ticle, sect. 3). This claim may be met with some incredulous denials; there are, after all, a cornucopia of theories in social psychology, many of which are discussed by K&F themselves. The theoretical vacuum truly does exist, however, and it resides in the need for functional explanations of processes, rather than mere enumeration of theoretical processes and properties. Functional explanations (e.g., what is phenomenon X designed to do?) are at a different explanatory level from process descriptions. In the information-processing model described by Marr (1982), functional explanations are at the computational level, whereas process explanations are at the algorithmic/representational level. Therefore, the further advancement of social psychology relies not only on the interaction of situational and dispositional factors, as K&F illustrate very well, but also on multilevel explanations.

Thus, in a very real sense, K&F are correct in pointing out that social psychology has little in the way of solid theoretical clothes with which to dress their findings. This brings up, even more urgently, the issue of what could constitute appropriate "clothes" for the discipline. Like pointing out that the emperor has no clothes, it can be difficult to acknowledge that something does not exist when many other people have a stake in saying that it does exist. The dilemma is not just stating that social psychology lacks a metatheoretical framework, but having this message come from someone who is above dismissal as one "unfit for his office or too stupid for any use" (in the words of Hans Christian Andersen [1916/1995]). K&F will hopefully be recognized for the credentials they do, in fact, possess.

A further problem, once the theoretical imbalance and shortcomings in social psychology are acknowledged, is to develop a proper theoretical framework for the functional abilities that constitute social psychology. K&F suggest a couple of directions for this work: the functional considerations derived from theories of bounded rationality and evolutionary psychology. But primarily, K&F have chosen to focus on the initial issue of the adequacy of theory construction in social psychology in terms of both internal and external consistency, and they have some excellent suggestions for methodological tools to improve social psychological research. They devote only a short section to the issue of how social psychology should proceed in developing a better framework for understanding the functional design of the mind. This commentary elaborates on this topic for a simple reason: With the realization that social psychology – like the fabled emperor – has no functional clothes, there is an acute need for clothes.

The emperor of the children's fable shows himself in the end to be a fool who cannot acknowledge his error, and is therefore held up for further ridicule. Let us not make the same mistake in social psychology, particularly when proper metatheoretical clothes are so close at hand. With the implementation of an evolutionary theoretical paradigm, social psychology phenomena become amenable to interactive and integrated functional explanations that cover the range of relevant behaviors. This is true not just for the brief examples given in the target article (i.e., the representativeness heuristic, the availability heuristic, consensus bias, self-enhancement, and the fundamental attribution error), but also for other aspects of social behavior and cognition that K&F noted.

For example, the study of helping behaviors in social psychology has an obvious parallel in the study of altruism in evolutionary biology. These parallels are strong and informative, despite the differences in initial orientation (violations of helping norms in social psychology; the development of any helping in biology) and behaviors typically targeted (emergency helping of strangers in social psychology; kin-based and repeated interaction helping in biology). Recent work to integrate these disciplines have produced strong support for their compatibility and have begun to show the way for an integrated overall theory of altruism/helping (e.g., Burnstein et al.; Cialdini et al. 1997). As emphasized in the target article, this integration and resulting functional theory produces an explanatory framework that covers the complete range of behaviors (i.e., all degrees of helping, from the life-threatening, to the mundane, to failures to help).

Another example is the evolutionary analysis of the fundamental attribution error (FAE; Andrews 2001), which has provided a functional-level description of attributional processes that can explain the historical adaptiveness of the FAE, its current maladaptive qualities, and predicts further functional design features of the FAE that are likely to exist based on this account. In addition to being consistent with existing knowledge of the evolutionary history of the human species, this description is also boundedly rational in that it specifies the conceptual reference class of conditions under which the FAE will be adaptive and accurate (and thereby the conditions outside those bounds as well).

Beyond the benefits of metatheoretical clothes for the existing body of research in social psychology, there are further benefits to adopting a functional-level framework for social behavior and cognition. Such a framework allows social psychology to become better integrated with other behavioral sciences, which will facilitate scientific progression (Tooby & Cosmides 1992). A more direct benefit to researchers is that a functional framework can facilitate the identification of rich topics and hypotheses that are both important and, heretofore, little researched. Daly et al. (1997), for example, point out that the topic of kinship – studied extensively in anthropology, biology, and sociology – is nearly absent as an important topic in social psychology.

