THE ESSENTIAL BRUNSWIK

Beginnings, Explications, Applications

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Ideas in Exile: The Struggles of an Upright Man

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44

Ideas in Exile: The Struggles of an Upright Man

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The sparkling intellectual atmosphere of early-twentieth-century Vienna produced Ludwig Wittgenstein, Karl Popper, Otto Neurath, and Kurt Gödel—in addition to a string of other great thinkers. Among them was Karl Bühler, who, when he founded the Vienna Psychological Institute in 1922, was one of the foremost psychologists in the world. Egon Brunswik began to study psychology in Vienna in 1923 and soon became an active participant in Bühler's famous Wednesday-evening discussion group; Thursdays he went to Moritz Schlick's Thursday-evening discussion group (Leary, 1987). Schlick was the founder and leading member of the European school of positivist philosophers known as the Vienna Circle. In 1927, Brunswik submitted his doctoral thesis to Bühler and Schlick, the same two advisers to whom Karl Popper submitted his thesis a year later.

The intellectual tension between Wednesday and Thursday evenings was vibrant. The logical positivist doctrine of the Vienna Circle posited that the relationship between scientific language and its sense-data referents should and could be unambiguous. Bühler, in contrast, had shown that the relationship between perceptual cues and their objects, as well as between words and their objects, was irreducibly ambiguous. Brunswik sided with Bühler. He did try, though, to resolve the tension by adopting the position of Hans Reichenbach, the leader of the Berlin school of logical positivism, who argued that all knowledge is probabilistic.

Influenced by Bühler's biologically motivated concern with the success of organisms in their world, Brunswik's research in the 1920s and 1930s aimed at studying "perceptual achievement" in the presence of ambiguous cues. The three traditional perceptual constancies—size, shape, and color—were the prototype for achievement, that is, how accurate perception is when aspects of the environment change. Brunswik extended the question of how well an organism infers size, shape, and color under varying context variables (such as illumination) to the more general problem of studying the invariance of the perception of one characteristic of an object when the others vary. For instance, he studied how the perceived size of coins changed when their value and the number of coins were varied—coins higher in value appeared to be larger in size and greater in number than those of lesser value (Brunswik, 1934b, p. 147). In Brunswik's terms, what we see are *perceptual compromises* that he attributed to the learning of cues from experience (e.g., coins of higher value actually do tend to be larger in size). He manipulated up to four variables simultaneously in factorial designs (he had not yet developed the idea of representative design) and measured how the perception of each variable depended on the values of the others. This Vienna program of "multidimensional psychophysics" measured the context-dependency of judgment (for an introduction, see Gigerenzer & Murray, 1987, pp. 61–81). In contrast, its independence from context was assumed in the one-dimensional psychophysics associated with G. T. Fechner and S. S. Stevens, in which one studied a variable in isolation (such as perceived size), held everything else constant, and then compared the perceived size to the actual size to obtain the psychophysical function.

In the early 1930s, Brunswik was far ahead of mainstream psychophysics in the study of context dependency. This is not to say that there was no room for theoretical development in his multidimensional psychophysics; for instance, Brunswik treated his two explanatory concepts, perceptual compromises and cue learning, as equivalent whereas these actually are different and can lead to contradictory predictions about the effect of context (Gigerenzer & Murray, 1987, pp. 70–74).

However, his Vienna program had virtually no impact on the future of psychophysics, except for a few scattered studies.

One reason for this lack of influence was that the Vienna Psychological Institute's program was destroyed soon after Brunswik had accepted a position at Berkeley in 1937. In early 1938, the Nazis entered Vienna and arrested and dismissed Bühler because of his political views, which were considered dangerous to the "peace and public order of the (Philosophical) Faculty" (Ash, 1987, p. 157). Eventually, he fled to the United States, but no one offered the once-celebrated Karl Bühler an adequate position; his brilliant career crumbled in exile. Schlick had died a few years earlier from gunshot wounds inflicted by a deranged student, and the political pressure of fascism caused the Vienna Circle to disband, with many of its members fleeing to the United States. Brunswik had to start practically from scratch at Berkeley.

