Thinking in Psychological Science

Ideas and Their Makers

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Introduction
How Our Future can be Observed in Our Past: Mental Bases for Scientific Inquiry

Jaan Valsiner

This book is about the future of something we have nicely labeled "the cognitive science." Labels are often confusing—and so this one. As the movements within the cognitive science reveal over the past few decades, the reliance on new technologies is not automatically giving us a head start on new ideas. Thus, the legacy of Vladimir Bekhterev's associative reflexes is transparent in our contemporary neural network modeling attempts, and much of contemporary problem solving themata were anticipated by Otto Selz and Karl Duncker (Simon, chapter 1 in this book).

Yet scientists usually dump their own history, considering it to be a graveyard of failures. Of course in terms of the *post factum* successes in a given area they may be right—if 99 percent of suggested solutions turn out to be wrong, the glory of the remaining one would be seriously undermined to show that scientific breakthroughs are outlayers in the normal distribution of efforts within the normal science. The demonstration of the actual slow and inefficient movement in ideas is far from the need of creating a hero image of the scientists for the lay public. Such hero images are undoubtedly on demand for granting research funding in democratically controlled governmental funding agencies. Hence the revelation of the failures of the past ideas may be detrimental for the future.

However, aside from the granting of money and prestige to new generations of scientists, the reality of science operates on the transformation of basic ideas. Some of such transitions are very slow—it took chemistry around two centuries to move from alchemy to science. Psychology—since the nineteenth century—may be seen to be undergoing a similar transformation—without a clear image of the end point in mind. In the process of such creative search for new solutions, a look back into the history of the given discipline can provide two kinds of guidelines for the future. First, it can show the futility of some directions of thought that were tried out in the past and failed to provide pro-

ductive solutions. However, these failures may have been caused by circumstances external to science—the lack of resources, or illness or demise of the scientists right before they were about to arrive at a solution. Thus, secondly, a scrutiny of history allows us to find the potentially productive—yet dismissed or unfinished—directions of our thought. For example, a careful scrutiny of the thinking that went into the construction of introspection experiments in the "Würzburg School" could have relieved our contemporary cognitive science from unproductive discussions about "the method."

History of sciences as I conceive it here is thus useful for the future of these sciences. Hence the creation of the From Past to Future project in 1998. At my arrival at Clark I decided it is a place where a careful re-look at psychology's history can be appropriate. The American Psychological Association was founded at Clark in 1892, and the whole University lives under the totemic symbol of the 1909 visit to Clark by Sigmund Freud. The latter's contribution is presented locally as a revolutionary challenge to conformity—rather than the basis for an alternative conforming community (see figure 1). Thus, the tensions present in psychology at large have had their local counterparts within that tiny private university in New England. Psychology has had its "ups" and "downs" at Clark since 1889, with the times of G. Stanley Hall and Heinz Werner clearly marking the unique productive periods

The From Past to Future project was initially born as a small-scale "technical reports" series that brought together a small set of historically oriented scholars who enjoyed contributing to the series. However, their contributions remained inaccessible to the wider world—the series was known only to few enthusiasts. Yet our work was productive—over seven years (1998-2005) we published ten issues on a number of topics. Its aim was

> … to chart out a new role for the study of psychology's history for contemporary and potential future research in the discipline. Many of the empirical research problems in contemporary psychology could benefit from a fresh look at how similar issues were approached in the past of the discipline. Therefore, we seek to analyze different ideas from the past through the prism of their potential usefulness for the future.

Psychology, similarly to many other disciplines, has been trying to forget its history. Or, when history is unavoidable, it has been rewritten so as to glorify the heroes of the past and overlook their tedious fight with themselves, their peers, their surrounding society, and—last but not least—for mere physical survival during hard economic times. The heroes of our past are usually depicted as able problem-solvers who rarely moved in unproductive directions in their thinking, but if they did, corrected themselves immediately. If that were true, then psychology should have solved its major problems a long time ago, thanks to all of our heroes. Yet this is obviously not the case. Psychology in our time is caught in the web of conceptual confusions and phenomenological oversights that often seem to resemble some of the issues with which the major psychologists of the past

attempted to wrestle. Therefore, it may be useful for contemporary psychology to trace the efforts of the past, precisely because we need that for our present efforts to solve our still unsolved basic science problems. Thus, analysis of the past can become productive for the construction of the future. Sure, taking this position amounts to creating yet another version of history of psychology.

Thanks to the initiative of Transaction Publishers (and Irving Horowitz who, after seeing some of the issues we had published, was fascinated by our intellectual efforts), the present book brings to the wider audience a selection relevant for the investigation into cognitive processes. A second book (Diriwächter and Valsiner, 2007) will also build on the results of this project. And—last but not least—From Past to Future becomes a new international annual series published by Transaction Aldine in 2007. Our local history of studying psychology's history at Clark thus becomes an international framework for such study on a wider scale.

References

Diriwächter, R. and Valsiner, J. (eds.). (2007). *Striving for the Whole: Creating Theoretical Synthesis*. New Brunswick N.J.: AldineTransaction.

Introduction Figure 1
Freud in his current presence on Clark University Campus


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Part I

Roots of Cognitive Science: Karl Duncker


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1

Karl Duncker and Cognitive Science

Herbert A. Simon[1]

This is a paper about Karl Duncker and his significance for American psychology. But, as Newton and others have pointed out, you can't talk long about scientists' discoveries without mentioning those on whose shoulders they stood, or those who stood on their shoulders. Nor need the shoulders belong to giants, for as there is a whole succession of shoulders, any desired altitude can be reached by conjoining persons of normal height. So I shall discuss Duncker as standing on shoulders of predecessors who extend back to the beginnings of modern psychology in Germany, and offering shoulders that connect these origins with the present—actually only four or five generations in all. Moreover, the story concerns the transmission as well as discovery of knowledge, and is complicated at many points by multiple independent inventions of the same ideas.

