

### ***Summary—Brief Letters from 1957***

On February 26, I asked Rapaport's opinion about publishing a brief account of the main findings of the Selection Project in a psychological journal (cf. Luborsky & Holt, 1957). Nothing seems to have come of it. The next day, he sent some information about a possible job for Crusa.

A note from Sue Annin on April 23 asked for any final input to the Koch monograph (I did not have any). A week later I sent a postcard about George's search for a reference, and on May 14 another brief letter about personal matters. In that I said I was sending a long version of my own review of Colby's 1955 book (Holt, 1956b), adding:

It's not altogether clear to me what you mean by "he did not listen to ps. metapsychology"—just that he does not have an expert familiarity with it? If he commits any real blunders in this respect, I failed to find them, but since my grasp of metapsychology is very far from expert I wouldn't expect to.

In a May 21 letter, Rapaport continued the discussion of autonomy and nutriment briefly. He expressed disappointment about Morton Levitt's (1959) proposed book, and said he was withdrawing his own paper. I had been considering reviewing the theory of primary and secondary process for that book; he suggested that I write it first and then think about where to publish it. This letter contains the first reference to George's plans for the *Psychological Issues* monograph series. Then, after an expression of regret about Robert W. White's "excursions into Adlerism," came these words:

Bob, I hope that we do not build walls between ourselves by bluntly talking out about weakness in each others' papers. I want you to be blunt with mine and I will be blunt with yours or [there] will be nothing. For all that, I am not sure but that you are right about autonomy and about identity that they really are in a

sense descriptive terms. Yet the term descriptive does not quite apply because there lies in these terms a true amount of generalization which ties many phenomena together and allows the field to be mapped out. This is somehow different than your description. I wished I knew precisely how it is different.

He ended with thanks for calling the Alger Hiss book to his attention; he will read it despite some reluctance.

My letter of July 2 reported "a brief visit from Peter Wolff, who gave an excellent talk to our staff on Piaget. . . . I think I understand a number of issues better now." Then hopes for a staff discussion with Rapaport on the new autonomy paper, and summer plans. I reported gossip from Topeka about imminent departures:

The Menningers' talent for finding good men is surpassed only by their capacity to drive them away. . . . They are so threatened whenever someone begins to emerge as a potential successor that they manage to get rid of him, though it is perfectly obvious that the long-range health of the institution demands they train someone to follow in their footsteps.

Paul Bergman was leaving, too, for Bethesda: "What a buoyant soul he is, and how attractive it is to see such mature wisdom combined with the ability to bounce gaily back after a series of harsh blows!" Else Frenkel Brunswik told me she might go there too. A brief report about the commemoration of the Harvard Psychological Clinic's founding and a forthcoming tribute to Murray, to which I hoped Rapaport would contribute. Finally some words about a reading course in metapsychology with "Sheldon Bach, one of our best assistants. . . . It's quite a splendid experience to see a first-rate young mind coming to grips for the first time with a set of important ideas and getting really excited by them."

A quick response from Rapaport on July 8, after acknowledging a week's illness, commented positively on all the above topics. "If the two years Wolff

and I put in on [Piaget] will serve to loosen up the . . . lack of understanding, then we will have done something which should have a remaining impact. You might think that I could have spent my time better, but in my own self appraisal this is good enough a 'midwife' job."

He then asked my help in starting his paper "about genetic psychology and Freud," to be given at Clark University:

What kind of books and papers am I supposed to know about genetic psychology besides Werner. . . What are the principles of . . . developmental study? How does it differ from simply historical study? How does it differ from study in terms of learning. . .? [. . .] I would very much like to hear your opinion on it and hear what I should cover if I want to give a picture on present day developmental psychology or genetic psychology and Freud, and both Freud's influence on it and a comparison of Freud's genetic psychology with present day genetic psychology.

Finally, he recounted having finished a seminar for the Western New England Psychoanalytic Institute on "Advanced Metapsychology," as well as an elementary one, both of which would be transcribed. (I was later to profit greatly from studying these and all of his other transcribed seminars on psychoanalytic theory.)

The letter ended with thanks for my first comments about his Activity-Passivity manuscript, and upbeat comments about the work of several mutual friends: "quite a few things ripening."

My response a couple of weeks later sent my regrets about not to be able to help with the Clark paper, genetic psychology being "an area in which my own ignorance is vast," but hopes to see him in September.

A note from Rapaport on August 1 sent along some textual corrections for Leo Goldberger's thesis outline. My reply at the end of the month reported three wonderful weeks with my daughters in Hawaii, then addressed his review of

Bruner et al. (1956), with a few stylistic suggestions, and this comment: "Why do you suppose Piaget went overboard so enthusiastically for this book? Is it perhaps because he is out of touch with the issues of method in terms of which this work seems to be so fallible? I hope you will send a copy of your review to him."

A brief exchange of notes in September regretted missing one another and anticipated Crusa's and my visit to Worcester to hear the developmental paper.

Finally, a Christmas card reported the good news from Crusa and me: "We're getting married on Dec. 27! Another proof that analysis does some good."