



Measuring Political Sophistication

Author(s): Robert C. Luskin

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## *Measuring Political Sophistication\**

Robert C. Luskin, *University of Alabama and Princeton University*

The vast, discordant literature on political sophistication, still divided over the variable's distribution in mass publics, is correspondingly divided over measurement. This paper, focusing on measurement, weighs the merits in these disputes. I first review the variable we all claim to be measuring, then the measures the literature affords. In the process I sketch several measures of my own and compare their empirical performance. Then, finally, I examine the distributional implications and offer some thoughts on future directions for sophistication research.

Few variables are more central to public opinion research than political cognitive complexity a la Campbell et al. (1960) and Converse (1964), increasingly known as "political sophistication." Most sophistication research has focused on the variable's distribution in mass publics, but since different measures have produced very different results, disputes over distribution have fueled and been fueled by disputes over measurement. The field is in epistemic disarray. Measures are criticized as too lenient; the same or similar measures are criticized as too strict; and despite all criticism, the same or similar measures continue to be used.

No one paper can possibly bring this melee to order, but I want here to do what I can—to revisit the variable we all claim to be measuring, to evaluate the measures we use, and to examine the distributional implications. The value of the enterprise would go without saying, were it not for Kinder's (1983) recently having pronounced the distributional question answered and urged the discipline to turn its attention from sophistication to more specific cognitions and cognitive structures. I certainly agree that the distributional question has been answered—I shan't spoil the suspense by saying at this point whether I agree with Kinder's idea of the answer—but that is not the same as its having been *settled*. Even as Kinder was reaching his verdict, Jackson (1983) was reporting contrary

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results, and Inglehart (1985) and Peffley and Hurwitz (1985) have taken further issue since. Disagreement continues. But suppose for a moment that we all shared exactly the same picture of sophistication's distribution. Even then, the abandonment of sophistication research would not follow. Other—in fact, more important—questions would remain. Scattered results suggest influences on other aspects of political thought and activity (e.g., Converse, 1964; Iyengar, Peters, and Kinder, 1982; Knight, 1985), but we need to know much more about sophistication's place in the causal scheme of things—what affects it, what it affects, and in what ways.

We shall not get very far without decent measurement. Distributional disputes may die down—I hope this paper will hasten the process—but the question of measurement will linger. What measures are worth retaining? What measures should be discarded? In what ways can usable measures be refined? Some of this ground has been covered elsewhere, but not always well and never fully. Even Kinder's splendid review stops well short of surveying the field. This paper attempts a more comprehensive evaluation. I shall argue that while some existing measures can serve adequately, many others have strayed hopelessly far from the concept and should be scrapped. Along the way, I shall sketch several measures of my own and compare their performance. And, at the end, I shall comment again on the directions in which I believe sophistication research should be heading and consider some possibilities for more radically improved measurement.

### Political Sophistication

Appropriately enough, most sophistication research skips rapidly past definition. Most papers barely outline the variable, trusting a citation to Campbell et al. (1960) or Converse (1964) to do the rest. Clearly, we all think we are talking about the same thing. But are we? Some usage of sophistication-related terms like “constraint” and “ideology” makes one wonder. Since measures succeed or fail only in relation to what they are supposed to be measuring, it is important to get these things straight from the outset. What, then, is political sophistication? Let me attempt to distill and clarify the Campbellian/Conversian variable we all claim to be talking about.