K&F allude to, but do not emphasize, that bounded rationality is consonant with evolutionary psychology. In fact, one aspect of the ecological rationality program of Gigerenzer and colleagues is that it is a specific application of evolutionary insights into the fields of judgments under uncertainty and decision-making (Gigerenzer & Todd 1999). Such a theory of evolved functional design, by virtue of its emphasis on what cognitive mechanisms are designed to solve (and the subsequent ability to place failures of cognitive mechanisms into context), yields an account that covers a range of behavioral and cognitive performances. For example, the theory that the mind is predisposed to register numerical information in natural frequency formats (Gigerenzer & Hoffrage 1995), not only provides an explanation for certain patterns of judgments under uncertainty, but also explains some of the difficulties children have in the course of mathematics instruction (Brase 2002a). This has led to further specifications about the nature of information representation in the mind (e.g., Brase 2002b; Brase et al. 1998).

ACKNOWLEDGMENTS

The author thanks David Over and Sandra Brase for helpful comments and advice in the development of this commentary.

Additional requirements for a balanced social psychology

Siu L. Chow

Department of Psychology, University of Regina, Regina, Saskatchewan S4S 0A2, Canada. **Siu.Chow@uregina.ca** **http://uregina.ca/chowsl**

Abstract: Ambiguous data obtained by deception say nothing about social behavior. A balanced social psychology requires separating statistical hypotheses from substantive hypotheses. Neither statistical norms nor moral rules are psychological theories. Explanatory substantive theories stipulate the structures and processes underlying behavior. The Bayesian approach is incompatible with the requirement that all to-be-tested theories be given the benefit of the doubt.

One may agree with Krueger & Funder's (K&F's) global thesis that social psychology depicts too negative a picture of human nature. However, they concede too much to the "negative psychology" camp. For example, they could have noted that Milgram's (1963) data were rendered ambiguous by the deception paradigm used. The subjects that were told to facilitate the learner-confederate's learning with electric shocks were confronted with two in-

https://doi.org/10.1017/S0140525X04240080 Published online by Cambridge University Press

Table 1 (Chow). *The deductive rules used in statistics and research*

Panel 1. Disjunctive syllogism used in making statistical decisions

	Statistical decision 1	Statistical decision 2
Major premise	Chance or nonchance (q or $-q$)	
Minor premise	Decide to reject chance (statistically significant result) (q is not the case)	Decide not to subscribe to nonchance (statistically nonsignificant result) ($-q$ is not the case)
Conclusion	Not chance ($-q$)	(q)

Panel 2. Conditional syllogisms used in drawing conceptual conclusions

	Modus tollens	Affirming the consequent
Major premise	If Theory T, then the data are in the form of Y in Condition C.	
Research	*Set up Condition C to collect data.*	
Minor premise	Data are not in the form of Y.	Data are in the form of Y.
Conclusion	Theory T is rejected.	Theory T is retained tentatively.

Panel 3. The inductive logic that warrants retaining the to-be-tested theory tentatively in case of affirming the consequent (assuming that there are only three recognized control variables)

		Control variables			
	Independent variable	Sex of participant	Age of participant	IQ of participant	Dependent variable
Experimental condition	To-be-tested level	M:F 5 1:1	18–45	95–105	Performance level
Control condition	Reference level	M:F 5 1:1	18–45	95–105	Performance level

compatible cues, namely, the pleading from the learner-confederate and the utter indifference of the experimenter-confederate. This conflict led to two possibilities. Those who were duped by the cover story defied the instruction. Those who were not duped "complied" as a pro forma exercise because they saw through the deception (Mixon 1976). In other words, Milgram's data were ambiguous as to whether or not there was any compliance at all.

K&F point out the inappropriateness of using moral rules as the theoretical foundation of empirical research. They could also have treated statistical norms in the same way, because negatively skewed distributions are not explanatory theories. What are required are well-defined explanatory theories that stipulate the psychological structures or processes underlying behaviors in various social contexts. At the same time, these theories are embedded in a global frame of reference. This seems to be consistent with the spirit of K&F's plea for studying the whole range of social behaviors.

The authors are correct in pointing out the futility of identifying the research hypothesis with the statistical null hypothesis. They could have been more thorough in pointing out that no statistical hypothesis (be it the null or the alternative hypothesis) can be identified with a research hypothesis, let alone a substantive theory (see Meehl 1967).