Accordingly, at least three different topics arise in assessing the significance of Karl Duncker's research on problem solving. The first topic is the direct influence of Duncker's publications upon the various strands of research that formed the "cognitive revolution." The second is the specific relation of Duncker's theory of problem solving to the information processing theories produced by the revolution. The third is the relation between the ideas that originated with Duncker and the ideas that were transmitted by Duncker but originated with other German psychologists, especially those of Leipzig, Munich, Berlin, Würzburg and Mannheim, during the first four decades of the twentieth century. The three inquiries lead to different, complementary pictures of Duncker's impact.

I will say something about each of these topics, not from the standpoint of someone who has made a professional study of the whole history, although I have examined most of the source materials, but from the standpoint of a researcher who participated in the cognitive revolution and has had occasion to think and write about it and its origins (Newell and Simon, 1972-, Historical Addendum,

pp. 873-889; Simon, 1981). After a prelude describing how the events appeared to our own research group, it will be convenient to continue with the second question about Duncker posed above, then take up the first, and finally address the third. In dealing with the second question, I will draw upon a paper by Allen Newell (1981) that used Duncker as a prototype of German research on problem solving during the first half of the twentieth century.

Prelude: A View of the Cognitive Revolution from Pittsburgh

The cognitive revolution[2] that took place in American psychology beginning in the 1950s, gradually reconstituting and replacing the then-dominant behaviorism with an information-processing formulation, has sometimes also been referred to as a counter-revolution, for it rebuilt ties to European psychology, notably to Gestalt psychology and Denkpsychologie, that had been greatly weakened during the behaviorist era. Whether revolution or counter-revolution, it is of considerable interest to see how psychology on the two continents became reconnected.

The new psychology involved not only conceptual innovations but new or rejuvenated methodologies as well. One wholly novel component in the information processing approach to cognition was to model human behavior by computer simulation. Only a minority of those constructing the new psychology used computer models, but they were central to the work at Carnegie Mellon. A rejuvenated method was to use verbal think-aloud protocols of subjects as data, to test the validity of the theories incorporated in the computer programs. Both of these procedures were viewed by behaviorists with great suspicion and skepticism.[3] Verbal protocols were usually considered a throwback to turn-of-the-century introspection, which had been almost wholly rejected as a valid source of empirical data. The computer programs were at first only admitted as "metaphors," rather than, as claimed by their proponents, theories in the form of systems of difference equations.

Both protocols and computer models of psychological processes occurring inside the brain were widely regarded as inadmissible manifestations of "mentalism." Even Edward Tolman, almost the sole American psychologist of that time whose views were close to those of *Denkpsychologie* (whether by direct influence or independent invention I cannot say), strove very hard to cast his writing in behaviorist terms. He evidently thought that this concession was a precondition to his remaining in conversation with his peers and thereby having his work taken seriously.

Experimentation also had an increasingly strict discipline, especially with the introduction of the new statistical hypothesis testing theories before World War II. Within this discipline, the purpose of experimentation was to verify or reject theories by testing whether the data could not be equally well explained by a "null hypothesis." There was little understanding of how theories could be generated in the first place, or of the role of observation and experimentation

in theory generation. No concept of exploratory experimentation was abroad; such experiments were dismissed as "counting the bricks in a wall."

When Allen Newell, Cliff Shaw, and I began research using computer simulation, around 1954, none of us had done graduate study in psychology, or had psychologists as mentors or as a principal reference group. Therefore, we did not need the courage that genuine psychologists would have had to muster (and that psychologists like George A. Miller and Carl Hovland did soon muster) to experiment with the new methods. The papers that grew out of our first efforts—the Logic Theorist (LT), the NSS chess program, the General Problem Solver—and the laboratory experiments associated with them, were all published in computer science journals, that had their own newly formed conceptions and standards of what constituted publishable research. These standards, mainly influenced by the physical sciences and mathematics, differed substantially from those prevalent in experimental psychology and were easier than the latter to reconcile with the new practices. In particular, they did not limit experiment to hypothesis testing, with "controls" and "null hypotheses," and were thoroughly comfortable with exploratory research.

When the time arrived, however, to publish in psychological journals, we had to be concerned with the legitimacy of our methods in the eyes of psychologists. For the initial plunge we choose *Psychological Review*, believing that a theoretical journal encompassing social as well as experimental psychology would be least bound by behaviorist scruples. I also knew the editor, Ted Newcomb, well. He was a sociologist as well as a social psychologist of distinction, and I was aware that he had a broad tolerance for diversity. Our first psychological paper, "Elements of a Theory of Human Problem Solving," was mostly devoted to describing the Logic Theorist program and its implications for human problem solving, but we devoted several pages to what we titled "Comparisons with Other Theories."

With respect to "associationism," we declared our adherence to the associationist belief that "the highest mental functions can be performed by mechanisms" but proposed "a theory of the information processes involved in problem solving and not a theory of neural . . . mechanisms for information processing." We suggested that the computer provided psychology "with a much profounder idea than we have hitherto had of the characteristics a mechanism must possess if it is to carry out complex information processing tasks."

Next we turned to "Gestalt" theories, saying, "The theory we have presented resembles the associationist theories largely in its acceptance of the premise of mechanism, and in few other respects. It resembles much more closely some of the Gestalt theories of problem solving and perhaps most closely the theories of 'directed thinking' of Otto Selz and Adriaan de Groot."[4] We then proceeded to enumerate and discuss seven specific respects in which the Logic Theorist could be mapped onto the problem solving theory of Selz, focusing on the relation between LT's processes and Selz's "schematic anticipation."

It cannot be said that either behaviorists or Gestaltists embraced the new theory or its methodology with enthusiasm, and several decades passed before it became a major current in the psychology of thinking and problem solving. To reach that status, behaviorists had to become reconciled with the use of theoretical terms that referred to events within the head (goals, strategies, discrimination nets, and so on) and to the "subjectivism" of verbal protocols and the frequent absence of the null hypothesis, while Gestaltists had to become reconciled with the idea of assembling Gestalts and thought processes from components realizable by mechanism. It would be an exaggeration to say that the reconciliation with either view is complete today, but information processing psychology has acquired a large following, to the point where it is the mainstream, at least in the domain of cognition and much of perception.