Brunswik in the Plural

Unlike the Vienna program, Brunswik's Berkeley program—probabilistic functionalism—is well known. It is so well known, in fact, that there is not just one Brunswik, but several. One is the Brunswik absorbed by contemporary psychology: he-was-one-of-us. These good-natured colleagues spell his name *Brunswick*, confuse his term *ecological validity* with generalizability from the laboratory to the environment, and his term *representative design* with the representativeness heuristic. In their friendly embrace, Brunswik comes out a forerunner and guardian of today's status quo. No conflict surfaces; all is quiet; nothing must be questioned.

There is a more sophisticated image, in which Brunswik's ideas basically boil down to three correlations and one unorthodoxy. The correlations are functional validities, ecological validities, and cue utilization coefficients, and the heresy is representative design—the frightening idea of sacrificing experimental control and, possibly even worse, of leaving one's laboratory to study people in their real-world environments. Correlations are fine; the unorthodoxy is repugnant. This view gets some work done, but it cuts right through the middle of Brunswik's intellectual heart.

There is a third view of Brunswik: opposition by neglect. This is not an active opposition against an intellectual enemy; Brunswik does not seem to have notable intellectual enemies, unlike many other scholars. The opposition takes the form of silence and a lack of understanding of what the fuss is all about. For instance, in his *Sensation and Perception in the History of Experimental Psychology* (1942), Edwin G. Boring, the dean of the history of psychology and an arch-determinist, covered Brunswik's work in Vienna, which had encompassed experimental control in multidimensional designs. But after Brunswik had fleshed out his probabilistic functionalism and representative design, he was not even mentioned in Boring's *A History of Experimental Psychology* (1957) and *History, Psychology, and Science* (1963). As Ken Hammond (1980, p. 9) reported, Boring's verdict was "Brunswik was a brilliant man who wasted his life." Informed neglect can be as toxic to new ideas as an uninformed embrace.

In the following, I describe what I think of when I think of Brunswik. I do not think of correlations; I think of the struggles of an upright man.

Intellectual Integrity

What impresses me deeply is Brunswik's uncompromising intellectual sincerity: the courage to think through the consequences of one's ideas carefully, and to speak out in public even when the scientific community does not want to listen and makes one pay a price for maintaining these standards. And Brunswik did pay dearly. Brunswik's personal struggle was, in my view, about maintaining his *intellectual integrity* in a scientific community in which his ideas fell on hostile ground held by ignorant troops. Great thinkers often learn, to their surprise, that new ideas are less than welcome.

What were these new ideas that inspired so much hostility? Brunswik's probabilistic functionalism can be summarized in the following concepts: achievement, ambiguity of cues, vicarious functioning, and representative design. That is, an organism needs to make inferences about its environment to adjust, survive, and reproduce (achievement); the proximal cues available to it to make these inferences about its environment are uncertain (ambiguity); the organism processes

ambiguous cues by substituting or combining them (vicarious functioning); in order to study achievement and vicarious functioning, researchers need to use representative designs. This is Brunswik's linking of biological purpose, environment, cognitive process, and research methodology.

The hostile ground itself was a minefield of dogmas: determinism, the Columbia Bible, and Fisher's experimental design. Determinism was a fading, but still strong, dogma, and the other two were newly emerging dogmas.

Refinancing Determinism

In their struggle to get psychology recognized as a science, many of Brunswik's fellow psychologists in America maintained an old-fashioned ideal no longer characteristic of modern science, from evolutionary biology to quantum physics. This ideal demanded *certain* knowledge and *universal* laws, as Newtonian mechanics had purported to deliver. As an example of this longing for certainty, Edwin Boring declared as late as 1963 that "determinism reigns" (p. 14).

The two debates of Brunswik's program, which were published in the *Psychological Review* in 1943 and 1955, illustrated the way Brunswik's probabilism collided with the leading experimental psychologists' belief in determinism (see Gigerenzer, 1987). Probabilism was interpreted as a confession of failure. For instance, Clark Hull (1943b) declared, in the first debate in Chicago, that he and Kurt Lewin believed in uniform laws of behavior that correspond to correlations of 1.00. Because the effort to isolate deterministic laws is laborious and time-consuming, "All of us may as well give it up, as Brunswik seems already to have done" (p. 204).

