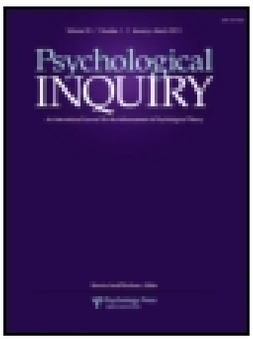

Psychological Inquiry

ISSN: 1047-840X (Print) 1532-7965 (Online) Journal homepage: http://www.tandfonline.com/loi/hpli20

Process Versus Content in the Study of Judgmental Accuracy

David C. Funder

To cite this article: David C. Funder (1990) Process Versus Content in the Study of Judgmental Accuracy, Psychological Inquiry, 1:3, 207-209, DOI: 10.1207/s15327965pli0103_6

To link to this article: https://doi.org/10.1207/s15327965pli0103_6

Published online: 19 Nov 2009.

Submit your article to this journal

Article views: 10

Citing articles: 6 View citing articles

Full Terms & Conditions of access and use can be found at
http://www.tandfonline.com/action/journalInformation?journalCode=hpli20

Process Versus Content in the Study of Judgmental Accuracy

David C. Funder
University of California, Riverside

Arie Kruglanski has offered a breathtaking integration of the varying rubrics under which psychologists have studied human judgment. In a single, simple, 2 × 2 model, he has integrated such traditionally diverse areas as attribution theory, attitude change, cognitive therapy, and even philosophy of science.

The benefits of his integrative model are many. For instance, by pointing out how consistency, consensus, and distinctiveness are all special cases of the covariation principle, Kruglanski's discussion of attribution theory brings that body of research much closer to being a comprehensive and plausible account of interpersonal judgment. And, Kruglanski provides a valuable insight when he discusses how shifts in scientific viewpoints and even paradigms can be accounted for through generic processes of attitude formation and change. In short, I believe that Kruglanski's model of knowledge acquisition (or "epistemics," to use his preferred term) is itself a significant contribution to knowledge.

However, the reach of Kruglanski's model is not unlimited, as I suspect its author would wish to be the first to acknowledge. I do not know what all its limits might be, but one seems rather obvious to me, perhaps because of my own epistemic motivations. Lay epistemic theory is an integrative model—an exceptionally wide-ranging model—of the *process* of judgment. As such, and despite the author's valiant attempts, it cannot address judgmental accuracy. As Kruglanski himself notes more than once, accuracy is a matter of *content*.

The distinction between process and content is both fundamental and subtle. Consider an example: Somebody once pointed out that if you want to understand completely how a chess player thinks, and in particular if you want to be able to predict his or her next move, it will not suffice to garner knowledge, no matter how extensive, about the chess player's cognitive processes (his or her "software") or brain functioning (his or her "hardware"). You will also have to acquire an understanding of *chess*.

I hope the point is clear. Cognitive mechanisms serve to process information, but they are not the information itself. Chess is a domain of content in its own right, and you will never be able to predict the player's future behavior, or even be able to determine whether somebody is a good player, by studying only his or her processes of judgment. You will also need to understand the game itself, what the player thinks about the game and how often, against what players, and how, he or she tends to win.

This analogy comes close to explaining why studies of judgmental process, even when they are viewed through such an admirably integrative model as Kruglanski's, will never get us far toward understanding accuracy. Kruglanski allows as much when he states that "it is doubtful whether a general *process* may be uncovered whereby accurate judgments are reached" (emphasis in the original). Exactly right, except that it's not just "doubtful"—it is impossible.

This is why I argued a couple of years ago (Funder, 1987) that research on judgmental error is utterly uninformative about accuracy: Studies of error tell us about the process of judgment rather than about its content. In particular, I argued, the existence of errors, demonstrated in laboratory settings, cannot be taken as evidence that judgment in real life is generally inaccurate. I also argued, in a seldom-noticed passage (1987, p. 87), that when subjects make judgments *correctly* in the lab, this cannot constitute evidence that they are generally *accurate* in real life.

