

A Balance-Logic Perspective on Kruglanski and Thompson's Single-Route Approach to Persuasion

Chester A. Insko

*Department of Psychology
University of North Carolina*

Kruglanski and Thompson's target article is both thoughtful and thought provoking. The article has two major parts. One major part is an exposition of their single-route perspective and a contrast with the dual-route perspectives of Petty and Cacioppo's (1986) Elaboration Likelihood Model and Chaiken's (1987) Heuristic Systematic Model. The other major part is a report of four studies that were designed to explore a possible confound between the contrast of argument strength with source expertise and the length-complexity-positioning of the information used to create the experimental contrast. I consider these two parts in reverse order. First, I review Kruglanski and Thompson's evidence that the previously obtained differential effects for arguments and for source expertise are due to the fact that the arguments have been long, complex, and presented last, whereas the source information has been short, simple, and presented first. Then, I consider the more general, theoretical issue regarding the viability of a single-route approach to persuasion. The theoretical issue will be considered from the perspective of a particular application of generalized logic to persuasion and other psychological effects.

Experimental Evidence for a Confound

Kruglanski and Thompson review a well-known, three-factor study by Petty, Cacioppo, and Goldman (1981) in which it was found that argument strength was more persuasive with high than with low personal involvement, whereas source expertise was more persuasive with low than with high personal involvement—all consistent with the dual-process notion that arguments are processed centrally, whereas heuristic cues, such as source expertise, are processed peripherally. Kruglanski and Thompson point out that the source information was presented prior to the arguments and, furthermore, was considerably shorter in "sheer number of words." Furthermore, both of these differences suggest that the "cue/heuristic information ... was much easier to process than the message argument information." Kruglanski and Thompson go on to point out that this "covariation" was "endemic" to many other experiments that examined dual processing and present a summary table of other studies in which it may have been easier to process the cue information.

In subsequent discussion, Kruglanski and Thompson suggest that complexity also affects information

processing. It is, of course, apparent that complexity, length, and positioning are different variables that may affect processing for somewhat different reasons. This is a matter that should be clarified and explored. Leaving that issue aside, it may be helpful to distinguish two perspectives on the confound between the communication arguments versus communication expertise variable and the length–complexity–positioning variable—a strong version and a weak version. According to the strong version, whether the information involves arguments or source expertise is irrelevant; the only thing that matters is the information length, complexity, and positioning. According to the weak version, whether the information involves arguments or source expertise does matter; it is just that the dual-route expected effects for argument strength will be relatively smaller when the arguments are short, simple, and presented first, and the dual-route expected effects for source expertise will be relatively smaller when the source information is long, complex, and presented last. Note that the weak version does not rule out the possibility of dual processing; the point is that the evidence for such dual processing occurs only when the length, complexity, and positioning of information do not produce contrary processing tendencies. I interpret Kruglanski and Thompson as advocating the strong version—although they do not distinguish explicitly between the two versions.

Definitive testing of the difference between the strong and weak versions of the confound could be conducted with an experimental design in which a manipulation of information length, complexity, and position is combined with the three factors investigated by Petty et al. (1981). This would enable us to determine whether the information manipulation eliminates any difference between the Argument Strength \times Involvement interaction and the Source Expertise \times Involvement interaction. Kruglanski and Thompson do not collect data for such a complex design; rather, they collect data for simpler designs that enable us to determine whether the data are at least consistent with the expected results for information length and a confound of length and position.

In Study 1 Kruglanski and Thompson crossed a manipulation of involvement with a lengthy (résumé) manipulation of source expertise. The results revealed an interaction such that source expertise had a greater effect with high than with low involvement. Such results are consistent with Kruglanski and Thompson's expectations. Note, however, that, because there was no manipulation of information length or complexity or position, neither the weak nor the strong version of their perspective was precisely tested.

Study 2 was like Study 1 except that a distraction manipulation was substituted for the involvement manipulation and involvement was held constant at a high level. The results revealed the expected interaction

such that the lengthy information regarding source expertise had a greater effect with no distraction than with distraction. Again, the results are consistent with Kruglanski and Thompson's expectations; however, again the absence of a manipulation of information length, complexity, or positioning does not allow for a direct test of their theory. I suspect that their evidence would not convince someone who was predisposed to be skeptical.

Finally, Study 3 did include a manipulation of information length, specifically information relating to source expertise. Study 3 was like Study 2 except for the addition of a third variable relating to length of information regarding the source; that is, there were manipulations of source expertise, distraction, and information length (with involvement held constant at a high level). The results revealed a triple interaction that, as expected, replicated the Study 2 results only when the source information was long. Such results are certainly consistent with the expected pattern of results relating to the information-length variable and are thus supportive of the weak version of Kruglanski and Thompson's theory. However, without a variable contrasting argument strength with source expertise there is no way to determine whether that variable might also have had an effect.

