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Self-Complexity and Well-Being: A Review and Research Synthesis

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We reviewed the extant literature examining Linville's (1985, 1987) self-complexity (SC) model. SC is a structural feature of people's self-knowledge. Linville (1987) proposed that SC serves as a cognitive buffer against extreme affective reactions to life events. We report results of two procedures: a classic meta-analysis and a more primitive vote-counting procedure. Overall, SC was negatively, but weakly, related to well-being, a relationship qualified by strong heterogeneity among studies. We found little support for SC as a stress buffer, but more support as a moderator of uplifting events. Several methodological and substantive variables (e.g., the type of well-being studied, the valence of SC, and characteristics of the samples and designs used) were associated with effect magnitude. We discuss implications for competing theories of self-structure and comment on the use of information theory in studying the self.

The construct of the "self" has had an uneasy position as a topic of psychological research, often being considered too elusive to be measured and studied (cf. Markus & Wurf, 1987; Westen, 1992). Nevertheless, psychologists from James (1890) on, have acknowledged the self's central role in behavior, affect, and cognition. James was the first to offer the distinction between the experiential self (the "I" or the self-as-knower) and the experienced self (the "me" or the self-as-known). This distinction is still employed today. Linville and Carlston (1994) referred to the notion of a "knower self" as the procedural knowledge that directs our actions, thoughts, and feelings and to the notion of a "known self" as the declarative knowledge we have about ourselves. The latter is synonymous with the term *self-concept.* This research synthesis is concerned with structural features of the self-concept, James's "me," or the declarative knowledge of the self.

Some theorists (e.g., Rogers, 1977; Wylie, 1974, 1979) viewed the self as a unitary construct. This view is implicit in the vast literature on self-esteem: Global self-esteem research is based, for the most part, on the predicate that people have a unitary self and that a single dimension can represent the valence of their feelings about their (unified) self-concept. Many social scientists, however, have presented an alternative, multifaceted, view of the self. Such a view can be found in the early works of James (1890), Kelly (1955), and Mead (1934). More recently, it has become a mainstay of social cognitive (e.g., Higgins, 1987; Markus, 1977), narrative (e.g., Gergen & Gergen, 1983), and even psychodynamic models (e.g., Westen, 1992). The self, as viewed by these theorists, is composed of various aspects, roles, perspectives, or "selves." Each of these selves corresponds to the knowledge we have about ourselves as we are in one role, relationship, perspective, and so on. From the multifaceted view of the self follows the understanding that individual differences may exist, not only in the content or the overall valence associated with the self (i.e., self-esteem; Wylie, 1974) but also in the organizational features of self-knowledge.

The exploration of cognitive organization or structure holds particular importance for psychologists interested in models of psychopathology and psychotherapy. It is a relatively unexplored frontier to cognitive clinical researchers, a hopeful venue after considerable disappointments elsewhere (Strauman, 1992). Despite the proven efficacy of cognitive therapy approaches (e.g., for depression; cf. Dobson, 1989; Hollon, Shelton, & Loosen, 1991), the etiological models informing such therapy have often not been supported. In the words of one group of reviewers (Haaga, Dyck, & Ernst, 1991), there is "little convincing support for causal hypotheses of cognitive theory" of depression (p. 231). Yet most tests of cognitive factors in depression have focused their sights only on a

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subset of the cognitive variables from the "taxonomy of cognitions" (cf. Hollon & Garber, 1988; Ingram, 1990): cognitive content (e.g., the thoughts that distinguish depressives from nondepressives) or cognitive processes (the mental procedures that the two groups utilize differently; cf. Mineka, Rafaeli-Mor, & Yovel, in press). Cognitive structural properties are the third and relatively understudied type of cognitions within this taxonomy. This relative inattention is surprising, given the frequent discussion of the depressive schema construct in clinical research and its definition as a structured knowledge set (Beck, 1967).

A positive outlier in this underexplored area, self-complexity (SC) is one cognitive structural feature that has received a relatively large amount of attention, particularly in its relationship to mood and psychopathology. Different SC models have emerged that have sprung from diverse sources, including constructivist psychology, social cognition, and the object relations school of thought. We focus on one definition of SC, a social cognitive one developed by Linville (1982a, 1985, 1987) and proving to be of great interest to both social and clinical psychology researchers. Linville's (1985) SC has been used to address topics as diverse as depression (Linville, 1987; Rafaeli-Mor & Brown, 1997), trauma (Morgan & Janoff-Bulman, 1994), escape from self (Dixon & Baumeister, 1991), narcissism (Rhodewalt & Morf, 1995), eating disorders (Knolbach, 1994), and coping with the successes and failures of everyday life (e.g., Campbell, Chew, & Scratchley, 1991; Miller, Omens, & Delvadia, 1991).

The aim of this research synthesis was to review the considerable literature that has amassed on Linville's (1985) SC construct and to glean from it insights about the nature of this construct that may not be apparent when studies are considered separately. In particular, we seek to clarify the relationship between Linville's (1985) SC and well-being—both within the context of life events, successes, and failures, as well as outside that context—as the simple ("zero-order") association. To begin this clarification, we review the history of the SC construct, placing it within the broader landscape of the literature exploring cognitive complexity in general.

Cognitive Structure and Cognitive Complexity

The study of cognitive complexity has its roots in cognitive and cognitive structure models of personality that gained prominence in the 1950s and 1960s. Some earlier examples of such approaches date back to the work of Lewin (1935) and the gestalt school, as well as the innovative neurocognitive work of Hebb (1949). However, direct examinations of individual differences in the structure of self-knowledge began with Kelly's (1955) pioneering work on personal constructs. Kelly defined *personal constructs* as the dimensions underlying particular knowledge domains, such as the self. Kelly's personal construct psychology can be seen as a precursor of modern social cognitive models (although many classify it as a humanistic, rather than a cognitive, approach). The essence of this theory is that the idiographic interpretations of reality and, more specifically, the distinctive dimensions each person uses to organize the perceptual world are particularly potent sources of individual differences in personality, emotion, and behavior.

One of the first explicit discussions of complexity appeared in Bieri's (e.g., 1955) work, where it was seen as a feature of the individual's perceptual system. Bieri (1955, 1966) approached the question of SC from a personal constructs psychology perspective and posited that complexity or simplicity reflects the number of construct dimensions with which persons can construe the behavior of others or of themselves. The more complex a person is, the more constructs he or she will use to describe or understand others.

Two elemental features of cognitive structure, differentiation and integration, figure prominently in Kelly's (1955) and Bieri's (1966) models and in subsequent models and should be explicitly defined. Both of these features characterize a person's knowledge of a particular domain: *Differentiation* refers to the degree to which a cognitive domain contains multiple distinct elements, whereas *integration* refers to the degree of coherence, interrelatedness, or unity in the cognitive domain.

In a comprehensive review of the cognitive complexity construct, Streufert and Streufert (1978) surveyed the work of Kelly, Bieri, Scott, Zajonc, and other students of cognitive structure. Their review helps clarify several important issues. First, in the late 1970s, after more than 2 decades of research, the field seemed to be plagued with confusing, inconsistent terminology, to the degree that "it [was] not possible to determine ... what the conceptualizations of one theorist mean in terms of the conceptualizations of another" (Streufert & Streufert, 1978, p. 20). This was particularly true regarding the central concepts of differentiation and integration: Subtly different theoretical definitions of these constructs led to profoundly different indexes, which were often empirically unrelated.

As a further confusion, Streufert and Streufert (1978) noted that the term *complexity* may actually refer to two separate constructs: the complexity of the beholder (i.e., an individual's complex perceptual system) or of the beheld (i.e., a complex stimulus domain). Indeed, the usage of the term within various theories perpetuated this confusion. For example, both Kelly (1955) and Scott (1962, 1969) defined complexity as a function of the number of binary dimensions

(i.e., constructs) used by a person to describe a particular domain: Complex individuals were those who utilized more numerous constructs in perceiving their social world. In contrast, Zajonc (1960) and Wyer (1964) focused on the complexity of the objects: Complex cognitive (or social cognitive) domains were those that included many distinguishable instances. In their view, therefore, complexity was not a property of the individual but of the object domain.

If this was not confusing enough, the very notion of complexity (whether of an individual or of a domain) was used to refer to differentiation, integration, or to some balance of these two features (cf. Emmons & King, 1989). For example, Crocket's (1965) index of complexity reflected the number of distinct elements in a particular domain, that is, differentiation. In contrast, Zajonc's (1960) index of complexity reflected the hierarchical organization of distinct elements, that is, a combination of differentiation and integration.

In short, Streufert and Streufert (1978) clearly demonstrated that the excitement about applying information theory to social psychological questions generated a rich, but often disorganized, literature. As we show, it is significant that investigations of SC arose with this rich but disorganized literature as a backdrop.

Self-Structure

Kelly's (1955) personal constructs psychology, as well as similar models (e.g., Bieri, 1955, 1966; Scott, 1969; Zajonc, 1960) did not focus exclusively on the structure of self-knowledge. Instead, these models suggested that structural aspects of cognition (e.g., cognitive complexity) would characterize a person in any perceptual domain. Thus, a person could hold a cognitively differentiated (or undifferentiated) view of the domains of nations, celebrities, his or her own acquaintances, and so on (Scott, 1969). Scott's (1962) theory of cognitive structure, as well as more recent cognitive theories of personality (e.g., Bandura, 1986; Cantor & Kihlstrom, 1987; cf. Cervone & Shoda, 1999), discussed the fallacy of assuming cross-situational consistency in such capacities as cognitive complexity. These writers have demonstrated the need for assessing domain-specific properties of cognition. Indeed, the decades following the advent of the basic cognitive structural models began yielding more context-specific analyses of cognitive organization. Chief among the specific contexts or domains explored was self-knowledge. (For other examples, see M. W. Baldwin [1992] on relationship schemata or scripts, Feldman [1995] on organization of semantic space of emotions, or Linville [1982b] on organization of knowledge about social groups.)

The structure of knowledge in any domain affects the processing of information about that domain. Nonetheless, even the early models (e.g., Bieri, 1955) recognized the particular importance of the organization of self-knowledge. Whether or not the organization was unique to the domain of the self, it is the organization of this important domain that was thought to mediate much of behavior and of emotional experience. The flood of interest in self-structure, and in SC in particular, is therefore not surprising.

Studies of structural features of the self examine how information regarding the self and specific self-beliefs are organized (Campbell et al., 1996). The last 15 years have brought a host of proposed structural characteristics of self-knowledge and of studies examining these characteristics. Showers (e.g., 1992) discussed the property of compartmentalization, or the degree to which one partitions differently valenced self-knowledge into distinct categories. Higgins and colleagues (e.g., Higgins, Bond, Klein, & Strauman, 1986; Strauman & Higgins, 1987), as well as Ogilvie (1987), explored self-discrepancies or the degree to which the real self diverges from the ideal self, ought self, or the undesired self. Campbell (1990) introduced the construct of *self-concept clarity,* the degree to which the self is clearly defined. Donahue, Robins, Roberts, and John (1993) examined the integration of self-dimensions. Finally, Linville (1985, 1987) coined the term *self-complexity*, which she operationalized as the dimensionality underlying the self-concept.

All of these self-structure properties have been discussed in relation to well-being, with various degrees of empirical support. Particular controversy has been associated with the property of SC, its relationship with well-being, and its role as a moderator of stressful or pleasant life events.

Models of SC

Linville (1982a) noted that "[self-]complexity can be thought of in a number of ways, depending on one's choice of representation ... the concept is multifaceted, model-specific, and sometimes fuzzy" (p. 81). Indeed, various authors who have explored complexity of the self-system have done so in diverse ways, often choosing diverging representations as the basis for their operationalization of SC. Most of these investigations sprang from the cognitive structure paradigms reviewed earlier, the same paradigms developed for the study of cognitive complexity. As a consequence, attempting to carry out a comprehensive review of the different SC models leads us to the same frustration encountered 2 decades ago by Streufert and Streufert (1978) in their review of cognitive complexity models. A consensual definition of SC is impossible because various models of SC have equated it with distinct operationalizations, with some emphasizing the differentiation of the self-concept, others its integration, and

still others a combination of both differentiation and integration. We briefly review the divergent SC formulations, which serve as a context for Linville's (1985) model, the focus of this research synthesis. Whenever possible, we mention the existing findings regarding the relationship between well-being and each of the SC versions. Following this brief review, we explain our rationale for concentrating solely on Linville's (1985) SC in the remainder of this research synthesis.

Several models of SC arose from the cognitive structure literature of the 1950s and 1960s. These include Stein's (1994), Anderson's (1992), and Ziller, Martell, and Morrison's (1977) versions of SC. Some of these models (e.g., Ziller et al., 1977) emphasized differentiation alone. Other models considered both differentiation and integration, at times separately and at other times uniting or combining them in conceptualizing SC. For example, Anderson adopted Zajonc's (1960) cognitive complexity procedure in which participants are first asked to generate self-descriptive traits, then sort and organize these traits into hierarchical groups, and finally determine which traits are interrelated. Anderson examined several indexes (of differentiation and of centrality) and kept them separate but referred to them communally as "complexity variables." Anderson's (and Zajonc's) centrality index affords a way of looking at the integration of a self-schema, namely the organization of the schema around one central issue (e.g., scholastic pursuits). Academic centrality scores were high when the scholastic traits were strongly interrelated to other traits. In Anderson's studies, all complexity variables (i.e., both differentiation and centrality indexes) were unrelated to the participants' response to self-relevant feedback (e.g., elementary school report cards).