Meanwhile, publication remained a problem, which we solved through the decade of the 1960s mainly by publishing in computer journals (at least eight major papers), the annual Spring Symposium that we initiated at Carnegie Mellon University (four papers), *Psychological Review*, which continued its tolerance (three papers), *Verbal Learning and Verbal Behavior* (two papers), *Journal of Math Psychology* (one paper), *British Journal of Psychology* (one paper), and invited papers for symposium volumes (at least fifteen papers). Invitations to symposia of psychologists began to arrive about 1962. The publication problem was further eased by the creation of *Cognitive Psychology* in 1969 and *Cognitive Science* in 1977, which, at least to some extent, were open to papers containing computer models and verbal protocols. Notice that none of the standard American experimental journals appear on this list. Nevertheless one cannot argue that the work went unnoticed, and by 1970, it was beginning to appear in the textbooks.

Duncker's Theory and Information Processing Psychology

Allen Newell (1981), in a paper prepared for the American Psychological Association's celebration of the centennial of the establishment of Wundt's laboratory in Leipzig, and using Duncker as a prototype of German research on problem solving before World War II, compared the central ideas of Duncker's monograph, *On Problem Solving*, with the new information processing ideas and methods that were introduced by the cognitive revolution. He summarized what he found in the following paragraph:

> Duncker interpreted everything through Gestalt spectacles. To first order what he says is familiar: task demands, functions, blind solving, understanding, insight. Yet, as an acute observer of the actual problem solving behavior of his subjects, in his understanding of process he moved beyond anything to be found in the other Gestaltists.[5] He understood much that we now see in modern information processing terms: the structure of general heuristic methods, means-ends analysis, search. He was even reaching for how to characterize the underlying processing structure—the architecture.

Newell also describes what is missing from Duncker and the other European psychologists, and had to be supplied by the cognitive revolution. These missing pieces include: 1) the demonstration of a process theory that was sufficient to account for successful problem solving; 2) the need for an underlying symbol system to carry out the processes; 3) a precise notion of how means-ends analysis works, by the use of operators to reduce differences between current situation and goal; and 4) recognition that selective (heuristic) search is at the core of all problem solving and is not to be equated with blind trial and error.

Newell then goes on to examine Duncker's notion of insight, concluding that "[it] was for [Duncker] a special computational device where, having encoded the knowledge, the computation could be made by recognition"—a view of insight that sounds entirely modern.

For the analysis that led Newell to these conclusions, I refer the reader to his article. With respect to Duncker's influence on American psychology, a point I will discuss more generally in a moment, Newell argues, and I think correctly, that the ideas just described had relatively little impact: that Duncker's main influence in this country was to focus a great deal of attention and experimental energy on a specific issue, the phenomena of functional fixity as a source of problem difficulty.

My own view is that, in finding many of the ideas of information processing psychology clearly embedded in Duncker's theories, Allen Newell was writing a slightly Whiggish history. It is reasonably easy, given the hindsight of operational computer simulations of human thought processes, including means-ends analysis, verbal learning, recognition, understanding and reorganization, and at least one form of intuition, to interpret the much less precise and process-oriented Gestalt language as embodying these concepts. It was much less easy before the appearance of computers, lacking the actual simulations and therefore working in the forward direction, to generate the modern concepts *de novo* from the Gestalt language that was used to describe the experimental findings.

In reaching this conclusion, I do not rely only on my own distant memories of how foggy and undefined the Gestalt and *Denkpsychologie* theories, with their "concretization" and "reorganization," appeared to me before we undertook our computer simulations. In Humphrey (1951), we have someone thoroughly sympathetic with the Würzburg and Gestalt theories but unfamiliar with modern information processing formulations. Yet in his final assessment of Selz's theory, he concludes:

> Selz's diagrams [of a schematically anticipated complex] really do no more than state the problem. They do not give any basis for understanding the psychology of its solution. . . . As an analysis, in general terms, of the form assumed by the process of thought at a certain stage, it is valuable. As a schema showing the working of particular acts of thought, it is as valueless as was the general statement of the problem of medicine—*given the disease, problem, to find the remedy*— . . . in showing the manner by which a particular disease is healed (Humphrey, 1951, p. 144).

In a similar vein, Humphrey says of the Gestalt theory of thinking, in which he includes Duncker's theory:

> At the present time, the Gestalt theory of thinking constitutes a program rather than a fulfillment. Nevertheless, the program is sufficiently suggestive, sufficiently developed, and, in part at least, insufficiently clear outline to merit somewhat detailed consideration (Humphrey, 1951, p. 150).

Among the major problems Humphrey regards as "left unanswered" are the distinction between productive and reproductive thought, and the fact that certain problems seem clearly to be motivated "from without" rather than "from the dynamics of the perceived-situation." He observes further that "the Gestalt theory of thinking has developed over a period of years . . . the contributors to the theory have stressed different things both in their theoretical statements and in their experiments. The accounts have not yet hardened into a coherent doctrine" (pp. 182-183.).

So, without having already available a set of mechanisms and a language for describing them with some precision, it was not at all easy, even for a sympathetic analyst, to extract a viable theory of problem solving from Selz's account, or from Duncker's, however informative and suggestive their experimental findings. This asymmetry between interpretation by hindsight (Newell) and by foresight (Humphrey) is familiar to every historian of ideas.

The Routes of Transmission

German psychology was by no means unknown to American psychologists prior to World War II. Indeed many of the most prominent "first-generation" American psychologists had studied in Germany—especially, during the early years of the century, with Wundt at Leipzig—and several of the Germans, notably Duncker himself, had made shorter or longer visits to the United States even before Nazism forced many of them to immigrate. In fact, Duncker earned his Master's degree at Clark University in 1925, and, after further study in Berlin leading to a doctorate under Köhler, returned to the United States about 1938, where he taught at Colgate during the last two years of his life.

