A Distinctive Approach to Psychological Research

The Influence of Stanley Schachter

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Psychology Press

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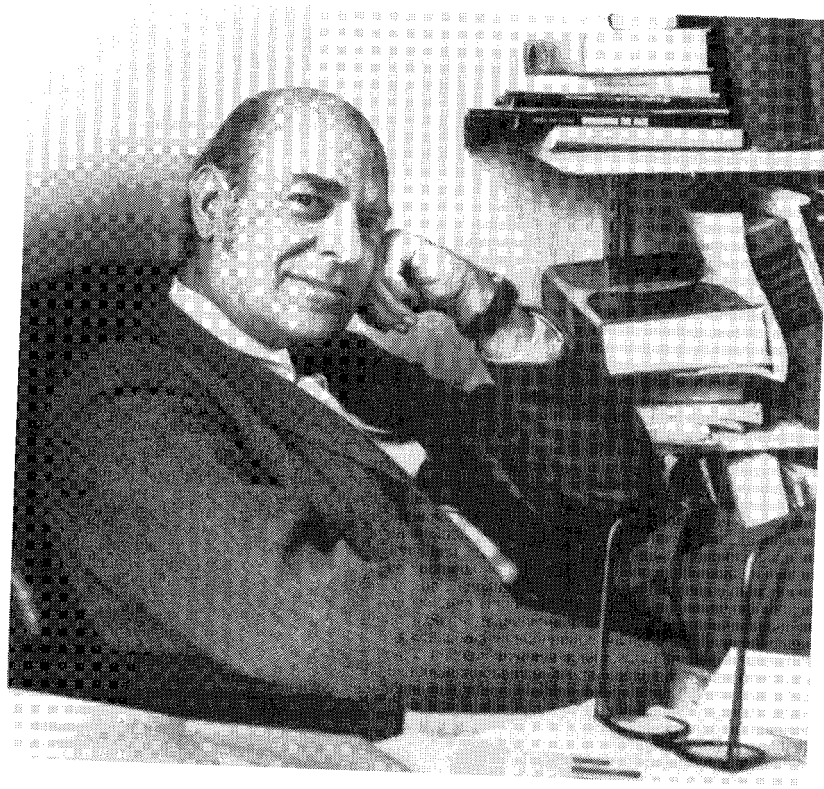

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The publisher has gone to great lengths to ensure the quality of this reprint but points out that some imperfections in the original may be apparent.

Contents

Preface

The contributions of any individual can be direct or indirect. For a scientist or scholar, direct contributions are made through research and teaching. Indirect contributions are made through the contributions of those individuals whom we teach. The influence of an individual depends on both types of contributions. To succeed at either type is notable; to succeed at both types is extraordinary.

Stanley Schachter's direct contributions are well-known and are widely cited in original investigations, scholarly reviews, and textbooks and courses in general psychology, social psychology, and health psychology. Schachter's distinctive approach to psychological research has broken new ground in the study of deviance, affiliation, emotions, obesity, cigarette smoking, and the psychology of money; has delighted and interested uncountable numbers of undergraduates; has impressed or infuriated uncountable numbers of colleagues; and has indelibly influenced the style and thinking of his graduate students.

In addition to the direct contribution of the particular content areas of his research, Schachter has made major contributions in unfolding and enlightening domains of psychology in which he is not usually associated. The growth of interest in social cognition and its basis in attribution theory are developments foreshadowed by Schachter's work in cognitive attributions as factors in emotional behavior. Indeed, much of the health-psychology attribution research has clear roots in Schachter's work. In an era when social psychology was an almost self-contained entity, divorced even from many aspects of general experimental psychology, Schachter has always considered the broader context in which social behavior was embedded. This is most apparent in his appreciation and consideration of biological processes in the under-

standing of seemingly social phenomena, for example, his consideration of the role of urinary pH in cigarette smoking at parties or under stress. His delight in embracing appropriate disciplines and weaving their contributions into social psychology has produced a general eclecticism that has influenced for the better the willingness of other psychologists to broaden the disciplinary foundations of their work.

