Note on George Klein and His Contributions

Since there is no Wikipedia entry for George S. Klein (1917–1971), nor any other online biography, it might be helpful to provide some background on him. First, let me say that there is a pretty good biographical piece in Gill & Holzman (1976), which is subtitled “Psychoanalytic Essays in Memory of George S. Klein.” That work also includes a complete listing of his 66 publications, many of which are gathered into two books (Klein, 1970, 1975). He was the founding editor-in-chief of the monograph series, Psychological Issues, a position he held until his death. A brief obituary appears in the Bulletin of the Menninger Clinic, 35, 207–208.

George came to Topeka in 1946, directly after his discharge from the U.S. Army Air Force. He had enlisted after getting his doctorate from Columbia in experimental psychology, 1942. By 1947, George had become my good friend, fellow member of Rapaport’s Research Department at Menninger’s, and someone I wanted to work with when and if we both left Topeka. He had attracted some first-rate graduate students in his role, like mine, of adjunct member of the Department of Psychology at Kansas University, located nearby in Lawrence. With Herbert Schlesinger and Philip Holzman, and under Rapaport’s general guidance, he launched a program of research on individual differences in ways of perceiving and conceiving the world and their grounding in what Rapaport called ego structures. In diagnostic testing as Rapaport taught it, we learned to recognize several clinically relevant types of such structures: notably hysterical, obsessive-compulsive, paranoid, and schizoid ego-organizations. Using the same basic conceptual approach but experimental designs and methods, George and his group began to isolate what they at various times called cognitive system principles, cognitive controls, and finally cognitive styles. The program was well under way in 1948 when Rapaport transferred to Stockbridge, and its first publication appeared the next year (Klein, 1949).
It also happened to be a time of ferment in American perceptual research, known as the New Look movement. The central theme of much of the new work was showing the effect of personality, especially motivation, on perceptual phenomena. It seems to have been launched by two papers: “Value and Need as Organizing Factors in Perception” (Bruner & Goodman, 1947) and “Symbolic Value as an Organizing Factor in Perception” (Bruner & Postman, 1948). The theories and facts conveyed under those titles seemed revolutionary in the staid world of sensation and perception, conceived the way the founding father of experimental psychology, G. T. Fechner, had formulated it in his psychophysics. Building on the work of a predecessor, E. H. Weber, he had made psychology into a quantitative science of testable hypotheses by postulating that a sensation was proportional to the logarithm of the stimulus intensity, the physical energy (e.g., of light or sound) causing it. Any deviation from the predicted results was treated as mere experimental error. Now, however, people were beginning to realize that such “errors” were not random but were caused (in part) by determinants within the person.

George and his group, who were among the first to start a program of such research, felt that the claim of some that this work was a belated recognition of Freud’s insights was a bit glib, and that cognitive processes were far from the passive plaything of needs, emotions, and the like. Indeed, that was one of the main reasons for calling the determinants they were discovering “cognitive controls.” The first of these to be published (Holzman and Klein, 1954), “Leveling-Sharpening” was measured by a classical procedure of getting people to judge the size of projected squares of light, a series of them gradually getting bigger but in random order. People who were slowest to notice the small increments were called Levelers; others who seized on slight changes, even exaggerating them, were called Sharpeners. Levelers were clearly making heavy use of what Piaget called “assimilation” (to a prior memory), as opposed to “accommodation” (of memory to the new information), which characterized the Sharpeners. Moreover, this cognitive control proved to be related, as predicted,
to the repressive defensive style of hysterical personalities as measured by independent assessment of their responses to Rorschach ink blots.