We can obtain some information on these matters by looking at frequency of mention of some prominent psychologists in the first edition of Woodworth's Experimental Psychology, published in 1938. Table 1.1 includes most of the German psychologists frequently cited by Woodworth, and, for calibration, some among the important American psychologists who were more or less contemporary with them. The German psychologists who played a major role in establishing modern psychology and many of its key experimental paradigms in sensory, perceptual (including form perception), motor, and learning psychology dominate the list. Müller, Wundt, Fechner, Weber, Helmholtz and Titchener, along with one Gestaltist, Köhler, a Russian, Pavlov, and two Americans, Thorn-

Table 1.1
Frequency of Citation in Woodworth

Müller	30	Helmholtz	20	James	9
Wundt	29	Titchener	20	Duncker†	8
Köhler†	27	Wertheimer†	14	Gibson	8
Fechner	26	Hull	13	Külpe ‡	7
Thorndike	24	Lashley	13	Selz‡	7
Pavlov	22	Koffka†	12	Ach‡	5
Woodworth	21	Bühler	10	Katona†	4
Weber	21	Brunswick†	9	Maier†	4

This is a partial list of German (and some American) psychologists cited by Woodworth in his Experimental Psychology, 1938. The numbers following the names are the total numbers of citations.

†Gestaltists ("Berliners")

‡Members of the Würzberg School

dike and Woodworth, constitute the top ten. Four more Gestaltists, Wertheimer, Koffka, Brunswik, and Duncker, are included in the next ten, along with the Würzburger Külpe. Selz (whom we can associate with the Würzburg school), Ach, also a Würzburger, and Katona and Maier, younger Gestaltists who both migrated to the University of Michigan, follow.

It is evident from the table, and confirmed by Woodworth's text, that the German psychology that lay within the mainline experimental tradition (whatever its exact theoretical orientation) was well-known in America and was part of the standard corpus. But while the psychologists who are associated with Gestalt theory and Würzburg ideas are cited by Woodworth, they are not, except for Köhler, cited with anything like the frequency of the pioneers in sensory, perceptual or motor psychology. Moreover, nearly half of the citations to Köhler are for work on sensory and perceptual phenomena not directly related to form perception or thinking.

All the citations of Duncker, Selz, and Külpe, and a majority of those of Ach occur in Woodworth's chapters on problem solving and thinking (only eighty pages in an 800 page book)[6], and all the citations of Wertheimer occur either in those two chapters or the one on form perception. Throughout, more emphasis is placed upon the experimental phenomena, then upon the associated theories. Some attention is paid to intuition, directed association, set and "constellation theory," but much more to Köhler's experiments with apes, Thorndike's with cats, and other problem solving experiments with humans. Woodworth discusses briefly, and not unsympathetically, the use of thinking aloud as a source of data, and contrasts it with introspection.

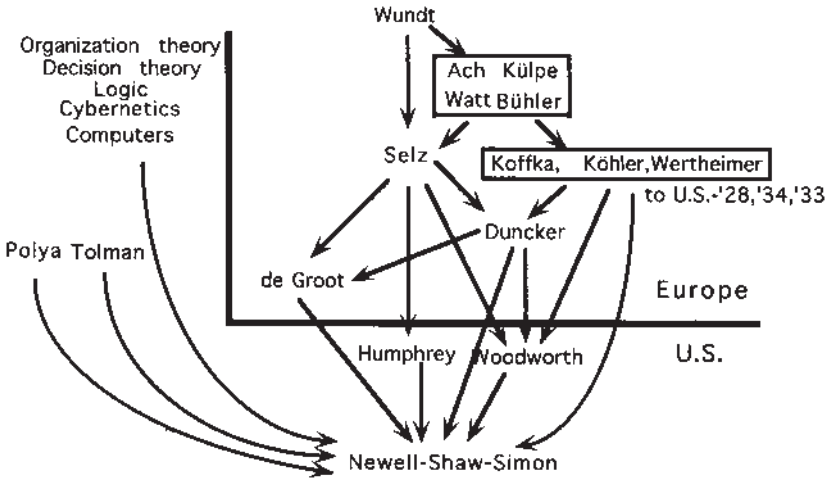
One might say that Woodworth was extracting from this research its empirical content and was not principally concerned with drawing out the theoretical implications of the experiments for behaviorism, "thought psychology," or his own functionalism. His emphasis is not inappropriate for a textbook titled "Experimental Psychology," but students who depended on Woodworth or similar books for their knowledge of theory would have only an exceedingly sketchy picture of the conceptual frameworks within which Ach, or Selz, or Wertheimer, or Duncker were working, or the hypotheses they were seeking to test. However, we must also recall that Köhler's Mentality of Apes had appeared in English translation in 1924 his Gestalt Psychology in 1929, and Koffka's Principles of Gestalt Psychology in 1935, prior to the time we are considering, so that Americans had excellent access to Gestalt theory if not to the problem solving theories of Duncker or Selz.[7]

The main problem was not access but the difficulty of providing an interpretation of the Gestalt and *Denkpsychologie* findings and concepts that would be consistent with the dominant behaviorism. With rare exceptions, mostly in developmental and social psychology and in form perception, the European theoretical ideas about thinking and problem solving, were largely ignored prior to World War II as unintelligible within the behaviorist framework and as violating its rules for both method and theory. What was not ignored were some of the experimental paradigms, particularly those concerned with "complex" behaviors, of which the "insight" problems of Maier, whose publications on these topics (in English) began about 1930, and Dunker's famous x-ray experiment are examples.

With the arrival in America of European refugees from Nazism, and with the beginnings of new interpretations and translations of European psychology just after World War II, new connections began to form. For example, the three principal Gestaltists, Koffka, Wertheimer and Köhler, assumed academic positions in American universities in 1928, 1933, and 1934, respectively, and Duncker spent time here in the middle-1920s. Figure 1.1 displays, in highly simplified form, some major routes of communication for problem solving research and theories before and after the war. A number of critically important European sources are shown above the horizontal line and two important potential bridges to our own (Newell-Shaw-Simon) work, Woodworth (1938), and Humphrey (1951), just below the line. Wertheimer's *Productive Thinking* (1945) is represented by a direct path across the ocean; as is de Groot's, *Het Denken van den Schaker*, (1946; English translation, 1965). Paths emanating from the names of Polya and Tolman and from a range of topics outside psychology represent major American in-fluences on our work that are not traceable to the German sources mentioned.