This volume reflects the influence of Stanley Schachter beyond his research. In particular, this volume presents the influence of Schachter on his students, even when their work may, on the surface, appear to bear little resemblance to Schachter's interests. It was created to honor his 65th birthday (April 15, 1987) in the most appropriate manner that we could imagine and produce. Unlike many festschrifts, this volume does not include a direct contribution by the honoree. That is no accident or oversight. Nor does this obvious omission reflect an inability of the honoree to make outstanding direct contributions. Stanley Schachter continues to be a productive and influential scholar and we expect that situation to continue for a long time to come. There are two reasons for compiling this volume without a direct contribution by the honoree: (1) the volume is meant to convey that aspect of Schachter's influence that extends beyond his own publications; and (2) it is unlikely that we would have been able to prepare this volume if Schachter had known of its existence. This second point is discussed in some detail in the Afterword.

The contributors to this volume are Stan's Ph.D. dissertation advisor and close friend, Leon Festinger, and ten of Stan's former graduate students. Schachter has produced more than forty Ph.D.s and many other graduate students assisted Stan on one or several studies. To include all of Schachter's productive students would have required a volume at least three times the length of this one. Therefore, we decided to limit the number of authors. The contributors to this volume were invited by the editors based on our attempt to provide the reader with some sense of Schachter's influence over the last thirty years. We purposely invited authors who worked for Stan during his study of each of his major research areas. All of Stan's students worked on several questions, but in terms of the major thrust of the Schachter lab during graduate student days: Wrightsman (Ph.D., 1959) worked on affiliation; Singer (Ph.D., 1961), Wheeler (Ph.D., 1962), and Latané (Ph.D., 1963) studied emotions; Nisbett (Ph.D., 1966), Ross (Ph.D., 1970), and Rodin (Ph.D., 1971) investigated obesity; Herman (Ph.D., 1972) studied obesity and then, with Kozlowski (Ph.D., 1975), studied cigarette smoking; Grunberg (Ph.D., 1980) studied cigarette smoking and the psychology of money.

The contributors were asked to write chapters that reflected the influence of Schachter on them. The standing instruction was to indi-

cate Stan's influence without discussing aspects of his work available in his own publications. In this way, this volume would provide additional, complementary information reflecting the professional Schachter otherwise unavailable to a wide audience. Everyone readily agreed to contribute; everyone had great difficulty preparing his or her chapter. We each rediscovered that Stan's influence was deep and had become virtually indistinguishable from our own values and approach. Each author chose different ways to convey Stan's influence. We encouraged authors to try to reveal their Schachter through their own work and experiences. We purposely avoided efforts at uniformity among the chapters. Some authors used their own research as the major vehicle for this task. Some took a more anecdotal approach. We believe that together they reflect some significant commonalities, while also revealing the diversity that represents Schachter himself. The order of the chapters is roughly chronological. We decided to reverse the chapters by Singer (Ph.D., 1961) and Wrightsman (Ph.D., 1959) because Singer's chapter provides a general introduction and overview while Wrightsman's chapter picks up with research details. All of the other chapters are in chronological order.

As a final note of introduction, we believe that this festschrift is distinctive in that it covers a rather wide range of substantive topics. That breadth is characteristic of Stanley Schachter. However, the approach to each area of study is remarkably similar. We believe that this collection of chapters accurately reflects the teachings of our mentor.

ACKNOWLEDGMENTS

No book is written exclusively by the authors and editors. It would have been impossible to assemble this volume without the assistance of many people.

Lee Ross participated as one of the original members of the editorial group. When the working arrangements became cumbersome and five-person conference calls next to impossible, he graciously removed himself from the editorial stream; fortunately his contributions remain.