Although Stein (1994) also adopted Zajonc's (1960) procedure, she formed an amalgam of the differentiation and unity indexes of self-schemata. Participants were seen as higher in complexity if their self-schemata were both differentiated and unified (i.e., integrated). Differentiation scores were high when participants generated more self-descriptive characteristics; unity scores were high when participants indicated that changes in any characteristics led to changes in other characteristics. Interestingly, Stein's low-complexity participants were those who rated themselves more favorably after a negative feedback.

Rosenberg (1977) and his colleagues (e.g., Woolfolk et al., 1999) developed a related yet somewhat more sophisticated approach to SC. This approach uses a hierarchical classes clustering algorithm (HICLAS, a method described at length by DeBoeck and Rosenberg, 1988) that derives from network theory. This approach represents the structure of cognition by categorizing the participants' responses into classes or clusters that correspond to "nodes of cognition" (Woolfolk et al., 1999). In other words, this approach could tap the degrees of both differentiation and integration. However, the indexes typically used to reflect SC in such studies are more directly affected by the degree of differentiation or discrimination among self-aspects and focus less on the integration of cognitive structure.

Several studies have used the HICLAS approach in examining the role of SC in affect and psychopathology. These studies, by Gara and colleagues (Gara, Rosenberg, & Mueller, 1989; Gara et al., 1993) and Woolfolk and colleagues (Woolfolk et al., 1999; Woolfolk, Novalany, Gara, Allen, & Polino, 1995), converged to indicate the need to separate the complexity of positive and negative self-knowledge. Specifically, these authors found that SC is not uniform across differently valenced information; individuals who are complex in their organization of positive information are not necessarily complex in the organization of negative information, and vice versa. Moreover, these authors repeatedly note (a) an absence of any relationship between positive SC and psychopathology and (b) a positive relationship between negative SC and psychopathology; we return to this question of valenced SC in the discussion of this meta-analysis.

Cognitive structure approaches based on Rosenberg's (1977) HICLAS model, or on Zajonc's (1960) influential work, mark one end of an implicit–explicit continuum; these models attempt to measure cognitive structure through implicit, nontransparent means. By contrast, a model presented by Evans (1994), which uses the self-report Self-Complexity Inventory, marks the explicit end. In this task, participants are provided with several scenarios that describe experiences in a particular domain (e.g., being ignored at a party) and are explicitly asked to note their effects on a variety of other domains (e.g., job competence, scholastic ability). This index sets out to assess the degree to which particular domains of self-concept are interrelated; thus, it is actually an index of integration. Using this index of complexity, Evans (1994; see also Evans & Seaman, 2000) found high-complexity participants to have more mature defenses and to report higher global self-worth.

Finally, complexity, integration, and differentiation have had important roles in psychoanalytic and developmental models (e.g., Blatt & Lerner, 1983; Leigh, Westen, Barends, Mendel, & Byers, 1992). Using techniques such as open-ended interviews, the Thematic Apperception Test (Murray, 1943), and the Rorschach, these authors coded complexity as a function of factors such as the perspective taking, ambivalence, and elaboration present in the free responses. They suggest that a complex perceptual tendency is related to chronological and psychological maturation. In support of this suggestion, these authors have found that individuals with severe psychopathology

have "objects" (i.e., self and other representations) that are not complex.

The main conclusion we draw from this limited survey of the existing alternative conceptualizations of SC is that the field lacks consensual organizing principles: There appears to be little agreement on what constitutes SC. Authors using the term *SC* have provided profoundly different answers to important questions: Is complexity in the self-as-known or the self-as-knower? Does it reflect differentiation, integration, or both? Must it assume a certain structural model (such as Scott's [1969] or Kelly's [1955]), or can it be explored without presupposing such a model? Given the disparate answers to these questions, it is less surprising to discover that SC can be advantageous, harmful, or neither for emotional well-being. If SC is not a single construct, why should it have a single relationship with well-being?

In an attempt to bring some organization to the SC field, we have chosen to focus this review exclusively on Linville's (1982a) SC model. The broader SC literature is so disparate that it would be difficult to pull together, and impossible to review, meta-analytically. In contrast, we believe that focusing on one prominent theory and using it as an anchor point can provide some organization. Of the various SC models, Linville's (1985) model stands out in several ways. First, it has generated to greatest amount of interest in social and clinical psychology; by sheer quantity, it provides the richest literature to review. Relatedly, Linville's (1985) version of SC is the only one that has reached broad recognition within psychology, entering important graduate and undergraduate texts. These two reasons alone warrant conducting a review of this model's empirical standing. Moreover, whereas most SC models have suggested that complex individuals process information differently and (in particular) respond in more moderate ways to life events, Linville's (1985) model has been unique in detailing the processes that bring about this relationship. Doing so, it brought together the cognitive structure tradition of Scott (1969), Bieri (1966), and others, with the language of more modern social cognitive approaches. In the next section, we review this prominent model in greater detail.

Linville's SC Model (1985)

In Linville's work, complexity is synonymous with differentiation; like some of the other versions of SC (e.g., Rosenberg and his colleagues' HICLAS model, 1977), it eschews a focus on integration. Linville (1985) defined SC as a "function of two things: the number of aspects that one uses to cognitively organize knowledge about the self, and the degree of relatedness of these aspects" (p. 97). Within this model, complex individuals are the ones who utilize more aspects in their self-description and who have little or no overlap among these aspects.

As was the case with several other models of SC (e.g., Stein, 1994; Ziller et al., 1977), Linville's (1985) SC arose from the cognitive structure literature, in this case from Scott's (1969) model of the structure of cognitive space. Linville's (1985) SC model followed her earlier work on in-group heterogeneity and out-group homogeneity (Linville, 1982b; Linville & Jones, 1980). In the earlier studies, complex (i.e., heterogeneous) concepts of other people were found to evoke more moderate affective reactions or evaluative judgments, whereas simpler (i.e., homogeneous) concepts were found to evoke evaluations and reactions that were more extreme. For example, Linville (1982b) found that the extremity of ratings of both favorable and unfavorable vignettes about an elderly target was significantly and negatively correlated with the complexity of the rater's concept of "elderly males."

Based on her in-group and out-group findings, Linville (1982a, 1985) posited a model that implicated SC in extreme affective reactions to self-relevant external events. Two cognitive-emotional mechanisms were thought to underlie this relation:

- Quantity of self-aspects: Linville (1982a, 1985) suggested that when a stressful event occurs, it affects the self-aspect most pertinent to the stressor. She argued that for a person with numerous self-aspects (high quantity), the affected self-aspect is but one of many aspects. Therefore, a relatively small proportion of the total self will be affected. By contrast, a stressor will negatively affect a greater proportion of the total self in persons who have fewer aspects in their self-concept.
- Overlap among self-aspects: Linville (1982a, 1985) posited that higher overlap (lesser distinction) among the various self-aspects allows for a spill-over effect; "feelings and inferences associated with the originally activated self-aspect spill over and color feelings and inferences regarding associated self-aspects" (Linville, 1987, p. 664). A stressor that affects one self-aspect will initiate a process of activation spillover to overlapping self-aspects; it cannot do so, however, if no overlapping aspects exist.

Linville (1987) viewed the propensity to differentiate between self-aspects as a trait-like feature. As such, it should show reasonable temporal and cross-situational consistency. Indeed, several investigations have reported adequate test–retest reliability for SC (e.g., $r = .72$, as reported in Linville, 1987). As a trait-like variable, SC should also be at least partly independent of the perceiver's reality; indeed, SC theory posits that different individuals experiencing the same social reality may manifest varying levels of SC. For example, a low-SC person may construe himself to have only two

roles—husband and teacher—and to be quite similar across both (lazy, friendly, and outspoken). As a more self-complex person (albeit with the same social reality), he might notice and cognitively represent differences among his relationshipships with his students, his colleagues, his department head, and in different facets of his relationshipship with his wife. He could note that he is anxious and subdued in some roles, dominant and affectionate in others.

To operationalize SC, Linville (1982a, 1985) adopted the dimensionality statistic (*H;* Attneave, 1959; Scott, 1969), an index borrowed from information theory. Information theory, which is a technique for the quantification of uncertainty and of information, was first developed to address problems in telegraphy and communications (Shannon & Weaver, 1949; cf. Brody, 1971). *H* can be thought of as a measure of the variance, or variability, in nominal-scale information: It provides a measure of the unpredictability of nonquantitative data (Brody, 1971). Because the *H* statistic is so central to most studies examining SC, its usage warrants some attention.

Participants in studies using this operationalization are typically given a list of trait words and are asked to sort these into groups that describe different aspects or roles in their lives. This trait sorting can yield a matrix, in which an aspect or role (column) is marked with 1s for every trait (row) that is descriptive of it and with 0s for the traits that are not. The *H* statistic is computed on an individual's trait-sort matrix and can be interpreted as reflecting the minimal number of independent binary attributes needed to reproduce a trait sort (i.e., the number of dimensions that underlie the sort). Hypothetically, and somewhat ironically, utter randomness or disorganization of self-knowledge could appear as complexity under this definition, a problem noted even by the developer of the complexity index (Scott, 1962). In other words, a high *H* score could indicate a complex (integrated) or a random (fragmented) organization.

Linville (1985, 1987) has suggested that high SC serves as a buffer against stress-related illness and depression; she proposed that its antipode, a simplistic view of oneself, is a diathesis for such ailments or disorders. Linville's own studies (1985, 1987), as well as some other studies following similar reasoning (e.g., Kalthoff & Neimeyer, 1993, Study 1), provided support for this hypothesized relationship. Others, using identical (e.g., Dixon & Baumeister, 1991; Koenig, 1989) or similar (e.g., Kalthoff & Neimeyer, 1993, Studies 2 and 3) designs, have provided only partial support or failed to support this model altogether. These inconsistent results have drawn the attention of several investigators who have critiqued the SC affective extremity model on a number of counts, at times suggesting modifications of the model.

Some (e.g., Woolfolk et al., 1995) have questioned the model's claim that complexity of the self-concept, independent of the content of the concept, is a meaningful predictor of affect or well-being. Studies along this line have found it useful to partition SC into the complexity of positive and negative self-knowledge and have noted systematic changes in *H* scores depending on the valence makeup of the trait list (e.g., Morgan & Janoff-Bulman, 1994; Woolfolk et al., 1995).

Some findings implicitly question the assumption that SC is a trait-like feature. For example, Salovey (1992) demonstrated SC's malleability by using it as a measure of state self-focused attention. Niedenthal, Setterlund, and Wherry's (1992) study similarly argued that SC may not be a stable unitary trait. Instead, they suggested that it is necessary to consider the tense of the self-concept; indeed, their obtained relationship between the complexity of the actual and the possible self demonstrated this nontraitedness.

Additional criticism has focused on Linville's (1985, 1987) use of the *H* statistic. For example, Brown and Rafaeli-Mor (2000) and Rafaeli-Mor, Gotlib, and Revelle (1999) analyzed the relation of the *H* statistic to the two presumed components of SC: quantity of self-aspects and overlap among them. These authors found *H* to be related in the predicted (positive) direction to the number of aspects but to be unrelated (Brown & Rafaeli-Mor, 2000) or related in the wrong direction (Rafaeli-Mor et al., 1999) to measures of overlap.

Theoretical Alternatives

Several theorists (Campbell, 1990; Campbell, Assanand, & DiPaula, 2000; Donahue et al., 1993) have suggested alternative hypotheses regarding the relationship of SC or differentiation and well-being. Donahue et al. posited that individuals who score high on measures such as Linville's (1985) are best characterized as fragmented rather than complex. Their view is based on Erikson's (1959) notion of identity and on a definition of complexity that highlights both differentiation and integration. In this view, fragmentation (the antipode to integration) is characteristic of poor well-being and is thought to play a part in several forms of psychopathology (e.g., anxiety, Block, 1961; depression, Gara et al., 1993; schizophrenia, Gara et al., 1989). Donahue et al. cite the findings of Block (1961) on ego identity, role variability, and well-being. They concur with Block's view, characterizing fragmented persons as ones with "role diffusion, where an individual is an interpersonal chameleon, with no inner core of identity, fitfully reacting in all ways to all people" (Block, 1961, p. 392). Thus, rather than buffering the adverse effects of life stressors, a complex organization of self-knowledge may put a person at increased risk for emotional suffering or physical sickness (e.g., Block, 1961).

Campbell and her colleagues (e.g., Campbell, 1990; Campbell et al., 1991) presented the construct of self-concept clarity, which, as one of its features, addresses the internal consistency of self-descriptions. Self-concept clarity is broader in its scope and encompasses the extremity, confidence, and temporal stability of the self-concept. However, the internal consistency component of self-clarity is quite similar in definition to the overlap component of SC. Both are indicators of self-concept integration. Nonetheless, the two were proposed to exert divergent effects on psychological well-being: a stabilizing, beneficial effect in Campbell's (1990) model, and an exacerbating, spill-over effect in Linville's (1985) model. Because of this contrast, we view Campbell's (1990) model as closer to Donahue et al.'s (1993).