Sophistication in this sense is a matter of cognition. The most elementary cognitions—of individual, tangible objects—are the bits of memory, the words in the grammar of thought. More complex cognitions link more elementary ones in much the same way as phrases or sentences link words. Cognitive psychology speaks of *associations*, social psychology of *beliefs* and *attitudes*. Whatever the terminology, such intercognitive

propositions are the beams and girders of cognitive organization. At the same time, of course, they are cognitions themselves. As Scott (1963) observes, the distinction between content and structure is not absolute: “though a structure constitutes a relation among elements, it may itself form an element in some superordinate structure” (p. 266). Simple associations may be joined in more complex associations, which may be joined in still more complex associations, and so on. Schema theorists refer to relatively large packages of associated cognitions as *schemata*. What is for present purposes the same wine comes in several bottles.<sup>1</sup>

These references to organization bring us to the edge of Converse’s (1964) notion of “constraint.” A *constraint* (as opposed to constraint) is simply an association—a belief or attitude. Presumably, most constraints are abstractive ( $x$  is a  $y$ , a more inclusive category), predicative ( $x$  is  $y$ , an attribute), causal ( $x$ —possibly in conjunction with  $z$  and  $w$  and . . .—induces  $y$ ), or quasi deductive (if  $x$ —and, possibly,  $z$  and  $w$  and . . .—then  $y$ ). They need not be strictly logical (hence the “quasi” in front of “deductive”). In Abelson and Rosenberg’s (1958) telling hyphenation, they are “psycho-logical.” They need only make sense to the person whose cognitions they are.

A person’s political cognitions, together with those with which they are constrained, are what is commonly known as his or her *political belief system* (PBS). At any given moment very little if any of a PBS is conscious—in working memory. Cognitions spend most of their time in longer-term storage, out of the way but subject to recall. The important thing, the thing that makes them cognitions, as opposed to *noncognitions*—a generalization of Converse’s (1970) *nonattitudes*—is that they are there to be recalled.

Now, some of a PBS’s cognitions are more *central* than others—more directly associated with more of the rest.<sup>2</sup> Cognitions are rather like quarks, but suppose, counterfactually, that we could isolate, identify, and count them all. For a PBS of  $N$  cognitions, arbitrarily numbered from 1 to  $N$ , we might reasonably define the centrality of the  $i$ th cognition as

$$m_i = \frac{1}{N-1} \sum_{j=1}^N c_{ij} \frac{1}{d_{ij}},$$

<sup>1</sup>I do not mean to depreciate schema theory or research, only to say that it does not matter here whether we define sophistication in terms of cognitive atoms (elementary cognitions) or molecules (schemata). Either way, sophistication is a matter of the belief system’s density, coverage, and organization (see below).

<sup>2</sup>For similar definitions, see Converse (1964, 1970), Scott (1969, 1974), and Rokeach (1968).

where  $c_{ij} = 1$  if the  $i$ th cognition is directly or indirectly associated with the  $i$ th and  $= 0$  if not, and  $d_{ij}$  is the “geodesic” or minimum number of intercognitive links to get from  $j$ th cognition to the  $i$ th (or vice versa).<sup>3</sup> Note that  $m_i$  ranges from 0 to 1, equaling 0 when the  $i$ th cognition is neither directly nor indirectly associated with any other, and 1 when it is directly associated with every other. The formula, however, is not the point. Any weight  $w_{ij}$  monotonic with  $1/d_{ij}$  will do.<sup>4</sup> So will various formulas designed to measure centrality within social networks (see Bonacich, 1972; Freeman, 1979). The important points, independent of mathematical detail, are: First, that centrality implies meaning. More numerous and closer connections mean more information, and closer to hand. And, second, that centrality implies usage. The more cognitive routes a cognition lies on, and the more direct, the more frequently thoughts will pass through it. A person who thinks of a great many politicians, groups, ideas, etc., as being to varying degrees “liberal” or “conservative” will find those terms more meaningful and will use them more often than will a person for whom their associations are fewer.