In his paper on Duncker, Allen Newell states that he first became acquainted with *On Problem Solving* shortly after we completed our work on the Logic Theorist and published our early papers on information processing psychology.

Figure 1.1
European and American Influences on Early Cognitive Simulation by Carnegie-Rand Group



My own recollections of my first encounter with Duncker are less clear than Allen's, but our two dates could not be far apart, for we were working very closely together, and whoever found it first would have immediately called it to the attention of the other. As I am rather compulsive about referencing relevant papers, even if first encountered when I am revising a paper for final publication, I find confirmation of his recollection in the fact that Duncker is not cited in two papers on decision and problem-solving processes that I published in 1955 and 1956, respectively. The second of these papers, however, has the following footnote on p. 21:

> Since writing [the 1955 paper], I have found confirmation for a number of its hypotheses in the interesting and significant study by A. de Groot (1946), of the thought processes of chess players.

This footnote indicates that *Denkpsychologie's* influence on us came from de Groot (and indirectly Selz) rather than Duncker, and that its main importance was in providing us with supporting evidence for our approach to cognition, based on heuristic search and simulation, which was already formulated by 1954. Hence we were now able to read and interpret Selz and Duncker with hindsight and without requiring foresight. I received de Groot's book and began reading it (in Dutch) in August 1954. It may be that it was acquaintance with the book that led us to acquire Duncker's monograph, but I have no evidence on that point.

So as far as the evidence goes, then, it does not appear that Duncker's work directly influenced the initial formulation of information processing psychol-

ogy or the implementation of the first computer programs, the Logic Theory Machine and the General Problem Solver that embodied the theories. In the historical appendix to *Human Problem Solving* (published sixteen years later), our description of the actual influences has mostly to do with: 1) the psychology of William James and E. C. Tolman, 2) the ideas of Polya about the role of heuristics in mathematical problem solving, 3) developments in logic, 4) the congeries of "cybernetic" ideas emanating from the recently invented digital computer, information theory, and feedback control theory, and 5) decision making theories in economics and statistics.

On the other hand, de Groot's book, which we first encountered in 1954, guided us into the extensive use of think-aloud protocols; and de Groot, in his book, credits Duncker with developing the think-aloud methodology well beyond the state in which the Würzburgers left it, and relies largely on Duncker's monograph for his discussion of protocols. De Groot, however, derives his substantive theoretical framework directly from Selz, and only makes occasional reference to Duncker on substantive points.

But much was going on during this period in other groups than ours. A number of experimental psychologists who were now moving into the domain of human problem solving came into contact with such immigrant Gestalt theorists as Wertheimer, and Katona, and with Maier, so that Duncker's work was only one communication channel among many. These Americans were encouraged by the writings of the whole group of Gestaltists to experiment on such phenomena as insight, functional fixity, and set, and to employ methods in their experiments that challenged behaviorist restrictions on what counted as data. They were much less influenced by the structuralist ideas of Selz, whether transmitted through de Groot, Humphrey, or Duncker.

Thus, comparing Newell's analysis of Duncker's theories, with this account of their impact in the United States, we come to the conclusion that although Duncker had, prior to the cognitive revolution, come a considerable way along its path toward a process theory of problem solving, his writings played only a limited role in bringing the revolution about. Here priority and influence must be distinguished.

Duncker and Selz

There remains one important chapter of the story. It is clear that de Groot's book on the thought processes of chess players did play an important role in the new developments in our group (and among other American psychologists after the translation became available in 1965), although mainly on the side of empirical methods for testing the new theories than upon the substance of the computer simulations themselves. Is this another route along which Duncker exerted substantial influence? De Groot is quite explicit in attributing the foundations of his theory of chess playing to Otto Selz. What, then, is the relation between Selz's theory of problem solving and that of Duncker?

Otto Selz is a tragic figure who died at Auschwitz in the Autumn of 1943, aged sixty-one. He received a Ph.D. in philosophy in Munich, then went to Bonn for his Habilitation and to work with Külpe. He was much influenced, also, by the theories of Bühler (1907/1908), Watt (1905), and Ach (1905), hence was a thorough "Würzburger," although he spent the main part of his career, a decade, in Mannheim. He published his major work, *On the Laws of Ordered Thinking*, in two parts, in 1913 and 1922, the interruption being caused by his military service in World War I.

Selz was a lonely person who never married and had only a few serious students, notably Julius Bahle and Adriaan de Groot (neither of whom were actually his doctoral students, but both of whom were strongly influenced by him). Having been removed from his professorship in Mannheim by the Nazis in 1933, he became even more isolated from the world than before.

Shortly after its publication, Selz read Duncker's *Zur Psychologie des Produktiven Denkens* , and wrote on April 4, 1935, to Bahle:

> You must read Duncker's book on the psychology of productive thinking. His terms are often confessedly translations of mine. He sticks close to me even when he claims to diverge. So apparently my whole Work, parts of it somewhat watered down, is now taken over by the Berliners. On the whole, he has behaved fairly, but did not send the book to me.

This has the familiar ring of "he stole my ideas," and this was not the first time that Selz had shown sensitivity on that score (a trait that does not distinguish him from many, perhaps most, other scientists). It is likely that his sensitivity had been acerbated by his having been declared a non-person by the Nazi government; his last defense against oblivion was being taken from him. But there is more to the matter than that. Selz acknowledges that Duncker behaved fairly, and generally he had: He acknowledged his debt to Selz in nine references, more than were given to any other psychologist except Köhler, and many of them accompanied by substantial discussions. This even required a measure of courage on Duncker's part in 1935, for references to Jewish scientists were not encouraged by the authorities, if not yet quite banned by law.

