employing organization's external system, especially its work technology.

Then I had a bright idea. To examine the development of an informal system it might be best to study a new union. Besides entree might be easier. Fortunately I had a stroke of luck. At a MIT affair I met the national presidents of two unions, each of which was engaged in an organizing drive, one at New England Telephone, the other at Boston Edison., With their endorsement I gained entree easily.

The telephone drive came first. I took on the role of a union organizer, making house calls on perspective members. Most of my prospects were young operators and often mommy or daddy would chaperone my interview. This didn't bother me much, since daddy typically was a unionist himself and would reinforce my message. Still I made little progress. As one woman told me embarrassedly, "I don't want to join a union. I want to get married." And another explained, "I can't join. My brother is a priest." But even among the older "girls" (as they adamantly insisted on calling themselves) loyalty to Ma Bell seemed high. Gradually it became clear that the key female leaders were going to sit this one out. The union lost 2-1.

Boston Edison was different. Here the Utility Workers Union was fighting an older so-called company (independent) union. The new union's leadership had a situation well in hand and had no need for an incompetent outside organizer. But they were delighted to explain what that were doing to a sympathetic academician who shared their views and was, besides, a good listener. McGregor's training paid off.

Actually it was an ideal research site. Technically, because of Labor Board rules, the company was divided into three "bargaining units": (1) "physical", blue collar, almost entirely male; (2) clerical, mostly female, and (3) professional, almost entirely male. Once the union won the election in all three units, as it did nicely, three separate locals were established, each with a distinctly different political life. I had a range of occupations from professional engineers, through bill collectors and overhead linemen, to coal handlers. And my groups differed nicely in ethnicity, gender, age, and, above all, status.

Systematically, on the basis of interviews and various forms of written data, I developed a profile of each of the some 30 major divisions into which the company's workforce was divided: its industrial relations problems, the occupation, age, and ethnicity of its formal and informal leaders, the positions they took with regards to the competing unions, and the extent and nature of its members' union participation. I was overwhelmed with data. Today I might have entered into my computer and tested my findings quantitatively. But there were no computers then, at least not for graduate students, and I didn't have the quantitative skills. So I roughly sampled my data for impressions and trends.

Boston Edison formed the heart of my dissertation, but I checked my observations with three other situations, the Ford assembly plant in Somerville, the then newly opened (now closed) GM plant in Framingham, and the three locals of the Boston Ladies' Garment Workers Union, (1) cutters, Jewish, (2) pressers, Italian and (3) dressmakers, mostly women. In all these my hypotheses worked out nicely

What did I find? In brief. consistent with The Restaurant Industry status and communications opportunities were of major importance in determining "Leadership and Participation in the Local Union," my dissertation's eventual title. Within groups, high status people were more likely to participate in union activities or be elected to union office. High-status groups had similar advantages over low-status groups. Communications had much the same impact. Individuals (such as tool-crib attendants) who could easily communicate with their peers were more likely to participate and be elected. Groups, such as electrical power operators, who could easily communicate with other workers in the course of their work, also tended to hold key positions in the union. Status and communications didn't explain everything of course. There were important differences among officers and stewards in the way they conceived and played their roles as well as the functions union activity played in their lives.

The Local Union.

By 1951 my dissertation was pretty well complete. Meanwhile my MIT classmate, Leonard Sayles, had left for the Cornell Labor School, to work on a project on "human relations in unions", funded by the W.T. Grant Foundation and directed by Bill Whyte. I joined the project in 1951 with a three-year appointment.

Our first task was to merge our dissertations into a single book. Though Len's dissertation was slightly more psychological than mine the two dissertations blended together beautifully. Today I can't tell which part of the resulting book, The Local Union (1952) was Len's and which mine. Along the way we milked our respective dissertations for at least a dozen articles, some written separately, most jointly, publishing in such journals as the Harvard Business Review (then considered a prestigious journal), Industrial and Labor Relations Review, and The American Journal of Sociology. In 1952 the Society for the Psychological Study of Social Issues awarded us a prize (funded by Alfred J. Marrow) for its contribution to "the scientific understanding of labor-management relations." Obviously we had a hot topic.

I spent much of the first year of my appointment at Ithaca. While there I had frequent contact with Bill Whyte, who had already influenced my work so heavily. We also occasionally saw such people as Chris Argyris, who was working on his own dissertation. But Len and I had our own project and we were pretty much alone.