A recent study by Rafaeli-Mor and Brown (1997) raised an interesting possibility regarding the SC/well-being relationship. In this study, SC (derived from the same card-sorting task but measured using alternative indexes, not Scott's [1969] *H*) was found to be a buffer only for severe stress. Under elevated levels of minor stressors (i.e., hassles), high SC proved to be a liability to participants' well-being. Thus, it is possible that Linville's (1985) SC model is accurate under conditions of acute stress, whereas Donahue et al.'s (1993) differentiation model applies under those of diffuse, or minor, stress.

Summary: SC Theory and Questions About Linville's Model

Many authors have been enthusiastic about the idea that the self has structural properties and that some of these properties (e.g., complexity) may play a part in affective reactions. A broad literature has developed, but this literature is hard (if not impossible) to integrate. Within it, Linville's (1985) SC model has emerged as unique in the amount of attention and research it has generated. However, although this model has been met with much excitement, it has also generated multiple critiques focused on the definitions, operationalization, and reliability of the model and of the measure typically used in it. These have included the possible separability of positive and negative SC (Woolfolk et al., 1995), the internal consistency problems of the *H* statistic (Rafaeli-Mor et al., 1999), the questionable traitedness of SC (Salovey, 1992), and the need to consider the severity of the stressful life events that are examined (Rafaeli-Mor & Brown, 1997).

Taking these critiques into consideration in further examinations of the SC/well-being relationship may indeed clarify some of the inconsistency in these findings. However, a natural and useful complement to these disparate suggestions would be a comprehensive research synthesis, bringing together much of the existing evidence on the relationship between SC and well-being. Such a review can identify substantial and methodological factors that account for the diversity of findings in the field. More important, such a review would allow us to contrast the alternative theoretical models reviewed previously in this article. With these goals in mind, we designed this research synthesis.

Overview of the Research Synthesis

In this research synthesis, we explore Linville's (1985) SC construct and attempt to find answers to the various methodological and theoretical debates in the field, particularly those raised in the previous section. To do so, the overall effect (aggregated across studies) is computed, but more important, study-level characteristics are coded and we examine the relationship between these characteristics and the sign and magnitude of the effect sizes.

Specifically, to contrast the prediction of the SC model with those of the self-concept clarity (Campbell et al., 1991) and differentiation (Donahue et al., 1993) models, we examine SC's relationship with well-being under various levels of stress. Linville (1985, 1987) has suggested that high SC will be associated with more positive outcomes following stressful or negative events, with more negative outcomes following pleasant or rewarding events, and with neither positive nor negative outcomes in the absence of life events. By contrast, implicit in the self-concept clarity and differentiation models is the idea that even under stressful conditions, maintaining a fragmented self-concept may not be advantageous and may in fact serve as a liability. Finally, research by Rafaeli-Mor and Brown (1997) demonstrated that different types of negative life events may interact differently with SC. Thus, one key study characteristic is the type and intensity of the stress examined in the study.

In addition to examining the way in which stress is incorporated into different studies, we examine whether other design, measurement, or population factors affect the magnitude of the relationship between SC and well-being. Some of these design factors (e.g., the use of overall SC in contrast to the distinction between positive and negative [valenced] SC) were mentioned earlier. These factors are derived from existing explanations of inconsistency in the field (e.g., in the case of valenced SC, Woolfolk et al.'s [1995] work). Other factors were identified during the planning of this research synthesis, following a comprehensive reading of the primary research reports. We discuss at length these factors, and the predictions based on them, in subsequent sections.

The synthesis includes two analyses: a meta-analysis of the broadest set of available studies that have related SC and well-being and a cruder (vote-counting) analysis of a smaller set of studies that specifically test the pos-

ited stress-buffering effect of SC. In the next section, we discuss design choices we made regarding the constructs of SC, well-being, and stress that cut across both the meta-analysis and the vote-counting analysis.

Issues in the Design of the Synthesis

Measurement of SC. As noted earlier, several methodologies have been used to measure SC. This synthesis includes only those studies that utilized Scott's (1969) *H*, the index of dimensionality derived from the self-descriptive trait sort. Most often, this index was computed following a card-sorting task modeled on the one used by Linville (1982a, 1987). Several different indexes have been computed on such matrices. Some are related to the SC concept (e.g., Block's [1961] role variability statistic, or Rafaeli-Mor et al.'s [1999] indexes of self-aspect quantity and overlap) and may be empirically or theoretically superior to *H*. Indeed, we have been, and continue to be, critical of the use of the *H* statistic as an SC index; in our discussion, we return to the question of what is the most appropriate operationalization of SC. Nonetheless, this effort was restricted to studies computing Scott's (1969) *H* for two reasons. First, *H* is purported to be a combined measure of the two components of SC: the number of self-aspects and the spillover among them. Using *H* as the SC measure allows for an overall test of the SC model. This goal is distinct from that of testing the two components, an interesting endeavor in its own right but one that falls outside the scope of this review. Second, *H* is the most commonly used measure and therefore allows for the aggregation of the largest number of studies.

Another debate in the SC literature revolves around the makeup, and particularly the valence, of the trait-word list sorted by participants in SC studies. SC and other self-concept structural parameters were originally thought to be orthogonal to the content of the self-concept. In other words, Linville's (1985) SC model would suggest that the actual identity of the words should not affect the dimensionality of the trait sort (for a similar reading of that work, see Campbell et al., 2000). Empirical evidence to the contrary has led some (Morgan & Janoff-Bulman, 1994; Woolfolk et al., 1995) to posit separate constructs of positive and negative SC (positive *H* and negative *H*). Operationally, these measures are identical to the *H* statistic but are computed only on the positive or negative traits in a sorting task, respectively. Rafaeli-Mor et al. (1999), in exploring the internal consistency of SC indexes, used positive-*H* and negative-*H* scores in computing the cross-valence, "worst split-half" reliability of *H*. In contrast to indexes tapping the components of SC (i.e., quantity and overlap of self-aspects), *H* indeed proved to have very poor cross-valence consistency.

To examine the effect of the valence of the trait words on the relationship between SC and well-being, we chose the following selection strategy. When studies provided information about the relationship of all three (positive, negative, and overall) *H* measures with well-being, the relationship based on overall *H* was chosen. When only the valenced (i.e., positive or negative) *H* measures were given, one was selected randomly. Whenever possible, we recorded the percent of negative traits within the list used to generate the *H* index.

Assessment of psychological well-being. Several types of well-being measures were included in this research synthesis: mood (or affect), depression, and self-esteem scales and relevant psychiatric diagnoses, specifically major depression.

Linville (1985) demonstrated a buffering effect of SC on both mood and self-evaluation (i.e., self-esteem). Thus, studies measuring or manipulating either mood or self-esteem were deemed to be within the scope of this synthesis as they attempted to replicate the original demonstration of the SC construct.

Linville (1987) expanded her predictions to the buffering effect of SC on (self-reported) depression, perceived stress, and health symptoms. The affective core of depression, especially as a subclinical phenomenon, makes studies that use measures of depression easy to compare to those measuring affect or self-esteem. Therefore, samples from such studies (e.g., Linville, 1987; Rafaeli-Mor et al., 1999) were included in the synthesis.

Because we have chosen to focus on the relationship of SC with psychological well-being outcomes, some outcome constructs were deemed inappropriate for this review. First, although the subjective perception of stress (as measured, e.g., by the Perceived Stress Scale [S. Cohen, Kamarck, & Mermelstein, 1983] in Linville, 1987) plays a major part in emotion and emotional disorders (cf. Lazarus & Folkman, 1987), it is best characterized as a mediating variable and not an outcome variable in itself. Thus, it falls outside the scope of this review. Second, physical symptoms (such as those measured by the Cohen Hoberman Inventory of Physical Symptoms [S. Cohen & Hoberman, 1983] in Linville, 1987) were not included because of our focus on psychological well-being. Finally, some investigations (e.g., Beatty-Leask, 1994; Gardner, 1997) explored the relationship between SC and psychiatric diagnoses. Only diagnoses of disorders with an affective core (i.e., mood disorders) were used as measures of psychological well-being for this analysis. For example, in Beatty-Leask's study, only the control group and the group with depression were used; the data for the patients with bulimia were omitted.

Naturally, the different well-being measures (i.e., depression, mood, self-esteem) need to be coded in a similar direction to be included in one synthesis. Be-

cause the superordinate term *well-being* denotes pleasant or appetitive feelings, we kept positive constructs (e.g., self-esteem, positive affect) as they were and reversed the scoring of all negative constructs (e.g., depression, negative affect).

One study (Salovey, 1992) differed from others in that its well-being variable (mood, in this case) was manipulated rather than measured. Salovey conceptualized SC as a dependent variable, a view that differs from that of most other authors reviewed in this synthesis. Because the synthesis focuses on the strength and direction (positive or negative) of the relationship between well-being and SC and does not necessarily assign a causal role to either, this study was retained. Had there been additional studies manipulating well-being and measuring SC, we would have been able to evaluate the causal direction, but doing so on the basis of one study would be premature.

The role of stress. As noted earlier, the SC model posits different SC/well-being relationships under different stress conditions. In adverse, stressful conditions, a complex self-concept is expected to buffer the stress and to be associated with less of a decline in well-being. In rewarding conditions, a complex self-concept is expected to moderate the benefit and to be associated with less of an increase in well-being (Linville, 1985, 1987).

A significant interaction effect of *H* and stress in a multiple regression model is the most appropriate test of the SC model's predictions. However, multiple regression coefficients cannot be used in meta-analysis, because regression weights (beta weights) reflect adjustment for the other predictors in any particular multiple regression model and cannot be separated into their individual effects (Hunter & Schmidt, 1990). Because stress does play an important role in the SC model, thus making it fundamentally multivariate, the inability to meta-analyze multiple regression results posed a challenge to us as research synthesists. This challenge was addressed in three ways. First, we included in the meta-analysis prospective studies that involved a shared stressor or uplift (e.g., a feedback manipulation, Setterlund, 1994; shared life event, Gallant, 1991). In those cases, the stressor (or uplift) can be thought of as a study-level feature rather than as a variable within the study. Thus, studies were included if they measured *H* and related it to well-being scores measured during or after some objective stressor. Although not a statistical interaction effect (for lack of a no-stress group), the relationship between prestress SC and poststress well-being serves as an adequate measure of the predicted buffering effect. If SC indeed buffers stress, high levels of prestress SC would be related to poststress well-being. Because Linville's (1985) SC model posits that high SC moderates both negative reactions (to stressors) and positive reactions (to uplifts), studies were included if they had either a challenging stressor or a rewarding uplift.

Linville demonstrated the SC buffering effect using both manipulated stress (Linville, 1985) and naturally occurring life events (Linville, 1987), suggesting that similar relationships between SC and well-being can be expected in both laboratory and naturalistic studies. For this reason, we grouped together laboratory studies that manipulated failure with studies containing naturalistic stressors. Because we located no studies containing naturalistic uplifts, we could not do the same in the prospective-uplift group. In the prospective-stress group, we also coded each study for the type of stress. This allowed us to compare natural and manipulated stress studies empirically.

A second way of addressing the problem of meta-analyzing diathesis-stress effects involves examining the relationship between the diathesis variable (in this case SC) and the outcome variable (i.e., well-being) at different levels of stress. For instance, in their study examining the relationship between SC and depressive symptoms, Rafaeli-Mor and Brown (1997) found different interactive effects for SC with stress of different severity. As Linville (1987) suggested, no relationship should exist between SC and well-being if stress is completely absent in a sample. However, when stress is simply not measured, it would be conservative to assume that some degree of stress is present in the lives of (at least some of) the individuals in a sample (Coyne & Gotlib, 1986; Coyne & Whiffen, 1995). Thus, although studies with no measurement of stress are less clear-cut in their implication, they offer a possibility of testing the predictions of the SC model under weaker (yet probably existent) levels of stress. For this reason, we included in the synthesis those studies that did not measure stress but did report the zero-order relationship between SC and well-being.

Some of the studies that yielded zero-order effects for the synthesis were designed by their authors as prospective tests of stress buffering and did include stress measures. However, these studies measured stress on continua and subsequently entered stress and SC into multiple regression models, examining their interaction effect. For the statistical reasons noted previously, only the zero-order relationship of SC and well-being could be converted to a standard effect size estimate that could be included in the meta-analysis. Nonetheless, as a third and final way of handling the issue of stress, we conducted a secondary vote-counting analysis that directly tested the buffering effect using studies with appropriate multivariate designs. This procedure is described at length in a later section; we turn first to the classic meta-analysis examining the relationship between SC and well-being.

Meta-Analysis

Method

Sampling of Studies

Several methods were used to locate studies for the meta-analysis. First, a search of computerized databases was conducted, starting from their beginning until March 1998. The searches used the key word *self-complexity.* The databases included were Psychological Abstracts Information Services (PsycINFO), Social Science Citation Index (SSCI), Dissertation Abstracts International, Educational Resources Information Center, and MedLINE. Second, a "descendent search" was performed in the print version (and, for later years, in the computerized version) of the SSCI, pursuing all of the articles that have cited any of three key studies (Block, 1961; Linville 1985, 1987). Third, an extensive search in PsycINFO was performed using the intersection of three word lists[1] to locate pertinent studies that failed to use the word *self-complexity* in their identifiers. Fourth, a request for information was mailed to two relevant e-mail discussion lists (one devoted to social psychology research, the other to clinical psychology research). We also contacted several prominent researchers in the field and requested their assistance in obtaining additional (possibly unpublished) reports.[2] Finally, in an ancestry search, we examined the reference lists of all obtained manuscripts to uncover any appropriate studies not otherwise found.