Diana Lord served unstintingly as editorial assistant, keeping drafts, revisions, and correspondence in order and on time.

Larry Erlbaum was more than a publisher. He helped, advised, and kept the book on a more permissive publication schedule than we had thought possible.

Sophia Schachter was an unfailing source of information in supplying facts, confirming dates, and keeping discussion of Stan Schachter's professional life from intruding on his personal one.

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Stanley Schachter: A Biographical Sketch

Stanley Schachter was born on April 15, 1922, in Flushing, New York to Nathan and Anna (Fruchter) Schachter. He attended elementary and secondary schools in the Bronx and, in 1939, attended Yale as an undergraduate. At Yale, Schachter became a protégé of Don Marquis in the Institute for Human Relations. In addition, Schachter was greatly influenced by Clark Hull and his Socratic teaching style.

In 1942, Schachter began graduate school in psychology at Yale. He worked for Walter Miles, doing research on night vision, until Schachter was drafted and served in the United States Army Air Force for the duration of World War II.

In 1946, Schachter decided to continue his graduate education at MIT because he had heard about Kurt Lewin's Research Center for Group Dynamics and the attempt to use psychology to study social problems. The Center's faculty included Dorwin Cartwright, Leon Festinger, Ronald Lippitt, Marian Radke, and (later) Jack French. The graduate students included Kurt Back, Morton Deutsch, David Emery, Gordon Hearn, Murray Horowitz, David Jenkins, Harold Kelley, Al Pepitone, John Thibaut, and Ben Willerman.

Schachter spent most of his time at MIT working on research with Leon Festinger. Most notable among Schachter's efforts in graduate school was his work with Festinger and Back studying social influence and communication in the Westgate student housing project. This study was central to Festinger's theories of informal social communication and social comparison.

In 1947, Kurt Lewin died and in 1948 the Research Center for Group Dynamics, including most of its faculty and students, moved to the

University of Michigan, Ann Arbor. Schachter did his Ph.D. thesis work at Michigan and received his degree in 1949.

In the Fall of 1949, Schachter became an assistant professor at the University of Minnesota in the Department of Psychology and the Laboratory for Research in Social Relations. The Laboratory was under the direction of Jack Darley and it was designed to integrate the activities of psychologists, sociologists, economists, political scientists, educators, and philosophers. At Minnesota, Schachter extended his dissertation research on deviance and began studies of social isolation that led to his classic work on affiliation. By this time (c. 1953), Festinger had joined Schachter at Minnesota and, with Henry Riecken, they conducted their well-known field study of cognitive dissonance that was published as a book, *When Prophecy Fails* (1956). Schachter's studies of affiliation and birth order were published in the monograph, *The Psychology of Affiliation* (1959). This book won the AAAS Social Psychological Prize in 1959 and the research described in the book led Schachter to his work on emotions. This line of research eventually led him to work on obesity and cigarette smoking.

In 1961, Schachter moved to Columbia University in the new Department of Social Psychology. The faculty at that time included Otto Klineberg and Richard Christie. The next year, Klineberg left and William McGuire and Bibb Latané joined Schachter and Christie. Schachter and Latané extended their work on emotions to the study of sociopathy, but that work ended because of logistical problems accessing prisoners as subjects.

Schachter decided to attempt to determine whether his findings on emotions (basically, that physiological states were subject to situational and cognitive labeling) would generalize to the state of hunger. This idea led to Schachter's work on obesity and his internality-externality theory.

In the late 1960s, the Social Psychology and Experimental Psychology departments at Columbia merged and Schachter moved into new quarters better suited for his work on eating and smoking. In addition, he married Sophia Duckworth (June 2, 1967). In 1969, Elijah Schachter was born to Stanley and Sophia Schachter.

In the early 1970s, largely because of Peter Herman's dissertation research, Schachter became interested in the role of nicotine regulation in cigarette smoking in different situations (e.g., at parties, under stress). Schachter's research on smoking became his most reductionistic approach to the explanation of any behavior that he had studied.

