

Michael Kubovy: Interview with William Epstein

Sunday May 6th, 2012

MK: Bill, how did you get involved with Gestalt psychology?

WE: I came of age at one of the high points of Behaviorism in North American psychology. My principal undergraduate teacher was a student of Kenneth Spence, at Iowa. His name was Howard Kendler and he hewed closely to the truth according to the word of Hull/Spence. And I was all set to attend Iowa as a graduate student when personal factors intervened and I found myself, late in the spring, wanting to remain in New York City for personal reasons. The options were very few, and I decided to make a tour of the local graduate programs – anyone who might consider having me at that late date. And one day I found myself at a school I had never heard of before called The New School for Social Research, and I was interviewed by a man I had never heard of before, his name was Irvin Rock. And Irv said “sure, we’ll have you”. And so I went through the procedure, and began my graduate study at The New School for Social Research not knowing anything about Gestalt psychology but finding myself at what was, probably, the single-most active center for the promotion of Gestalt psychology – with Hans Wallach (of Köhler & Wallach fame), Mary Henle – who was a devout advocate of classical Gestalt theory. Wolfgang Köhler had been a professor at “The New School” and had just left not too long before I arrived. And that was my introduction to Gestalt theory. So by the time I finished, a few years later, I had received a thorough indoctrination in Gestalt theory and found myself being shipped out by Hans Wallach to the Midwestern bastion of Gestalt theory at the University of Kansas, where Fritz Heider took me under his wing and other second-generation students of Lewin became interested in my future. And that is my contact with Gestalt psychology, unplanned and unprepared, and I must say, by the time I left, also unconverted. There was some part of it that appealed to me and other parts of it that either I couldn’t understand or I couldn’t embrace. So what about you, Michael, how did you get from The Hebrew University to Gestalt theory?

MK: It’s not a simple path. There were several sources of my interest in Gestalt psychology. One of them was my study of philosophy at The Hebrew University, which got me interested in phenomenology, my interest in philosophical and psychological aesthetics, which I thought were topics that required phenomenology since they were about experience and about phenomenal objects. And to some

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extent, Danny Kahneman, who was extraordinarily eclectic in his presentation of the fields of psychology, and, especially when he talked about social psychology, phenomenology appeared quite naturally. The aspect of Gestalt psychology that has always been most interesting to me has been the study of emergent properties. I never was happy with any reductionistic approach to psychology, and I always thought that psychology was neglecting the phenomenal through behaviorism, but even when I came to know Cognitive Psychology, it felt, and still feels like Behaviorism warmed over. There is little trust in reports of phenomenal experience. This comes from a long-standing suspicion regarding introspection that sank psychology into a morass of methodological conflicts. But psychology threw the baby out with the bath water. Psychology seems to leave its humanity at the door of the laboratory. It refuses to recognize that third person information needs to be merged with first person knowledge to produce a complete theory. I got engaged in the study of Gestalt psychology in the late 70s when I was at a meeting of the Psychonomic Society. I went from talk to talk where people mentioned the word gestalt and apologized for using it, as if it was a dirty word. Having had that experience, that people were pining for the notion of gestalt and had no way of incorporating it into their paradigm, into their scientific program, they were left without the benefits of phenomenological information. From that came my collaboration with Jim Pomerantz on a conference called “Perceptual Organization” and then a book called *Perceptual Organization* that was the first of a series of books that were published at about the same time – in the late 70s, early 80s – all dealing with similar problems of perceptual organization and gestalt. So...