Kruglanski and Thompson investigated argument strength in their fourth and final study. In addition to argument strength the study included two additional factors: personal involvement and initial brief arguments versus subsequent lengthy arguments. The latter factor confounds length and position, thus preventing us from knowing whether any effects for that factor are due just to length, just to position, or to both length and position. Of course, from Kruglanski and Thompson's perspective, length and position should have similar effects on the tendency to process. Still, as indicated earlier, at some point the separate effects of these variables should be clarified. In any event, their theory predicts a triple interaction among the three factors such that the double-interaction pattern of greater persuasion for strong than for weak arguments with high rather than low involvement should be relatively greater with subsequent lengthy arguments than with initial brief arguments. Unfortunately, Kruglanski and Thompson do not report whether this triple interaction is significant. I find that puzzling. Even more puzzling is the fact that they describe their design as involving an "Initial Argument Strength \times Subsequent Argument Strength \times Issue Involvement ANOVA." Because initial argument strength is nested within one half of the design and subsequent argument strength is nested within the other half of the design, these clearly cannot be analysis of variance (ANOVA) factors—at least as ANOVA is typically conceived. So what was found? Judging from the simple-effect tests, the expected triple interaction was descriptively present. With subse-

quent lengthy arguments, strong arguments were significantly more persuasive than weak arguments only with high involvement. On the other hand, with initial-brief arguments, strong arguments were significantly more persuasive than weak arguments only with low involvement.

Although Kruglanski and Thompson do not report a single experiment allowing for a definitive contest between the argument-strength versus source-expertise variable and the information length–complexity–position variable, the general pattern of results is consistent with the assumed importance of an information variable and the weak version of their theory. I agree with Kruglanski and Thompson that their model provides “considerable generative potential” for further research. Such research would ideally include a test of the strong version of their theory.

Unimodel Perspective on Persuasion

Kruglanski and Thompson point out that their unimodel is based on Kruglanski’s (1989) “Lay Epistemic Theory (LET) of the processes governing the formation of subjective knowledge.” The unimodel assumes that persuasion flows from “rule-based” inferences that assign a “central role to (syllogistic or probabilistic) reasoning flowing from evidence to conclusion.” They point out, furthermore, that “Kindred notions of ‘evidence’ appear in dissonance and balance theories,” as well as in “the theories of reasoned action (Fishbein & Ajzen, 1975) and planned behavior (Ajzen, 1991).”

Because I have been associated with a balance-logic perspective, it will come as no surprise that I am basically sympathetic with much of what Kruglanski and Thompson have to say. I do believe that approaching this issue from a balance theory (or generalized logic) perspective is potentially illuminating. Much of what I have to say relates to my personal history, and has been said many times before. For that reason I feel more comfortable writing in first person.

Multiplicative Rule and Two-Valued Logic

Abelson and Rosenberg (1958) published an influential article in which they distinguished between logic and *psycho-logic* (their term for balance). A few years later, Runkel and Peizer (1968) pointed out that, contrary to Abelson and Rosenberg’s argument, if relations are restricted to two-valued, plus or minus, distinctions (consistent with Heider’s, 1946, 1958, original discussion), the multiplicative rule is perfectly mapped by two-valued logic. In Runkel and Peizer’s own words, “once it is realized that at most two catego-

ries are available for assigning perceptually associated elements, the practical distinction between *psycho-logic* and ordinary logic becomes superfluous from the point of view of the behavioral scientist” (p. 61).

Traditional, deductive logic makes only two-valued distinctions; that is, something is either included in the class of or not included in the class of, related to or not related to, equal to or not equal to, and so on. Why such an oversimplification? Consider a nonsexist version of the classic syllogism: “All humans are mortal; Socrates is a human; therefore Socrates is mortal.” Given just the major premise, if we view Socrates as only somewhat human we cannot, with logical certainty, infer that he is mortal.¹

Because Runkel and Peizer’s (1968) argument has been almost universally ignored, or unappreciated, by social psychologists, a version of it bears repeating. Much of Runkel and Peizer’s discussion is quite technical. However, a simple way to illustrate the mapping is with the similarity dimension after it has been restricted to the two values of same and different. Note that if a is the same as b and b is the same as c it follows logically that a is the same as c , and this, of course, is an implication of the multiplicative rule ($++ = +$). Also, if a is different from b , and b is different from c it follows that a is the same as c ($-- = +$); if a is the same as b , and b is different from c it follows that a is different from c ($+ - = -$); and finally if a is different from b and b is the same as c it follows that a is different from c ($- + = -$). To minimize the possibility of misunderstanding, let me emphasize that the aforementioned mapping depends on the assumption of only two values. Thus, if a relation is not the same, it is different and if a relation is not different, it is the same.