In 1976, Schachter became interested in the psychology of money and his research focus shifted away from the study of appetitive and addictive behaviors. However, in 1982 Schachter published an influen-

tial article in the *American Psychologist* on relapse among ex-smokers and former obese. Recently, his research interests have returned to this topic.

Among Stanley Schachter's honors are: Fulbright Fellowship (1952–53); Guggenheim Fellowship (1967–68); APA Distinguished Scientific Contribution Award (1969); Cattell Fellowship (1974–75); Robert Johnston Niven Professor of Social Psychology at Columbia. Schachter has authored or co-authored *Social Pressures in Informal Groups* (1950), *When Prophecy Fails* (1956), *The Psychology of Affiliation* (1959), *Emotion, Obesity, and Crime* (1971), *Obese Humans and Rats* (1974). He has written more than 60 journal articles. Stanley Schachter currently resides in New York City and East Hampton, Long Island.

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1

A Personal Memory

Leon Festinger
New School for Social Research

I do not know what is the proper kind of thing to write for a volume that is intended as a tribute to a man's distinguished professional career. I'm sure other contributions to this volume reflect that career better than I can. Hence, arbitrarily, I have decided to relate the most vivid (to me) instances of the long association between Stanley Schachter and myself, keeping within the professional context. I write this entirely from memory, without reference to any letters, notes or records of any kind. If someone cares to examine the correspondence between what I write here and verifiable facts, discrepancies will undoubtedly be uncovered. Human memory is fallible; being a psychologist I know this from many studies in the literature that have demonstrated that fact. I am also aware of the convincing data that indicate a lack of relationship between confidence in a particular memory and the likelihood that it is accurate. Nevertheless, I prefer to rely on my memory. I have lived with that memory a long time, I am used to it, and if I have rearranged or distorted anything, surely that was done for my own benefit.

In 1944 Kurt Lewin, with whom I had studied at the University of Iowa, moved to the Massachussetts Institute of Technology where he established the Research Center for Group Dynamics. He had wanted to establish an institute but how can you have an institute within an institute? So it became a Center. Soon after the conclusion of the second World War, I joined Kurt Lewin, Marian Radke, Dorwin (Doc) Cartwright and Ronald Lippitt at M.I.T. as an Assistant Professor. One aspect of my situation there was highly peculiar. I had remained a civilian throughout the war and had completed my Ph.D., thereby becoming a member of the faculty. The vast majority of the students who came there had served with the military for some number of years, interrupting their

graduate work, and were mostly my own age, some a little younger, others a little older. This was the context in which I first met Stanley Schachter; I a faculty member, he a student.

One of the things that made Stanley stand out amid that group of highly talented students was that he always walked around carrying books and papers in a murky-green cloth bag. If I had been more knowledgeable about the world I would have known, as I was later told, that it was a Yale Book Bag from the place he had gone to school before the war. In the first conversation I ever had with Stanley he told me in detail about research he had done at Yale that had something to do with hangnails—yes, I remember that distinctly, hangnails. I remember very little else about that conversation, perhaps because after hearing "hangnails" I stopped listening.

M.I.T. had built two housing projects for married students, one of which was innovatively designed, according to the people in the Architecture and City Planning Department. They approached Kurt Lewin to explore his interest in doing a study of those living in the projects to assess the effects of the innovative design. There was money available for the study and Kurt passed the buck to me. I then asked Stanley and Kurt Back if they would like to work with me on the study. Kurt Back immediately said "yes," Stanley was rather unenthusiastic and went off to think about it. He finally agreed but remained unenthusiastic, which was not surprising—who could be really enthusiastic about a study that was so vaguely formulated.