Twelve years later, in the second debate in Berkeley, David Krech (1955) confronted Brunswik with his personal confession of faith: "I have always made it a cardinal principle to live beyond my income. And although I have yet to find a one-to-one correlation in psychology . . . I am always ready to make another promissory note and promise that if you bear with us we *will* find uniform laws. . . . And if I can't pay off on my first promissory note I will come seeking refinancing. . . . I have faith that despite our repeated and inglorious failures we will someday come to a theory which is able to give a consistent and complete description of reality. But in the meantime, I repeat, you must bear with us" (p. 230).

Refinancing went on for some time. The fixation on uniform laws of behavior was one of the reasons why many of the commentators did not understand the nature of Brunswik's probabilism—which was located neither in the environment, as Krech (1955) and Hilgard (1955) interpreted Brunswik, nor in the organism, as Hull seemed to do, but rather in the relationship *between* the organism and the environment.

The dogma of determinism did not survive Brunswik very long, but the next two methodological faiths did. They are still entrenched in the minds of most experimental psychologists—and in their hearts, because these methodologies have been taught as if they were moral principles.

The Columbia Bible

The Henry Holt publishing company advertised in 1938, "THE BIBLE IS OUT." Robert S. Woodworth had finally published his long-awaited *Experimental Psychology*. This textbook, which was popularly known as the "Columbia Bible," narrowed the many existing practices of experimentation (see Danziger, 1990) to one and only one legitimate form: Vary an independent variable (or a few), hold all the conditions constant, and observe the effect on the dependent variable. In Brunswik's copy of the Columbia Bible (which Ken Hammond so kindly lent me), on page 2, the passage "all the conditions constant, except for one" is underlined twice, and Brunswik's pencil notation "imposs[ible]!" is in the margin. It is not without irony that Brunswik taught courses for years using Woodworth's textbook, as the notes in his copy indicate. An estimated 100,000 North American psychology majors and graduate students learned what experimental research is from the Woodworth bible and its revised edition (Woodworth & Schlosberg, 1954). The book was translated into many languages and widely used around the world (Evans, 1990). It was enormously successful; many psychologists can no longer envision more than one experimental method in science.

In this book, Woodworth excluded correlation methods and individual differences from the domain of experimental psychology. The bible separated the murky waters of correlation, which ob-

scure the causes of behavior, from the bright sun of experimentation, where cause and effect can be distinguished clearly. The result was a strange institutional partition into "two disciplines of scientific psychology" (Cronbach, 1957), the "Tiny Little Island" of experimental psychology and the "Holy Roman Empire" of correlational psychology.

Brunswik's probabilistic functionalism fitted into neither of these disciplines. His intellectual vision was one of coherence between theory and methodology: to start with the purpose or function (achievement in natural environments) and a subject matter (vicarious functioning of perception and judgment), and to choose a matching methodology (representative design).

There is no such intellectual vision behind the creation of the two "scientific disciplines." Each was, and still is, a historically arbitrary collection of purpose, subject matter, and method that have no necessary logical or psychological affinity with each other (Gigerenzer, 1987). For instance, there is no psychological reason why the study of intelligence is linked with individual differences and correlations, while the study of thinking is linked to general laws and experiments. Nor is there a reason why one group should rarely read or cite the other group's work. Like most ordinary humans who bond with their peers, psychologists in one camp looked down on their colleagues in the other camp, declaring their adversaries' methods inferior and their purpose of little scientific interest and public value. The correlation between psychologists' esteem for their colleagues in one camp and their colleagues in the other camp was –.80 (Thorndike, 1954)—alas, a substantial, but not perfect, correlation.