It is not clear how the authors may justify their preference for the Bayesian approach to choosing between two theories, H_1 and H_2. The choice is determined by the larger of the two posterior probabilities, $H_1|\text{Data}$ and $H_2|\text{Data}$. At the same time, the two probabilities are a function of the prior probabilities, $p(H_1)$ and $p(H_2)$, respectively. The mathematics is impeccable. There is yet no non-arbitrary answer to the questions, "How are the two prior probabilities determined?" Relying on past research is not satisfactory, because what is true in the past does not guarantee that it is true now. Relying on the researcher's subjective probability is not acceptable, because beauty is in the eye of the beholder. Hence, the Bayesian approach allows the possibility of giving a heavier weight to one theory at the expense of the other when one evaluates the data.

The Bayesian approach is also incompatible with Tukey's (1960) distinction between making a statistical decision and drawing a conceptual conclusion. Statistics is about data (viz., whether or not the data can be explained by chance influences). Such a decision has nothing to do with the past history or track record of the theories being tested. The deductive rule used in making this decision is disjunctive syllogism (see Panel 1 of Table 1). The major premise makes explicit two mutually exclusive options, chance influences on the data or some non-chance factors. The well-defined null-hypothesis significance-test procedure (NHSTP) provides the minor premise for the syllogism.

Choosing between explanatory theories is a conceptual endeavor that is governed by logic (both deductive and inductive). As may be seen from Panel 2, the major premise is the implicative relationship between the theory and the to-be-tested hypothesis. The latter is a stipulation of what the expected data are like. To conduct research is to set up the appropriate data collection conditions (see the italicized entry in Panel 2). The outcome of the statistical decision in Panel 1 supplies the minor premise for Panel 2.

The *modus tollens* rule is applied to exclude theories whose implications are inconsistent with the data. When the data are consistent with the theoretical expectation, the "affirming the consequent" rule is used to retain the theory tentatively in the presence of all recognizable controls (see Panel 3 of Table 1). The significant difference between the experimental and control conditions in Panel 3 can be said to result from the to-be-tested level of the independent variable when sex, age, and IQ are excluded.

The exercise described in Table 1 is objective because (1) all

https://doi.org/10.1017/S0140525X04240080 Published online by Cambridge University Press

contending theories are given the benefit of the doubt, (2) the decision and inference rules are independent of the researcher's vested interests, and (3) any undue influence brought about by the researcher's theoretical stance can be detected readily.

In sum, K&F could have made a stronger case for a balanced psychology had their advocacy stance been less zealous. It would also have helped had statistical issues been distinguished from methodological or conceptual issues.

Psychologists seek the unexpected, not the negative, to provoke innovative theory construction

John Darley and Alexander Todorov

Department of Psychology, Princeton University, Princeton, NJ 08544-1010.
jdarley@princeton.edu **atodorov@princeton.edu**

Abstract: Krueger & Funder (K&F) see social psychologists as driven to demonstrate that people's behavior falls below relevant moral and intellectual standards. We suggest that social psychologists search for demonstrations of when it is that people's actual behaviors and decisions deviate from expected or ideal behaviors and decisions, and what these "deviations" tell us about general decision processes, including those that do not produce unexpected actions. Often the discoveries are of positive rather than negative behaviors.

According to Krueger & Funder (K&F), social psychologists and judgment and decision-making researchers seek to demonstrate how people violate norms of decent human behavior and sensible human reasoning. K&F seem to suggest that this negative emphasis is rewarded, because these demonstrations of flawed behavior and reasoning are valorized by research peers and seized on by textbook authors. In any event, "a distorted view of human nature emerges" (target article, sect. 1, para. 3).

K&F point out that one of the sources of the negative picture of human behavior is the comparison of some behavior with some normative standard, revealing that the behavior, on moral or intellectual terms, falls below the standard. However, not all such comparisons result in negative views. According to the rational actor model in economics, people are solely motivated by self-interest. One of the contributions of social psychology has been to show that in situations of economic games, people act in ways that are more cooperative, and more considerate of the well-being of others, than is expected from the normative standard (Dawes et al. 1988). Here then is a demonstration of human strengths, one that K&F should be pleased about.