WE: I just want to volunteer an observation about the group at The New School for Social Research. I think there was only one member of that group who could fairly be said to be a true blue Gestalt psychologist, and that was Mary Henle. Hans Wallach, who was an established visual scientist, and Irv Rock, who was up and coming, really shied away from Gestalt theory, and eventually, as you know, Irv gave it all up in favor of a Neo-Helmholtzian approach to perception, and Hans I think was a-theoretical, certainly a-theoretical in the broad sense – not that he rejected the value of small scale accounts of phenomena, but the broad approach of Gestalt psychology never came up in his lectures, and never came up in his tutorials. Solomon Asch, you mentioned social psychology, well Solomon Ash, while he was primarily at Swarthmore, would come to The New School every week and give a seminar. His seminars clearly reflected the Gestalt theoretical orientation and yet, there’s something about the way Shlaym writes, the way he formulated questions, that was less doctrinaire. In this New School group Mary Henle stood out as the standard bearer of Gestalt theory. She taught history of psychology, and whenever she touched on other approaches, she was harshly critical; she rarely had anything critical to say about Gestalt theory.

MK: Excuse me for interrupting, but let me try to explore a little bit more the question of what you weren't by... Was there anything in the Gestalt approach that did appeal to you?

WE: I was principally, and still am principally, interested in understanding perception. And I like the way Koffka framed the questions for a student of perception. I was less, much less, enamored by the answers that he volunteered, which I found too vague. Now, I have to admit that I may not have understood much of what Koffka was volunteering, and for that matter, may not really have understood much of what Köhler was volunteering. I was very attracted – this overlaps with the observation you made about even current cognitive psychology leaving humanity at the laboratory door – I was very much attracted to a book that very few people have read that Köhler first, I think, presented as a series of lectures at Harvard. The book is called *The Place of Value in a World of Facts*, and there, it seemed to me, Köhler was addressing the questions that any thoughtful person who thinks about the human condition and the capabilities that we have would address. And... so that's what I liked about... it pretty much resembles a little bit about what I liked about Freud. I went through my Freud period – seems to me probably every young person goes through a Freud period, maybe no more, but when I was young – and what I liked about Freud were not his answers but that he didn't shy away from any questions. And in that respect, I like Gestalt theory. I thought when Koffka raised the question of "Why does the world look as it does?" I thought that was a thrilling question. And it's the question I've tried to address for the last 50/60 years. So when I left The New School, there I was on my own. I had been taken into a friendly atmosphere, sort of the Midwestern branch of Gestalt theory. Roger Barker was there, Fritz Heider was there, other students of Lewin were there, and there was a person there whose name I think has been forgotten, but who had a profound effect on Irv Rock. His name was Martin Scheerer – a little gamecock of a fellow. And to see Martin, who must have been all of four foot eleven, thin and wiry, consorting with his best buddy Fritz Heider, who has seemed to me the tallest psychologist that lived, very formal, very dignified, was a joy to behold. And so for a while, I wonder sometimes whether it was imagined by people like Mary Henle that I would be the standard bearer of Gestalt theory, but I just couldn't make it work for me. For me it didn't generate a research program, and it didn't really – it didn't allow me to cope with the questions that interested me. And so I just gave it up. It wasn't until later on that I tried to rethink Gestalt theory, and did, and came away with a greater appreciation for their program – which I have expressed in a number of different places. But none of this was available to me when I finished my graduate studies.

MK: So, has the study of perception really pursued the question 'why does the world look the way it does'? It seems to me that it has turned its back on that question, and it's only on occasion, as a by-product of other questions, that the

appearance of the world, the phenomenal world, comes into play. You mentioned *The Place of Value in a World of Facts*. I know we're going to get to Gibson at some point, so let me inject him right now. The closest anyone has come to saying something about the place of value in a world of fact is Gibson. He inherited the notion of affordance from the Gestalt psychologists but transformed it into a pillar of his approach. He claimed that the world around us is seen in terms of its affordances – namely the ways in which it affords our interaction with them. The world that surrounds you is different when you're walking through the woods and you're fresh, and walking through the woods and you're tired and looking for a place to sit. At that point, various objects in the forest are queried to find out whether they afford a sitting place, a resting place. Gibson never emphasized that an affordance is shot through with value. That is, one way of saying that something is sittable-upon is to give it a value that it wouldn't have when you're not looking to sit. To describe it in that way is to bring back the phenomenology into Gibson's approach. In his early work he did talk about the visual world and how it's different from the visual field, but in his later work, those issues of the characterization of experience were not emphasized, although they were always implicit. That's an example of how phenomenology was tacitly present, but couldn't be spoken – couldn't be really part of the conversation.