A helpful reader of an earlier version of this article offered the following rejoinder: $5 \neq 6$ and $6 \neq 7$ does not imply that $5 = 7$. The problem here is that the reader had violated the two-valued assumption and assumed that 5, 6, and 7 constituted three different values. If we only allow two values (and thus have a binary number system), it is indeed the case that $5 \neq 6$ and $6 \neq 7$ implies that 5 symbolizes the same quantity as does 7.

¹Some contemporary logicians have attempted to develop formal systems that go beyond two-valued distinctions to, for example, three-valued distinctions (cf. Rescher, 1969). Consider the statement: “The King of France is bald.” Because there is no King of France, from a three-valued perspective the statement is neither true nor false but indeterminate. Note, however, that even if logicians eventually reach consensus on the validity of a three-valued approach, it will still be the case that the two-valued approach will remain valid and that the multiplicative rule will overlap with such logic.

An Initial Attempt to Apply the Multiplicative Rule to Counterarguing and Central Processing

A number of years ago I was invited by Petty, Ostrom, and Brock to write a balance theory chapter for their edited volume, *Cognitive Responses in Persuasion*. Because I was intrigued by the apparent fit between counterarguments included in participants' listed thoughts and the multiplicative rule, I accepted the invitation. I approached the problem from the perspective of Rosenberg's (1956, 1960) concept of an attitude structure, and illustrated this structure with the example of someone who was favorable (+) toward the building of nuclear-power generating plants, believed that nuclear-power generating plants facilitated (+) the conservation of oil and gas (+), and so on. Rosenberg, very much like Fishbein and Ajzen (1975), conceived of attitudes as varying as a function of many such belief linkages. Rosenberg referred to the linkage between the central attitude object, the belief, and the related attitude object (or "value") as a *cognitive band*. Note that this is another way of describing what Heider referred to as a *p-o-x* triad except that there are two nonperson objects (*x*s) and the person is not explicitly symbolized. Thus, in Heiderian terms, the aforementioned cognitive band can be expressed as "*p* has a positive sentiment relation toward the building of nuclear-power generating plants, accepts a positive unit relation between the building of nuclear-power generating plants and the conservation of oil and gas, and has a positive sentiment relation toward the conservation of oil and gas."

Given that individuals have a tendency to hold consistent attitude structures, what happens when they encounter a counterattitudinal message? The basic assumption of the cognitive responses approach is that individuals generate thoughts—hopefully thoughts that can be assessed with the thought-listing technique. Suppose that someone with a negative attitude toward nuclear power is exposed to a pro-nuclear-power message. In my chapter, I pointed out that one possibility would be for the individual, consistent with his or her attitude structure, to list the thought, "problem of radioactive waste." This listed thought is an implied cognitive band: "nuclear power is bad (-) because it generates (+) dangerous radioactive waste (-).

The chapter went on to discuss other matters but, for purposes here, the most important of these was a Venn diagram version of Runkel and Peizer's argument. Thus the implication was that logic, not just the tendency to maintain balance, may guide central processing.

Sometime later, Petty and Cacioppo (1986) asked me to read a draft of a chapter they were intending to publish, and did eventually publish, in *Advances in Experimental Social Psychology*. In this chapter they presented an interesting discussion of how various theories can be

described as having different locations on the continuum from peripheral to central processing. What particularly struck me about this was that they located balance theory toward the peripheral end of the continuum. As I struggled to understand why they had done this, it occurred to me that, despite the aforementioned argument, they regarded balance theory as suggesting simple heuristics, or rules of thumb, such as "we tend to agree with those we like." After I described my concern to Petty and Cacioppo they retained their characterization of balance theory as involving peripheral processing but added a footnote stating that Insko had extended or broadened the theory beyond peripheral processing. Actually, all I really did was to make explicit what was implicit in Heider's original formulation.

I should clearly acknowledge that my thoughts about this matter were not really original and, in fact, drew directly from the contributions of others. Note that it was Cartwright and Harary (1956) who developed the multiplicative rule; it was Runkel and Peizer (1968) who demonstrated the mapping of the multiplicative rule by two-valued logic; and it was Peak (1955), Rosenberg (1956), Fishbein (1963), and Fishbein and Ajzen (1975) who specified attitude-belief linkages. Even earlier, both Thurstone (1928) and Likert (1932) had recognized the linkage between beliefs ("opinions") about an object and evaluation of an object.