Social and judgment and decision-making researchers sought to produce results that challenged the prevailing model of human functioning at the time. In social psychology, the prevailing image was that of the individual functioning with some independence of the social context, and in decision-making, the image was of a fully rational, information-seeking individual. The social psychologists substituted the conventional image for the image of an actor integrating the information concerning "what is going on" to determine her interpretations of the situation, and therefore her reactions to it. The insight shared by these researchers was that the behavior of other actors who were facing the same "objective" situation gave a good deal of information about the essential nature of the situation and therefore how to react to it. By using experimental variations that should create different interpretations in the participants' minds, the researchers attempted to enter the minds of the participants, carrying out what we now call a phenomenological analysis; determining why they acted as they did.

K&F imply that it is the researchers who cast the behaviors of their participants as normative and ethical failures. The implication strikes us as untrue in regard to the conformity, obedience, and bystander research that is the centerpiece of their censure. To demonstrate this in the bystander area, here are two quotes, drawn from the two studies that they cite.

In one study (Darley & Latané 1968), subjects knew that many other subjects also heard the cries of a person in distress. They failed to intervene because their own responsibility for doing so was diffused. The experimenters commented that

> Subjects who failed to report the emergency showed few signs of the apathy and indifference thought to characterize "unresponsive bystanders." . . . Why then didn't they respond? It is our impression that non-intervening subjects had not decided *not* to respond. Rather they were still in a state of indecision and conflict concerning whether to respond or not. (Darley & Latane 1968, pp. 381–82)

In the Darley and Batson (1973) study, seminarians who were hurrying to another building to give a short sermon on the parable of the Good Samaritan often passed by a person in distress on their way to give the sermon. Here is what the researchers reported about why seminarians did so:

> According to the reflections of some of the subjects it would be inaccurate to say that they realized the victim's possible distress, then chose to ignore it; instead, because of the time pressures, they did not perceive the scene in the alley as an occasion for an ethical decision. . . . For other subjects it seems more accurate to say that they decided not to stop. Why? . . . Because the experimenter, *whom the subject was helping,* was depending on him to get to a particular place quickly. In other words, he was in conflict between stopping to help the victim and continuing on his way to help the experimenter. . . . Conflict rather than callousness can explain their failure to stop." (Darley & Batson 1973, p. 108)

The participants are characterized as good people, who, caught up in complex situations, act in ways that they themselves would not wish. The ameliorative point of the bystander literature is that these actions are the products of situational forces, and people can train themselves to resist these forces. Although we do not have space to demonstrate it, we think that the conformity and obedience researchers also characterized the thought processes of their subjects in similar and similarly sympathetic ways. Milgram's (1974) chapter on "binding forces" is an example of this (Ch. 12). In the decision-making research tradition, as Tversky and Kahneman (1983) comment,

> Our studies of inductive reasoning have focused on systematic errors because they are diagnostic of the heuristics that generally govern judgment and inference. In the words of Helmholtz, "It is just those cases that are not in accordance with reality which are particularly instructive for discovering the laws of the processes by which normal perception originates." The focus on bias and illusion is a research strategy that exploits human error, although it neither assumes nor entails that people are perceptually or cognitively inept.

K&F similarly mischaracterize social cognition research by suggesting that the metaphors underlying social cognition research have been predominantly negative. However, they fail to point out that the metaphors of the 1970s and 1980s (the "naïve scientist" and the "cognitive miser") have been replaced by the metaphor of "motivated tactician" (Fiske & Taylor 1991, Ch. 1). This metaphor emphasizes the pragmatic and functional aspects of social cognition, that is, that "thinking is for doing" (Fiske 1992; 1993).

Given the purpose of K&F's article, it is certainly justified to selectively review the literature. But writing about the status of contemporary social cognition without mentioning a single work on dual-process models (e.g., Chaiken & Trope 1999) is puzzling. The rise of the dual-process framework is a major recent development in the field of social cognition. Dual-process models attempt to integrate forms of heuristic reasoning with presumably more rational forms of reasoning into a single framework, without assigning evaluative labels to any of these forms of reasoning. This integration encompasses both social cognition and judgment and decision-making (e.g., Chaiken & Trope 1999; Kahneman & Frederick 2002; Sloman 1996). Again, the thrust of this work is inconsistent with the picture of social cognition depicted by K&F.

https://doi.org/10.1017/S0140525X04240080 Published online by Cambridge University Press