To be included in the meta-analysis, studies had to meet the following criteria:

1. The study included a self-descriptive task from which the author(s) computed Scott's (1969) *H.* Although the *H* statistic can be computed from any matrix-like organization of aspects by traits, several additional constraints were used. First, only studies that allowed a variable number of self-aspects to be generated were included. Several investigations have demonstrated that *H,* the dimensionality statistic, is highly related to the number of aspects constructed by a subject (typical correlations: .70–.75; e.g., Linville, 1987; Rafaeli-Mor & Brown, 1997; Rafaeli-Mor et al., 1999). Some studies (e.g., Jordan & Cole, 1996) restricted or fixed the number of aspects. Such restrictions eliminate a very large portion of the variability in *H.* We therefore excluded such studies from our analysis. Second, some studies did not provide a fixed set of trait words to be sorted in the complexity task and instead elicited these from the participants. This practice allows the trait-list length to vary within a study, calling into question the meaning of individual differences in the resulting *H* scores. Such studies were also excluded from the analysis.

2. The study included at least one measure of well-being. As discussed previously, well-being measures include self-report measures of mood, affect, self-esteem, and depression, as well as psychiatric diagnoses of depression.

3. The study reported zero-order correlations between the well-being and SC measures obtained concurrently, or prospective relations between an initial SC score and a well-being score obtained subsequent to an objective stressor or uplift.

Scope and Primary Prediction

Based on our approach to addressing the presence or absence of stress, the studies falling within the scope of the meta-analysis fell into three design groups:

1. Prospective (stress) studies, which included an objective specification of a shared stressor (e.g., a failure feedback manipulation; Linville, 1985, Study 1). Some studies with shared naturalistic stressors (e.g., the months following birth of first child; Gallant, 1991) were included as well. The valence of the events in these studies is not as clear-cut as a laboratory manipulation of stress. Nonetheless, we viewed them as pertaining to stress and not to uplifts (see following). The theoretical basis for this decision was the definition of *stress* as a demand for resource allocation (e.g., Lazarus & Folkman, 1987). Thus, situations that clearly demand coping resources were categorized as stressful. In this group of studies, SC had to have been measured prior to the stressful event or manipulation, and well-being had to have been measured following the stressor (although in several cases it was measured both before and after, yielding a change score or a partial correlation).

2. Prospective (uplift) studies, which included an objective and shared rewarding situation (e.g., a success feedback manipulation; Linville, 1985, Study 1). In this group of studies, SC had to have been measured prior to the uplift, and well-being had to have been

[1]For the extensive search, we followed the suggestions of Reed and Baxter (1994). Three word (or word fragment) lists were composed, tapping the three relevant notions of self, complexity, and well-being. An intersection of these three lists yielded more than 14,000 citations. The first 50 cited abstracts for each year (1967–1997) were reviewed. Although some of these studies fell within the scope of this review, they had all been found using the other (more efficient) retrieval techniques. The word lists used were as follows (word fragments are appended by the dollar sign, $): List 1: self, ego, identity, role, aspect, schema$, concept; List 2: complex$, structur$, coheren$, variability, consisen$, differntiat$, integrat$, overlap, stability; List 3: well-being, emot$, depression, mood, affect, dysphoria, esteem, coping, symptom, neurotic$.

[2]We are grateful to the following authors who were kind enough to assist with locating studies or with providing additional information for this review: Jane Buder-Shapiro, Jennifer Campbell, Jill Gardner, Ronnie Janoff-Bulman, Linda Koenig, Patricia Linville, Alan McConnell, Ari Solomon, and Robert Woolfolk.

measured following the uplift (although, as in the case of negative stress, several studies reported measures both before and after, yielding a change score or a partial correlation).

3. Zero-order studies, which did not include shared or specified life-events or circumstances. Some of the studies in this category (e.g., Rafaeli-Mor & Brown, 1997) were in fact prospective in their design but measured stress in a dimensional (usually self-reported) manner, which were then submitted to multivariate analyses. In those cases, after establishing that the data cannot be parsed (so as to compute a prospective effect even on a subsample) zero-order correlations of SC and well-being were extracted.

Earlier, we reviewed Linville's SC model (1985, 1987) and presented theoretical alternatives to it. Linville's (1985, 1987) model and these alternatives yield distinct (and in some cases competing) empirical predictions for the relationship between SC and well-being in the three design groups. Linville's (1985, 1987) SC model suggests a positive relationship under conditions of stress. Thus, studies that fall into the stress group are expected to yield positive effect sizes, particularly in those prospective studies in which stress is interjected between the measurement of SC and that of well-being. The same model posits a negative relationship between SC and well-being under conditions of reward or uplift: The higher the SC level, the more buffered (less positive) one's positive reaction to an uplift. Thus, studies that fall into this uplift ("eustress") group are expected to yield negative effect sizes. Lastly, in studies without appropriate measurement of stress, where only a zero-order relationship was computed, Linville (1987) declined to predict any directional relationship.

Alternative models of the role of self-structure (e.g., Block, 1961; Donahue et al., 1993) predict different effects than those of Linville's (1985) SC model. According to such approaches, high self-schema differentiation actually reflects fragmentation and lack of ego identity. In contrast to Linville's (1985) view of SC as an emotional moderator, proponents of these alternative models predict a negative relationship between complexity (or fragmentation) and well-being. This relationship is expected not to be contingent on stress and should therefore hold for all three groups of studies.

Following Linville (1985, 1987), our primary prediction was finding differences among studies from the three (prospective stress, zero-order, or prospective uplift) design groups in the strength, and sign, of the relationship of SC and well-being.

Additional Coding of Studies and Secondary Predictions

Each study was coded for several characteristics in addition to being grouped into one of the three theoretically distinct design groups (i.e., stress, uplift, and zero-order studies). Because there are different directional predictions for these three groups, we examine the effects of the consequent coding variables separately within each design group. For each study we noted these coding variables as well as a brief summary of the study, statistical information about the directionality and significance of the effect, and the reported statistics that were used to compute the effect size. We performed all coding independently. Agreement regarding continuous variables (e.g., effect size) was complete; disagreements about categorical coding variables were rare and were resolved by consensus after reviewing the study in question. Following is a brief description of the key study characteristics[3] and their hypothesized impact on the SC/well-being relationship.

General information. Three characteristics that we coded conveyed general information concerning the studies, their authors, and the populations used within them. One of these is the publication source (i.e., whether the study appeared in a peer-reviewed journal, an unpublished manuscript, or an edited chapter). As has been found in numerous literature reviews in the past, published studies were expected to yield stronger effects than nonpublished studies. We also coded the subdiscipline (social vs. clinical or applied psychology) from which each study's first author hailed. This comparison tested the possibility that SC had been examined using diverging techniques or designs in different subdisciplines and the related possibility that these designs led to divergent results. The final characteristic in this group was the clinical status of the study's population (clinical, nonclinical, or a comparison of one to the other). Variation in this characteristic was present only in the zero-order group of studies: All the prospective (both stress and uplift) studies utilized nonclinical participants. Recall that following Coyne and Whiffen's (1995) reasoning, we would expect a positive relationship (albeit a weak one) between SC and well-being. We were interested to see whether SC would have a zero-order relationship to well-being even within groups that are relatively homogenous in their mood symptoms (or lack of them), or whether only a major qualitative distinction (e.g., the one between patients with clinical depression and controls who were not depressed) would reveal this relationship.

Internal reliability. We coded several measurement and procedural characteristics that reflect the reliability and accuracy of the SC or well-being indexes. One such characteristic is the internal consistency

[3]For the coding sheet used, and a complete list of coded study characteristics, please contact Eshkol Rafaeli-Mor.

(e.g., alpha) of the well-being indexes. Another is the number of adjectives in the trait-sort list used to compute SC: With bigger lists, finer and more reliable distinctions are expected. A third characteristic is the number of participants completing the SC task: Participants run individually are thought more likely to complete the open-ended task in an unbiased way, whereas multiparticipant sessions may be tainted by peer-induced speeding or slowing. Because reliability bounds validity, we expected greater reliability and lesser procedural noise to be associated with bigger effect sizes.

External validity. Certain features of the designs used in studies may increase their external validity or generalizability. We were interested in examining whether those studies with greater external validity would yield effects that are as strong as those with weaker external validity, or whether the typical trade-off between internal and external validity exists in this literature as well. Specifically, we were interested in the role of time lag in the buffering effect of SC on well-being. Whenever possible, we coded (categorically) whether SC and well-being scores were obtained at the same session or at different times. In the prospective groups, we coded (as a continuous index) the time lags between the measurement of SC and the occurrence of the stressor or eustressor, as well as the time lag between the stressor or "eustressor" and the measurement of well-being. Within the prospective groups, we also coded whether the stressor was experimentally manipulated or naturalistically occurring and, when manipulated, whether the manipulation was determined to be successful.

Type of well-being. One important study-level variable involved the utilized well-being index (depression, mood or affect, self-esteem, or a combination of these). This was interesting because of the differences in the scope of well-being implicit in different indexes. Specifically, we expected that studies using mood or affect items would show the strongest relationship, followed by those using depression, and only then by those using self-esteem indexes. This prediction reflects the temporary versus chronic nature of the well-being indexes. Mood or affect tend to be transitory and to fluctuate more and may therefore be most affected or buffered by a factor such as SC. In contrast, depression and, to an even greater extent, self-esteem tend to be more long-standing and would be less likely to be affected by SC levels.

Thus, when directional predictions were made, effects based on mood or affect were expected to be more powerful than those based on depression or self-esteem. It is notable that this prediction diverges from Linville's (1985) suggestion that similar buffering occurs both with mood and with self-esteem.

SC valence. Another substantive measurement question involved the valence of the SC index, as reflected in the percentage of negative traits within the trait-sort adjective list used to compute SC scores. As noted earlier, several authors, starting with Morgan and Janoff-Bulman (1994), have suggested that the complexity of positive self-information may function differently than the complexity of negative self-information. Coding this variable allowed a systematic examination of this concern not only within one sample (e.g., Woolfolk et al., 1995) but also across samples.

Computation of Study Effect Sizes

The analysis focused on the relationship between two continuous individual difference variables: SC and well-being. As could be expected, most studies (59 of 70) examining this relationship used correlation or regression computations. This practice was maintained in this synthesis, and the common effect size metric used in this analysis was *r*, the correlation coefficient. A positive correlation signified that SC was associated with greater well-being, whereas a negative correlation signified that SC was associated with poorer well-being. Naturally, the sign of the relationship was reversed when the well-being index was depression or negative affect.

Several reports (e.g., Linville, 1985; Niedenthal et al., 1992) included more than one study, each conducted with a separate sample; in such cases, we computed an effect for each of the samples and treated them as separate studies. Some studies used more than one measure of well-being within the same sample. Although such additional information allows for a better estimation of the strength of the relationship between SC and well-being, the dependence of the measures prohibits their use as different effect sizes in a meta-analysis (Rosenthal & Rubin, 1986). Following Rosenthal and Rubin's (1982, 1986) suggestions, a combined effect size (accounting for the correlation between the well-being measures) was computed when possible.

Whenever possible, the effect size was computed on a prospective relationship (i.e., SC's relationship with well-being, with the latter measured during or after a shared stressful or pleasant event). If more than one type of relationship was reported, the relationship in the presence of negative events (stress) was preferred. If this was not available, the relationship in the presence of positive events (uplift) was used; otherwise, the relationship in the absence of events (zero-order) was computed. We used this hierarchical preference because of our interest in considering the SC/well-being relationship in the context of stress. A stress condition is both the focus of Linville's (1985) original prediction and the context in which the contrasting predictions of Linville's (1985) SC and Donahue et al.'s (1993) fragmentation models can be best evaluated.

Sixteen of the studies (mostly prospective ones) reported only change scores or partial correlations between SC and Time 2 well-being (partialling out the Time 1 well-being score). In primary research, using such an approach is statistically preferred because it allows for a within-person control and decreases irrelevant variance in the outcome scores. However, this valid primary research consideration poses a difficulty for the meta-analyst. Specifically, studies reporting partial correlations or change scores are harder to integrate in research syntheses. The reasons are similar to those pertaining to the question of meta-analyzing multiple regression coefficients (Hunter & Schmidt, 1990; see the following discussion in the section devoted to vote-counting). Because such a large proportion of studies within the scope of this synthesis did report only such statistics, we decided to include them in the review. The main consideration in favor of this decision is that such procedures (e.g., partial correlations) are not likely to reverse the sign of the SC/well-being relationship. However, a note of caution is needed: Although the sign of the relationship probably remains the same, partialling procedures will tend to inflate the magnitude of the effect. In other words, by reporting the relationship of SC with the residual well-being only, these studies are mathematically producing greater effects than would be found under zero-order correlations. With this caveat in mind, we proceeded with including these studies while coding the use of partial statistical relationships so that its effect could be examined.

Effect sizes were computed and synthesized with the aid of a dedicated computer program (DSTAT; Johnson, 1989). The computation of effect sizes was based on correlations (or partial correlations) reported for 59 studies, and t scores (or means and standard deviations) reported for 11 studies. Three of the studies reporting means and standard deviations conceptualized SC as the continuous dependent variable and well-being as the categorical independent variable. In these cases, the t statistic was converted to a point-biserial correlation. This was corrected to a biserial correlation, an estimate of the product–moment correlation (r). In total, 70 effect sizes from 46 different manuscripts were computed.