Brunswik found himself and his ideas exiled from his discipline. Ernest Hilgard (1955), an eminent experimental psychologist, put his lack of regard for Brunswik's methods in no uncertain terms: "Correlation is an instrument of the devil" (p. 228). But methods per se are neither good nor bad; the question is whether they match a theory or not. Brunswik's intellectual integrity demanded that he think for himself, deciding what the proper method was, rather than just climbing on the bandwagon. The tragedy is that he found himself in a no-man's land between the two newly established disciplines.

Fisher's Straightjacket

B. F. Skinner once told me that he had thought of dedicating one of his books to "the statisticians and scientific methodologists with whose help this book would have never been completed." He had second thoughts and, in fact, dedicated the book to those who actually were helpful, "to the pigeon staff." Skinner had had those statisticians in mind who imposed Sir Ronald Fisher's doctrine that the design of an experiment must match the statistical method, such as analysis of variance.

Fisher's randomized control group experiments were tailor-made to Woodworth's ideal of experimentation, and analysis of variance allowed one to study more than one independent variable. Skinner's resistance arose when researchers started to use Fisher's method compulsively rather than in a thoughtful way, that is, as a tool, which is—like all tools—useful only in specific situations. Editors began to make what they believed was good scientific method a sine qua non for publication: factorial designs, large numbers of subjects, and small p values.

Statistical thinking was replaced with a mindless ritual performed in the presence of any set of data (Gigerenzer, 1993). Skinner confessed to me that he once tried a factorial design with some two dozen animals. But only once. He lost experimental control because he could not keep so many animals at the same level of deprivation, and the magnitude of error in his data increased. Why increase error just to have a method that measures error?

The Skinnerians escaped the emerging pressure of editors to publish studies with large numbers of animals by founding a new journal in 1958, the *Journal of the Experimental Analysis of Behavior*. This was not an isolated case. One of the reasons for launching the *Journal of Mathematical Psychology* in 1964 was also to escape the editors' pressure to perform institutionalized null hypothesis testing (Luce, 1989). Somewhat perversely, the best mathematical psychologists were told by their statistically less sophisticated colleagues that they should carry out (as Duncan Luce, 1988, put it) "mindless hypothesis testing in lieu of doing good research: measuring effects, constructing substantive theories of some depth, and developing probability models and statistical procedures suited to these theories" (p. 582).

Brunswik, however, had no following with which he could found his own journal. Like Skinner, he remarked drolly that "our ignorance of Fisher's work on factorial design and its mathematical evaluation . . . paid off" (1956b, p. 102). As almost all great psychologists did, he analyzed individuals rather than comparing group means, and he continued to employ his own nonfactorial representative designs. But he also sometimes felt that he should make concessions, for instance, when he performed "a routine analysis of variance for the factorially orthodox part of our experiment" (1956b, p. 106).

In Brunswik's struggle with Fisher's ideas, unlike Skinner's and Luce's, a classic controversy repeated itself. Karl Pearson, who, with Francis Galton, had founded correlation methods, was involved in a terrible intellectual and personal feud with Fisher. This fight between these towering statisticians repeated itself in psychology between the proponents of their respective tools. Just at the time when Brunswik adopted Pearson's correlation methods around 1940, Fisherian methods began to spread. By 1955, when Brunswik died, Fisherian methods had overrun, conquered, and redefined every branch of experimental psychology (Gigerenzer, 1993).

Then the newly institutionalized tools evolved into new theories of mind. When Brunswik's vision of the mind as an intuitive statistician finally became a great success in experimental psychology, the mind's intuitive statistician was not of the Karl Pearson school, as Brunswik had imagined. Rather, the homunculus statistician used the new laboratory tools, such as analysis of variance. For instance, according to Harold Kelley's (1967) causal attribution theory, the mind attributes a cause to an effect in the same way as researchers have come to do—by calculating an intuitive version of analysis of variance (Gigerenzer, 1991). Brunswik had never been able to persuade his colleagues from experimental psychology that the mind would use the techniques of the competing discipline of correlational psychology.