Fairport

By June 1952, with two years of the project to go, I took off for another study, this time in an American Can plant in Fairport, New York. Fairport, a town of only 5,000, was pretty lonely, but fortunately the kindly union president and his wife somehow adopted me, especially for much appreciated Sunday dinners. Yet, as a study of union behavior my can plant had little to offer. The union was weak, union-management relations were placid, and no new interesting hypotheses emerged. Instead my interests turned in two directions:

How people adjust to their jobs. As my Labor Board secretary so dramatically taught me, not everyone wants job challenge. Yet many jobs are very boring. How do people come to terms with their work? Being sure that more than Mayo's obsessive reveries were involved I began asking workers, "What were you thinking

about just now" (a version of a "penny for your thoughts"). "About God," one told me. "I like to think of my cans as little babies," a childless woman said. "When I put tops on them I am diapering them." (Several thousand diapers a day!).

In one department high-status women had a choice of two sorts of jobs: the first was completely routine and repetitive; the second involved considerable variety. Women who elected routine jobs were happily married for the most part; their reveries turned on family matters ("What will I serve on Sunday?"); and they viewed work as respite from screaming children. Those who picked non-routine work were generally single or had unhappy family lives. The reveries they told me dealt with changing jobs or other forms of fantasy. Obviously, personal adjustment and orientation to work were related. Further, most people made peace with their job, but at various costs to their psyche. This was a theme I was to enlarge later.

Working supervisor. My second study involved set-up men, who traditionally had served as working supervisors, coordinating and providing technical direction for work teams. College-trained foremen were usurping their functions on the dayshift, resulting in much turmoil. Meanwhile, on the largely foreman-less night shift production and quality were higher and stress-induced illnesses and accidents were lower. Later, using other material, I documented how the working supervisor's role was being eroded through much of American industry, caused in part by the ever-sharpening distinction between labor and management. Only recently, have observers noted that this distinction is much weaker in Germany and Japan, contributing to greater manufacturing flexibility in these countries.

Rochester

After a eight months in Fairport I moved to Rochester. There I began two additional studies. The first was of white-collar unions. Why were workers so reluctant to join them? How could this reluctance be overcome? How did the behavior of these unions, once organized, differ from that of blue-collar unions? Besides drawing on my Boston telephone experience, I observed a series of organizing drives (all unsuccessful) and several functioning unions.

In addition I followed closely, through a long cold winter, a bitter insurance agents' strike against Prudential. These were "five and dime" industrial agents who sold small policies, collecting their premiums monthly. Though ostensibly striking over compensation, their chief concern was what they perceived as their company's unrelenting pressure to "produce" and especially to compete with each other, for instance (according to stories told me) by holding weekly sales meetings in which the low producer of the previous week had to sit with a dunce cap in the corner. Well after the strike was over, this union had by far the best monthly meeting attendance of any union I have ever observed -- over 70% month after month. Why? Because selling insurance is a lonely, frustrating job. 95% of their prospects turned them down. Company meetings were even more ego damaging. The union meeting offered solidarity and to chance to exchange stupid-customer stories.

My second study was of construction unions. I spent three fascinating years with Business Agents (BAs), typically meeting one at the union hall at 6:30 AM, when he "dispatched" men to the various jobs, then driving with him through the country as he inspected jobs and resolved grievances. Often in the evening there would be a meeting, followed by drinks with the members. (I gained some six pounds through excess beer drinking).

Though I was unevenly successful in gaining these BAs' confidence, many were delighted to have someone accompany them on their rounds, someone to whom they could spill their frustrations. Occasionally I would get a late night call from a BA, just to talk over how he'd handle sticky problems the next day.

Some fifteen unions were involved, with striking differences among the occupational characteristics of their members and the way the BAs played their roles. What made these unions especially interesting was that each was an occupational community (with much intermarrying) and most were marked by internal feuds within the "family". Among the more worthwhile questions: status hierarchies and how they were expressed; the constantly changing relationships among the BAs of various unions as they cooperated for some purposes and then fought each other tooth and nail over jurisdiction; the process by which BAs decided when to "shade" the standard wage rates to save particular jobs; and the unstable power balance between BAs and their members. Since BAs controlled jobs they could punish political enemies; but if they punished too many they might lose the next election, and election fights were frequent. All this affected the bargaining process. This was rich material which is almost never examined today, perhaps because economics and sociology are now such separate disciplines and perhaps because, unless you are on a grant, it is difficult to find the time to spend endless days driving around with BAs or walking insurance agent picket lines.

Buffalo

In 1954, when my grant had expired, I went to the University of Buffalo Business School. At last I belonged to a community rather than merely being an observer. Buffalo, at the time, was not a research-oriented university, which meant that despite a heavy teaching load there was plenty of time for lunch and evening social activities. I quickly joined a close-knit group of friends whose interests spanned the humanities and the social sciences. One lunch we might discuss Chaucer or Joyce, the next we sampled each other's pates over wine. Since my friends did it, I even began attending string quartets and the symphony. Everybody knew everybody, and it was nice.