Results

Characteristics of the Studies

Table 1 lists the 70 studies included in this synthesis with some of their coded characteristics. The studies are divided into the three design groups. Twelve studies with an objective and uniform stressor were included in the stress group. Nine studies with a positive eustressor (uplift) were included in the uplift group. The remaining 49 studies provided a zero-order relationship.

Table 2 summarizes the frequencies, means, or ranges of study-level variables. As would be expected, the range of publication dates of the aggregated studies begins with Linville's (1985) influential study and runs through the present. Unpublished studies, many of which were doctoral dissertations, narrowly outnumbered published or edited ones. These facts reflect the interest with which the SC concept was met in both the social and the clinical psychology domains, particularly as a worthwhile topic for dissertation research.

Also of note is the disappointing fact that the majority of studies conducted by clinical or applied researchers did not utilize clinical samples. This of course limits the possibility of generalizing the conclusions from SC studies to disordered affect or pathological levels of (poor) well-being.

Overall Relationship of Well-Being and *H*

The summary of overall and study-level effect sizes is presented in Table 3. As is evident, an overall modest negative relationship is suggested by the mean effect size ($r = -.04$). The median effect size was comparable in magnitude, as was the unweighted mean overall effect. However, there are several reasons to reject this estimate as representative of the literature as a whole. As described earlier, different effects are expected in studies using different designs. This theoretical expectation was supported empirically: A great deal of heterogeneity existed in the study-level effect sizes. This heterogeneity could not be remedied by exclusion of several effect sizes; in fact, outlier analysis revealed that only removal of 21 (30%) of the studies yielded a set of studies with heterogeneity that does not exceed chance. Finally, as Table 1 reveals, a simple count of the studies yielding positive (28) and negative (42) effects suggests considerable heterogeneity.

Test of the Primary Prediction

We predicted that studies using different designs would differ in the effect sizes they yield. The categorical model of study design proved significant ($p < .0001$). Effect sizes from stress studies (prospective negative designs that included a stress manipulation) were positive, though weak (mean weighted $r = .03$, $k = 12$). Effects from the zero-order group of studies (those with neither a stressor nor an uplift) were negative and equally weak (mean weighted $r = -.04$, $k = 49$). Recall that these two groups of studies can both inform us about the stress-buffering capacity of SC. Effect sizes from the uplift group of studies (prospective-positive studies that included an objective eustressor) were more robustly negative (mean weighted $r = -.27$, $k = 9$).

Table 1. *Studies and Their Characteristics (In Order of Effect Size) Within Each of the Three Designs*

Stress (Negative Event) Studies (in Order of Effect Size)

Authors	Year	Additional information	*N*	Pub	Msr	Dcp.	Trts	P. Neg	Rel.	*d*	*r*
Dixon	1989	(failure)	30	U	A	soc.	33	0.36	0.9	–0.46	–.23
S. H. Smith & Cohen	1993		56	P	E	clin.	33	0.36	0.8	–0.30	–.15
Koenig	1989	(failure)	38	U	A	clin.	48	0.4	—	–0.29	–.15
Buder-Shapiro	1992	(failure)	30	U	E	clin.	33	0.36	0.8	–0.23	–.12
Gallant	1991	(female)	47	U	D	clin.	33	0.33	0.9	–0.12	–.06
Gallant	1991	(male)	45	U	D	clin.	33	0.33	0.9	–0.06	–.03
Fankhauser	1991		74	U	D	clin.	33	0.21	0.8	0.14	.07
Niedenthal, Setterlund, & Wherry	1992	(Study 1, fail actual selves)	25	P	A	soc.	39	0.46	—	0.31	.16
Niedenthal et al.	1992	(Study 2 fail possible selves)	20	P	A	soc.	39	0.46	—	0.43	.22
Niedenthal et al.	1992	(Study 3 fail actual selves)	20	P	A	soc.	39	0.46	0.8	0.67	.33
Niedenthal et al.	1992	(Study 3 fail possible selves)	15	P	A	soc.	39	0.46	0.8	0.71	.35
Linville	1985	(failure)	29	P	A	soc.	33	0.36	—	0.85	.40

Cross-sectional Studies (in Order of Effect Size)

Authors	Year	Additional information	*N*	Pub	Msr	Dcp.	Trts	P. Neg	Rel.	*d*	*r*
Woolfolk, Novalany, Gara, Allen, & Polino	1995	(Study 1)	60	P	D	clin.	—	1	0.8	–1.00	–.45
Woolfolk et al.	1995	(Study 5)	55	P	D	clin.	42	1	0.8	–0.58	–.28
Rafaeli-Mor & Pineles	2000	(Study 2)	45	U	D	clin.	44	0.48	0.9	–0.53	–.26
Salovey	1992		96	P	A	soc.	33	0.36	—	–0.43	–.21
Kalthoff & Neimeyer	1993		127	P	D	clin.	33	0.39	0.9	–0.41	–.20
Edwards	1997		117	U	A	clin.	36	0.42	—	–0.38	–.19
Horton	1995		69	U	D	clin.	32	0.34	0.9	–0.38	–.19
Green	1993	(restrained)	89	U	Dx	clin.	40	—	0.8	–0.37	–.18
Hayashi & Horiuchi	1997		126	P	E	—	20	1	—	–0.36	–.18
Woolfolk et al.	1995	(Study 3)	65	P	Dx	clin.	28	0	0.8	–0.36	–.18
Widner	1994		34	U	Dx	clin.	34	0.5	0.8	–0.33	–.17
George	1997		164	U	E	soc.	33	0.36	0.7	–0.32	–.16
Linville	1987		106	P	D	soc.	33	0.36	0.9	–0.32	–.16
L. H. Cohen, Pane, & Smith	1997	(Study 1)	59	P	D	clin.	33	0.36	0.8	–0.28	–.14
Green	1993	(unrestrained)	224	U	Dx	clin.	40	—	0.8	–0.22	–.11
Morgan & Janoff-Bulman	1994		242	P	Dx	clin.	80	0.43	0.9	–0.23	–.11
Jolly	1987		39	U	Dx	soc.	33	0.36	—	–0.20	–.10
Rafaeli-Mor	1999		55	U	A	clin.	44	0.48	0.8	–0.20	–.10
L. H. Cohen et al.	1997	(Study 2)	157	P	E	clin.	33	0.36	0.8	–0.18	–.09
Rafaeli-Mor & Brown	1997		69	U	D	clin.	45	0.33	0.9	–0.18	–.09
Rhodewalt & Morf	1995		114	P	E	soc.	33	0.36	0.8	–0.18	–.09
Fisher	1990		117	U	E	soc.	33	0.36	0.8	–0.16	–.08
Wiss	1991		83	U	Dx	clin.	33	0.36	0.8	–0.16	–.08
Rhodewalt, Madrian, & Cheney	1998	(Study 1)	53	P	Dx	soc.	33	0.36	0.7	–0.09	–.05
Knolbach	1994		304	U	D	clin.	33	0.36	0.8	–0.08	–.04
Miller, Omens, & Delvadia	1991		98	P	E	soc.	33	0.36	0.8	–0.06	–.03
Campbell, Assanand, & DiPaula	2000	(Study 3)	62	C	Dx	soc.	30	0.27	0.8	–0.04	–.02
Rafaeli-Mor	1998		169	U	D	clin.	44	0.48	0.7	–0.04	–.02
Campbell et al.	2000	(Study 2A)	71	C	Dx	soc.	33	0.33	0.8	0.00	.00
Solomon	1994		90	U	D	clin.	33	0.36	0.8	0.01	.01
Niedenthal et al.	1992	(Study 2 actual selves)	40	P	A	soc.	39	0.46	—	0.04	.02
Rafaeli-Mor, Gotlib, & Revelle	1999		75	P	D	clin.	44	.48	0.9	0.08	.04
H. S. Smith	1994		68	U	E	clin.	33	0.36	0.8	0.08	.04
Hershberger	1989		110	U	E	clin.	33	0.33	0.8	0.18	.09
Jeannote	1993		64	U	D	clin.	30	0.37	0.8	0.20	.10
Dozois	1995		80	U	D	clin.	15	1	0.8	0.22	.11

(continued)

Table 1. *(Continued)*

Cross-sectional Studies (in Order of Effect Size)

Authors	Year	Additional information	*N*	Pub	Msr	Dcp.	Trts	P. Neg	Rel.	*d*	*r*
Takagi	1996		85	U	D	clin.	59	0.44	0.7	0.22	.11
C. L. Baldwin	1996		38	U	Dx	soc.	36	0.39	0.8	0.23	.12
Campbell et al.	2000	(Study 4)	64	C	A	soc.	35	0.43	—	0.24	.12
Gardner	1997		40	U	A	clin.	33	0.36	—	0.30	.15
Shyuu	1990		40	U	E	clin.	12	—	0.7	0.42	.21
Weiss	1988		97	U	D	clin.	33	0.36	0.8	0.49	.24
Rhodewalt et al.	1998	(Study 2)	78	P	Dx	soc.	33	0.36	0.7	0.51	.25
Campbell et al.	2000	(Study 1)	67	C	E	soc.	50	0.3	0.8	0.55	.27
Brown & Rafaeli-Mor	2000		72	U	D	clin.	45	0.33	0.9	0.58	.28
Rafaeli-Mor & Pineles	2000	(Study 1)	85	U	D	clin.	44	0.48	—	0.65	.31
Campbell, Chew, & Scratchley	1991		67	P	Dx	soc.	27	0.33	—	0.75	.35
Koenig	1989	(Control)	35	U	A	clin.	48	0.4	—	0.77	.37
Beatty-Leask	1994		26	U	D	clin.	20	—	—	1.05	.48

Uplift (Positive Event) Studies

Authors	Year	Additional information	*N*	Pub	Msr	Dcp.	Trts	P. Neg	Rel.	*d*	*r*
Niedenthal et al.	1992	(Study 1 success actual selves)	24	P	A	soc.	39	0.46	—	–1.49	–.61
Niedenthal et al.	1992	(Study 3 success actual selves)	17	P	A	soc.	39	0.46	0.8	–1.13	–.51
Niedenthal et al.	1992	(Study 2 success possible selves)	20	P	A	soc.	39	0.46	—	–1.11	–.50
Niedenthal et al.	1992	(Study 3 success possible selves)	19	P	A	soc.	39	0.46	0.8	–0.86	–.41
Linville	1985	(Success 2)	31	P	A	soc.	33	0.36	—	–0.64	–.31
Linville	1985	(Success 1)	30	P	A	soc.	33	0.36	—	–0.55	–.27
Koenig	1989	(Success)	37	U	A	clin.	48	0.4	—	–0.16	–.08
Buder-Shapiro	1992	(Success)	30	U	E	clin.	33	0.36	0.8	0.26	.13
Dixon	1989	(success)	30	U	A	soc.	33	0.36	0.9	0.37	.19

Note: *N* = the number of participants in the study; Pub = publication source; P = published in a peer-reviewed journal; U = unpublished, submitted, or doctoral dissertation; C = chapter; Msr = the type of well-being measure used in the study; A = affect or mood; E = self-esteem; D = depression; Dx = psychiatric diagnosis; Dcp. = the subdiscipline of the first author; soc. = social; clin. = clinical; Trts = the number of traits used in the self-complexity task; P. neg = the proportion of negative traits within the trait list; Rel = the reliability of the well-being measure; *d* = Cohen's *d* value for the study; *r* = Pearson's *r* value for the study.

Chi-square pair-wise comparisons of the uplift studies with the other two groups were significant ($p < .0001$), whereas a statistical trend ($p < .06$) was obtained for the comparison of zero-order and stress studies.

Thus, the primary prediction of the meta-analysis was supported: Major differences were found between studies without measured stress compared to those exploring the buffering of negative stress or of positive uplifts. As planned, all secondary predictions were examined separately within each of the three design groups.

Tests of Secondary Predictions Within the Three Design Groups

Table 4 summarizes the findings of the secondary predictions within each of the design groups. For categorical coding variables with a significant effect, the mean effect and the number of studies on which it is based are noted. For continuous coding variables, the effect of the coding variable on the SC/well-being relationship (e.g., more positive) and the *z* value associated with this effect are noted. Findings that run counter to our prediction are italicized. Following is a discussion of the secondary findings.

General information. As expected, published studies yielded more positive results than unpublished ones in the stress group and more negative results in the uplift group ($p < .05$ and $p < .0001$, respectively). The effects of published studies in the zero-order group were also more negative ($p < .0001$).

Also as predicted, studies originating in social psychology laboratories yielded results that differed from those originating in clinical or other applied laboratories. In both the stress and the uplift groups, results from social psychology laboratories were stronger in the predicted direction ($p < .001$ for positive

Table 2. *Summary of Study Characteristics*

Characteristic of studies	Value
Publication Source	
Journal Article	29
Edited Chapter	4
Unpublished/Dissertation	37
Study Population	
Adults	10
College Students	59
Adolescents	1
First Author's Subdiscipline[a]	
Social	40
Clinical/Other Applied	29
Range of Years	1985–1999
Design of Study	
Prospective (With Negative Stressor)	12
Prospective (With Positive Eustressor)	9
Zero-order	49
Well-Being Measure	
Depression	22
Mood or Affect	22
Self-Esteem	13
Combined Measure	13
Clinical Status	
Nonclinical	65
Clinical	2
Comparison of Clinical to Nonclinical	3
Average Number of Trait Words (n = 69)[a]	36
Average Proportion of Negative Words in the Trait-Sort Task Used (n = 66)[a]	.42

Note: For categorical variables, the numbers represent the frequency of studies in this category.
[a]This information was unavailable for some studies.