Three of the dimensions on which PBSs vary define political sophistication. The first is *size*: the number of cognitions the PBS contains ( $= N$ ). Some people might as well be living on Neptune for all they know about politics; others are walking *Washington Posts* or *New York Timeses*. The second is *range*: the PBS’s coverage of the political universe. At any given level of knowledge, some people specialize rather narrowly (in race relations, or social security benefits, or political personalities, for example), while others sample more widely. If the political domain could be partitioned into mutually exclusive, exhaustive, and indiscernible categories—one for each sort of issue, each sort of actor, and so forth—we might reasonably define a PBS’s range as the dispersion of cognitions among categories. The third dimension is organization, or *constraint*. This, as Converse (1964) intended it, is the extent to which the PBS’s cognitions are interconnected. More precisely, we might define constraint as

<sup>3</sup> Some weighting for distance is necessary because without one all cognitions would be equally central, except in the case of PBSs decomposable into unconnected clusters (and even there all the cognitions within a given cluster would be equally central).

<sup>4</sup> For  $w_{ij} = 1$  for direct connections and 0 for indirect connections,  $m_i$  becomes the unweighted proportion of the other cognitions to which the  $i$ th cognition is directly linked. This is Freeman’s (1979) “degree” of centrality. Appealingly, constraint (as defined presently) would then equal mean centrality. Not so appealingly, however, this weighting would slight indirect connections. A cognition whose direct associations are few but whose associations at one or two removes are many would be defined as peripheral.

the mean proportion of the other  $N - 1$  cognitions with which any given cognition is directly associated:

$$C = \frac{1}{N} \sum_{i=1}^N \frac{1}{N-1} \sum_{j=1}^N a_{ij} = \frac{1}{N(N-1)} \sum_{i=1}^N \sum_{j=1}^N a_{ij}$$

where  $a_{ij} = 1$  if the  $i$ th cognition is directly connected with the  $j$ th and  $= 0$  otherwise. Or, equivalently, as the proportion of all possible pairs of cognitions in which the elements of the pair are directly associated:<sup>5</sup>

$$C = \frac{\sum_{i=1}^N \sum_{j=1}^N a_{ij}/2}{N(N-1)/2} = \frac{1}{N(N-1)} \sum_{i=1}^N \sum_{j=1}^N a_{ij}.$$

A PBS is maximally constrained ( $C = 1$ ) when every cognition is directly (and therefore indirectly) associated with every other, and minimally constrained ( $C = 0$ ) when no cognition is directly (or therefore indirectly) associated with any other.

*Sophistication* is the conjunction of these dimensions. A person is politically sophisticated to the extent to which his or her PBS is large, wide-ranging, and highly constrained. The continuum extends from PBSs consisting of "large, intricate lattices" of cognitive material to those consisting only of "scattered croutons floating in undifferentiated cognitive soup."<sup>6</sup> In schematic terms, similarly, political sophistication is the number, diversity, and organization (both internal and interschema) of a person's political schemata (cf. Conover and Feldman, 1984; Graber, 1984). The rephrasing from micro- to macro-cognition requires only the acknowledgment of internal organization.

This definition should be clear, but in view of the conceptual mists overhanging this literature, a number of points may need amplification:

(1) Constraint is an individual-level phenomenon, psychological and at least potentially conscious, in the sense of dwelling in long-term memory. This is perfectly obvious when we refer to "cognitive organization," but easy to forget when we substitute "constraint." Perhaps it is just the usual shorthand of naming measure after concept, but many authors seem at times to treat constraint as if it denoted the observed "consistency" of a person's opinions (Nie and Anderson, 1974; Nie and Rabjohn, 1979; Darcy, 1980) or the aggregate "consistency" or patterning of opinions across individuals (Nelson, 1977; Bennett, Oldendick, Tuchfarber, and Bishop, 1979; Klingemann, 1979b). "Consistency" of either sort may reflect constraint (*inter alia*), but is not the same thing.

<sup>5</sup>In this form, constraint is Zajonc's (1960) "degree of unity."

<sup>6</sup>Images I wish were mine, but are from Abelson (1968, p. 528).