I agree with Kruglanski and Thompson that logic is applicable to processing involving varying degrees of time and effort. Just because an inference follows some heuristic does not mean that that inference is not guided by logic. Kruglanski and Thompson deserve credit for making this point explicit.²

Petty and Cacioppo's apparent lack of appreciation of the linkage between the multiplicative rule and two-valued logic is seemingly shared by many others. Note, for example, that Eagly and Chaiken's (1993) impressive summary of the attitude change literature, *The Psychology of Attitudes and Beliefs*, does not include balance theory in the section of their book dealing with various logic approaches to attitudes but in a different section dealing with "motivational processes."³ And Heider, for that matter, did not appreciate, or accept, the linkage with logic. I became very

²Cacioppo and Petty's (1981) nonacceptance of this general point is illustrated by their argument that because a manipulation of the amount of time given participants to rate *p-o-x* triads (10 sec vs. 30 sec) had no effect on the attraction effect (*p* to *o* main effect) and agreement effect (*p* to *x* by *o* to *x* interaction), but did affect the three-sign balance effect (triple interaction), the attraction and agreement effects cannot be balance effects. The amount of time utilized in drawing an inference is a dubious indication as to whether the inference is or is not guided by logic. The possibility that the attraction and agreement effects rely on overpracticed bands relating to the self makes it quite understandable why the inferences would require less time.

³After I communicated my thoughts about this to Alice Eagly, she graciously acknowledged the point.

much aware of this when I reviewed for *Contemporary Psychology* (Insko, 1990) the volume of Heider's notebooks (Benesh-Weiner, 1988) dealing with balance theory. Heider explicitly attempted to track the linkage with logic but appears to have made an error. Here is the quote:

$a = b, b = c, a = c$ that is valid, possible
 aLb, bLc, aLc that is balanced
 $a \neq b, b = c, a \neq c$ that is valid
 $aDLb, bLc, aDLc$ balanced
 but: $aDLb, bDLc$
 $a \neq b, b \neq c$ that does not lead to $a = c$. (p. 59)

To the contrary, in a simple, two-valued world, if a is unequal to b , and b is unequal to c , it does follow that a is equal to c , just as a dislikes (DL) b and b dislikes (DL) c implies that a likes (L) c . Again, let me emphasize that two-valued means that if a relation is not equal, it is unequal and that if a relation is not unequal, it is equal. Because traditional logic is two-valued, balance theory cannot be a logic theory unless it is restricted to two-valued distinctions.

Beyond Two-Valued to Many-Valued Distinctions

An obvious need for development involves going beyond traditional logic to the many-valued distinctions of human thought. This is not an issue that Kruglanski and Thompson explicitly consider, although they do favorably reference the probabilistic models of McGuire (1960) and Wyer (1974). Indeed, combining probability theory and logic is one way of going beyond two-valued distinctions. Still another way is with the tetrahedron model—a model first formulated in geometric form by Wiest (1965) and given algebraic expression by Wellens and Thistlethwaite (1971a, 1971b). Because the tetrahedron model is not well-known, it merits a brief description.

Consider the three dimensions of a p - o - x triad, p to o , p to x , and o to x . Suppose that we do not restrict attention to just two-valued, plus and minus signs, but consider the array of possible plus to minus values for each relation. Geometrically the three relations create a cube. If the corners of the cube represent the pure, two-valued cases, then the multiplicative rule implies that four of the corners of the cube ($+++$, $+-$, $-+-$, $---$) are balanced and four ($-++$, $+-$, $++-$, $---$) are not balanced. What Wiest did was to use straight lines to connect the four balanced corners inside the cube. He thus used simple, straight lines to generalize beyond the pure two-valued triads. In doing this, what he created inside the cube was a tetrahedron—a three-sided pyramid. Wiest assumed that any triad on the surface of the tetrahedron or inside the tetrahedron was balanced. What

Wellens and Thistlethwaite did was to develop alternative, algebraic expressions, or models, that enabled the prediction of any third relation given values for the other two relations. The alternative models differed simply in the assumptions regarding the array of points inside the tetrahedron. Of course, as the relations become less and less polarized the interior of the tetrahedron increases in size and the possible predictions will differ more widely. Research by Wiest (1965), Wellens and Thistlethwaite (1971a, 1971b), and Tashakkori and Insko (1979, 1981) yielded encouraging initial support for the general approach (cf. Insko, 1984, pp. 119–140, for an overview).

The tetrahedron model provides an alternative interpretation of some results obtained by Klar and Pol (reported by Kruglanski & Klar, 1987), indicating that balance effects occur more strongly with sentiment relations implying the most rated “agreement.” For example, they report that “venerates” was rated as suggesting the most agreement, “is impressed by” as suggesting medium agreement, and “fancies” as suggesting weak agreement. These relations appear to differ in polarization. If this is, in fact, the case, Klar and Pol's results can be accounted for by the tetrahedron model.