The crucial additional fact is that Duncker touched a spot in Selz that was already sore.[8] In 1925, in the *Manual of Philosophy*, Koffka had written a hundred-page article on psychology that was so biased from a Gestaltist's viewpoint that he was strongly reproved for having borrowed Selz's ideas without citing him. Bühler, in a review of Koffka's article, uses strong language: "How can Koffka venture, in his manual, to describe the whole theory as a product of 'Gestaltpsychology,' without mentioning the name of Selz except to criticize him on insignificant points?" Two years later, Koffka replied with the claim that there was no need to cite Selz, as the Berlin "structure theory" and the Würzburg "process theory" were completely different! With this exchange

still in recent memory, Selz had good reason to be sensitive to the intellectual imperialism of the "Berliners."

By Duncker's own account in *Zur Psychologie des Produktiven Denkens*, his theory was based squarely on Selz's, although developed further in several respects and supported by substantial new evidence gathered by means of innovative experimental designs and methods. Without insisting on an either-or decision, we have to regard the contributions of de Groot and Duncker, apart from the empirical evidence they provided (an important qualification), as primarily acts of transmission rather than innovation in theory. Their contributions do not thereby become any less significant or original. In contemporary views of science we are all too prone to idolize theory and describe empirical contributions as "mere fact-finding," or at best, simply confirming (or refuting) what the theorist has wrought. That is a bad reading of the history of science, where important theories have as often emerged from imaginative (or even lucky) experiments that revealed new phenomena, as have "crucial" experiments from theory.

The central advance that Selz made in our understanding of problem solving involved the concept of "schematic anticipation," which he diagrammed with the relational structure, aR?b, where a is a given concept, ?b a desired concept and R a relation (*Aufgabe*). Given a and R, the problem is to produce an appropriate b: e.g., given "cat" and "coordinate," to produce "dog" or "tiger." In its simplest form, this is simply the "directed association" of Watt and Ach. What Selz does is to show how problems in general may be solved by replacing an initial schematic anticipation with successive new ones, derived from a and R, that approach closer and closer to the desired b. What a logician might say (exercising a little of the hindsight that I complained about earlier in Newell's account of Duncker) is that Selz had rediscovered the great power of simple two-argument relational structures and their applicability to problem solving. What a computer specialist might say is that he had anticipated the "description lists" or "property lists" of list processing languages. What an information processing psychologist might say is that he had found the underlying structure of means-ends analysis and thereby of heuristic search. This is the basic structure that Duncker learned from Selz and applied in his own important research.

So we come to the end of our examination of Duncker's work on problem solving, with findings much more complex than a simple enumeration of "innovations" and "influences." Duncker was an excellent and innovative experimentalist; he understood Selz's theory and its basic importance, and he extended it in its application to the processes of problem solving. He was an important channel of communication of the European ideas of problem solving to the American psychologists who were questioning behaviorism. The early instigators of the cognitive revolution came to understand clearly how Duncker's experiments and concepts fit their theories; they would have found it much harder to generate the theories from his formulation of them.

We have seen Karl Duncker standing on shoulders, and we have seen others standing on his shoulders. At each level as we ascend the pyramid, both our factual knowledge and our understanding become richer. Nor is there just a single pyramid, but rather many parallel paths, and a considerable redundancy of rediscovery and reinvention. It would be a pity if it were otherwise, if everything were neatness and efficiency. For then far fewer of us would have the opportunity to enjoy the adventure of lives in science, the opportunity to contribute to the building of its fantastic structure.

Notes

1. Previously published in: *From Past to Future, Vol. 1*(2), *The Drama of Karl Duncker*, pp. 1–11. ©1999 Frances L. Hiatt School of Psychology, Clark University. Author: Herbert A. Simon, late of Department of Psychology, Carnegie Mellon University, Pittsburgh, PA.
2. I will use this term throughout as a convenient and conventional label for the changes in American cognitive psychology that began in the two decades following World War II. The revolution is sometimes dated from the Dartmouth Conference of 1956, sometimes from the publication of Neisser's textbook, titled *Cognitive Psychology* (1967). I adopt the earlier date as corresponding more closely with the critical initial events.
3. However, J.B. Watson himself accepted thinking aloud as a legitimate source of data, contrasting it with introspection. "The present writer has often felt that a good deal more can be learned about the psychology of thinking by making subjects think aloud about definite problems than by trusting to the unscientific method of introspection" (Watson, 1919, p. 91).
4. Notice that we refer to Selz as a "Gestalt" theorist, indicating how broadly that term was then used by American psychologists to embrace virtually all German non-behaviorist psychological theory, including the Gestalt psychology (in the narrower sense) of Berlin, the Denkpsychologie of Würzburg and its offshoots, and a number of other schools. Selz himself drew a clear distinction between his own theories and those of the Gestaltists, whom he referred to as "the Berliners." The Würzburgers, by and large, were reductionists who sought explanations in the form of mechanisms and thought of themselves as biologists; the Berliners were much more holistic in their outlook and often anti-mechanistic in their language.
5. Here, we must interpret "Gestaltists" in its narrower historical sense: essentially the group around Koffka, Köhler, and Wertheimer, for, as we shall see, this assertion surely does not apply to Selz.
6. Woodworth begins his Chapter 29 (wryly?) with the statement: "Two chapters will not be too many for the large topic of thinking . . . "
7. Köhler's books were on the reading list for my single psychology course at the University of Chicago in 1935, but I cannot reconstruct now how much of their content I assimilated.
8. I take this account from Seebohm (1970, pp. 271-272). The translation of the quotation from Bühler, like the earlier one of Selz's letter to Bahme, is mine.

References

Ach, N. (1905). *Über die Willenstätigkeit und das Denken*. Göttingen.

Bühler, K. (1907/1908). Tatsachen und problemen zu einer psychologie der denkvorgänge. *Archiv für die Gesamte Psychologie, 9*:297-365; 12:1-122.