We interviewed the project residents about their satisfactions and dissatisfactions with their apartments, with school, with their social life and with every other thing we could think of. Because asking people who their friends were was one of the latest revolutionary methodological breakthroughs, we also asked about that. We then sat down, sometimes separately, sometimes together, to try to find some order in the mass of data we had collected. To fully appreciate the situation one should point out that not one of us had ever taken a course in Social Psychology, nor had we any previous experience doing such research.

That was when Stanley and I started playing cribbage together. No one ever said anything openly to us about it but I imagine there must have been some who wondered about such an activity when we were supposed to be doing research. I don't think people really understood that only with the assistance of cribbage were we able to begin to put order into the chaos of the data. Stanley had a special talent, one that I have rarely seen in such a highly developed state. Give Stanley a set of data to pore over and he will invariably emerge with interesting ideas which, furthermore, have a fair chance of being correct. You couldn't have asked for a study or a set of data more in need of Stanley's talent.

Without going into great detail, the write-up of the study, authored by the three of us, didn't look too bad; actually parts of it looked good. I don't think we could have done it without the assistance of cribbage.

Kurt Lewin died suddenly in 1947 and we discovered that without him M.I.T. was quite disinterested in this little group that was truly peripheral to the main concerns of that institution. Without his prestige and the glue he provided for the group the normal course of events would have been for it to break up, each one going his separate way. This didn't happen, however, for we were, faculty and students alike, determined to preserve the Center that Kurt had created. We elected Doc Cartwright as substitute glue provider and, after considerable shopping around, moved the whole thing, faculty and students together, to the University of Michigan.

Stanley still had to produce a dissertation for his degree and the only things that either of us could think to do at that time were laboratory experiments on whether or not the things we had observed (or imagined) in the housing project would show themselves under controlled conditions. So Stanley designed an experiment to see whether people rejected others who disagreed with them on some issue simply and solely because of the disagreement. It was a difficult experiment to do; he had to produce in each group within any condition the identical disagreement and hold constant such factors as the personality of the one who uttered the deviant opinion. It was also very time consuming, but it was neither the difficulty of the task nor the time involved that created a major problem between Stanley and myself.

By the time Stanley was finished with some pilot runs to iron out whatever methodological bugs existed, he knew how the data would come out and he knew this with an intensity backed by his firmest intuitions. He had convinced himself that the results we expected would not appear and, hence, the rational thing for him to do was to abandon this experiment and find some other topic that would make an acceptable Ph.D. thesis. Our arguments about this never seemed to cease; after each experimental group he finished I was informed that the data from that group showed the same absence of anything, and the argument continued. This problem was never solved; it was dealt with at a practical level by simply increasing the number of hours a day that we played cribbage. It has always been a puzzle for me to understand how people cope with all the problems of doing research if they do not employ interesting ways of doing nothing when they should be working. Ultimately, all the groups were run, the data analyzed, and the results proved to be far from trivial. The outcome of all this was that he wrote the dissertation, received his degree, wrote an article for publication,

and accepted a position as Assistant Professor at the University of Minnesota.

Those years at the University of Michigan coincided with the time when money to support research started to flow more freely from the Federal Government. It was not yet much more than a heavy trickle but, because the Research Center for Group Dynamics had to live primarily from the income of research grants, we all spent lots of time writing proposals. The fact that many of these proposals were approved and funded had good and bad aspects to it. The good ones are easy to identify: we could pay our own salaries, pay student assistants and were able to do our research. The bad aspects are more delicate. To increase the likelihood that one would have enough money for oneself and the people to whom there were commitments and obligations, it seemed reasonable to apply, every year, for more money than was needed or wanted. After all, some of the proposals might not be funded. When I found myself with more funds than I wanted I had to hire more people and then, of necessity, apply for even more money. At a time when money was becoming more and more available one could readily become a research administrator rather than remain a researcher.