Some readers may be confused by the fact that I initially discussed the multiplicative rule by explicitly limiting it to two-valued, plus or minus, distinctions. Thus, as in the initial example, I restricted the similarity–dissimilarity relation to the same–different distinction. Such a restriction is necessary to demonstrate the overlap with two-valued logic. This restriction, however, is an obvious oversimplification. Human thought is capable of more than just two-valued distinctions. On the other hand, the tetrahedron model implies that as long as the relations are at least moderately polarized, the implications of the multiplicative rule will approximately hold. The aforementioned example from the Klar and Pol experiment actually makes this point quite well. We could regard inferences in situations in which the relations are not completely polarized as a kind of “fuzzy logic.”⁴

⁴Within philosophy the term *fuzzy logic* is used to refer to a controversial form of logic (cf. Haack, 1978, pp. 165–167; Haack, 1996, pp. 229–258). Haack (1978) characterized fuzzy logic as partially flowing from fuzzy set theory. In fuzzy set theory, as opposed to traditional two-valued set theory, the set membership of an element is a matter of degree represented by a real number between 0 and 1, where 0 represents no membership and 1 represents full membership. Thus if Person A belongs to the degree 0.3 to the set of tall people, it follows in fuzzy logic that the statement “A is tall” has the value 0.3, or has a low degree of truth. Haack (1996) stated that “I remain convinced, first, ... that truth does not come in degrees, and, second, that fuzzy logic is not a viable competitor of classical logic” (p. 230). My concern, of course, is in describing the psychological basis for an inference and not its logical appropriateness. If the tetrahedron model were characterized as a type of fuzzy logic, the fuzziness would be a function of the distance between the upper and lower tetrahedron boundaries and thus increase with a decrease in the polarity of the relations.

Potential Applicability of the Multiplicative Rule to Various Psychological Effects

If indeed the multiplicative rule is applicable to thought processes relating to persuasion, the rule should also be applicable to other psychological effects. Superficially, at least, this appears to be the case. Note that, in the older literature, the so-called secondary reinforcement effect (cf. Miller, 1951) involves a previously neutral stimulus taking on the valence of a circumstance (e.g., electric shock or food) with which that stimulus is repeatedly linked. Repeated pairing of a stimulus with a positive or negative affective state polarizes, or strengthens, the positive unit relation of the originally neutral stimulus and the affective state. Note also that, in the more recent literature, the so-called mere-categorization effect (e.g., Tajfel, 1970, 1981) involves an individual expressing a preference for another individual with whom he or she is categorized. In balance theory categorization is referred to as a *unit relation*; in symbolic logic categorization is expressed as *class inclusion*. Thus, assuming positive self-evaluation, it is consistent to have a positive evaluation of a person with whom one is categorized.⁵

Assuming positive self-evaluation—or high subjective well-being (cf. Diener & Diener, 1996)—a large class of effects appear to fit the multiplicative rule. Notable among these are various dissonance effects. Consider, for example, the well-known insufficient reward effect in which it has been demonstrated that exercising an apparent high choice to accept a small reward for engaging in counterattitudinal advocacy (e.g., telling a waiting experimental participant that the experiment, which is actually dull, will be fun) results in attitude change consistent with the direction of advocacy—if the advocacy results in negative consequences (e.g., Calder, Ross, & Insko, 1973; Cooper & Worchel, 1970; Hoyt, Henley, & Collins, 1972; Nel, Helmreich, & Aronson, 1969). For such a situation the relevant cognitive band is: “I (+) am responsible for (+) misleading the waiting participant (–).” One way of escaping the inconsistency would be to deny responsibility, but the presence of low reward (which suggests an internal cause) and high choice (which also suggests an internal cause) makes such denial implausible. Thus, assuming that the participant is reluctant to lower self-evaluation (“Let’s face it, I am the kind of immoral person who does this sort of thing”) or to believe that it is perfectly acceptable to mislead people in some important way, the participant takes the only remaining option of concluding that “I did not mislead the waiting

⁵This is not to deny that other balance processes (e.g., relating to reciprocated sentiment; cf. Rabbie, Schot, & Visser, 1989) might also be involved.

participant because the task really is enjoyable.” This, of course, destroys the third element in the aforementioned cognitive band and thus eliminates the inconsistency. Still another well-known dissonance effect relates to the demonstration that not playing with a forbidden toy in the context of a mild threat results in greater devaluation of that toy than does not playing with the toy in the context of a severe threat (e.g., Aronson & Carlsmith, 1963; Freedman, 1965). In this case the mild threat produces more convincing evidence that the decision not to play with the toy has an internal cause. Thus: “I (+) have chosen to avoid (–) an unattractive toy (–).”

There are many other examples from the dissonance literature, but the aforementioned illustrations will suffice. Of course, an emphasis on positive self-evaluation as an explanation of dissonance effects is not novel. Such an approach has been advocated by others—for example, Aronson (1968) and Schlenker (1982). I agree with this emphasis. Note, however, that an emphasis just upon a positive self is not sufficient. There needs to be a rule indicating why the self is threatened. The multiplicative rule is an obvious candidate—as illustrated by the aforementioned example of the self (+) being responsible for (+) misleading the waiting participant (–).