De Groot, A. (1946). *Het Dencken van den Schaker*. Amsterdam: Noord-Hollandsche Uitgevers Maatschappij.
De Groot, A. (1965). *Thought and choice in chess*. The Hague: Mouton
Duncker, K. (1935). *Zur Psychologie des productiven Denkens*. Berlin: Springer.
Duncker, K. (1945). On problem solving. *Psychological Monographs* 58 (Whole No. 270). Washington, DC: American Psychological Association.
Frijda, N. H. and de Groot A. (Eds.) (1981). *Otto Selz: His contribution to psychology*. The Hague: Mouton Publisher.
Humphrey, G. (1951). *Thinking.* London: Methuen.
Katona, G. (1940). *Organizing and memorizing*. New York, NY: Columbia University Press.
Maier, N.R.F. (1930). Reasoning in humans I: On direction. *Journal of Comparative Psychology 10*:15-143.
Maier, N.R.F. (1931). Reasoning in humans II: The solution of a problem and its appearance in consciousness. *Journal of Comparative Psychology, 12*:181-194
Newell, A. (1981). Duncker on thinking: An inquiry into progress in cognition. In S. Koch and D. Leary (Eds.) *A Century of Psychology as Science: Retospections and Assessments*. New York, NY: McGraw-Hill.
Newell, A., Shaw, J.C. and Simon, H.A. (1958). Elements of a theory of human problem solving. *Psychological Review 65*:151-166.
Newell, A. and Simon, H.A. (1972). *Human Problem Solving*. Englewood Cliffs, NJ: Prentice-Hall.
Seebohm, H.-B. (1970). *Otto Selz: Ein Beitrag zur Geschichte der Psychologie*. Inauguraldissertation, Universität Heidelberg. Mannheim: Published by the author.
Selz, O. (1913). *Über der Gesetze des geordneten Denkverlaufs* Erster Teil*: Eine experimentelle Untersuchung*. Stuttgart: Speman.
Selz, O. (1922). *Über der Gesetze des geordneten Denkverlaufs* Zweiter Teil: *Zur Psychologie des productiven Denkens und des Irrtums*. Bonn: Cohen.
Simon, H.A. (1955). A behavioral theory of rational choice. *Quarterly Journal of Economics 69*: 99-118.
Simon, H.A. (1956). Rational choice and the structure of the environment. *Psychological Review 63*:129-138.
Simon, H.A. (1981). Otto Selz and information processing psychology. In N. H. Frijda, and A. de Groot (Eds.), *Otto Selz: His contribution to psychology*. The Hague: Mouton Publisher.
Watson, J.B. (1919). *Psychology from the standpoint of a behaviorist*. Philadelphia, PA: Lippincott.
Watt, H.J. (1905). Experimentelle Beiträge zu einer Theorie des Denkens. *Archiv für die Gesamte Psychologie, 4*:289-436.
Wertheimer, M. (1945). *Productive Thinking*. New York, NY: Harpers
Woodworth, R.S. (1938). *Experimental Psychology*. New York, NY: Henry Holt.

2

Life as the Problem: Karl Duncker's Context

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Among the people who left their mark on cognitive psychology, Karl Duncker's intellectual legacy is certainly outstanding (Newell, 1985; Zimmer, 1989). Some of the tasks on problem solving that he designed as part of his M.A. thesis when he was a twenty-three-year-old graduate student at Clark University in 1926 are still discussed now—more than seventy years later—in basic psychology textbooks. Most students of introductory psychology are probably quite familiar with the "candle problem," or the "radiation problem," but probably most of them (and most of us) only know very little about the background of the extraordinarily promising scholar, Karl Duncker, whose life took such a tragic course and ended in suicide when he was only thirty-seven years old. In this paper I will focus on the context in which Karl Duncker's life and work were situated, ranging from the context of Berlin in the 1920s and 1930s, to Clark University in 1925-1926, as well as his depression and forced emigration from Germany.

Karl Duncker's Life

Karl Duncker was born on February 2, 1903, the son of Hermann and Käthe Duncker, in Leipzig. Both his parents were active Marxists, a fact that would become central to Karl Duncker's life later on. From 1923 to 1928, Duncker was a student at the Friedrich-Wilhelms-University in Berlin, where he worked with Wolfgang Köhler and Max Wertheimer, among others. When Köhler was appointed to spend a year as visiting professor at Clark University in Worcester, Massachusetts, in 1925-1926, he selected Karl Duncker to join him there and Duncker was awarded a Clark University Fellowship. Duncker received an M.A. from Clark in 1926 with his thesis on "An Experimental and Theoretical Study of Productive Thinking (Solving of Comprehensible Problems)," which

was published the same year under a slightly modified title in *Pedagogical Seminary* (Duncker, 1926).

In spring 1927, Köhler selected his "best student" (Wendelborn, 1996) to temporarily replace his University Assistant Kurt Gottschaldt. Both Köhler and Wertheimer were very impressed with Duncker's exceptional abilities, as can be gathered from an undated letter of recommendation in which Wertheimer described Karl Duncker as his and Köhler's favorite student, and one of the best younger psychologists (Wertheimer, n.d., cited in King, Cox and Wertheimer, 1998). Köhler predicted a splendid university career for him (Wendelborn, 1996), and a former fellow student of his recalled that he was undoubtedly the brightest and most versatile of the students (Metzger, 1976).

After completing his dissertation on induced motion in 1929, Duncker became University Assistant in 1930, a position that he held until he was dismissed from the university for political reasons in 1935.

In 1933, the year of the "Machtergreifung" of the National Socialists, Duncker had applied to the university for his "Habilitation." In Germany, the Habilitation is a substantial thesis that is required to apply for professorship and to be allowed to teach at a university, after having completed a doctoral dissertation. Duncker's application for his habilitation thesis on problem solving [Zur Psychologie des Denkens beim Lösen von Problemen] was not accepted. A look into his personal file reveals that the reason for this decision was Duncker's communist connections, and the fact that he had been married to a Jewish woman, although, by that time, he had already gotten a divorce (Wendelborn, 1996). A re-application for his habilitation in 1935 was rejected as well, making it impossible for him to move up on the ladder of academia in Germany. Furthermore, by September 1935, his contract as an assistant was terminated.