Not that I gave up on politics. My social set included most of Buffalo's liberals. I was active in both the local American Civil Liberties Union (this was the McCarthy era) and the campus chapter of the AAUP, serving a term as chair of each.

Contributing to my rosy memories of this period, in 1957 I got married. After a four month European honeymoon Helene and I bought a wonderful Victorian house, had two children, and were prepared to settle down for a lifetime among our friends in Buffalo, interspersed, as we planned, with many trips throughout the world.

I enjoyed my teaching. I taught 12 hours a week in the regular day school and, to earn extra money, four extra hours Friday night for a total of seven preparation. (I flaunt these figures at my colleagues when they complain of having to teach two courses in the same semester.) Besides such topics as labor relations, labor history, and labor law I handled a three semester sequence of human relations, wage administration, and personnel administration. Showing how so slim the materials we had at the time, in Human Relations I used Whyte's Restaurant Industry (rejecting Gardner's Human Relations in Industry, 1949, the field's first

text, as too limited). Wage administration I renamed Incentives and Productivity, assigning Whyte's Money and Motivation and stressing group standards, introducing change and incentive systems. For Personnel I used Pigors and Myers. All my texts were supplemented by journal articles.

None of this stopped my research, at least prior to my marriage. I went to Rochester one or two days a week to follow up on my building trades research, carrying it through three negotiating cycles. Meanwhile I was actively publishing the work I had done previously.

Personnel

Sometime in 1955 Len Sayles suggested we write a text together. It took us five years. Without giving the matter much thought we called the work "Personnel: The Human Problems of Management". We avoided "Human Relations" because the term was under attack (see below). On the other hand, in the schools we knew, "Human Relations" was still called "Personnel". Regardless of title, our text was designed to cover the topics which are called today organizational behavior and human resources management. Since it was published in 1960 ours was not the first OB book. Hoslett (1948), Gardner (1949), and Leavitt (1958) came before us. But at 750 pages ours was certainly the most comprehensive.

What did it cover? Everything we knew in 1960: Maslow on needs, McGregor on motivation, Hawthorne and Whyte on groups, status, and informal leadership, Lewin on group dynamics, the British studies on job design, Michigan on supervision plus heavy doses of industrial relations and labor economics. Our general framework was McGregor's class of the late 1940s, but we added work done during the 1950s, especially by sociologists such as Blau, Gouldner, and Bendix.

There were two main differences between our text and Leavitt's beautifully organized work. First, he focussed on psychological principles; we were concerned with sociology and industrial relations as well. Secondly, we stressed the problems connected with application and implementation (something which even today most texts ignore). We viewed implementation as a political process requiring endless accommodation. Indeed an accurate title for our text might have been "Human Relations and Its Limits." Forinstance, in addition to McGregor's three forms of motivation, Be Strong and Be Good (later called Theory X) and internalized motivation (later called Theory Y), we added a fourth, implicit bargaining (a concept borrowed from Gouldner). Though internalized motivation might ideally be best, in many instances bargaining was the best one might achieve.

As it turned out, we were lucky in our choice of topics and publication date. Eventually we split the book into three different versions: one purely personnel, the second purely OB and the third combining both topics. After 30 years our various editions have sold over a half-million copies in four different languages. The last edition appeared in 1980.

In rereading the book today it seems to have stood up very well; I am surprised how much we knew in 1960 (though this may be merely an indication of how little I have learned since then). We certainly knew a lot more in the 1960s than in the 1950s.

## THE FIELD IN THE 1960s

By the mid-1960s the field was beginning to jell. First, the "field" was no longer called "Human Relations". Secondly, it had changed greatly since 1950 and was changing still. Finally, it was rapidly becoming accepted as a key part of most business school curricula.

### Human Relations Becomes Organizational Behavior.

In the mid-1950s human relations came under attack from both the left and the right. From the left it was attacked by labor economists and sociologists (especially radical sociologists) who claimed that it ignored economic motivation, unions, and the class struggle; that it put the group over the individual, substituting Organizational Man for anomie; that it treated mangers as rational "elite" and workers as irrational "aborigines" (Kerr and Fisher, 1957); and that it assumed conflict was simply the result of misunderstanding, to be eliminated by discussion. (As late as 1989, in a discussion of the role of human resources management in the Industrial Relations Research Association, John Dunlop insisted on calling HRM, "human relations", which, as he saw it, was just a method of union busting.)