Someone might argue that we recognize a threat to the self, not because of logic, but because of self-defensive thought. Or in a somewhat similar vein, one might argue, as did McGuire (1960), that there is a fundamental distinction between logical thinking and wishful thinking. Note, however, that such a perspective is not parsimonious because it requires the postulation of one kind of thought for inferences that are self-relevant and another kind of thought for inferences that are not self-relevant. It is simpler to assume that logic (or a multivalued extension of logic) applies to both self-relevant and non-self-relevant thought.⁶

Steele (1988) and Steele and Liu (1983) provided a somewhat different perspective on dissonance, or choice, effects. They obtained evidence indicating that choice effects will not occur if the participant is allowed to affirm his or her self-worth independently. I regard this as an interesting illustration of Abelson’s (1959) point that sometimes inconsistency with an attitude object is not resolved but the attitude object is bolstered, thereby “drowning out” the inconsistency. If there is independent evidence for self-worth the partic-

⁶Schlenker (1982) supported his self-perspective with interesting evidence that the so-called reinforcement effect in the insufficient-reward situation (with high choice and high, or negative, consequences) is due to the avoidance of evidence for having been bribed (“I was not bribed with money to mislead the waiting participant because I really did enjoy the task”). Note that the avoidance (–) of having been bribed (–) is consistent with a positive (+) self. We tend to make the logical inference without realizing why that inference is so compelling.

ipant may, in some admittedly unspecified circumstances, be able to bolster self-esteem (“So what if smoking causes lung cancer, I am a basically decent person who is living a happy and productive life.”)⁷

Finally, I mention two self-relevant examples outside of the dissonance literature. These are the “concern with being liked” and the “concern with being right” explanations of conformity effects. Note that it is consistent for a positively evaluated self (+) to be characterized by (+) being liked (+) and also to be characterized by (+) being right (+). There is evidence that both of these concerns play a role in the production of Asch-type conformity effects (Insko, Drenan, Solomon, Smith, & Wade, 1983). However, these cognitive bands were not originally suggested to explain Asch-type conformity effects, but rather as the most promising of several possible explanations for so-called agreement effects in studies of hypothetical triads (cf. Insko, 1984, pp. 89–119). In retrospect, however, they bear an obvious similarity to Deutsch and Gerard’s (1955) widely cited notions of normative and informational social influence, as well as to Kelman’s (1961) concepts of compliance and internalization.⁸

Of course, the fact that the multiplicative rule appears to fit such a diverse array of effects does not demonstrate that the rule is the correct explanation of such effects, or even part of the correct explanation. Certainly, however, the fit is sufficiently striking to indicate that the rule merits serious consideration.⁹

Balance Theory, the Self, and Hedonism

According to Kruglanski and Thompson, “the LET assumes that all instances of knowledge formation, including persuasion, are potentially impacted by a broad variety of motivations that affect the course of the judgmental process; that is, its extent (or depth) and direction.” I find much of what they have to say about motivation, and various needs, quite reasonable. It

⁷Abelson (1959) provided an interesting discussion of various reactions to inconsistency. An obvious problem, however, is that balance theory has not been developed to specify which reaction will occur in any specific situation.

⁸Kelman’s third concept of identification is an even more obvious example of a balanced relation.

⁹Greenwald and Banaji (1998) reviewed an even larger array of effects that they convincingly related to self-esteem. They, however, did not discuss each effect in the context of the multiplicative rule. On the other hand, they did point out that these effects have been interpreted from a variety of theoretical perspectives and acknowledge that such interpretations “describe plausible mechanisms by which implicit self-esteem may operate” (p. 14). A footnote attached to this statement gives “Heider’s (1958) balance theory” as “one example.” Cialdini and De Nicholas (1989) similarly reviewed a set of self-relevant studies. They, however, explicitly offered a balance theory interpretation.

does appear, however, that the postulated importance of various needs flows less from clearly specified initial assumptions and more from additional assumptions. This is in contrast to the deduction of general hedonistic tendencies from the multiplicative rule and an assumed positive self.

Note, quite simply, that it is both balanced and hedonic for a positive self (+) to receive (+) a reward (+), and also to avoid (–) a cost (–). Likewise, it is imbalanced and anhedonic for a positive self (+) to receive (+) a cost (–) and to avoid (–) a reward (+). The parallel between balance and hedonism flows from the assumption of positive self-evaluation. Although this overlap between the multiplicative rule and hedonism may strike some as bizarre, note that the essential idea is captured in everyday, common parlance by the reference to “rational economic decisions.” With negative self-evaluation, of course, the relation between hedonism and balance is reversed.