However, during that year, building on his Master's thesis, Duncker published his seminal book *Zur Psychologie des produktiven Denkens* [Psychology of productive thinking] (Duncker, 1935a), which later would be listed among one of the "key events in one hundred years of the study of cognition" (Newell, 1985, p. 394). The same year, he published an article on learning and insight in the service of goal attainment [Lernen und Einsicht im Dienst der Zielerreichung] (Duncker, 1935b).

After being expelled from the university, Duncker left for England, and started doing work on pain with Sir Frederick Bartlett in Cambridge in 1936. Duncker's mental health had been bad, probably for at least a decade (King et al., 1998) and was deteriorating, and by 1937, Duncker was treated for endogenous depression by the psychiatrist Ludwig Binswanger in Kreutzlingen, Switzerland, where he stayed for two months (King et al., 1998). Wertheimer and Köhler, who both had academic appointments in the U.S. by that time, were very concerned about Duncker's state, and tried to find work for him. In 1938, Köhler arranged for Duncker to follow him to Swarthmore College, where he

himself had been a professor from 1935 onward. Duncker spent two years as instructor at Swarthmore. While there, he published an article together with Isadore Krechevsky "On solution-achievement" (Duncker and Krechevsky, 1939), as well as a paper on taste perception (Duncker, 1939a) and a paper on ethical relativity (Duncker, 1939b). Apparently, Duncker's mental health was getting worse, and following several "nervous breakdowns," the Köhlers tried to take care of Duncker, but alas, in vain (King et al., 1998). He took his own life shortly after his thirty-seventh birthday.

The Berlin Institute

What could it have been like for somebody like Karl Duncker to be a graduate student at the Berlin Institute in the 1920s and 1930s?

In order to better understand the circumstances of his life, consider the historical and political context of the time. After the painful defeat in World War I, the first democratic German republic, the "Weimar Republic" had been proclaimed by Social Democrat Philipp Scheidemann in November of 1918. The war had left the country with high debts, an extremely high inflation, unemployment and a shortage of food and goods. Because of the dissatisfaction with the concessions made in the Versailles treaty, the government stood on shaky grounds, with upheavals from both rightist and leftist extremists threatening the young democracy. The fact that the government had changed twenty-one times by the time Hitler came into power in 1933 serves as a good indicator of how unstable the general political situation was.

Things promised to get better with the introduction a new currency in 1923, and indeed, the economic situation consolidated and the living conditions for the majority of the German population improved considerably thereafter. The "Golden Twenties" swept the country, with technical and cultural innovations, such as mass production of cars and "Rundfunk für alle" [radio for everybody]. A new form of mass entertainment was born with the rise of the film industry, and movies became popular all over the country, and especially in the city, Berlin (Kracauer, 1995). By the end of the decade, the standard of living had reached its level from before the war (Thieme, 1994).

In the mid-1920s, according to Gay (1969), Berlin was the center of cultural, technological, and scientific life not only of Germany, but of Europe.

> To go to Berlin was the aspiration of the composer, the journalist, the actor: with its superb orchestras, its 120 newspapers, its forty theaters, Berlin was the place for the ambitious, the energetic, the talented (Gay, 1969, p. 83).

Among "the talented," to name only a few, were Berthold Brecht, Erich Kästner, Alfred Döblin, and Robert Musil (who had in fact received a Ph.D. from the Berlin Psychology Institute) in literature, and Max Pechstein, as well as Max Liebermann in the visual arts. The Dada movement had its first international fair in Berlin in 1920, exhibiting works by Otto Dix, Max Ernst,

George Grosz, and others (Ferrier, 1988). In addition, many distinguished scientists and Nobel price laureates lived in Berlin, and Wolfgang Köhler himself maintained friendly relationships with the physicists Max Planck and Otto Hahn (Jaeger, 1992; Ash, 1995), as did Max Wertheimer with Albert Einstein (King and Wertheimer, 1995).

Gay (1969) captures the cultural atmosphere of Germany during the time of the Weimar Republic in the following way:

> The excitement that characterized Weimar culture stemmed in part from exuberant creativity and experimentation; but much of it was anxiety, fear, a rising sense of doom. … [I]t was a precarious glory, a dance on the edge of a volcano. Weimar culture was the creation of outsiders, propelled by history into the inside, for a short, dizzying, fragile moment (Gay, 1969, p. 12).

Stumpf's Institute

During the Weimar period, the Berlin Institute was certainly the most influential psychology institution in Germany (Ash, 1985a). Carl Stumpf (1848-1936) had been appointed as chair of psychology within the three chairs of philosophy of the Friedrich-Wilhelms-University in Berlin in 1893. He had studied philosophy with Franz Bretano (1838-1917) and with Hermann Lotze (1817-1881), and had also attended lectures in physiology with Georg Meißner (1829-1905), and physics with Wilhelm Weber (1904-1891) (Sprung and Sprung, 1995).

At the time, working in psychology with a strong background in philosophy was not only common, but in fact mandatory. Although the foundation of Wilhelm Wundt's laboratory in Leipzig in 1879 is often mentioned in the context of the emergence of psychology as an empirical, "new" science, psychology as a discipline was by no means independent from other disciplines. On the contrary, at most German universities, psychology was approached as providing an innovative methodological tool for solving the problems of one more traditional science, namely philosophy (Ash, 1985a; Geuter, 1984a).

For instance, Carl Stumpf in Berlin, as well as Wundt in Leipzig, hoped to find answers to questions of epistemology by using experimental psychological methods (Ash, 1984b). Thus, in contrast to the U.S. where independent psychology departments already existed before World War I., in Germany, psychology was more of a sub-discipline, or "Propädeutik" (Ash, 1985a) to philosophy. In fact, the competition between philosophy and psychology was considerable, and professors of philosophy feared that experimental methods would contaminate the pursuit of "pure" philosophy, or, at the least, not contribute to any new insights. The conflict between members of the two science reached its peak in 1912 in a declaration of more than two thirds of the academics in philosophy who protested against hiring any more psychologists within any of