Thus, I think Kruglanski misrepresents my point of view at least slightly when he cites my 1987 article as having claimed that "persons' judgments are more likely to be accurate in natural versus artificial settings." That was not the point I was trying to make.[1] Rather, my point was that the accuracy of judgments that subjects render in artificial settings is simply *not informative* about the accuracy of their judgments in real life. For all I know, in real life, accuracy is even worse. But evidence is not being gathered one way or the other as long as research remains inside the laboratory, where it can address only process and not content.

That said, and at the risk of reinstating the misunderstanding I have just tried to eliminate, let me add that the laboratory situations in which judgmental errors are demonstrated are not merely artificial in some neutral way. Rather, they are specifically designed to fool subjects. (Procedures that don't succeed at fooling subjects are eliminated in the pretesting phase.) Subjects are shown impossible combinations of stimuli, or are presented irrelevant information under circumstances designed to imply that the information *is* relevant, or even are told outright lies (e.g., "This sociable-unfriendly person is *real,* and was evaluated by trained psychologists") that cannot help but lead subjects down the primrose path toward what the experimenter will triumphantly label an erroneous inference (see Funder, 1987, for several specific examples).[2] I own up to a belief that subjects' judgments in real-life settings, which often are confusing but are seldom so perverse, will be more accurate than they are in artificial settings like these. But, let me reiterate, it would be trivially easy to design a study that would lead subjects to make *correct* judgments of experimental stimuli. Unfortunately, such a study would not prove that people reason will in real life, for exactly the same reason that studies of error do not show that people reason poorly.

So, how does one study judgmental accuracy? If we want to understand the judgments of a chess player, we must understand chess. It would follow that to understand the judgments of a weather forecaster, we should learn something about meteorology (e.g., Lusk, Hammond, & Steward,

[1] I leave to the other authors cited on this point (McArthur & Baron, and Swann) to decide whether they think they were summarized correctly.

[2] One of the errors most commonly demonstrated in these experiments is the "fundamental attribution error," the putative tendency to overestimate the importance of persons relative to situations in the determination of behavior. Ironically, it could be argued that by attributing errors to shortcomings of subjects (e.g., Ross, 1977) instead of to the deliberate rigging of experimental situations, researchers are themselves committing a particularly grievous instance of the fundamental attribution error.

1989). And, to understand the judgments of a home appraiser, we should learn something about real estate. My own research field is the accuracy of judgments of personality. To understand that topic, I believe, we must make a serious attempt to understand personality.

Apparently, this is a rather radical position. Psychologists who study, say, the perception of color tend to know an awful lot about the physics of colored light. For some reason, psychologists who study the perception of persons feel themselves under no similar obligation to inform themselves about the nature of personality. This is unfortunate, because the study of accuracy in personality judgment has a lot more to do with personality than with judgment.

The study of accuracy in personality judgment is, by rights, no other than a branch of the more general field of applied personality assessment, such as was described in the classic text by Wiggins (1973). The field of personality assessment has as its goal the formulation of meaningful and useful dimensions of individual differences, and the development of techniques for the accurate measurement of those differences. For instance, you might try techniques such as the Minnesota Multiphasic Personality Inventory, or the Rorschach, the Type A Inventory, or the Hardiness scale, or perhaps self-monitoring. Whatever technique you utilize, you will need to gather evidence as to its reliability and validity. The psychometric technology and more substantive rules of this game are well documented in Wiggins and in Cronbach and Meehl (1955).

Or, you could try a different technique—you could ask people who know your subjects well to describe what the subjects are like. If you were of a mind to take these judgments seriously as possible indicators of individual differences in personality, again you would need to know about their reliability and validity, as you would with any measurement tool (Funder, 1983). You would assess whether the judgments were repeatable, or generalizable across judges (e.g., Funder & Dobroth, 1987). Most important, you would find out what else the judgments correlate with. For instance, do peers' judgments of personality predict self-judgments, or more direct measurements of the subjects' behavior? In short, you would do everything Cronbach and Meehl (1955) say to do when validating any assessment device.