I had become very restive in this situation when, two years after Stanley went to Minnesota, that University offered me a position of Research Professor with minimal teaching duties and an internally provided, guaranteed, modest research budget. Why they made such an attractive offer to me I do not know. I have never asked people about it. I simply assumed at the time, and I still believe, that Stanley had been unable to find an adequate cribbage partner. The prospect of not having to write more grant proposals, of being able to do my own research without responsibilities for the livelihood of others, was so attractive to me that I jumped at the chance. And so Stanley and I resumed our near daily cribbage competition.

The situation at Minnesota was nearly ideal for me and, I believe, also for Stanley. Our years there were very productive, for me perhaps the most productive four year period of my life. The person who created this situation was Jack Darley, the Dean of the Graduate School, and one of the most powerful men at the University. Jack wanted to build Social Science, at least behaviorally oriented Social Science, and build it he did. This also meant providing whatever was needed for research and protecting those who were doing it.

Stanley and I, although we saw a great deal of each other at the University, and away from it also, worked independently. On only one study did we actively collaborate, but that was a memorable study indeed. We got wind of a small group of people, in a town not too distant, gathered about a woman who was in direct communication with

gods who travelled around in flying saucers. One god, in particular, communicated a great deal and had revealed to her that on a given date the Earth would be totally inundated by flood; all would perish except for those few chosen to be saved. Before the flood a flying saucer would land to evacuate these survivors to be and this woman would in due time be told where to meet the saucer.

Henry Riecken, Stanley, and I discussed the matter and reassured each other that the flood was unlikely to occur. It seemed possibly interesting to find out how this group would react to that nonevent. Despite a relative lack of enthusiasm for the project, Jack Darley somehow came up with money to support it. Henry, who was the one among us most skilled at presenting himself favorably to strangers, made a trip to where the group was gathered to get further information but received a rather cold reception. They weren't interested in revealing things to strangers. It was a rather private group, and they felt no need to spread the news because those who were chosen would be saved willy nilly. Henry was sure that the only way we could study the group was to join them and it wasn't even clear how one could maneuver that. We tried the tactic of frequently dropping in on them and gradually we were treated as any other member of the small assemblage.

On the first visit that all three of us made to the group an event occurred that was to make a difficult study much more so. Stanley, with his own characteristic style of humor, introduced himself as Leon Festinger and there I was left to be Stanley Schachter. Once done it could not be undone but had to be maintained throughout—I've wondered if they even noticed the inevitable small lapses that occurred. At times it seemed they must have noticed and we worried but had no idea what they thought about it. Four days before the promised event was due, the three of us began a 24 hour a day continuous presence in the home of the leader of the group together with most of the other believers. In preparation we had rented a room in a nearby hotel where we left recording equipment so that we could periodically dictate what had occurred from notes.

The following days were hectic. The talents of the three of us complemented each other very well. Henry, with his easy social presence and smooth manner of ready interaction started to be seen as almost a minor leader, others confiding in him and informing him of everything. We were, hence, not often caught by surprise but were on hand, as alert as we could manage, to observe and record. Stanley revelled in exercising his intuitive talents. His characteristic position was at the edge of any group, observing relentlessly, and perceiving things that no one else would have detected. Some new person might put in an appearance and Stanley would know their occupation without

being told; some occurrence would puzzle us until Stanley explained that a liaison had been building since the previous day; a minor struggle for power in the group by one of the members was predicted ahead of time in Stanley's uncanny way. I was neither socially easy or very intuitive and insightful in my observations. My characteristic position was not at the edge of a group but in the back of the room, looking, listening and remembering. I had an excellent memory and used it to the utmost.

We got very little sleep or rest and had some prescription from a doctor to keep us going. At intervals infrequent enough not to arouse comment each of us would go to the toilet to make notes in private—that was the only place in that house where there was any privacy. Periodically, one or two of us together would announce we were taking a short walk to get some fresh air. We would then dash madly to the hotel room to dictate from our notes, perhaps grab a hamburger quickly (they were all vegetarians), and rush back so that our absence would not have been implausibly long. By the time the study was terminated we all literally collapsed from fatigue.