In a personal communication (December 28, 2011), Herb Schlesinger recalls the time when Klein left Topeka in 1950:

I believe George was in [had just begun training] analysis with Hans Jokl, whose command of English was nominal at best; not one of KAM [Dr. Karl]'s best recruits. It would have been in 1947 or 1948 that George got in touch with Jerry. I joined George in late 1946, delighted to discover that he too had the notion that people see the world differently and that we ought to be able to find systematic variation in what researchers dismissed as error. We set up the perception lab in North Office basement and began our experimental program. Phil joined us shortly after. [. . .] Our research program began with a replication and critique of Jerry’s article on the effect of personal values on perception: a swastika, etc. on a hand-held disc affected its perceived size. We showed that individual differences among subjects, the subject variability that averaging could wash out, were partly a function of what we called their perceptual style. In the course of this work, George arranged to meet with Jerry (Bruner) at Harvard to discuss our findings, [. . .] and took me along; a very heady experience. I recall that Jerry wasn’t much impressed.

Nevertheless, not very long afterwards, Bruner invited George to join his own informal research group at Harvard for a year in the spring of 1950. Recently, Jerry commented:

He was certainly an amazing guy intellectually, and a lovely human being. And, believe me, he gave me courage to go more deeply into the more psychodynamic aspects of the perceptual process—and of cognition generally.

What I particularly loved in George was his ability to infuse the so-called scientific process with an imaginative playfulness. I never knew him to be “heavy-handed” in his thinking about psychological research. And what a relief
that was there at heavy-handed old Harvard!!! What a great companion he was!

He returned to Topeka periodically to supervise the work of his own team: Holzman, Schlesinger, and in 1951 Riley Gardner. The initial Harvard fellowship was for only one year, but he had transferred to the Boston Institute for Psychoanalysis, entering training analysis with Grete Bibring, which was so much more successful that he felt he could not go back to Jokl. Most likely, Jerry helped him get some sort of grant or adjunct appointment from year to year, through the first semester of 1953, to continue their research collaboration. He joined me to share the leadership of the Research Center for Mental Health that September, then completed his analytic training in the New York Institute. He seemed to have come to a satisfactory conclusion of his psychoanalytic treatment; he did not enter another in the rest of his regrettably short life. He died suddenly from heart failure on April 11, 1971, at the age of 53, in Stockbridge, MA, where he, Bessie, and their daughter Rachel had a summer home.

Nothing above gives a hint of the personality of this unique, charismatic, magnetic figure. He was very sociable, a warm, funny, and caring friend, with a strong interest and some talent in graphic art. He could get so intensely absorbed in some new idea that he might hardly notice people around him. Graduate students greatly prized him as a dissertation sponsor. He had one great asset which Rapaport and I both envied, recognizing that despite our strong wishes we lacked it: the kind of creativity plus lab know-how that generated many feasible experiments—practical ways of putting theoretical propositions and hunches to test against the reality of hard data. Klein was so fertile of such productive ideas that I often had to argue for finishing what we had already begun before launching off into another, exciting direction. His

1 None of the survivors of that era recalls precisely how it was managed. I am indebted to Jerry Bruner, Herb Schlesinger, and Leo Goldberger for their help in assembling the above information.
enthusiasm for new “studies” had an infectiousness that made the atmosphere of the Research Center for Mental Health electric with excitement—one of the Center’s most memorable attributes. He brought some of that spark along with him to the meetings of the Rapaport Study Group. The organization’s name was appropriately changed after his death, becoming the Rapaport-Klein Study Group.

A letter of July 17, 2012, from Robert S. Wallerstein provides a nice finale:

George [. . .] is the one who started my whole research career. Gardner Murphy had just arrived in Topeka as Director of Research and and looking for a research-minded psychiatrist who would fill the same kind of role that Merton Gill played under Rapaport in the Research Department. George enthusiastically proposed my name to Gardner on the basis of a research program I had engineered at the VA Hospital studying the treatment of chronic alcoholism. Gardner had not known me, but on George’s advice interviewed me and offered me the job with him at the Menninger Clinic, half-time in the Research Department. (The other half being clinical work.) For me that was the start of everything [notably Wallerstein’s work directing the Psychotherapy Research Project] and I owed it all to George’s sponsorship.