(2) Sophistication involves size and range as well as constraint. Most people have tightly knit clusters of cognitions about the political objects that most intrude on their lives. Farm, educational, and racial policies are likely foci of constrained cognitions for farmers, teachers, and blacks, respectively. But such clusters may be either small and scarce or essentially confined to some narrow sector(s) of the political universe. And we should hardly regard a person who holds a small handful of cognitions about each of a small handful of political topics as politically sophisticated, however interwoven those cognitions might be.

(3) Size, range, and constraint are related. The size-range relationship is obvious: larger PBSs tend to cover more ground, and vice versa. But both are also related to constraint. Experiments show that people store information largely by organizing it or adopting an organization that comes with it (Tulving, 1962; Mandler, 1967; Bransford and Johnson, 1972; Buschke, 1976). The more copious or diverse the information, the more necessary, and the more highly organized, the organization is likely to be. Along the reverse causal arrow, organization makes retention easier and hence more likely. Thus Neuman (1981) finds a correlation of .67 between measures of constraint, on the one hand, and size and range, on the other.

(4) Political sophistication is just the political case of a more general variable. Within any stimulus domain—religion, baseball, fashion—some people's belief systems are larger and denser than others'. One name for this variable is *cognitive complexity* (as in Scott, 1963, 1969; Schroder, Driver, and Streufert, 1967; Tetlock, 1983, 1984). Political sophistication is cognitive complexity about politics. Constraint translates as *integration*; size and range, loosely, as *differentiation*.<sup>7</sup> Another name for this same variable, in the information-processing literature, is *expertise* (as in Larkin et al., 1980; Fiske and Kinder, 1981; Fiske, Kinder, and Larter, 1983). Expertise is extensive, organized knowledge. Political sophistication is political expertise.

(5) In principle, a person can be politically sophisticated without making much use of such high-order abstractions as "liberal" and "conservative" and can be unsophisticated despite using them. The centrality of such concepts is not part of the definition (cf., Nie, Verba, and Petrocik, 1976; Holm and Robinson, 1978; among many others). Yet, contrary to Lane (1973) and Bennett (1975, 1977), sophistication and abstraction

<sup>7</sup>Definitions of "cognitive complexity" vary considerably (see Streufert and Streufert, 1978, for a catalog). Most frequently, "differentiation" refers to the dispersion of phenomenal objects on phenomenal attributes, rather than the phenomenal objects' and attributes' coverage of the objective domain, as here. For a definition like mine, see Neuman (1981).

are theoretically and empirically entwined. As Campbell et al. (1960, p. 193) remark, “any cognitive structure that subsumes content of wide scope and high diversity must be capped by concepts of a higher order of abstractness.” How else can such content be retained? Converse (1964, p. 214) may come nearer the nub: “economy and constraint,” he says—and he means here the economy achieved by abstraction—“are companion concepts.” This much is raw if plausible assertion, but numerous experiments lend support. The grouping and regrouping of information into increasingly abstract categories is among the commonest and most effective means of cognitive organization (Bower et al., 1969; Santa et al., 1975). Thus the more highly organized the PBS, the more abstract the most abstract of its relatively central elements should be, and the more central such high-order abstractions as “liberal” and “conservative” should be to it.

(6) An “ideology,” in the Conversian sense of this literature, is a particularly sophisticated PBS—large, wide-ranging, and highly organized.<sup>8</sup> We should most of us agree that an ideology is a PBS of at least near-eliteworthy complexity, although the line that separates ideologies from less-than-ideologies is ultimately arbitrary. Regrettably, the literature has concentrated on this dichotomy. Even with polytomous measures, attention usually centers on the distinction between the topmost or “ideological” category and the rest. But what matters more—what demands explanation and helps explain other variables—is “a person’s actual degree of sophistication, not merely whether or not it exceeds some high and necessarily arbitrary threshold” (Luskin, 1987a).

(7) Strictly speaking, then, it is entire PBSs, not individual concepts or the words that label them, that are or are not “ideological.” “Liberal” and “conservative” are only “ideological” in the borrowed sense of tending to be more central to ideological than to nonideological PBSs. The use of such terms may suggest but does not define an ideology.

