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Original Article

Causality vs generality: Judgment and decision making struggles to become a scientific discipline

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No doubt my long life as a student of judgment and decision making influences my choice of topics for this issue of the journal. I began my life as a scholar in 1945, immediately after WW2, when I returned to the University of California at Berkeley, to find Egon Brunswik a professor of psychology, undoubtedly as unpopular as ever as a teacher and a colleague, and as masterful and brilliant as ever as a scholar, whose depth and commitment were unmatched, yet unrecognized by few if any of his contemporaries except Edward Tolman, fortunately then the Chairman of the department. I say "fortunately", for if it were not for Tolman, the students, and regrettably, I must add, his colleagues would have forced him out of the department. He was just too much for them. Tolman wrote a letter to the President of the University stating in unmistakable terms (I have seen the letter) that if Brunswik were forced out, as colleagues and students demanded, he would resign immediately. The president held firm, Brunswik was retained, and the psychology building at the university is named the "Edward C. Tolman" building.

Although few knew about this episode (I learned about it 20 years later) everyone knew about Brunswik's demanding scholarship and unpopularity among the students, graduate as well as undergraduate. He was not unpopular because he was a rude or unpleasant person, however; his manners were impeccable. But when it came to scholarship, he simply was a perfectionist, and few chose to become his student. Even I could see the risks in his demand for perfection, and I did my dissertation with another faculty member. I could not risk becoming a perennial student. We communicated fairly often, but then, a few years later (1955), he committed suicide. That means that there will be no history of those

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* *Editorial note*: We were extremely delighted when Ken Hammond, founder of the Brunswik Society, the second President of the Society of Judgment and Decision Making, and founder of the Institute of Behavioral Science at the University of Colorado, accepted our personal invitation to contribute to this special issue. In fact, he was the first one who submitted a manuscript! In our response we said: "... Your focus is on Brunswik, specifically, on representative design. The focus of our Special Issue is on intuitive decision making, in particular in organizational settings. Obviously, there is a link, and we listed Brunswik's representative design as one of the bullet points on which authors could focus. Ken, we would be grateful if you could make a bit more effort to elaborate on this link. You started your piece with your personal story: You as a student of Brunswik who inspired you and who had a major influence on your career. Imagine we print your manuscript as it currently is and a young student, maybe the same age you had back then when you interacted with Brunswik, is approaching you with the following question: 'Ken, thanks a lot for your wonderful and inspiring essay. I am interested in intuition and the role it plays in organizational decision making. I have the feeling that what you wrote needs to be considered in my further studies of this issue, but could you please give me some guidelines, concrete ideas, or recommendations how to do this exactly?" What would you tell to this student? We think this would be a nice end of your story that would link the past (when you have been a young student) to the future (of those who are young today)."

Ken Hammond replied "Your request sent me to look for my recent article "Quasirationality Yes, Intuition No". I don't think I can do any better than this. Take a look at it. You might even like it!—Ken". Of course we liked it and we felt the ideas that he discussed in this paper (Hammond, 2010) would be an excellent fit to the present special issue. So we asked whether it would "be possible to somehow integrate the bottom line of this Quasirationality paper into your manuscript for our special issue? Maybe as a short section at the end?" His reply: "I'm working on this—Ken".

This was the last mail we received from him. Knowing that he had to go to the hospital a while before, but not knowing to what extent he recovered and how good his health actually was, we did not dare to send reminders. On April 28th, 2015, Ken Hammond passed away at a "biblical" age of 98 after a career spanning seven decades of theoretical and empirical contributions to the field of judgment and decision research (for obituaries, see http://www.fabbs.org/index.php?cID=165 and http://www.legacy.com/obituaries/dailycamera/obituary.aspx?pid=174763911). The present paper, for sure one among the last (if not *the* last) he has written, takes us back to the very beginning of his career – back to the 1940s at the University of California, Berkeley – but then covers six decades of discussion centering around the concept of representative design (Brunswik, 1956; Dhami, Hertwig, & Hoffrage, 2004; Hammond, 1954; Hammond & Wascoe, 1980) and its implications for psychological research.

We would like to thank Tom Stewart who helped us with editing this manuscript and with identifying some of the references and quotes Ken Hammond included.

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days written by Brunswik. So I have taken upon myself to present my view, and that is what leads us to the content of this article, and to my explanation of why psychology is in such trouble today.

Psychology began as an offshoot of philosophy that wanted more empirical evidence for the truth or falsity of the assertions of its spokespersons regarding the causal relations among its many interdependent variables. In order to seek that evidence, it found the practice of experiments that varied one variable while holding all others constant to be practicable and convincing. As soon as that practice became standard, psychology became a scientific discipline, and all its students learned how to do their work in that fashion. It remains at the core of all research based on experiments today. But many have recognized that its day is, or should be, over, and what follows explains why that is so and what needs to be done to maintain the scientific character of this profession, one that now seems to be in trouble because of the standard design of its experiments implicitly remains in favor of establishing causality over generality (all this is laid out in detail in Hammond & Stewart, 2001).

