The Place of the Conditioned Reflex in Psychology and Psychiatry:
Reply to Reese, Dykman, and Peters

GREGORY RAZRAN
Queens College of The City University of New York

Abstract—Razran's "The Place of the Conditional Reflex in Psychology and Psychiatry" delivered at GAP Symposium No. 9 traced historically the influence of Russian physiology on American behavior systems, first in the classical Watson period and then in present-day Neobehaviorism. The address collated Pavlovianism and Behaviorism, and mentioned recent Soviet emphasis of interoceptive, semantic, and compound-stimulus conditioning. Within the limits of time, it dealt with group developments and not with individual contributions. And it assumed that a behavior system comprises both normal and abnormal aspects; that is, the basics of both psychology and psychiatry. The assertion by Reese, Dykman and Peters that "Psychiatrists tend to avoid contributions of experimental psychologists and vice versa" is not shared by Razran, nor, he believes, by American psychiatry and experimental psychology as such. And the assertion is contrary to the Pavlovian tradition. Pavlov's first, 1903, publication on conditioned reflexes was entitled "Experimental Psychology and Psychopathology in Animals" which he later renamed "the physiology and pathophysiology [or pathophysiology] of higher nervous activity."

In an article in the American Journal of Psychiatry (December, 1964) entitled "A Gap in GAP Symposium No. 9: Pavlovian conditioning and American psychiatry," Reese, Dykman, and Peters quote a May 1964 Newsletter of the American Psychiatric Association as stating: "The impact of Pavlov in American psychiatry is elaborated with admirable clarity in Symposium No. 9 . . . It may well prove a significant contribution to the intellectual history of American psychiatry . . ." (13, p. 555). The authors declare, however, "the two central papers [of the Symposium] seem to demonstrate immense historical amnesia coupled with phenomenal ignorance of the contributions of American psychiatrists and psychologists, yet both are very knowledgeable [sic!] about conditioning literature. . . . The discussants provide useful information, but accomplish little toward

filling the gap . . . the publication [Symposium] is a monumental insult to the leading American Pavlovian, W. Horsley Gantt . . . magnifies the probability of misleading those readers who are not conversant with conditioning literature" (ibid).

Continuing with the “Critique of Razran’s Paper,” the authors write: “According to Razran, the American branch of Pavlovian genealogy [sic.] comes to us mainly through John Watson. In truth, Watson made conditioning theory unacceptable to the mainstream of American psychiatry and psychology . . .” And—presumably to highlight my amnesia and ignorance—the authors (a) quote statements by Koshtoyants, Pavlov, Lashley, and Watson and (b) enumerate more than a score of contributions by Gantt and his co-workers and by other American students of the field that are not in my paper. “Razran,” they say, “is apparently much more conversant with the Russian literature than with the American literature . . . the Russians do not need his assistance in distorting medical history . . . It was Gantt, and not the Russians, who pioneered the work in cardiac conditioning” (pp. 557-558). My view that in visceral conditioning “American laboratories have yielded no more than a pitiably meager amount of research”—as compared with other modes of conditioning, with known Russian output in the area, and with Watson’s original intent and interest—is labelled “blatant” and “utter nonsense.” (I mentioned in the paper that by now the Russians have reported 98 studies of interoceptive conditioning while we have 2, and should add—or should have added—that the number of their salivary conditioning experiments in animals is approximately 1800.)

Reese, Dykman and Peters say, in essence, very little against what I said at the Symposium—indeed even pay us some compliments. What has disturbed them is what we did not say, and also the very fact that we were asked to do the saying. The first statement in their Conclusion reads: “American psychiatrists have paid insufficient attention to Pavlov and to Russian contributions in general—” (p. 560). As the very title of their paper reveals, their crux hinges on a gap and the gap is primarily Gantt. A documented reply will, it is hoped, fully clear the matter—gap, GAP, and Gantt—and shed light on what seems to be a benighted chapter of American psychiatry.

Let me begin with saying that the gap in my paper should not be construed as sequelae of amnesia or agnosia. I am fully familiar with each publication from Gantt’s laboratory and with each of the other non-Russian publications in the “Critique,” including the chapter, “Freud’s Psychodynamics,” in Hilgard’s Theories of Learn-