The last day before the flood was full of activity. Messages kept coming in from her favorite god containing information about procedures to be followed in boarding the saucer, procedures that we then had to practice because errors could be catastrophic. Then information came about the importance of having no metal on anyone because the force fields that drove the saucer wouldn't operate effectively in the presence of metal; I've always been annoyed that I had to cut out the zipper on my trousers while Henry and Stanley simply maintained they had already done so; especially annoyed because later I was refused reimbursement for the cost of the ruined pants. Anyhow, the flying saucer didn't come and the world was not flooded. My last treasured memory of that experience concerns the following day when everyone was calling newspapers and offering interviews. There was Stanley sitting in a comfortable chair, observing everything, and softly whistling Mein Yidishe Mama.

We spent the next summer together in Palo Alto organizing the material and writing a book. To explain how we got there requires a little bit of backtracking. In spite of nearly ideal professional circumstances and a social community that was very congenial, there were some problems with Minnesota. The winters were very cold and I could not overcome my absolute aversion to wearing a hat. But even more than this, the winters were long. I was accustomed to begin expecting spring when the month of March rolled around but that did not fit reality in Minnesota. This made March a dismal time. I am convinced that if one wants to steal someone away from the University of Minnesota, the best

time to make the job offer is during the first two weeks in March. Consequently, when I was offered a job at Stanford University, I was dazzled with images of warmth and sunshine. That may have been my only consideration because I seem to have leapt at the opportunity. So we wrote the book that summer at Stanford.

With me in California and Stanley in Minnesota we didn't see too much of each other. I know how I dealt with the problem of not having anyone with whom to play cribbage; I started playing GO regularly with Doug Lawrence. The two games are radically different and I would not assert that one can freely substitute one for the other but it was the best I could manage and it had to do. How Stanley solved the problem I do not know but after a few years he left Minnesota for Columbia where, as I later discovered, he found a more adequate substitute, namely, backgammon.

The most frequent professional occasions for getting together with Stanley during the years I was at Stanford were periodic meetings of a marvelous committee that I chaired for the Social Science Research Council. Social Psychology at the time was almost exclusively an American (more properly, United States) enterprise. There were very few persons anywhere else in the world that thought of themselves as Social Psychologists and even fewer who did research in the area. Under these circumstances how could we know whether or not the body of knowledge we were developing was entirely, or partially, a United States culture bound body of knowledge. The Committee on Transnational Social Psychology was established to encourage the development of active researchers in Social Psychology in other parts of the world. Stanley, who had spent about a year in Amsterdam coordinating a joint research project by persons in England, Holland, France and Norway, was indispensable to the committee.

To be credible and maximally effective the committee itself had to have an international membership and, fortunately, could not meet solely at the quarters of the Social Science Research Council in New York. Some of the meetings were, of course, held in New York but Stanley and I also were forced to go to places like Italy, Spain, Holland, France and Austria to attend meetings. It was through this committee that Stanley and I first met people such as Henri Tajfel, Serge Moscovici, and Martin Irle. In the course of these meetings we learned such things as how good the seafood was in Barcelona, how uninteresting some Italian wines can be and, outside of music, how dull Vienna is. In that city we made the mistake of holding the meeting at a new, modern hotel where everything went wrong. Stanley described the hotel management as showing a startling combination of Viennese efficiency and American charm.

We did, however, also work hard on that committee, locating interested Europeans, bringing them together at meetings and encouraging research activities. I think the committee performed an invaluable service to the field of Social Psychology and, although I am philosophically aghast at the notion, I am also gratified that there are now many Europeans who are intent on developing a European Social Psychology.