Evolution and the Problem of Low Self-Evaluation: A Primal-Positive Sign

The parallel between hedonism and balance theory with an assumed positive self raises an interesting, but highly speculative, question concerning which of these processes was selected for in the course of evolution. This is an issue that John Thibaut and I used to enjoy discussing. Consider an organism without the capacity to seek out rewards (such as food and water) and to avoid costs (such as predators). Clearly such an organism (or its genes) would be selected against. The interesting question, however, is whether the selection would be for hedonism or for balance, or for something more primitive than either. Actually, it is hard to conceive of anything more primitive than a simple consistency rule. On the other hand, it defies credulity to assume that one-celled organisms possess a self. Perhaps, however, there was an early adaptation that resulted in one or more genes operating on an algorithm that assumed a positive sign for the organism, thereby making survival logical. As long as I am engaging in “off-the-wall” speculation, note, further, that perhaps such an assumed positive sign was a primitive beginning for what evolved into a self. Such an assumption is consistent with Sedikides and Skowronski’s (1997) argument regarding the evolution of a self in nonhuman animals—although they do not postulate anything quite as simple as a primal-positive sign.

A consideration in favor of the balance theory perspective is that it is more general than hedonism and can assume hedonism as a special case. First, hedonism only applies to self-relevant circumstances, whereas that is not true of two-valued logic. Second, unlike two-valued logic, hedonism does not apply to

circumstances in which there is low self-evaluation. There is, however, a question as to whether self-esteem is ever truly low.

One argument against enduring low self-esteem comes from balance theory itself. As Wiest (1965) pointed out, it is imbalanced for a person to evaluate negatively his or her own self-concept. In this case the semicycle (or dyad with directional relations) includes the person and the person's self-concept—with a person to self-concept sentiment relation and a person to self-concept unit relation (“I highly regard (+) my (+) self.”). Of course, this is only one of many possible considerations relating to self-evaluation—everyone has experiences implying low self-worth.¹⁰ Leary and his associates (Leary & Downs, 1995; Leary, Tambour, Terdal, & Downs, 1995), in fact, have proposed the interesting idea that variations in self-esteem are a gauge, a “sociometer,” of social exclusion.¹¹

The self-concept can be construed as a type of attitude structure in which the self-symbol at the center of the structure is an attitude object characterized by various abilities, physical characteristics, accomplishments, values, friendships, group memberships, and so on. The believed characterizations are unit relations between the self-symbol and various other attitude objects. Like all attitude structures, the self-structure may change as a result of new information or experience. On the other hand, change may be resisted. In the case of the usual attitude structure such resistance can take the form of *counterarguments*. In the case of a self-structure a counterargument is sometimes labeled a *rationalization*. Further, the attitude structure may be *fragmented* (Rosenberg, 1960). In the case of a self-structure, fragmentation is sometimes labeled as *dissociation*.

What is the difference between the self-structure and other attitude structures? One difference is that other attitude structures do not have a primal-positive sign associated with the central attitude object and thus can include positive belief linkage to negative objects

¹⁰In commenting on the therapeutic value of writing about personal experiences, King and Pennebaker (1998) observed that: “Our data suggest that it is critical for individuals to construct a story or narrative to explain the traumatic experience.” I am intrigued with the possibility that such narratives are a way of addressing the self-esteem inconsistencies with negative experiences.

¹¹Leary, Tambour, Terdal, and Downs (1995, p. 529) further argued that “rather than serving primarily to maintain one's inner sense of self, the self-esteem motive prompts people to behave in ways that maintain their connections with other people.” While not denying the obvious importance of connections with other people, my point is to call attention to the tendency toward consistency between self-esteem and all valenced experiences—such as the experiences that undoubtedly were associated with variations in the self-esteem of Robinson Crusoe even before the advent of Friday. (See Harter, 1993, for interesting evidence that self-esteem has an additive association with both social support and competence in important domains.)

(or negative belief linkages to positive objects) without being inconsistent. Note, quite simply, that it would be inconsistent to believe that “I” have failed an important test, but it would not be inconsistent to believe that a disliked other has failed an important test. Another difference relates to the fact that the person has a positive unit relation to the “central” object in a self-structure, whereas this may or may not be the case for other attitude structures (cf. Aron and associates' perspective regarding the inclusion of other in the self; Aron, Aron, & Smolian, 1992; Aron, Aron, Tudor, & Nelson, 1991).