1. Psychophysics

Known to many as the most boring topic they ever encountered, psychophysics also enjoyed, if that is the right word, a reputation for being the most disappointing subject ever encountered. Students enthusiastically signed up for courses in psychology, anticipating intriguing material about sex and other forbidden topics, but found themselves listening to lectures about perceptual stimuli and auditory responses. Brunswik entered this situation about 1937 at Berkeley and was appointed Professor in 1938. That was because Edward Tolman, then Chairman of the Department, discovered Brunswik during a sabbatical year in Vienna. He much admired Brunswik, and they published a significant paper together (Tolman & Brunswik, 1935) that reflected Brunswik's general views in the *Psychological Review*.

There could hardly have been a worse time for this appointment. Brunswik was certainly ready to break away from psychophysics, and Tolman certainly was ready to accommodate that break, although it would have to be on Brunswik's terms, which means that it would have to be done on the basis of scholarship. But students were not ready for discussions of "distal–proximal" relationships and the like. As a result they were left wondering why they were listening to this obscure material that, worse still, was presented in Brunswik's heavily German-accented English. I recall peeking into a Brunswik lecture while wandering down a corridor and finding his wife, Else Frenkel–Brunswik, sitting in his classroom offering English words as substitutions for his German ones. These were not happy days of learning.

The language situation improved rather smoothly but the heavy, abstract psychological content remained a problem that was largely impossible for undergraduates, and few graduate students found any reason to put up with it, other than anticipating encountering more of the same on progressive admission tests. For my part, I found it all very stimulating, and together with a Japanese student (Joe Kamiya) developed strong interest in learning more. But Brunswik's seminars were a real challenge because of the high level of performance he demanded and in these seminars students found it necessary to tightly organize their responses. I recall (this must have been about 1947) that on one occasion in which I thought I had found a point that would have been very troubling for him, I plotted with my colleague Kamiya just exactly what the content of our presentation would be. Then, after my first sentence, we kept track of the time that Brunswik spoke, and noted with considerable glee, that he took up the *entire* remaining time of the seminar (which, by the way, I recall my student colleague and I thoroughly enjoyed, mainly because he brought up the topic of the design of experiments)!

This should have been a fascinating topic for graduate students, for it runs deep as the basis for research—but it wasn't. As far as I can understand it, the reason for that was the stubborn (I use the word advisedly) resistance of faculty members at every institution I have ever been associated with, to resist whole-heartedly his approach. Students recognized this faculty resistance, and few ever became devotees (at least in public) of Brunswik. It got to the point that if you showed your interest in *him* it meant that you had no interest in *them*. And they were right about that. For his concept of the *representative design of experiments* simply means that one should be as rigorous about one's inductive inferences over the object, or stimulus, side of the experiment as over the subject side. This means that one is interested in generalization as well as causality. Acting in accordance with that principle changes (almost) everything that the student thought s/he had learned about research. (This concept is developed in detail in his "Perception and the Representative Design of Psychological Experiments" (Brunswik, 1956); in my view a marvelous book; see also Hammond & Stewart, 2001).

Few, if any, recognized this 1956 book as containing the seeds of a revolution in science. Much the same thing had happened in physics when Einstein (1905) at the turn of the century opened the eyes of physicists to far-reaching space, and tiny particles, thus changing the frame of reference from a "small world" (in which Newton's physics worked very well) to a "large world" (in which they didn't—as he and others) pointed out in 1938; Einstein & Infeld, 1938/1961). Einstein put it this way: "When formulating the principal clews of mechanics we omitted one important point. We did not state for which coordinate system they are valid. For this reason, the whole of classical mechanics hangs in mid-air since we do not know to which frame it refers" (Einstein & Infeld, 1938/1961). Who would have believed the "whole of classical mechanics hangs in mid-air" in 1938 if Einstein hadn't said it?

Few did believe it at the start despite the detailed mathematics (see Kumar, 2008). But the real question for the readers of this article is: Who would have believed that this distinction between Newton's "small world" and Einstein's "large world" would have any relevance for experimental psychology? The answer to that question is easy: "no one" (at least no one I know of mentioned it).

But in 2012 two psychologists did mention it; Brighton and Gigerenzer (2012), in a badly needed, brilliant paper used this very distinction – "small world" vs. "large world" – in a step to clarify the distinction between the "coordinate systems" of two cognitive environments. They wished to emphasize that each "world" requires different cognitive activity. The logical ("small") world requires the development of different principles of cognitive activity than the "large" ecological one. They don't claim that "small worlds" principles are wrong for those small worlds; they simply require replacement by different principles when the subject must deal with "large worlds", by which Brighton and Gigerenzer mean the ecological worlds encountered by human judgment. And since the frame to which classical psychophysics belong was never specified (as was also the case in classical mechanics), should we say that the *psychophysics* – as well as classical mechanics – of the 20th century and earlier "hangs in mid-air"? Perhaps that was the cause of the students' boredom?

Be that as it may, Brunswik (1943), and Brighton and Gigerenzer (2012), set about arguing for a psychology of judgment and decision making that would apply to an *ecological* ("large world") instead of a *logical* ("small world")—to which Kahneman (2011) wished to apply his ideas about judgment and decision making. That required them to develop a description of the properties of a "large world", which they did. These were the same ecological properties that Brunswik tried so hard to convince his colleagues were the important elements of judgment and decision making,