The Price of Intellectual Integrity

Woodworth's bible had excommunicated Brunswik from experimental psychology, and the institutionalization of Fisher's methods as the sine qua non of scientific method set Brunswik's ideas outside the realm of what was considered proper scientific method. Brunswik must have soon realized that the edifice he had erected had become, as Ken Hammond (1966b) expressed it succinctly, a significant landmark that "was virtually empty; there were visitors, it is true, but no one stayed" (p. v). Although Brunswik had chosen freely to leave Vienna for the United States, unlike the exiled Bühler, he found his ideas in exile. Unlike in Vienna, at Berkeley he seems not to have had a group of students who worked on his ideas, nor did his working atmosphere support the philosophical and interdisciplinary spirit that continued to enhance his writings. But there was no way back; the Vienna program and the Vienna Circle had been destroyed, and Brunswik himself had moved beyond multidimensional psychophysics. What is one to do if one has lost the old companions and failed to enlist new ones? The obvious easy choice would have been to conform to the new Zeitgeist, but the option of surrendering his ideas seems never to have occurred to Brunswik. It is easy to be true to one's ideas if everyone applauds—I admire Brunswik's intellectual integrity because, in his case, only very, very few applauded. Standing upright must have been difficult, lonely, and depressing.

Do the Ideas Matter?

American psychology would hardly remember Brunswik's ideas had not one of his students, Ken Hammond, kept his memory alive for over half a century. But is the memory of Egon Brunswik of more than historical interest? Are his ideas still exiled, and if so, does it matter?

Representative Sampling

Brunswik (1956b) sadly reported that his success in persuading fellow researchers to shift to representative sampling of stimuli was "very slow going and hard to maintain" (p. 39). He complained that his colleagues practiced "double standards" by being concerned with the sampling of subjects but not of stimulus objects. Representative sampling of stimuli is one aspect of the more general notion of representative design.

It would be an error to introduce representative sampling as a new dogma to replace current methodological dogmas. The point is to choose the appropriate sampling method for the problem

under discussion. For instance, representative sampling of objects from a class is indispensable if one wants to make general statements about the degree of "achievement," or its flip side, the fallacies of perception and judgment concerning this class of objects. But if the purpose is testing competing models of cognitive strategies and flat maxima obscure the discriminability of strategies, then using selected stimuli that discriminate between the strategies may be the only choice (see Rieskamp & Hoffrage, 1999).

Is the idea of representative sampling of any relevance to present-day research? Imagine Brunswik browsing through recent textbooks on cognitive psychology and looking for what we have discovered about achievement in judgment—now more fashionably labeled *fallacies* and *cognitive illusions*. It would catch his eye that the stimuli used in the demonstrations of fallacies were typically selected rather than representative, among them, the five letters in Tversky and Kahneman's (1973) study from which the availability heuristic was concluded; the personality sketches in Kahneman and Tversky's (1973) engineer-lawyer study from which base-rate neglect was concluded; and the general-knowledge questions from which the overconfidence bias was concluded (Lichtenstein, Fischhoff, & Phillips, 1982). Brunswik would have objected that if one wants to measure achievement or demonstrate fallacies in a reference class of objects, one needs to take a representative (or random) sample of these objects. If not, one can "demonstrate" almost any level of performance by selecting those objects where performance is at its worst (or at its best). In fact, when one uses representative (rather than selected) samples in these three studies, performance greatly improves: The errors in estimating the frequency of letters largely disappear (Sedlmeier, Hertwig, & Gigerenzer, 1998); the estimated probabilities that a person is an engineer approach Bayes's rule (Gigerenzer, Hell, & Blank, 1988); and the overconfidence bias completely disappears (Gigerenzer, Hoffrage, & Kleinbölting, 1991; Juslin, Winman & Olsson, 2000). These celebrated cognitive illusions, attributed to the subjects, are in part due to the selected sampling done by the experimenters.

These examples illustrate that representative sampling of stimuli is still a blind spot in some areas of research. In survey research, it would be a mistake to present the odd views of a few selected citizens as public opinion; that the same applies to stimulus objects is still not commonly acknowledged. Unreflectively selected samples can produce apparently general phenomena that occupy us for years and then finally dissolve into an issue of mere sampling.