Although this is not an appropriate context for reviewing the emerging literature on self-esteem, I point out that some of this literature is consistent with the assumption that low self-esteem involves inconsistency. For example, Campbell and Lavalley (1993) summarized evidence indicating that low-self-esteem individuals exhibit less stability in their trait ratings and in their moods than do high-self-esteem individuals, that low-self-esteem individuals are accepting of positive and negative feedback whereas high-self-esteem individuals are only accepting of positive feedback, and that, in general, low-self-esteem individuals have “self-concepts that are poorly articulated, confused, and uncertain” (p. 13).¹² Still other evidence consistent with the idea that low self-esteem involves inconsistency is Baumeister, Tice, and Hutton's (1989) finding (see also, Tice, 1993) that what some investigators label as *low* self-esteem is actually a score toward the middle of the range of possible self-esteem scores and is thus low in a relative sense but not in an absolute sense.¹³

Testability Dilemma

Finally, I would like to discuss the problem of testability, a problem that is shared by both the probabilogical models and the tetrahedron models. This is, again, an issue that is not addressed explicitly by Kruglanski and Thompson.

In a general sense the probabilogical models and the tetrahedron models are not testable. They are not testable because they do not specify for all situations the relevant relations or assumptions. Thus, if some partic-

¹²There is an interesting parallel between Rosenberg's (1960) idea that an attitude structure exists in a homeostatic state in which there may be either cognitive change followed by affective change or affective change followed by cognitive change and Campbell and Lavalley's idea that there is reciprocal relation between mood and the self-concept. Rosenberg used posthypnotic suggestion to induce affective change and Campbell and Lavalley used a mood induction to produce self-relevant feelings.

¹³I, however, do not wish to rule out the possibility that a few individuals may have self-concepts that in important domains are more negative than positive.

ular prediction fails, an investigator can always postulate some additional consideration to account for the failed prediction. This testability dilemma is common to the general issue as to whether persons do, or do not, always think logically and also to a hedonistic, or reward-cost, approach to human behavior.

Henle (1962) has a perceptive discussion of the general issue as to whether persons do or do not think logically. She pointed out that this is an old philosophical issue that is, in fact, not empirically resolvable. To document her case, she presented participants with a series of syllogisms relating to various everyday problems and then asked them to write out their judgments regarding the logical adequacy of the deductions and the grounds for their judgments. Additional data were collected through interviews. The data indicated that indeed many apparently illogical errors were made. She, however, illustrated how these errors could be accounted for by, for example, judging the conclusion on a factual basis, altering the meaning of a premise, omitting a premise, or adding additional premises. An example of this was mentioned earlier in the discussion of Heider's failure to recognize that $a \neq b$ and $b \neq c$ implies that $a = c$. Is this a result of illogical thinking, or, rather, Heider's failure to assume only two-valued distinctions (as he had previously done in his formulation of balance theory).

The testability problem is, of course, well-recognized in the context of the hedonistic, or reward-cost, perspective. Is someone's decision to be burned at the stake a disproof of hedonism, or, rather, an indication that the person is concerned with receiving the rewards of an afterlife, or avoiding the costs of renouncing deeply held beliefs, and so on? In view of the fact that the reward-cost perspective and the multiplicative rule (together with the assumption of a positive self) have similar implications, the fact that they share the testability dilemma is hardly surprising.

So where does this leave us? The fact that the general reward-cost perspective is not testable does not mean that we cannot test reward-cost predictions in specific situations. These predictions, however, will of necessity include assumptions regarding what rewards, costs, or both are relevant in the particular situation being investigated. An example is the test of the prediction that one reason why, in the context of the Prisoner's Dilemma Game, relations between groups are more competitive than are relations between individuals is the greater fear of another group than of another individual (cf. Insko & Schopler, 1998). An example relating more obviously to balance theory is the test of the prediction that the ability of attitudinal verbal reinforcements to modify interview responses flows from the reinforcement's conveying of information regarding the interviewer's approval of a certain direction of responding (the o to x relation) and the creation of positive rapport with the interviewer (the p to o

relation), thus setting up a p - o - x triad (Insko & Cialdini, 1971). Still a further example is Aronson and Cope's (1968) clever demonstration that "my enemy's enemy is my friend" and also that "my friend's friend is my friend."

Kruglanski and Thompson point out that their unimodel provides "considerable generative potential as a source of novel testable, predictions." I agree that this is true for certain specific situations—given additional assumptions regarding the relevant considerations in those situations. As a general model, however, it is no more testable than other logic-related formulations.

Epilogue

Thinking back over the last several years makes me wonder whether I would have better communicated with my colleagues if I had abandoned the term *balance* and simply referred to *two-valued logic*, and considered the problem as to how best to generalize beyond two-valued logic, and the problem of how to deal with self-evaluation. If I had done so, it would have been more obvious, perhaps, that any major disagreement with Kruglanski and Thompson is more apparent than real. Please note, however, that I do not mean in any way to detract from the originality of their argument regarding the confound between the communication-arguments versus source-expertise distinction and the length-complexity-positioning variable. The issue as to what determines processing is obviously an important problem—and a problem that is not solvable by the multiplicative rule alone.

Notes

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Chester A. Insko, Department of Psychology, University of North Carolina, Chapel Hill, NC 27599-3270.

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