These attacks still sting. As someone studying human relations in unions I felt it grossly unfair to charge human relations with being anti-union. Quite the contrary. My 1948 McGregor class notes make it clear that he viewed human relations without unions as a sham. Unions were needed for participation. True, McGregor hoped for some sort of Theory Y union-management relations in which each side recognized the other's needs and that differences would be worked out through good listening. This was idealistic. It wasn't anti-union. Mayo, it is true, might be viewed as paternalistic and even a bit fascistic. Roethlisberger hoped that management would work so well that unions weren't necessary. But for him union were merely the bearers of bad news. One shouldn't shoot the messenger.

The other attack came from the right. It was best illustrated by Malcolm McNear's article, "What Price Human Relations?" (1957). Briefly the charge was that human relations was goody-goody, namby-pamby and no management could survive with just good intentions. This was the kind of criticism which management (and many economists) could accept.

The term "human relations" was out in any case. It would be interesting to study systematically what the people-oriented course was called in most business schools in the 1950s. In some it was Personnel, in others Management, but this typically included very little Human Relations. By the mid-1960s the title Organizational Behavior had taken hold, though some schools retained Management.

### A Changing Field.

By 1960 OB, as I will call it, was much changed from 1950 and was still changing rapidly. Important books had been published by Argyris, Likert, McGregor, and Fred Herzberg. The field had become considerably more psychological. Compared with the earlier period there was more interest in individual motivation and mental health than in group behavior; there was more concern for ego and self-actualization than for social needs. Some talked of a human resources as opposed to a human relations model (Miles, 1965).

Industrial sociology as a field was dying out and organizational sociology was still to be developed. A number of earlier sociologists -- Homans, Whyte, Blau, and Gouldner -- were writing on exchange theory, a theory which didn't get very far in sociology, but was revived and extended by psychologists as equity theory and procedural justice.

The focus of 1960s OB was more on management than on blue-collar workers. Supervisory training was transformed into management training and then to organization development (OD). Further, the field became more quantitative and methodologically sophisticated. Survey questionnaires were widely used, especially to study supervision. Research was becoming deductive rather than inductive.

By the 1960s OB had developed a widely accepted paradigm, perhaps best articulated by Argyris and Likert. Put simply it was as follows. Workplace behavior is motivated by Maslow's needs-hierarchy. Individuals seek to satisfy progressively higher levels of need -- and to do so on the job, not necessarily through family or community. Specifically they seek to exercise autonomy and to develop their unique personalities with freedom. Organizations, on the other hand, seek to program behavior and reduce discretion. Subordinates react to this pressure in a variety of ways, from union activity to psychosomatic illness. To keep employees in line, management must impose still more restrictions and force still more immature behavior. Thus human assets are wasted and a vicious cycle begins.

A subtle management, which provides "hygienes," such as high wages and decent supervision may well induce workers to think they are happy and not dissatisfied, but in fact they are apathetic. Really effective solutions include job enrichment, participative management and above all T-groups and OD.

This 1960s paradigm was even more normative and applied than early human relations. As I noted (Strauss, 1968b), the field's key figures were crusaders. They were interested in research, not for its own sake, but because they wanted to better the human lot. They were optimistic, even utopian. Being crusaders their work was directed to managers, as well as scholars. This normative dedication also influenced the questions which these scholars asked and the variables which they considered. Likert was explicit that the causal variables with which he was concerned include only these "which can be altered or changed by the organization and its management." (1967, pp. 29).

Finally, 1960s OB had almost naive faith that there was a "one best way" for most problems. I recall sitting next to Ren Likert at dinner. "Ren," I asked. "Would System 4 work equally well in all countries?" Pointing to his hotel-sticker plastered attache case, he replied. "I've been to dozens of countries throughout the world. System 4 works everywhere I have been. Sometime I may find some Hottentot tribe where it isn't appropriate. But I haven't found it yet."

The 1960s paradigm presented a glowing image of a world in which happy employees worked productivity and enthusiastically for management's goals. As I describe below, I became increasingly uncomfortable in teaching this paradigm in its unadulterated form.

OB Wins Acceptance in the Business School Core.

By the late 1960s OB (or Management) had become an independent field. In most business schools it was a required course at both graduate and undergraduate levels. How this occurred still needs to be documented -- and before the key participants pass away. Certainly the Gordon-Howell report played a major role. So did the American Association of Collegiate Schools of Business (though chiefly in ratifying a process almost complete). It would be useful, if someone could describe the change process in a broad sample of schools. To start the ball rolling, I describe what happened at Berkeley below.