Natural Sampling

Imagine Brunswik looking at the studies on Bayesian reasoning, which emerged about ten years after his death. When he learned that people neglect base rates, he might have been surprised because his rats did not (Brunswik,1939c). His rats were not perfect, but they were sensitive to the difference of the base rates of reinforcement in the two sides of a T-maze, and to the ratio as well. Sensitized by the frequentist Reichenbach, Brunswik's eye would have caught an essential difference between his study and the base rate studies of the 1970s and 1980s: His rats learned the base rates from actual experienced frequencies, whereas the humans in almost all studies that reported base rate neglect could not; they were presented summary information in terms of probabilities or percentages. Rats would not understand probabilities, and humans have only recently in their evolution begun to struggle with this representation of uncertainty. Does representation matter? Christensen-Szalanski and Beach (1982) presented base rates in terms of actual frequencies, sequentially encountered, and reported that base rate neglect largely disappeared. This process of sampling instances from a population sequentially is known as *natural sampling*. Natural sampling is the everyday equivalent—for rats and humans alike—of the representative sampling done by scientific experimenters. When observed frequencies are based on natural sampling—that is, on raw (rather than normalized) counts of events made in an ecological (rather than experimental) setting—then one can show that Bayesian computations become simpler than with probabilities, and that people have more insight into Bayesian problems (Gigerenzer & Hoffrage, 1995).

Structure of Environments

A most important insight I gained from Brunswik's writings is the relevance of the structure of information in environments to the study of

judgment. Brunswik tentatively proposed measuring environmental structure by ecological validities, and measuring these in turn by correlation coefficients. Brunswik, though, almost as much as Skinner, hesitated to look into the black box, and so he failed to see the important connection between the structure of environments and that of mediation. Adaptive mental strategies can exploit certain structures. For instance, if there is a match between the structure of the environment and that of a strategy, a simple heuristic that processes much less information than multiple regression can nevertheless make as many (or more) accurate inferences about its environment (Martignon & Hoffrage, 1999). Herbert Simon had emphasized the link between cognitive processes and environmental structure in his famous 1956 *Psychological Review* article on bounded rationality. However, in recent years, bounded rationality has been reduced to cognitive limitations, and the structure of environments has been largely forgotten as an indispensable part of understanding bounded rationality, sometimes even by Simon himself (e.g., 1987). The study of the structure of environments is still in its infancy.

Much of psychology after the cognitive revolution is about what is in our heads: Which logic does human reasoning embody? How many primary emotions should we distinguish? It is little concerned with what cognition, emotion, and behavior are for, and with how they relate to the structure of environments, both physical and social. Brunswik's focus on achievement, in contrast, is functional, focusing on the accuracy of perception and judgment. Accuracy is not the only goal; to be able to act quickly, to come in first, or to establish social relations of trust and cooperation also exemplifies achievement in a broader sense.

The structure of environments is essential for understanding cognition and behavior in terms of adaptation, because adaptations are relative to (past) environments. To flesh out the Darwinian aspect of Brunswikian psychology, one needs to distinguish between past and present environments, between ecological validities in past and in present environments, and between social environments composed of conspecifics (where cues are actually signals) and other environments (e.g., physical environments in which humans do not cooperate or bargain with their inhabitants). For instance, smooth skin in female humans may have been a highly valid cue for reproductive capability during most of human evolution, signaling good health (Buss, 1987). In current environments with abundant medical technology, the ecological validity of smooth skin may have decreased to almost nil, but men's proximal mechanisms, cognitive and emotional, may still rely on such cues. A Darwinian psychology is a historical psychology, one that looks into the past to learn about the present (e.g., Cosmides & Tooby, 1992).

Brunswik repeatedly alluded to Darwin, and the notions of function, achievement, and environmental structure all relate to evolution by natural selection. Brunswik, however, never developed or carried these ideas any further. Neo-Brunswikians have done little to develop the Darwinian fragment, consistent with the prevailing anxiety about evolution in the American psychology establishment. Given that even Pope John Paul II finally announced in the *Quarterly Review of Biology* (1997) that evolution (of the body, not of the spirit) is a plausible hypothesis, more psychologists might find the courage to think about what we can learn from modern evolutionary theory—even if some of them still consider such thoughts politically incorrect.