Occasionally, when I was in the New York area, I visited Stanley at his summer place in Amagansett. It was there that he introduced me to backgammon. On the surface it appears to be a rather dull and simple, almost trivial game. Below the surface there are vital tactical issues, strategic considerations and psychological subtleties. Without explicit tutelage it takes considerable experience before one begins to grasp some of these things. Stanley taught me the game and under his quite inexplicit tutelage it did take me a long time. Whenever we played Stanley won. Our reactions to this were quite different; I was perplexed and mildly frustrated while Stanley seemed to enjoy it. On one or two occasions when I expressed my feelings about the unfairness of the apparent inequality, Stanley gently reminisced about the past and how that had been the exact situation, with roles reversed, when I was teaching him to play cribbage.

After thirteen years in the clean sunshine and easy living of California, I left to return to New York where the New School for Social Research had offered me a very nice job. I was sorry to leave Stanford but I had developed an ununderstandable hunger for the noise, dirt and inconvenience of living in New York City. Irrationally, I am contented and happy to be here. I am convinced that humans, genetically, have homing urges as strong or stronger than pigeons.

Stanley and I see each other now with moderate frequency—not as much as one might expect of two persons living in the same city who have been friends for about forty years. The problem lies in the nature of New York City. Contrary to the impression of most people who have never lived here, New York is not a large city. It is really a collection of moderate size villages and the residents of each village rarely visit other villages. I live on Twelfth Street while Stanley lives on 90th Street. That distance is not great, it is approximately four miles but in New York that is a problem. The major means of going from one place to another is to find a Taxi, but finding one is a process filled with uncertainty. Sometimes you simply do not see any, at other times you see them but for some reason the drivers do not seem to see you. There is an alternative, the subway, but it requires the sensibilities of a hardened crowd lover to use it. We do, however, get together and, in addition to playing backgammon, each of us periodically tries to get the other interested in what he is currently doing. We each succeed partially.

I have lightly sketched some events from the time when I first met Stanley until the present but I fear that I have not done justice to the quality or tone of our interactions. I have two excuses to offer—to the reader and to Stanley. If I think about it at all, which mostly I do not, I prefer to imagine that Stanley and I are both in our mid-thirties, which makes me feel awkward writing a contribution for a Festschrift for him. There is a second and more important difficulty, a final irony. I am in the habit of getting criticisms of anything I write before completing a final draft. Good, detailed, constructive criticism is very hard to come by; most colleagues aren't interested enough to devote the necessary time. It is much easier to make a few general comments ending with effusive praise. I have always been able to get good, useful criticism from Stanley and, over the years, I have come increasingly to rely on him. Yet, because this volume is supposed to have been a secret from Stanley, I have had to forego this vitally important step in producing this piece. Hence, I ask tolerance from the readers and especially from Stanley.

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The Role of the Mentor[1]

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My undergraduate education was a mixture of chemical engineering and social anthropology. When I started graduate school at the University of Minnesota in an interdisciplinary training program in the behavioral sciences, I had never taken a course in psychology and my transcript attested that I was certifiably ignorant of the field. Several years later, I held a Ph.D. in Psychology, and my curriculum vitae proclaimed that I was a psychologist by profession. Somewhere and somehow, while at Minnesota and since, I have learned something about psychology. When I ruminate about how that part of my education transpired, I always return to one striking fact: the truly important part of my education came from my mentor, Stanley Schachter.

I was fortunate enough to have had several vital and gifted teachers—Harold Kelley, Henry Riecken, Ben Willerman—to name but a few. As much as I learned from them, my real education was the realization, after I went off to my first job, that I had to apply not just psychological knowledge to my teaching and research, but also a way of thinking about the problems that made up the field and a set of standards and values about the work of my colleagues and peers. These beliefs constituted for me the real legacy of a superior education. They are the least changing aspect of my professional demeanor; they are a branch of my apprenticeship for which I remain most grateful; they are the essence of what I hope to pass along to my own students. Nevertheless, I have rarely reflected on how people are socialized into being psychologists, as opposed to being trained in psychology. Now, as I renew the acknowl-

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