Suddenly, you would find yourself studying judgmental accuracy, but in a way radically different from anything envisioned in Kruglanski's article or in most of the literature on social judgment. You would not be examining how the judgments were made—that is an interesting question, but it is not an accuracy question. Rather, you would be unpacking the content of the judgments themselves. And, you would begin the daunting but reasonable task of gathering the evidence you will need to approach an answer to this question: Are the judgments right, or are they wrong?[3]

Further questions will follow naturally. Are some kinds of people better judges than others? Are some kinds of people more *judgeable* than others? Are some kinds of traits harder and easier to judge (Funder & Dobroth, 1987)? How well acquainted, and under what circumstances, do judges and targets need to be before judgments have a reasonable chance of being valid (Funder & Colvin, 1988, in press)? And perhaps most important, what, exactly, do judges observe in their targets that makes accurate judgment possible (cf. Neisser, 1980)?

Kruglanski wonders when if ever relying on one's initial intuition will be correct. The answer, which he does not give, depends on the content of the intuition. If your intuition is that extraverts can be detected by their soft voices and small, timid gestures, you would be well-advised not to rely on it. But this advice does not stem from studies of process, but rather from studies of content—specifically, in this case, from research on the nonverbal correlates of extraversion showing it to be associated with a loud speaking style and expansive gesturing (Scherer, 1978).

My comments have focused rather specifically on the accuracy of personality judgment, a topic of particular interest to me. Of course, Kruglanski's article ranges across much broader terrain. But I suspect the same process–content distinction that I have emphasized is important in other areas of psychology as well. Interestingly, Kruglanski himself shifts gears significantly in this direction when he talks about cognitive therapy. He points out that the essence of what he calls "psychological malaise" is not a certain thinking process, or even inaccurate thinking. Rather, Kruglanski hypothesizes—quite plausibly—that malaise is a "function of . . . the *contents* of the individual's beliefs" (emphasis in the original), specifically, "beliefs connoting the frustration of important objectives."[4]

Let me just note that to test this hypothesis, we would not conduct research, using artificial stimuli, into *how* depressives think. Rather, we would try to find out *what* depressives think about their chances of obtaining the objectives that really are important in their lives. Appropriately, and in line with Kruglanski's suggestion in this case, we would focus on content rather than on process.

Lay epistemic theory is an extremely broad, integrative view of the process of judgment. What I have treated in this commentary as a limitation of the theory is really not due to any shortcoming of the theory itself. Lay epistemic theory fails to address judgmental accuracy in a convincing manner only because of the fundamental difference between process and content. Studies of the process of judgment can be rich, insightful, and integrative, as lay epistemic theory is. But they have nothing to do with the content of judgment. And accuracy is a matter of content.

Note

David C. Funder, Department of Psychology, University of California, Riverside, CA 92521.

References

Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin, 52,* 281–302.

Funder, D. C. (1983). The "consistency" controversy and the accuracy of personality judgments. *Journal of Personality, 51,* 346–359.

[3]Surprisingly often, I hear expressed the point of view that this accuracy question is impossible to answer and therefore accuracy research is meaningless. It is not clear to me whether those who adhere to this nihilistic position also believe that measurement validity is impossible to determine and therefore validity research is meaningless. The study of accuracy in judgment is exactly the same thing as the study of measurement validity, where the measurements being validated are interpersonal judgments.

[4]Even though this is Kruglanski's own hypothesis, it seems to come from outside his lay epistemic framework because it concerns content rather than process. Lay epistemic theory does become relevant a bit later in Kruglanski's discussion, when he describes the various processes by which a therapist might try to alter a patient's depressive cognitive content.

Funder, D. C. (1987). Errors and mistakes: Evaluating the accuracy of social judgment. *Psychological Bulletin, 101,* 75–90.
Funder, D. C., & Colvin, C. R. (1988). Friends and strangers: Acquaintanceship, agreement, and the accuracy of personality judgment. *Journal of Personality and Social Psychology, 55,* 149–158.
Funder, D. C., & Colvin, C. R. (in press). Congruence of self and others' judgments of personality. In S. Briggs, R. Hogan, & W. Jones (Eds.), *Handbook of personality psychology.* Orlando, FL: Academic.
Funder, D. C., & Dobroth, K. M. (1987). Differences between traits: Properties associated with interjudge agreement. *Journal of Personality and Social Psychology, 52,* 409–418.
Lusk, C., Hammond, K., & Stewart, T. (1989). Weather forecasting in Colorado focus of research. *Brunswik Society Newsletter, 4,* 6.
Neisser, U. (1980). On "social knowing." *Personality and Social Psychology Bulletin, 6,* 601–605.
Ross, L. (1977). The intuitive psychologist and his shortcomings: Distortions in the attribution process. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 10, pp. 174–221). New York: Academic.
Scherer, K. R. (1978). Personality inference from voice quality: The loud voice of extraversion. *European Journal of Social Psychology, 8,* 467–487.
Wiggins, J. S. (1973). *Personality and prediction: Principles of personality assessment.* Reading, MA: Addison-Wesley.