## My Move to Berkeley

In 1960 I was invited to spend a year as a visitor at Berkeley. Having been turned down for a Fulbright in Italy, California seemed a tolerable second best (I knew it well from my Army hitchhiking days). Helene and I loved the Bay Area. With two squabbling children in the back of our VW, we explored the area thoroughly. I liked my immediate colleagues but found the University as such quite cold, compared to the intimacy of Buffalo. Then, to my surprise, given Berkeley's historical antipathy to human relations, I received an offer to stay. That posed quite a dilemma. Helene and I cherished our life, friends, and wonderful house in Buffalo. I had already turned down offers to Columbia, Chicago, and Illinois. But Berkeley, at the time, was the best IR center in the world and Berkeley's weather had much to recommend over Buffalo's, especially for growing kids.

So we vacillated. I went for long walks. I made long lists with the advantages and disadvantages of each choice. At two points I told Buffalo I would definitely return -- and then 48 hours later called them to say I had changed my mind. In the end we returned to Buffalo for a year, sold our wonderful house, went back to Berkeley, lived for two years in a rented house while we agonized, took two years of sabbatical (1965-66; 1970-71) in Buffalo, and finally gave up our Buffalo dream. Crass ambition (Berkeley was more prestigious than Buffalo) triumphed. For Helene, with family in Buffalo, the sacrifice was greater.

### Teaching

I was hired to be the first tenure-track faculty member to teach "Personnel". This was under the auspices of the Industrial Relations Group (Berkeley's Business School has no formal departmental subdivisions). I did this my first year, interpreting Personnel as being mostly Human Relations. In 1962 I was reassigned half-time to the Administration and Policy Group, a strange mixture consisting chiefly of decision theorists, game theorists, and management scientists The intent was that this group would provide the behavioral emphasis envisioned by the Gordon-Howell report. For a year I actually chaired the group. There I taught a required course of Organization and Administration (previously this course had been known as Business Policy). Though no one told me, I was probably expected to teach Simon. Instead I used my new textbook.

Over the next three years, after much political maneuvering, the Administration and Policy Group was renamed Management Science, the Organization and Administration requirement dropped, the Industrial Relations Group renamed Organizational Behavior and Industrial Relations, and the required undergraduate and MBA-level industrial relations courses enlarged to encompass

OB. After several years of trying to teach both OB and IR in the same introductory course, the IR part was dropped. Meanwhile the personnel course was transformed into what we might call HRM today. The net result: through some Pac-Man type activity, IR obtained jurisdiction over OB, but became more OB than IR.

Finishing up research

Before leaving Buffalo the first time, I had started four projects, all of which I continued both at Berkeley and during my year's return to Buffalo. Each of these were based on extensive field work.

The first dealt with the changing patterns of industrial relations in Buffalo's twenty largest factories . This was a period when union strength peaked and management gradually moved from the defensive to the offensive (Strauss, 1962a). The second, "adolescence in organizational growth", documented the stresses which occurred as growing start-up companies shifted from informal to formal, bureaucratic organization (Strauss, 1974). A third study involved purchasing agents (PAs) and their "lateral relations" with other departments, especially engineering and production scheduling (Strauss, 1962b, 1964a). I was concerned with two related questions, first, how to design organizational systems so as to lessen workflow problems, and secondly, what were the bargaining and political skills required to be successful in lateral relations.

The PA research contributed to a fourth study, this time examining professionalism and the role of professional associations viewed as interest groups or quasi- (or even real) unions (Strauss, 1963a). My focus was on engineering unions and associations, but I also looked at such groups as foremen's unions and the National Association of Purchasing Agents. My aim was to develop a conceptual scheme linking professional, managerial, white-collar, and blue-collar roles, but stressing role conflicts suffered by in-between occupations, such as engineers, whom I viewed as part-professionals, part-workers-and part-managers.

While all these studies led to publications, I was never able to develop them as extensively as I had hoped. The first two studies were designed to be longitudinal, covering five or ten years of development. My move to Berkeley left me with only three years data. As to the last two studies, having studied engineers and purchasing agents, I planned to move on other professionals, such as accountants. Other developments got in my way.

Apprenticeship

In 1962 the Institute of Industrial Relations received a large Ford Foundation grant to study unemployment. Feeling under great pressure to participate, I postponed (temporarily, I thought) my other projects.

Given my background I was at a loss as to how to study unemployment (other actually observing/interviewing the unemployed, but this had been done already). Eventually, I latched on to apprenticeship, especially in the building trades. How did presumably skilled workers acquire their knowledge? Why was apprenticeship so weak in the U.S., compared to Germany, for example? What could be done to reform it?

So I spent several years trying to understand how apprenticeship worked. Aside from reading the voluminous but largely boring literature, I interviewed employers, BAs, apprenticeship coordinators, vocational school instructors and many apprentices themselves -- all my standard stuff. I sat through long meetings of joint union-management apprenticeship committees, had coffee with vocational school students in their breaks, travelled with apprenticeship coordinators as they made their rounds, and spent time with BAs as they checked apprentices at work. Having exhausted the Bay Area I spent part of a sabbatical (1965-66) going through similar paces in Buffalo.