Table 3. *Summary of Overall and Study Level Effect Sizes*

Variable	Value
Number of studies	70
Overall Number of Participants	5,354
Weighted Overall r[a]	–.04**
95% Confidence Interval for r	–.02/–.06
Homogeneity (Q) of Study Level rs[b]	308.81**
Unweighted Overall r	–.03*
Median Study Level r	–.05**

Note: Effect sizes are positive when self-complexity and well-being are positively related and negative when self-complexity and well-being are negatively related.
[a]Effect sizes are weighted by the reciprocal of the variance. [b]Significance for Q indicates rejection of the hypothesis of homogeneity.
*$p < .05$. **$p < .00001$.

results in stress studies; $p < .01$ for negative results in uplift studies).

Only the zero-order group included studies differing in the clinical status of the participants. As expected, there was a significant difference between their average effects ($p < .0001$). Specifically, the three studies that involved comparison of clinical and nonclinical groups yielded strong positive effects of SC, whereas studies done with either clinical or nonclinical samples reported weak negative effects.

Internal reliability. Whenever possible, we extracted the reliability of the well-being index used in studies or located external sources for the index's reliability. Within the zero-order group, studies using more reliable well-being indexes yielded stronger negative effects ($p < .05$). A surprising finding within the two other design groups was that studies using more reliable indexes yielded significantly weaker results (weaker positive results in the stress group, $p < .01$; weaker negative results in the uplift group, $p < .05$). Two possible explanations are that these effects are based on nonrepresentative subsets of already small groups of studies (8/12 stress studies, and 4/9 uplift studies) or that they may be biased by the restricted range of reliability estimates (all greater than .80).

Two features of the methodology were used as indexes of the internal reliability of the SC index. The first, the number of adjectives used in the trait-sorting task, was weakly related in the predicted direction ($p < .10$, two-tailed) in the uplift group. However, it proved unrelated to the magnitude of the SC/well-being effect in the zero-order and the stress groups. The second estimate of internal consistency was the number of participants run simultaneously in the trait-sorting task. Our prediction, that more reliable results (and hence, stronger effects) would be obtained when participants were run individually, was supported in the stress and uplift groups ($p < .05$ in both). In the zero-order group, studies with individually run participants tended to have positive effects, whereas those with more participants per session had more negative effects ($p < .0001$). In other words, those zero-order studies with a more reliable SC score tended to find a positive relationship between SC and well-being.

External validity. When SC and well-being were measured within the same session, effects tended to be stronger (more positive in the stress group, more negative in the zero-order and uplift groups; $p < .10$, $p < .10$, and $p <. 01$, respectively).

In the two prospective groups, the longer the time lag between the measurement of SC and the onset of the stressor or eustressor, the weaker the effect ($p <. 10$ in the stress group, $p < .05$ in the uplift group). Similarly, the longer the time lag between the stressor and the measurement of well-being, the weaker the effect ($p < .10$ in the stress group). The studies in the uplift group did not differ in this (latter) time lag: All measured well-being immediately after the occurrence of the eustressor.

In the stress group, studies using false feedback yielded stronger effects than those using naturalistic stressors ($p < .05$; all the studies in the uplift group used false feedback, and thus no comparison could be made). Interestingly, in both the stress and the uplift

Table 4. *Secondary Predictions Within Each of the Three Design Groups*

Coding Variable	Stress Studies (Expected Positive Effects)	Zero-Order Studies (Unclear Prediction)	Uplift studies (Expected Negative Effects)
General Information			
Publication	Published (+.14, 6) > Unpublished (–.04, 6)	Published (–.10, 17) < Unpublished (–.01, 28) < Edited chapter (+.09, 4)	Published (–.41, 6) < Unpublished (+.02, 3)
Subdiscipline	Social (+.23, 6) > Applied (–.05, 6)	No effect	Social (–.36, 7) < Applied (–.02, 2)
Clinical Status	—	Clinical (–.07, 2) = Nonclinical (–.05, 44) < Comparison (+.23, 3)	—
Internal Reliability			
Well-Being Reliability	Less positive ($Z = -2.59$)	More negative ($Z = -2.89$)	*Less negative* ($Z = 2.47$)
Number of adjectives	*No effect*	No effect	More negative ($Z = -1.73$)
More Participants per Session	Less positive ($Z = -2.22$)	More negative ($Z = -5.44$)	Less negative ($Z = 2.38$)
External Validity			
Same/Different Times	Same (+.11, 7) > Different (–.03, 5)	Same (–.06, 27) < Different (–.02, 17)	Same (–.36, 7) < Different (–.02, 2)
Greater Time Lag			
Before Stress	Less positive ($Z = -1.67$)	—	Less negative ($Z = 2.57$)
After Stress	Less positive ($Z = -1.70$)		—
Manipulated vs. Natural	Manipulated (+.12, 8) > Natural (–.03, 4)	—	—
Manipulation Check	*No effect*	—	*No effect*
Statistical Procedure	Partial r (+.24, 4) > Change score (+.06, 4) > Time 2 only (–.10, 4)	Partial r (–.28, 1) < Zero-order (–.03, 48)	Partial r (–.50, 4) Change score (–.16, 4) < Time 2 only (+.18, 1)
Measure of Well-Being	Mood (+.14, 7) > depression (+.01, 3) > self-esteem (–.14, 2)	No effect	Mood (–.30, 8) < self-esteem (+.13, 1)
Self-Complexity Valence (More Negative Traits)	More positive ($Z = 2.47$)	More negative ($Z = -3.91$)	More negative ($Z = -4.32$)

Note: For categorical models, each category is listed with the mean effect size and the number of studies in that category. For continuous models, the Z score for the effect is listed. Only significant effects are listed here (for significance levels, see text). Findings that run counter to our predictions are italicized.

groups, effects did not differ as a function of the success or failure of the manipulation.

Type of statistic used to compute effect size. As we noted in our discussion of the computation of study effect sizes, several of the studies reported the results only as change scores or partial correlations and not as zero-order correlations between SC and well-being. As expected, studies using these (perfectly appropriate, yet powerful) statistical corrections yielded much stronger relationships in all three groups ($p < .01$ for the zero-order and stress groups, $p < .0001$ for the uplift group). Studies using statistical corrections found the relationship between SC and well-being to be more positive in the stress group and more negative in the zero-order and uplift groups.

Type of well-being. As expected, narrow-range well-being indexes such as mood or affect yielded stronger SC/well-being effects than broader range indexes such as depression or self-esteem. This pattern was observed in both the stress and the uplift groups ($p < .05$ for both). It was not observed within the zero-order group in which effects were most heterogeneous.

SC valence. Consistent with the findings of Morgan and Janoff-Bulman (1994) and of Woolfolk et al. (1995), we found a continuous effect for the valence of the trait list used in SC tasks. In the stress group, studies utilizing SC tasks with a greater proportion of negative traits yielded more positive effects. In both the uplift and the zero-order groups, studies utilizing such tasks yielded more negative effects ($p < .001$ in zero-order group, $p < .05$ in the others).

Vote-Counting Analysis

Overview

The most appropriate test of Linville's (1985, 1987) theoretical model is a significant interaction effect of SC and stress in a prospective-design, general linear model (GLM). After all, it is within the context of stressful life-events that SC is posited to have its buffering effect on well-being. Statistically, a buffering effect translates to an interaction term in a multiple regression equation (or any similar GLM). For example, Linville (1987) demonstrated a negative interaction be-

tween SC and stressful events in predicting depression: Under high levels of the buffer variable (SC), the positive relationship between stress and depression was weaker than under low levels of SC.

Unfortunately, interaction terms taken from multiple regression analyses are unsuitable candidates for input into a classic meta-analytic review (Hunter & Schmidt, 1990). The primary problem with the use of such analyses is that researchers often include different predictor variables in regression equations testing the same theory. For example, Linville (1987) included a pretest score of the outcome measure (depression), stress and *H* scores from the pretest, and their interaction. In contrast, another researcher (Weiss, 1988) included, in addition to *H* and stress, the participant's age, several measures of coping style, and a measure of the education level of his participant's fathers. A consequence of the different makeup of the equation is that the interaction term has differing amounts of variance, from varying numbers of sources, partialled from it. This precludes true mathematical aggregation of beta weights from different studies.

Because of this problem (and two additional ones[4]), the data available are often not sufficient to reach a valid estimate of effect sizes; at most, they inform us about directionality and significance (Bushman, 1994). Nonetheless, 24 studies have reported findings pertinent to the stress-buffering role of *H* in a multiple regression format. Rather than disregarding them altogether, we conducted a weaker (yet informative) vote-counting analysis on these data, as outlined by Bushman. This is a relatively crude technique that acknowledges all of these difficulties but makes use of existing studies to the degree possible.

Method

Twenty-four studies were included in this analysis (see Table 5). To be included, studies needed to have reported conducting a multiple regression or analysis of variance model with one of the well-being measures (mood, self-esteem, or depression) as a dependant variable. Seventeen of the 24 studies used depression scales (e.g., Center for Epidemological Studies-Depression Scale; Radloff, 1977), 6 used mood scales, and 1 used a self-esteem measure. The studies also had to have computed SC scores in the same manner described earlier. In addition, studies needed to have included some index of stress. In 16 of the studies, stress was operationalized using continuous measures (e.g., the count of negative life-events events on the Adolescent Perceived Events Scale; Compas, Davis, Forsythe, & Wagner, 1987). Seven studies manipulated stress (e.g., providing failure feedback; Linville, 1985). One study (Morgan & Janoff-Bulman, 1994) used the occurrence of a traumatic event as the stress variable. As a final inclusion criterion, each study needed to have reported a GLM (usually multiple regression or analysis of variance) model, including the interaction of *H* and stress.

In most cases, studies reporting a multiple regression model reported information regarding zero-order (concurrent or prospective) relationships as well and were reviewed in the Meta-Analysis section of this article. However, several studies included only multiple regression models and appear here for the first time.

When possible, both the regression coefficient for the interaction term (or a similar statistic: *t* value [e.g., Aarsvold, 1991] or partial *r* [e.g., Weiss, 1988]) and its significance level were gleaned from the studies. In several instances (four studies), this was not possible, as the interactions were reported simply to be nonsignificant. These studies were considered to show a null effect.

Results

To support the buffering hypothesis, interaction terms of SC and stress in predicting poor well-being need to be negative. Conversely, positive interaction terms suggest an exacerbation effect of SC. Twelve of the located studies reported a negative interaction term, and 7 reported a positive term. Five additional studies reported only the nonsignificance of the interaction and were therefore considered to report null results. Of the 12 "negative studies," 7 reported the interaction to be significant. Of the 7 "positive" studies, 4 reported the interaction to be (at least marginally) significant.

Bushman (1994) warned against a simple reliance on the modal result in deciding the true population re-

[4]Any attempt to integrate multiple regression results across studies faces two additional problems: that of interpretation of interactions and that of mathematically constructing the interaction term. As discussed by Cohen and Cohen (1983), and more recently by Aiken and West (1991), reporting the significance of an interaction term, even when accompanied by its sign, is not informative enough regarding the nature of the interaction. Specifically, a significant interaction tells us little about the issue of the ordinality ("cross-over") of the interaction or about significant differences among groups that differ in one or the other of the interacting variables. For example, a significant interaction term in a SC study fails to inform us whether the high-stress, high-SC participants had lower well-being than the high-stress, low-SC participants. To solve this problem, Aiken and West guided researchers through a simple probing procedure that helps uncover the true nature of the effect. Unfortunately, many authors, including some of those whose studies are integrated here, failed to conduct (or at least report) such probing.

A related statistical problem, which compounds the need for probing interactions, is that of constructing the interaction term. Mathematically, interactions are the product of the two (or more) main effects. However, if these effects are not standardized (or zero centered), collinearity of main and interaction effects occurs. Although the interaction effect (and its significance level) are unchanged, both the coefficients of the main effects and their significance levels may be drastically different (Aiken & West, 1991).