WE: I was going to say that I agree, but I'm not sure. The first part of Gibson's 1979 book is, in my view, an exercise in phenomenology. Gibson set out to redefine the objects of perception from the perspective of an active perceiver. So, it may be that phenomenology is alive and well, but not by that name, that it still plays an important role. In fact, I think you might get a surprising degree of concurrence that phenomenology plays a role in all cases in which perception is being studied, and even more importantly, in all cases in which successful explanations of perceptual experience are offered.

MK: It seems to me that people are recognizing that they can't escape phenomenology, but they're merely paying lip-service to it. Take, for example, the idea that perception is in the service of action – because it's in the service of action, phenomenology is less important. You can side-step the phenomenological part of it. For example, Gibson – however much he may have wanted to propose a theory of picture perception – down-played the importance of pictures because they are not objects for action. They may be objects for hypothetical action, but he never developed that idea... [Gibson] came from a phenomenological tradition and phenomenology was important to him implicitly, but explicitly he turned his back on phenomenology. So, tell me why – how and why – you came to identify yourself so strongly with the Gibson tradition.

WE: I came rather late to being a Neo-Gibsonian. I had to travel a circuitous route in which I stopped at intervals to sample other theoretical orientations. I

knew by the time I had finished my graduate study that I couldn't sign on as a Gestalt theorist. And so I looked elsewhere, and at the time and with the example of Irv Rock, I began to try my hand at Neo-Helmholtzian theory. And, for a decade or so, I pressed the case pretty strongly, mostly in empirical work. But I soon became unhappy with that, and I began to reflect on: what were the sources of my unhappiness? It wasn't simply that a particular set of experiments had not worked out. So it wasn't that I had come upon, or been come upon, either in my own work or in the work of others, empirical disconfirmation of these points of view of Gestalt theory, of Transactionalism, of Neo-Helmholtzian theory. I came to realize that what was distressing me and making me most uncomfortable was that these theories, and others like them, were too complicated, were too elaborate. And I became convinced, rightly or wrongly, that the more elaborate an account is, the more it banks on assumptions – some of which are untested assumptions – the more likely it is to be misleading. And when I came upon the startling simplicity of early Gibson, I thought that I had struck gold. "This is a very simple, straight-forward account of perception. I like it." By the time I came to late Gibson, he had left me behind. I simply couldn't understand much of what he was claiming. I didn't really understand the theory of affordances. I remember being invited by Gunnar Johansson in Uppsala, Sweden, where I had done a post-doctoral, to come to Uppsala to give a seminar on Gibson's 1979 book, and I agreed. I liked Uppsala very much, and here was an opportunity. But by the time I showed up, I realized that I didn't understand what Gibson was claiming, and so I had to confess before the seminar group that I had very little to say because I couldn't understand it. It took me a long while to get to the bottom of Gibson. And once I did, and got some confirmation from others that I had understood him correctly, I came to like it very much because I thought that among all the options, it was the one that committed me to fewest questionable assumptions. Naturally, anyone who signs on with Gibson is signing on to some premises, no question about that. And a number of them are in some respects either un-testable or have never been tested, or never been pressed hard enough. But, in my assessment, that was the recommendation for Gibson, so by maybe 5 or 6 years after the 1979 book, I thought I understood it well enough and that I liked what I understood.

MK: You were talking about your allegiance, or your sympathy, with Gibson's approach, and you were happy to identify yourself with a unitary theoretical approach. You have often contrasted that with what you perceive to be my approach. Would you like to...?