The result? A number of minor league publications (I should have set my aims higher). However, a monograph I wrote on the policy aspects of the topic got turned down (see below) and I finally gave up on a three-quarters finished book. It wasn't worth the time.

The apprenticeship study was my last empirical research. The projects mentioned earlier were abandoned. Why? In the first place, ethnographic research (which was the only kind I could do) was tremendously time consuming. You couldn't do it in a free half hour. Distances were greater in the Bay Area than in Buffalo. To make a Silicon Valley trip worth while required a complete free day -- and I had few of them. Not only was I a father, but I was taking on a growing amount of administrative work. Family opportunities combined with committee work left me few large blocks of time.

Equally important, my methodology was out of date. This was illustrated by my manuscript rejection. The critical question was why construction apprenticeship was so low. According to classical economic theory, unions restrict apprenticeship in order to reduce labor supply and so raise wages. I argued that unions raised wages, not through their ability to restrict supply but through their ability to cut it off altogether by striking. More to the point, my case studies suggested that the key impediment to apprenticeship was employer unwillingness to train people who might find work elsewhere as soon as they finished their training. To the extent unions actually restricted apprenticeship it was to reduce the number of apprentices who couldn't find jobs.

My evidence consisted of lengthy interviews in two communities, extensive union records as to employment and apprenticeship numbers, and attendance at joint apprenticeship committee meetings in which the admission decisions were made. My critics said that their regressions, based on macro-nationwide data proved it couldn't work the way I said it did. My findings were based on only two cities and a small number of unions. My findings were "conjectural"; theirs were based on numbers.

## OB IN THE 1970s

In the mid-1970s the 1960s paradigm began breaking down. By 1980 the field had greatly changed. The 1960s "one best way" was particularly vulnerable. McClelland postulated differences in need structures that might affect what people wanted from their jobs. Blauner, Turner and Lawrence, and Hulin and Blood found deviant work communities in which job satisfaction and job challenge were not positively correlated. Fiedler and the Ohio State studies suggested that there was no one best way to supervise. In the macro area it became quickly clear that

there was no one best organization structure; this depended on the nature of the task and the environment.

As computer costs dropped, research designs became more elaborate and more variables were considered. Some concepts, such as Maslow's hierarchy proved impossible to test rigorously. With regard to others, causal relationships seemed to become increasingly complex. The relationship between job characteristics and satisfaction, for example, was moderated by a wide variety of other factors. Many conscientious OB scholars lost the confidence to give the simple solutions many managements wanted. I stopped doing consulting myself.

By the mid-1970s OB was split. On the one hand were the "scientists", on the other the "humanists" (Back, 1972). A new group of younger scholars was entering the field, many trained as psychologists. They wanted to make the field more rigorous. Their success was facilitated by competition from other business school groups which encouraged OB people to appear harder, both more hard-hearted and more quantitative and scientific. By contrast with Likert, who was interested only in variables he could improve, the new scholars studied an ever wider range of variables. They were concerned with what <u>is</u> rather than what's <u>best</u>. Indeed, productivity and satisfaction were less frequently the dependent variables.

In sharp contrast to the scientists were the humanists who carried the normative optimism of the early 1960s to the extreme. For them, there was a very simple one best way: it involved sensitivity training, encounter groups, OD and the like. Consistent with the counterculture values of the period it was "as if their purposes were to create a 'Love Generation" of managers, a hippie organization in which all relations are 'trusting,' 'authentic,' and 'open'; each employee 'can do his own thing' in his own unique way, hostility can be expressed openly, and everybody works for organizational objectives -- all at the same time" (Strauss, 1968b, p. 265). Hard research was not for them.

The differences were dramatically illustrated in a meeting which David Bradford, Hal Leavitt, Ray Miles and I organized in Berkeley around 1973 of a group that later became the Organizational Behavior Teaching Society. At one point we divided into subgroups to discuss what OB should teach. One group, with Bob Tannenbaum as its guru, sat on the floor. The other, which included Larry Cummings as spokesperson, sat around a table. The reports of the groups were predictable, but still should been preserved for history. (Where was I? I think as an organizer I shuttled between the groups. I liked the Tannenbaum group's colorfulness. I was repelled by its rhetoric).

By the late 1970s the younger idealists came up for tenure. In school after school we saw the slaughter of innocents. The humanist movement survives in only a few schools today. It is no longer a part of OB's heartland.