Table 5. *List of Studies (and Their Characteristics) From Vote-Counting Procedure*

Study	*N*	WB	WBcov	Add	Probe	Stress Measure	Lg	Frm	Eff	Sig
L. H. Cohen, Pane, & Smith (1997, Study 1)	59	Dep.	Yes	—	Yes	CSLES	70	61	B	Yes
Knolbach (1994)	304	Dep.	—	—	—	SRRS	0	—	B	Yes
Linville (1985, Study 1)	59	Mood	Yes	—	—	Manipulation	0	—	B	Yes
Linville (1987)	106	Dep.	Yes	—	—	CSLES	14	14	B	Yes
Brown & Rafaeli-Mor (2000)	69	Dep.	Yes	—	Yes	General Stress Item	8.5	8.5	B	Yes
S. H. Smith & Cohen (1993)	56	Mood	—	Yes	Yes	CSLES	0	31	B	Yes
Solomon (1994)	90	Dep.	Yes	—	Yes	NLEQ	14	14	B	Yes
Aarsvold (1991)	107	Dep.	—	—	—	LES	0	183	B	No
Buder-Shapiro (1992)	60	SE	—	—	—	Manipulation	0	—	B	No
L. H. Cohen et al. (1997, Study 2)	157	Dep.	—	Yes	—	Manipulation	0	—	B	No
Rafaeli-Mor and Brown (1997)	169	Dep.	Yes	—	Yes	NEI	60	60	B	No
Takagi (1996)	85	Dep.	—	—	—	LOPES	0	183	B	No
Gallant (1991, Males)	47	Dep.	—	—	—	Parenting Stress Index	0	183	—	No
Koenig (1989)	111	Mood	Yes	—	—	Manipulation	1	—	—	No
Morgan & Janoff-Bulman (1994)	242	Dep.	—	—	—	Trauma	0	365	—	No
H. S. Smith (1994)	68	Mood	—	Yes	—	Manipulation	0	—	—	No
Woolfolk, Novalany, Gara, Allen, & Polino (1995, Study 5)	55	Dep.	Yes	—	—	CSLES	14	14	—	No
Fankhauser (1991)	74	Dep.	Yes	—	—	GSRLE	31	183	E	No
Weiss (1988)	97	Dep.	Yes	Yes	—	A-FILE	14	183	E	No
Widner (1994)	34	Dep.	Yes	—	Yes	LES	31	365	E	No
Gallant (1991, Females)	48	Dep.	—	—	—	Parenting Stress Index	0	183	E	Trend
George (1997)	164	Mood	—	—	—	Manipulation	0	—	E	Trend
Dixon & Baumeister (1991)	60	Mood	—	—	—	Manipulation	0	—	E	Yes
Rafaeli-Mor (1998)	69	Dep.	Yes	—	Yes	APES	14	14	E	Yes

Note: WB = well-being; Dep. = depression; SE = self-esteem; WBcov = Time 1 WB as covariate; Add = additional covariates in the model (besides well-being, *H,* and stress); Probe = clear statement of effect probing; CSLES = College Students Life Events Scale; SRRS = Social Readjustment Rating Scale; NLEQ = Negative Life Events Questionnaire; LES = Life Events Scale; NEI = Negative Events Inventory; LOPES = Louisville Older Person Event Scale; GSRLE = Geriatric Scale of Recent Life Events; A-FILE = Adolescent–Family Inventory of Life Events and Chances; APES = Adolescent Perceived Events Scale; Lg = lag between Times 1 and 2 (in days); Frm = time frame of stress measure (in days); Eff = the direction of the interaction term consistent with a buffering (B) or an exacerbation (E) effect; Sig = significance.

lationship. Accordingly, the preceding frequency information was entered into two sign tests, which yield a probability estimate for a given set of results. In the first test, we dichotomized the data into positive and negative results. The five null results were excluded from this analysis. A binomial test was conducted with the expected proportion of negative studies set at $p = .5$ (because under the null hypothesis, the mean coefficient should have a value of 0, with half of the observed coefficients falling to each side of it). The observed proportion of negative studies was 12/19, or 63%; this was far from a statistically aberrant finding ($p > .25$).

In the second sign test, we dichotomized the data by placing the nonsignificant studies along with the positively significant studies in one group and contrasting it with the negatively significant studies. Recall that Linville's (1987) buffering hypothesis posits a significant negative interaction of *H* and stress. A binomial test was conducted, with the expected probability set at $p = .025$. (With a two-tailed significance level set at $p = .05$, the null hypothesis would provide an expected probability of one half of that, or $p = .025$, for statistically significant results.) The observed proportion of negatively significant studies was 7/24 (or 29%), considerably greater than a chance occurrence, $\chi^2(1, N = 24) = 70.02$, $p < .001$. However, a similar test for the positively significant studies yielded comparable results: 4/24 studies (17%) were positive and significant, a finding that is considerably greater than chance as well; $\chi^2(1, N = 24) = 19.76$, $p < .001$.

Summary

We conducted the vote-counting procedure as a "quick-and-dirty" test of the stress-buffering role of SC. Both a simple inspection of the results, as well as two sign tests, offered little support for the buffering hypothesis. Although several studies yielded negative interaction effects, most did not, and several yielded positive interaction effects—giving equal credibility to an exacerbating role for SC.

Some pitfalls of multiple regression models, and particularly of meta-analyzing such models, have already been noted. The vote-counting procedure itself sidesteps some of these problems but is powerless to address an additional pitfall, cogently warned against by Solomon (1994). Solomon reported a multiple regression model that seemingly supported the buffering effect. Importantly, the author performed one of the few adequate post hoc probes of this interaction effect. In doing so, he discovered that the negative interaction coefficient was due to high baseline depression in the

highly self-complex group rather than a buffering of the reaction to stress. In discussing this finding, Solomon went on to question whether other samples, including the one in Linville's (1987) study, might not have the same pattern of nonsupportive relationships underlying seemingly supportive results. Only 7 of the 24 studies aggregated in the vote-counting procedure reported post hoc probing of the interaction effects, but this smaller group of studies failed to converge on a single conclusion. In fact, as in Solomon's (1994) case, they add to the ambiguity by suggesting that even seemingly supportive results could, on probing, run counter to the predicted buffering effect.

Discussion

We had two purposes in this article. The first was to conduct a comprehensive research synthesis, bringing together as much of the existing evidence on the relationship between SC and well-being and clarifying the conditions under which SC buffers stress. For this purpose, we chose to focus exclusively on the most extensively studied version of SC, namely, Linville's (1985, 1987) social cognitive model of complexity. Two types of analyses were conducted: a classical meta-analysis on a larger number of studies and a vote-counting procedure on a smaller set of studies that reported interaction effects of stress and SC.

The second and more theoretical purpose was to critically review the emergence of research on SC. Earlier we placed it within the broader landscape of research exploring information theory, cognitive structure, and cognitive complexity. We reviewed several alternative models of SC, noting the importance of thinking about integration and differentiation as key constructs in examining self-structure.

Our discussion ties together these two purposes. We begin by addressing the results of the meta-analysis and the vote-counting analysis focused on Linville's (1985) SC. In discussing the results of the primary prediction of the synthesis, we examine implications to Linville's (1985) SC model, as well as to competing theoretical models. In discussing the results of the secondary predictions, we offer possible substantial and methodological explanations for the considerable heterogeneity of effect sizes in this field. We then shift our attention to theoretical implications that extend beyond Linville's (1985) model to the study of SC and self-structure as a whole. We conclude with some suggestions for future research.

Reviewing Results

Primary predictions. Our primary prediction in the meta-analysis was finding differences among studies from the three (prospective stress, prospective uplift, or zero-order) design groups in the strength and sign of the relationship between SC and well-being. Differences were indeed the case, although they only partly conformed to the prediction. Studies that included a uniform negative stressor yielded a weak but positive relationship between SC and well-being. Studies that included a uniform positive uplift yielded a stronger negative relationship between SC and well-being. Finally, studies with neither stressor nor uplift yielded a weak, but negative, zero-order relationship between SC and well-being.

The primary prediction of the vote-counting analysis was that SC would buffer the effect of stress (in the 24 studies using multivariate designs and variable levels of stress). Although several studies reported the appropriate interaction terms, the analysis as a whole did not support the buffering hypothesis: Most studies yielded nonsignificant effects, and some actually supported an exacerbation effect.

The mixed findings of the meta-analysis, as well as the lack of support for the buffering hypothesis in the vote-counting analysis, can be used to examine the competing theories bearing on the SC/well-being relationship. Earlier, we reviewed two models with competing predictions; these predictions, along with the obtained results, are plotted in Figure 1. Linville (1985, 1987) presented the construct of SC as a buffer of extreme affective reactions to both positive and negative life events. Her model predicts positive effects in the presence of stress and negative effects in the presence of uplifts but makes no strong prediction regarding the zero-order relationship of SC and well-being in the absence of stress measurement.

Donahue et al. (1993; cf. Block, 1961, Campbell et al., 2000) presented a model that de-emphasizes stress levels. Instead, this competing model focuses on the deleterious effect of a differentiated (i.e., complex or fragmented) self-concept. According to this model, SC

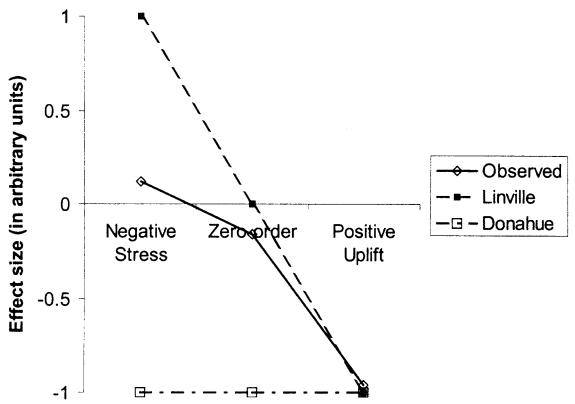

Figure 1. Comparison of obtained results with predicted effect sizes by study design (predictions based on the two competing theories).

should be negatively associated with well-being at all levels of stress or uplift.

As Figure 1 clearly shows, neither theoretical model perfectly predicted the pattern of results obtained across the three design groups. Contrary to Donahue et al.'s (1993) model, SC was not associated with poor outcomes under stressful conditions and was only weakly associated with poor outcomes in zero-order studies. Contrary to Linville's (1985) model, there is very weak evidence for a buffering relationship in the negative stress studies as well as a weak but consistently negative relationship in the zero-order studies. Moreover, the vote-counting analysis failed to support the predictions of the SC model regarding stress buffering.

Both models predicted the obtained negative relationship between SC and well-being in uplift studies, but neither predicted its asymmetry with the (unobtained) positive buffering relationship in stress studies. Does this asymmetry imply that SC only buffers the effect of positive events, or at least is a more powerful buffer of such events? If so, are the mechanisms proposed by Linville (1985) to cause the SC effects (i.e., affect containment and spreading-activation processes within the self-schema) different for positive and negative affect? These questions cannot be explored within this synthesis but can serve as the basis of future work.

Although the absence of robust buffering effects in the stress studies and in the vote-counting analysis is the most notable finding of this research synthesis, another one is the weak but consistently negative zero-order relationship between SC and well-being. This finding does not fit squarely within either Linville's (1985, 1987) or Donahue et al.'s (1993) models but may result from a combination of two competing forces. On the one hand, as Donahue et al. put it, differentiation or fragmentation may at times indicate a (pathological) lack of a core identity or self. Moreover, the mere maintenance of a complex self may be a costly enterprise. Indeed, Linville (1987) herself raised that possibility in stating that "maintaining multiple distinct self-aspects may be a source of chronic, low-level stress, perhaps because of role conflicts or multiple demands on time and attention" (p. 672). Additionally, some positive life events are likely to be present in daily life, even when such uplifting events are not measured. Because SC and well-being are negatively related in the context of positive life-events, the (trace) presence of positive events may account for some of the negativity of the SC/well-being zero-order relationship. On the other hand, just as the absence of measured uplifts does not guarantee an absence of uplifts in the zero-order group of studies, the absence of measured stress does not guarantee an absence of stress in the lives of the studies' participants. Thus, consistent with the arguments raised by Coyne and Whiffen (1995), a buffering effect (albeit a weak one) may be taking place in such studies. Thus, it is possible that the weak but negative relationship seen in the zero-order studies group results from a combination of an overall negative (zero-order) relationship between SC and well-being, tempered by a moderate positive (buffering) effect when stress is actually present (though unmeasured).

To summarize, very little support was found for the proposed mood-buffering effect of Linville's (1985, 1987) SC on negative life events or stress; in contrast, more support was found for the buffering of SC on positive life events or uplifts. At the same time, little support was found for an across-the-board negative relationship between SC and well-being, as was expected under the self-differentiation model of Donahue et al. (1993); however, in zero-order studies, high complexity was associated with mild negative affect.

Secondary predictions. In addition to the primary prediction regarding SC under different stress conditions, we examined several secondary predictions regarding potential sources for the differences in effect magnitude within each of the study groups. For the most part, these predictions (which are summarized in Table 4) were confirmed.

Several of the secondary predictions addressed the effects of the scope and generalizability of the studies on the strength of the SC/well-being relationship. As expected, the more constrained and internally consistent and the less externally valid the study is, the stronger the obtained relationship between SC and well-being. This was borne out when comparing the stronger findings based on mood (i.e., relatively narrow band) with the weaker ones based on self-esteem or depression (i.e., relatively broader band well-being). This inverse relationship between generalizability and effect size was also borne out when considering the time lags used in the designs (weaker effects with time lags that extend beyond the experimental session); the integrity of the experimental sessions (more controlled sessions, with fewer participants, yielding stronger effects); and the veracity of the stressor used in negative stress studies (false feedback studies yielding stronger effects than naturalistic stressors).

The effect of a temporal separation between the measurements of SC and of well-being was examined both as a categorical comparison (same session vs. different sessions) and as a continuous model (time lag). As noted previously, both results were consistent with the conclusion that greater generalizability (i.e., protracted temporal separation) was associated with weaker effects. An anonymous reviewer suggested an interesting explanation for this time lag effect. According to this explanation, high SC serves as a buffer to the extent that the self is activated. In single-session studies, we can assume such activation, if only because participants spend some time completing a self-descrip-

tive task. In contrast, according to this explanation, any residual activation of the self would have dissipated by the time well-being is measured in time-lagged studies. This hypothesis certainly warrants future attention (although it does not seem to flow directly from Linville's [1985] model).