(8) Ideology as high sophistication is comprehensive. A “racial,” “environmental,” or “New Politics” ideology (as in Aberbach and Walker, 1970; Miller and Levitin, 1976; Kritzer, 1978; or Pierce and Lovrich, 1980) is oxymoronic. An ideology runs more or less the gamut of political affairs, or it is not an ideology: Many a humbler word can denote mere regions of a PBS: “opinions,” “views,” and “ideas” spring to mind. At

<sup>8</sup> More idiosyncratically, Lane (1962, 1969, 1973) uses “ideology” to denote a PBS. A Conversian ideology is in Lane’s terms a “forensic” as opposed to a “latent” ideology: a PBS capable of generating “the articulate, well developed political arguments put forward by informed Marxists or fascists or liberal democrats,” as opposed to one capable of generating only “the loosely structured, unreflective statements of common men” (1962, p. 16). For Converse’s view of his differences with Lane, see Converse (1975a); for Lane’s, Lane (1973). Kinder (1983) provides an incisive discussion.



least, if the term must be used in this lesser sense, "ideology" as part should not be confused with "ideology" as whole.

(9) It similarly dismembers the notion to distinguish "conceptual," "informational," and "affective" ideologies (as in Pierce, 1970) or "conceptual," "informational," and "behavioral" ones (as in Coveyou and Piereson, 1977). These distinctions are based on a sort of implicit operationism: "conceptual ideologues" use "ideological" terms; "informational ideologues" define such terms abstractly; "affective ideologues" feel much more positively toward liberals than conservatives or vice versa; and "behavioral ideologues" vote for the presidential candidate to whom they place themselves closer on a liberal-conservative scale. These are different measures, not different kinds, of ideology. *All* ideologies are conceptual and informational, storing and organizing information with concepts. *All* involve affect and have behavioral consequences. An ideology, as Gertrude Stein might have put it, had she written more about public opinion and less about flowers, is an ideology, is an ideology.

(10) The dimensionality of highly sophisticated PBSs is an open and in ways unanswerable question (cf. Stimson, 1975; Herzon, 1980; Conover and Feldman, 1981; Judd and Milburn, 1980). Every PBS involves many concepts, on many dimensions, at many levels of abstraction. At the lowest levels the conceptual dimensions are narrow but numerous (although how numerous varies from person to person). At higher levels these narrow dimensions are successively subsumed by smaller and smaller numbers of more and more general dimensions, until at the very top, the dimensions are few and broad (although, again, how few and how broad varies from person to person). Do fewer or more numerous dimensions suggest greater complexity? Stimson (1975) takes the first position; Marcus, Tabb, and Sullivan (1974) the second. But the answer is probably both—more numerous toward the base (greater differentiation) but fewer toward the apex (greater integration). Impressionistically, many of the most sophisticated PBSs do seem to narrow to a single dimension at the peak, as witness Herzon (1980) and Conover and Feldman (1981) themselves. While arguing the multidimensionality of highly sophisticated PBSs, they interpret their results in terms of a single liberal-conservative dimension.<sup>9</sup>

<sup>9</sup>Conover and Feldman cite a correlation of only  $-.17$  between feeling thermometer temperatures of "liberals" and "conservatives" as evidence of multidimensionality. But a far simpler and sounder interpretation, and one in keeping with much of the rest of their analysis, is that this mild correlation is just another sign of the terms' lack of appreciable meaning to a hefty fraction of the population. Knight (1984) shows that the correlation depends strikingly on sophistication—strongly negative among the most sophisticated, distinctly positive (a result, she shows, of "positivity bias") among the least sophisticated.

(11) The “sophistication” in political sophistication has nothing to do with acuity. Its opposite is not naiveté or false consciousness but sheer ignorance. John Birchers and Trotskyites alike may be highly sophisticated in this sense. Sophistication is a matter of how much and how a person thinks about politics, not what.