Lay Epistemic Theory and the Relation Between Motivation and Cognition

E. Tory Higgins
Columbia University

My commentary is directed not only to Arie Kruglanski's article in this issue, "Lay Epistemic Theory in Social-Cognitive Psychology," but also, and especially, to his book on which the article was based, *Lay Epistemics and Human Knowledge: Cognitive and Motivational Bases* (1989). This work is very impressive. Over the years, I have found both the "endogenous–exogenous" partition and the "process–contents" distinction very useful, and their value is clearly demonstrated once again. The significance of this work goes beyond even these contributions, however. Recently, social and personality psychologists have regained their historical interest in understanding the relation between motivation and cognition. The aspect of this relation that has received the most attention concerns how information processing variables influence motivation. When the opposite direction of influence has been considered, most research has focused on how motivation influences specific processes, such as attention or reconstructive memory.

Kruglanski's theory presents a fascinating and novel alternative perspective on the motivation–cognition relation. Basically, the theory suggests that cognitions can be treated as if they were real-world objects, such as a car or a bottle of wine, to whose properties people have motivational orientations. Just as a bottle of wine may be more or less preferred across people or situations as a function of its complexity, so too may a piece of knowledge be more or less preferred across people and situations as a function of its complexity. Just as people vary in whether they prefer a car with a single uniform color or a car with two-tone contrasting colors, people vary in whether they prefer conclusions that are univalent and unambiguous or conclusions that are multifaceted and contingent.

Thus, it is not just that motivational processes can influence cognitive processes and vice versa, but *cognitive properties can be treated as targets of motivational processes.* And what makes "cognitions as objects" especially interesting is that cognitions are often more malleable than real-world objects. Like real-world objects, cognitions can be approached or avoided, selected or rejected. But they can also be transformed mentally. It is difficult to make a simple wine taste like a complex wine either in actuality or in imagination. But one can add new information that will make simple knowledge more complex or that will transform a noncontingent proposition into a contingent one. What motivates people to engage in such transformations? This is one of the fascinating questions that his theory addresses. More generally, his work represents an original and important perspective on the motivation–cognition relation that both provides new insights into basic psychological phenomena and suggests new and exciting research directions. Indeed, the book (Kruglanski, 1989) contains compelling examples of such significant insights and research directions.

With a theory as broad and encompassing as this one, it is not possible to address all issues fully. Thus, in pointing out a few gaps, I only wish to suggest areas for future consideration rather than to suggest that there are serious limitations in the theory. I believe that there are three basic issues that need to be addressed more fully in the future.

First, the theory considers two types of motivating closure: (a) the need to seek or avoid nonspecific closure and (b) the need to seek or avoid specific closure. The former type of motivating closure involves the most general processes of lay epistemic motivations. The second type of motivating closure involves contents and not just processes. Indeed, there is no theoretical limit to the number of kinds of specific closures. How does the theory of lay epistemics per se identify or delimit the relevant kinds of specific closures? Must the theory rely on other theories to identify or discover meaningful or important kinds of specific closures? And once such kinds of specific closures are identified, does the theory of lay epistemics alone permit predictions to be made concerning their consequences for information processing and behavior?

It seems that additional assumptions from other middle-level theories are necessary to make clear predictions. Thus, additional theories concerning different kinds of contents, and not just general lay epistemic processes, are necessary for different domains of psychological phenomena to be addressed. This raises a critical question for the theory. What does the theory of lay epistemics contribute to the study of specific-closure phenomena beyond the middle-level theories explicitly designed to address these phenomena? Certainly, the theory does suggest important similarities in underlying processes among phenomena previously treated as totally distinct. And consideration of such similarities is surely useful. But what about the differences between the