## My Activities: 1970-90

<u>Administration and Professional Work</u>.

By 1970 my glory days as a field researcher were over. Much of my time was spent administrating. I was editor of <u>Industrial Relations</u>, Associate Director and for four years Director of the Institute of Industrial Relations, Associate Dean of the Business School, and for five years member and chairman of the City of Berkeley Personnel Board. In this latter role I was the butt of more than my share

of Berkeley's ideological politics as we moved into an era of collective bargaining and equal employment.

The most interesting of my committee assignments was the campus Budget Committee, a vital part of Berkeley's faculty self-governance system which made near-final decisions as to faculty appointments, promotions, and pay hikes, subject only to top administration's rare veto. Tricky judgments here: how does one evaluate scholarship in departments as diverse as drama and chemistry? It required an omniscience I lacked, but the process was fascinating.

Over the years I have been reasonably active in my primary professional association, the Industrial Relations Research Association (IRRA). Recently I served on a special "Review Committee" charged to rethink its function and role. I argued (with only moderate success) that the IRRA was too narrowly focused on labor-management relations and that we should place greater emphasis on HRM and economics. Further, we should copy much of the Academy of Management's divisionalised, broadly participative format. Having been elected IRRA president for 1993, I may be able to implement some of these ideas. Wish me luck.

Travel.

Once the kids were old enough to be left alone Helene and I started travelling -- over 40 countries so far. Sometime I could claim these trips had some academic purpose, other times they were frankly tourism. Though we have systematically explored much of Western Europe, Australia and the South Seas are our favorites. In 1979 I had a Fulbright to Australia, giving some 25 lectures at 18 institutions. In 1986 we returned, this time as Visiting Professor at the University of Sydney, helping revive a troubled department (in the process learning much about the difference between US and Australian-British academic administration,)

Teaching

While at Berkeley I have taught almost every OBIR course offered, from Human Resources Management through introductory OB courses to Collective Bargaining, including our first graduate macro-OB course, taught jointly with Chick Perrow. My favorite is the Ph.D. seminar on the history of OB (from which much of this paper is taken).

My Collective Bargaining class made considerable use of bargaining games. Drawing on this experience, in 1984, I introduced a MBA elective on Negotiations and Conflict Resolution. As with similar courses elsewhere this course makes extensive use of simulations, starting with bargaining over a used car and proceeding through progressively complex negotiations to a final multi-party case involving developers, environmentalists, city officials, and a mediator.

As the School's only course which permits students to develop interpersonal skills, it is popular for the same reason that T-group courses were popular in the 1960s; but this time students are taught to be tough, rather than be nice. I have mixed feelings here. I am a terrible bargainer myself. While I spend a good deal of class time on conflict resolution and integrative, win-win negotiations, it's the distributive, win-lose elements which students take away. I don't like the values I teach. Nevertheless, I enjoy the course and it's nice to be a popular teacher (even if for the wrong reasons). My ambivalence reflects a tension throughout my career, beginning at MIT. It's a tension between idealism and cynicism, between

the empowering values of Theory Y and the reality that in the real world it helps to have clout. It's a tension that I have only partly resolved through my belief that unions empower.

Writing.

Since 1970 my writing has consisted of a combination of review articles, think pieces and criticism. Usually I throw in some suggestions for future research. For the most part these have appeared as book chapters or in invited journal symposia. Thus I avoid the review process.

My subjects have been rather broad (have word processor, will write). They have included performance appraisal (1972a), organization development (1976b - one of my favorites), developments in human resources generally (1982a, 1987b), concession bargaining (1984), union government (1977b), comparative industrial relations (1988b), and construction labor relations (1972). Industrial Relations, as an academic field, is a key interest to me and I have written a set of critical articles evaluating its development (Strauss and Feuille, 1978, 1981; Strauss 1989, 1990a) and suggesting links to psychology (1979a) and conflict resolution (1982a).

Criticism the OB paradigm. In 1963 I began a series of papers asking questions about the dominant OB paradigm (e.g., 1963, 1968). In them I argued generally that the paradigm overstressed individual desire for autonomy and meaningful work as well as the ability of organizations to provide such work. On the other hand, it underestimated individuals' ability to adjust to routine work through various forms of social activity, game-playing, and reverie. Hitting below the belt, I argued that the paradigm "bears all the earmarks of its academic origin. Professors place high value on autonomy, inner direction, and the quest for maximum self-development...for them, creative achievement is an end in itself and requires no further justification.. [They] see little incongruity in imposing [their values] on the less fortunate" (Strauss, 1963, pp. 47-48).