WE: Sure. I have on occasion challenged you with being an eclectic – someone who draws whatever is needed from different theoretical approaches to patch together a single account of a set of phenomena, for example. And I have long been worried about eclecticism. My concern about eclecticism originated in

a lecture that Mary Henle gave when I was a graduate student, in which she pointed out to us – in a way that I still find convincing – that, by patching together bits and pieces of disparate theoretical approaches, one runs a serious risk of putting together an account which embodies internal inconsistencies among fundamental premises that underlie the different approaches. And, because of that, an eclectic approach is an approach that is bound to fail. Inevitably it will collide with these internal contradictions, and won't be able to remain standing. But I confess that I have grown more sympathetic toward your position. Maybe it's all those lunches, and you've been slipping me something in our tandoori chicken that has brought me around. So, I thought I'd share this with you. I'm convinced that Gibson doesn't have the complete story. But then I was convinced that Gestalt theory didn't, and I was convinced that the Neo-Helmholtzian theory did not, and so it went. And I think Gibson has made remarkable strides, but has stopped far short of providing us with an understanding of the richness of perception. Perhaps Gibson would not be upset to hear this; after all, surely he didn't imagine that he had gotten the whole story. And so, I'm not offering this observation with the intent to embarrass him, embarrass Gibson if he could overhear me, or to embarrass contemporary ecological theorists. But, now I wonder if the whole story is to be found in an eclectic approach – but not one that's piece-meal eclecticism (so there's my Gestalt background – you recognize the term piece-meal), not piece-meal eclecticism, but a principled eclecticism. And that would be where I would go if I was still in the business. And if there's one person who can do this, it's you!

MK: Well it seems to me that, in part provoked by you, I have given a great deal of thought to the question of theoretical eclecticism. So, it's *theoretical* eclecticism that we're talking about.

WE: Mmm, right.

MK: You can find your phenomena eclectically, but they're phenomena, they're facts, they're observations. So, the eclecticism must be about theory. I have reached the conclusion that is, in some ways, similar to yours in the rejection of eclecticism, but for different reasons. I think that psychologists are much too quick to call an idea a theory. You find different figures in contemporary psychology associated with their own theory of whatever it is that they're studying. Since I think of myself as a mathematical Neo-Gestalt psychologist, I'm led to think a little bit about other models of science that are different from the one prevalent in psychology. It seems to me that we might learn from physics, however often people have said that psychology cannot be modeled on physics. One thing we can learn from physics is the relationship between the law and theory. In physics, theories are much less common than they are in psychology. They come only after a very large body of observations, lawful observations, comes into being. So, for instance, the laws of the gasses, in which you relate pressure,

volume and temperature, preexisted thermodynamics and an understanding of how molecules in a gas behave under various conditions and that the three – volume, pressure and temperature – can be explained by the motion of particles. Psychology has few laws. The practice of research in psychology encourages the development of opinions that are given the honorific title of theories. The so-called theoretician, being convinced of the correctness of that theory, looks for conditions under which that theory can be, as it were, tested, but in fact, confirmed. In our field, there's a drive toward the confirmation and little toward the refutation of one's own opinions. I think of myself as relatively a-theoretical, not eclectic. I don't propose theories; I want to propose laws of whatever it is that I'm studying and then let someone – if I'm lucky, me – take these observations that form a coherent whole and formulate a theory about them. Once we have a solid body of lawful relations, quantitatively solid relations, we can start debating about the appropriate theoretical framework to account for these phenomena. That requires an approach that is different from what psychologists practice. I said that psychologists have their "theory," from which they deduce some point-predictions, and then proceed to confirm what they thought, and they're done with their job. If, instead, their task were to carve out a fairly large domain, determine that they were covering that domain with relevant variables, and then formulated lawful relations, our progress would be more rapid. I lucked out in choosing the problem of grouping by proximity – and the problem of grouping by similarity – where I could, in fact, characterize all possible dot-lattices and then explore the space of dot-lattices and come up with lawful relations. But I'll have you note – I don't have a theory. It does turn out that the mathematics suggests neural mechanisms that *might* produce my data; that is neither a theory nor reduction. I recommend that psychology be modest in its use of the term "theory" and that it be more ambitious with respect to data, lawfulness and phenomenology.

Works Mentioned in the Interview

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