Models of Vicarious Functioning

Schlick's Thursday-evening discussion groups seem to have had a lasting effect on Brunswik. The methodological objectivity of the Vienna Circle helped Brunswik to focus his work on the measurement of objective achievement rather than on cognitive processes ("mediation"). He hesitated to speak about the unobservable process of mediation and, even still in 1937, declared that psychology is a science of "what" rather than of "how." The question of how mediation works should be studied only insofar as it throws light upon the question of what an organism achieves. Only later did Brunswik (e.g., 1957) grant a place, though only a second place, to the study of cognitive processes.

Given his reluctance to open the black box, I am not sure how Brunswik would look at the process models of vicarious functions that were inspired by his ideas: multiple regression models, on the one hand (e.g., Hammond, Hursch, & Todd, 1964), and the theory of probabilistic mental models (PMM theory) and the fast and frugal

lens model, on the other (Gigerenzer et al., 1991). When Brunswik coined the metaphor of the "intuitive statistician," he tentatively suggested that the process of vicarious functioning might be like multiple regression (Doherty & Kurz, 1996). Brunswik's measurement tool turned into a theory of cognitive processes. In the neo-Brunswikian revival, multiple regression became *the* model of vicarious functioning, and, unfortunately, it remains so. Ken Hammond, like Brunswik, has had second thoughts, but by and large, the tool has become part of the message. It structures our thinking about Brunswik.

Brunswik's reluctance to think about processes may explain why his examples for vicarious functioning vacillated back and forth between two different processes, substitution and combination. Some of his examples (e.g., Hull's habit family hierarchy and the psychoanalytic substitution mechanism in which one cause can manifest itself as various symptoms) referred to substitution without combination, others to the combination of cues. The fast and frugal lens model, based on PMM theory, assumes substitution without combination, emphasizing that judgments need to be made quickly and on the basis of limited knowledge (see Gigerenzer & Kurz, this volume). Here, Egon Brunswik meets Herbert Simon, creating models of bounded rationality in which simple cognitive heuristics exploit environmental structures.

A Love of History, Philosophy, and Methodology

Just as the human species has a history, so do our theories and methods. Not knowing where they come from can blind one to understanding why one propounds a particular theory or uses a specific method. Nevertheless, looking down at history is symptomatic of much of current psychology. Brunswik had written about the history of his field and had published in philosophical journals; possibly, it is just that background that helped him to see that there are differences between methodologies, and that one actually needs to make informed choices. Many psychologists do not seem to make these choices; rather, they take on the methodological practice of their field and then defend it as if it were religious dogma. If one reads Brunswik, one finds a constant stream of thought about methodology, from preferring matching tasks over numerical response tasks in order to minimize the confounding of perception with judgment, to the larger program of representative design. In contrast, the enthusiasm with which some methods have been mechanically applied as general-purpose tools—factor analysis, multidimensional scaling, and analysis of variance, among others—springs from ignorance of history, philosophy, and the methodologies of other scientific disciplines. Methodology is an art, not the science of compulsive hand washing.

This is not to say that every psychologist must be a master of history, but history can protect one against confusing present-day methodological conventions with the sine qua non of scientific research.

The Search for Objectivity in the Twilight of Uncertainty

John Locke (1690/1959) remarked that "God . . . has afforded us only with the twilight of probability; suitable, I presume, to that state of mediocrity and probationership he has been pleased to place us in here" (vol. 2, p. 360). Bühler's psychology opened the door for Brunswik to the twilight of uncertainty, and the Vienna Circle inspired him to search for objective knowledge behind that door. What Brunswik found there: that we know. What he was looking for is more: not answers, but the right questions. From him, one can learn to rethink that which is taken for granted. I have.

Yet there is another, deeper message in the work of Egon Brunswik: the value of the struggle for intellectual integrity—daring to think ideas through, with all the consequences, and remaining true to them even if they are condemned to exile. Kant's final two words in his lovely essay on the Enlightenment capture the essence of this struggle: *sapere aude*, that is, have the courage to know.