Chris Argyris generously agreed to comment on several of these papers. His criticisms were generally quite useful but at times quite sharp. We differed quite considerably. With regards to one paper, he said "I am impressed with how freely you are willing to cite your personal view as gospel. You may be pessimistic about using intergroup in difficult situations. However, we have experimented (in preliminary fashion) with three nations who intend to go to war and have had some effect." With regards to another, "I believe you are struggling hard to be fair. However, it reads as if you are suppressing your anger (until the end). Consequently, ambivalence, hostility, bending backwards, etc. seem to predominate." Touche.

Blue-collar Blues. In the early 1970s, with a sudden concern with "blue-collar blues" and a so-called revolt against work there was a revival of interest in job redesign, job enrichment, autonomous work groups and the like. When asked to write on these topics I had a Yogi Berra deja vu feeling. We had explored these issues thoroughly in the 1940s and they were more than adequately covered in our text and my previous writings. But for the benefit of those who hadn't read these (and for the glory of getting my name in print) I repeated them, but with new evidence. None of the standard data as to attitudes, turnover, or productivity indicated any blue-collar revolt against work, once business cycles and the composition of the workforce were taken into account. True, there had been much industrial unrest in Europe during the late 1960s, but in the U.S. the campus

turmoil never spread to factories. Once again, professors overgeneralized from their narrow worlds.

Wasn't my somewhat cynical view toward the 1960s paradigm inconsistent with my earlier idealism? Yes and no. It was the naive, sweeping claims made by some of the protagonists which turned me off. Mine was a contingency approach. Theory Y would work in some situations, Theory X (tempered with paternalism) in others, and in almost all situations worker-management relations were determined through implicit bargaining. As I saw it, Barnard's zone of acceptance was typically quite large. Workers were neither frustrated by their jobs or were they enthusiastic about them. A job was a job and they accepted it. Ideally all workers should enthusiastically accept management's objectives, but it was unrealistic to expect such enthusiasm to become universal.

Participation. I am not completely cynical. Workers' participation in management (WPM) has been a continuing interest. Mine is (I hope) a balanced, contingency view. Under the proper, perhaps fairly limited circumstances, WPM can improve labor-management relations and product quality and even job satisfaction and productivity. Among the main conditions are the presence of a strong union. Over the years I have looked at a variety of WPM forms, from Israeli kibbutzim to U.S. quality circles and have attempted to integrate the research, conducted in many countries and using many methods, as to the conditions under which WPM will work (and in what ways) and as to its impact on relevant actors, from rank and file workers to boards of directors and national unions. (What will I ever do with my seven file drawers of WPM notes?)

Unions again. Recently I returned to my first love, the internal life of unions. Having just edited a book, The State of the Unions (Strauss, Gallagher and Fiorito, 1991) to which I contributed chapters on union democracy and participation, my next project is to apply macro-OB concepts to union structure, but to do so prescriptively so as to be of some use to the union movement. I am also contemplating a new field study of union leadership. Back to where I started!

## OB IN THE 80s AND BEYOND

By the 1980s, with humanists purged and the contingency approach wrecking the simplicities of the 1960s, OB entered a distinctly new phase -- or so it seemed from Berkeley. There were totally new approaches. From a micro-OB approach the world was socially constructed. Many questions were almost unresearchable since the answers depend on the contexts in which the questions are asked. Job attitudes are the result of predisposition from childhood. Leadership is almost a figment of the imagination.

The macro-level has changed even faster. In the 1950s the main macro issues were staff-line relations, decentralization, and departmentation. In the 1960s and early 1970s Perrow, Lawrence and Lorsch, Joan Woodward, Jim Thompson brought in the impact of technology and the environment. Miles and Al Chandler tied in strategy and structure. Today the stress is on the relationship between organizations and their environment, with resource dependency, organizational ecology, and institutionalism being the rage. At Berkeley a new field of strategy is being developed, under the leadership of Glenn Carroll and Oliver Williamson. This merges bits of OB, sociology, and transactional economics.

Unfortunately little of this is teachable to introductory students looking for simple answers. Neither is it of direct help to management. Back in the 1940s the field was relevant in part because it didn't know how to be rigorous. Today rigor has largely replaced relevance -- at least immediate relevance. In the long run, however, the new research may make OB more relevant than it has ever been, just as cell biology is more relevant to the physician today than are medicine dances, even though the latter were designed for immediate application.

We have gone a long way since Hawthorne. Much of the seminal work in both micro- and macro-OB was done by my present and former Berkeley OB colleagues, Glenn Carroll, John Freeman, Jim Lincoln, Ray Miles, Charles O'Reilly, Karlene Roberts, Trond Peterson, Jeff Pfeffer, and Barry Staw. I can truly say that I was present, not just at OB's beginning but also at its rebirth.

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