The comparison between naturalistic and manipulated stress also warrants attention. Some readers may question whether laboratory and naturalistic studies should be combined into one category. However, as we explained earlier, Linville (1985, 1987) explicitly tested her model in both kinds of designs. In addition, although naturalistic stress studies offer greater external validity and laboratory stress studies greater internal reliability, they all purport to examine the same phenomenon—the buffering effect of a participant's SC level on real (or simulated) stress. Indeed, our finding that only laboratory stress studies yield the SC buffering effect, and the questions it poses regarding the generalizability of the effect, would not have been documented unless we had included both laboratory and naturalistic studies.

Another prediction involved a statistical index problem faced by many meta-analysts. Specifically, several of the studies that were of greatest interest to us (i.e., the stress and uplift studies) failed to provide zero-order correlations, and we were forced to rely on the reported statistics, typically change scores or partial correlations. As we expected, the effect size estimates based on such statistical corrections were much stronger than those based on zero-order correlations. Short of excluding these studies, we can only note the existence of this possible artifact and encourage authors to include both corrected and uncorrected statistics in their reports.

One prediction applied only to studies within the zero-order group but is nonetheless important to discuss. We contrasted the findings of studies using purely clinical samples, purely nonclinical samples, or a comparison of a clinical with a nonclinical sample. We found the same weak-but-negative zero-order relationship between SC and well-being appeared within both clinical and nonclinical groups. However, in the few studies comparing clinical to nonclinical samples, a consistent difference emerged: Clinical participants (usually those with depression) obtained lower SC scores than nonclinical participants. Although many factors could be at play here, we find two explanations to be particularly plausible. First, this difference might support the view of SC as a diathesis for psychopathology. Specifically, as Linville (1987) suggested, individuals vulnerable to affective psychopathology might indeed differ from nonvulnerable individuals in the degree to which their self is complex. However, a second possibility is that this difference may reflect not so much a vulnerability to psychopathology as an effect of it. Specifically, it may reflect the decreased motivation of individuals with major depression or other forms of psychopathology. Because scores in the open-ended SC task are often affected by the willingness of the participant to persist in creating more self-aspects, the group difference may simply reflect a symptom of the pathology. Only if SC were assessed prior to the onset of depression would it be possible to differentiate these possibilities.

The difference observed between studies conducted by social psychologists and those conducted by clinical or applied researchers warrants some attention. In both the stress and the uplift groups, social psychology reports yielded effects that were stronger (and often time opposite in direction) to those emerging from applied psychology reports. This difference is clearly not compelling for its causality; it is most likely that the differences between the two subdisciplines could be reduced to differing methodologies or to the differences in scope and generalizability of studies that we discussed previously. However, this "subdiscipline effect" is important for its cautionary value. If the two literatures were reviewed separately, or if investigators from one subdiscipline were exposed only to findings from within their area, the conclusions drawn would clearly be lopsided.

Finally, a theoretically significant secondary prediction involved the effect of trait-list valence on the SC/well-being relationship. As expected, varying the content of the trait list used in the SC task did affect the obtained relationship with well-being. Specifically, when the SC task included a greater proportion of negative traits, the buffering effects were stronger. In other words, if anything, it appears that it is particularly the complexity of negative self-descriptive information that buffers the impact of positive or negative events. Starting with Linville's (1985) split of two thirds positive, one third negative traits, different investigators have used varying proportions of positive and negative traits, most often de-emphasizing the negative ones. If anything, our secondary finding argues to the necessity of including more negative traits in the self-descriptive sorting tasks. It also supports the findings of Morgan and Janoff-Bulman (1994), Rafaeli-Mor et al. (1999), and Woolfolk et al. (1995) in emphasizing the sensitivity of the *H* statistic to valence.

Measuring SC

It is important to raise two general considerations regarding the measurement of SC, which apply to all studies utilizing the *H* statistic (Scott, 1969) in examinations of SC. As demonstrated by Rafaeli-Mor et al. (1999), *H* is problematic as a measure of the complexity construct. First, *H* has poor internal consistency: Scores based on positive and negative self-information are only weakly correlated. Indeed, the significant continuous model for proportion of negative words in the trait list, reported earlier, confirms this.

A second and more serious shortcoming is that the *H* statistic does not tap both of the hypothetical mechanisms underlying complexity. Specifically, *H* scores are strongly related to the first mechanism, the number of self-aspects (typically showing a correlation of about .7: e.g., Linville, 1987; Rafaeli-Mor et al., 1999). However, these scores are weakly related in the wrong direction to the second mechanism: the overlap of aspects.

We noted these major psychometric limitations in our introduction. At the same time, we explained that the popularity of *H,* and its purported ability to represent both of the mechanisms underlying SC, led us to select the literature using it to be the focus of this synthesis. This focus has required a balancing act between being descriptive (of the literature as it is) and being prescriptive (of how we envision SC research should be conducted). If the sections preceding this one were more descriptive, we now shift the balance and turn to a more prescriptive emphasis.

We believe that the limitations in the psychometrics of complexity require caution in the interpretation of studies utilizing such measurement; indeed, they may be responsible for some of the surprising findings in this research synthesis. For example, it may be that the active buffering component of complexity is actually the overlap among aspects and not the number of aspects. Because individual differences in overlap are simply not reflected in the *H* statistic, the small overall effects found in this synthesis, particularly for negative stress studies, may be a poor reflection of the real effect of complexity. Several studies (e.g., Buder-Shapiro, 1995; Gardner, 1997; Rafaeli-Mor & Brown, 1997) have addressed this topic and may aid in clarifying the differential effects of overlap (integration or unity) and of the numerosity of self-aspects (differentiation). Rafaeli-Mor and Brown, for example, reported that the buffering effect is limited only to low overlap and to the interaction of low overlap with high number of aspects; in contrast, the number of aspects (and the closely related *H* statistic) failed to serve as buffers. Moreover, this buffering occurred only to severe, but not to minor, daily stress.

Conceptualizing and Assessing Self-Structure

Why is the *H* statistic so empirically problematic? One possibility noted earlier is that *H,* as a measure of dispersion or variability, does not truly capture complexity, at least not in the sense of both differentiation and integration. Instead, as Scott (1962) noted, it could reflect randomness and disorganization. A second possibility, presented by Rafaeli-Mor et al. (1999), was that the *H* statistic, originating as it does from information theory (Attneave, 1959), entered into psychology within a multidimensional model of knowledge structures (Scott, 1969). Thus, it seems best suited for multidimensional models of knowledge. In contrast, Linville's (1985) SC model uses a categorical approach to self-knowledge. The two underlying mechanisms driving the SC buffering process (the existence of alternative self-aspects and the degree of spreading activation among them) rely on a hierarchical, categorical view of the self-schema. Significantly, both of the alternative measures suggested by Rafaeli-Mor and his colleagues (1999) assume a categorical nature: The quantity measure (number of self-aspects) is a count of the categories used, and the overlap measure reflects category similarities. The theoretical appropriateness of the alternative measures, but not of the *H* statistic, may explain the empirical problems encountered with it.

We have not been alone in suggesting improvements in the assessment and operationalization of self-structure. Notably, Campbell et al. (2000) provided a thoughtful analysis of the self-structure field and arrived at several suggestions with which we agree. For example, they suggested moving away from the binary (categorical) notion of either including or excluding a certain trait from any self-aspect. Indeed, they suggested moving to a dimensional framework in which each self-aspect is placed along the continua of all self-relevant dimensions. This would allow computation of other, parametric, statistics, such as Scott's (1969) dimensionality (*D*) index. In effect, Campbell and her colleagues (2000) have argued for the resumption of a multidimensional model of cognitive space, as it was conceptualized by Kelly (1955), Scott (1969), Shrauger and Patterson (1976), and others.

It is noteworthy that Linville did not include multidimensional models of cognitive space as one of the alternatives in a review identifying possible cognitive models of self-knowledge representation (Linville & Carlston, 1994). Indeed, such multidimensional models seem to get little (if any) attention today. This is not to say that they are not worth (re-)developing. However, until such a dimensional framework becomes the norm, we are stuck with a mismatch between a middle-level categorical model (Linville's [1985] SC model), using a dimensional index borrowed from a high-level dimensional theory (the 1950s and 1960s multidimensional approach to memory and self-knowledge).

In discussing this mismatch, we move from intratheoretical limitations of Linville's (1985) SC model to limitations of the whole SC field, which are of a more metatheoretical and historical nature. We are struck by the similarity between the problems in the recent literature examining SC and those in the earlier literature examining cognitive complexity. The two questions that caused confusion in the study of cognitive complexity

in the 1950s and 1960s have returned to plague the study of SC in the last 2 decades.

First, as we indicated in a review of different SC models, the SC construct has been taken to mean the integration of the self-concept, its differentiation, the two of them separately, or the two combined. Together with Campbell et al. (2000), we hope the field will be able to adopt a more consensual terminology. Of course, however, such a terminology requires agreement on the underlying model of self-knowledge representation, an agreement yet to be developed.

Second, as was the problem with the cognitive complexity literature, the question "complexity of what?" has not been uniformly answered. When we say an individual is complex (or differentiated, or integrated), do we mean that he or she views a particular domain (e.g., personal relationships) using a complex encoding process? This would imply that complexity is a cognitive process. On the other hand, does it simply mean that personal relationships are represented in a complex way, and therefore that complexity is a feature of cognitive structure? When the domain in question is "the self," the same question can be asked as follows: Is SC a feature of the I, the self-as-knower, or of the me, the self-as-known? Even with the restricted focus on Linville's (1985) SC, at times within the same article, authors sometimes refer to the complexity of the perceptual system (the I) or the complexity of the perceived domain (the me).

Summary

Choosing to focus on Linville's (1985) SC model has meant examining a literature reliant on one particular index, the *H* statistic. That statistic reflects differentiation or randomness and not complexity as a whole. Hence, we believe the conclusions of this synthesis (and of all studies using Scott's [1969] *H* as an index of complexity) inform us only about the role of one type of differentiation, its zero-order relationship with mood, and its ability to buffer stressors or uplifts. Differentiation, and its role vis-à-vis well-being, are certainly worthy of study in their own right. Indeed, it is useful to know that highly differentiated individuals may be mildly more distressed, will react somewhat less negatively to stressful life events, and will respond considerably less positively to positive life events. Recognizing this pattern generates further questions about the dynamic processes that govern this relationship, its temporal organization, and the reasons for the perplexing asymmetry between positive and negative events. Further research could attempt to distinguish between the effects of actual social-role differentiation and of perceived, or cognitive, self-aspect differentiation.

The intratheoretical and metatheoretical limitations notwithstanding, an extensive literature has utilized the *H* statistic to date. We have reviewed the relevant theoretical models, provided a review of the extant findings in this field, and attempted to couple the two together. To summarize the findings: Under conditions of objective and identifiable stress, a higher standing on Linville's (1985) SC dimension was only weakly associated with superior well-being. Even this weak relationship may be a product of statistical overestimation. Under conditions of objective and identifiable uplifts or when life events were not measured at all, higher SC was associated with poorer well-being.

The results of the synthesis suggest three insights into the role of Linville's (1985) SC (or by "differentiation," as we argue it should be identified) in psychological well-being. First, very little support was found for the proposed mood-buffering effect of Linville's SC on negative life events or stress (Linville, 1985, 1987). To most readers interested in the SC construct, this insight would be the most striking finding of this meta-analysis. The construct of SC was enthusiastically welcomed as a compelling candidate for stress buffering by both social and clinical psychologists. Some clinical researchers (e.g., Segal, 1988) expected SC to provide "important data ... from which deviations associated with a depressed state can be better characterized" (p. 156). Thus, to many the lack of clear support for a buffering effect would be disappointing.

Second, as suggested by Donahue et al. (1993), it appears that high complexity, or self-concept differentiation, does have a mild depressogenic effect. This conclusion is borne out by the combined results of the zero-order studies, the most numerous group of studies in this analysis. Given this conclusion, it seems that only a combination of the two models, Linville's SC model (1985, 1987) and Donahue et al.'s self-differentiation model, can account for the findings of the aggregated studies. In particular, a combined model would need to pay particular attention to the divergent role of Linville's (1985) SC in the contexts of major and acute life events and of more quotidian and diffuse stress.

The third and final insight emerging from this research synthesis echoes the ideas of Campbell et al. (2000). We see it as essential that future examination of differentiation, integration, and of the SC construct try to eliminate the ambiguity in interpreting results by explicitly addressing the underlying representational model and by adjusting the measurement to avoid a mismatch with the theoretical model.

We embarked on this research synthesis with two purposes. The first was to find out the empirical status of Linville's (1985, 1987) SC/affective-extremity model. The second was to look beyond this model at the SC field as a whole and at its roots in the study of cognitive structure and cognitive complexity. Although we conclude that there is little support for some of the major positions of Linville's (1985, 1987) specific model, we remain enthusiastic about the general field. In particular, we be-

lieve that research incorporating the conceptual, methodological, and psychometric suggestions made herein would benefit our understanding of individual differences in cognitive structure.

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