(12) Sophistication is not the same as rationality. *Procedural* rationality requires only that people behave and believe in accordance with a sort of personal cost-benefit analysis, in which they assign costs and benefits as they choose. Only the deranged are procedurally irrational. Not everyone, however, is highly sophisticated. Procedural rationality is a constant, sophistication a variable. Indeed, their very rationality must lead many people to be *unsophisticated*, since many people are in fact politically unsophisticated, and whatever is, is procedurally rational. The perceived costs and benefits of amassing and organizing large quantities of political information must be such as to make most people aim very low (Downs, 1957; Riker and Ordeshook, 1973; Popkin et al., 1976). *Substantive* rationality, which requires acting as if from calculations on modeler-defined costs and benefits, is in contrast a variable. Whatever a modeler may assume, some people are more substantively rational than others. And, depending on the substance, this variable sort of rationality should be a function of sophistication. More sophisticated people, with their greater knowledge of political means and means-end connections, should do better at maximizing their “objective” interests. The relationship is causal, however, not definitional.

(13) Issue-orientation, too, is a separate variable, though related. At higher levels of sophistication, people are more likely to care about, have opinions on, and act in accordance with their opinions on policy issues (Miller et al., 1976; Knight, 1985). These relationships should be especially strong for “harder,” more abstract, as opposed to easier, doorstep issues (Carmines and Stimson, 1980; also Converse, 1964). Nevertheless, a person may care passionately about some issue(s) and act accordingly without being particularly sophisticated.

### Measuring Sophistication

Political cognitions, alas, are not directly apprehensible. We cannot actually count them; we cannot compute their dispersion among topics; we cannot calculate their mean connectedness. We can only infer them, and their properties, from what a person says or does. To take Browning out of context, we measure the mind’s height by the shade it casts. But some measures are far more direct, and far better, than others.

*Consistency*

Perhaps the most popular approach is to infer sophistication from the "consistency" of responses to attitude questions.

*Correlations between policy attitudes.* The earliest and most frequently used such measures are the mean correlations between policy attitudes (e.g., Campbell et al., 1960; Converse, 1964; Nie and Anderson, 1974; Miller and Levitin, 1976; Carmines and Stimson, 1982). This approach assumes, first, that the correlations reflect the aggregate patterning or "consistency" of responses and, second, that that in turn reflects the distribution of constraint. To the extent that the items are numerous and varied, the correlations may also reflect size and range, but more distantly and obliquely. I shall treat correlations at some length because they share and illustrate the problems of other consistency-based measures.

The most obvious problem is that correlations are only aggregate measures. They speak to the sophistication of populations or subpopulations, not of individuals (RePass, 1976; Coveyou and Piereson, 1977). Worse, they do not speak true. Correlations and other aggregate measures impose an effective sampling bias. The correlation between any two items is necessarily confined to the intersection of those answering each. But only some 70 to 90 percent of a mass sample will typically provide substantive, on-the-scale answers to political questions. The rest confess ignorance ("Don't know," "No opinion"). At best, therefore, correlations measure only the sophistication of the most sophisticated two-thirds or three-fourths of the population (Bennett et al., 1979; Rapoport, 1979).

But how well do they measure even that? Let us consider the two-part assumption on which the use of correlations rests. Critiquing the first part, Weissberg (1976) has argued that when items are sufficiently "consensual" (low-variance), near-zero correlations may coexist with near-perfect consistency. This is possible, says Weissberg, because correlations measure covariation, not consistency. Despite fairly wide acceptance (Barton and Parsons, 1977; Darcy and Aigner, 1980; Wyckoff, 1980), this point is moot. The relationship between correlation and consistency depends on the kind of correlation and the definition of consistency. I for one should feel comfortable defining consistency as "correspondence" (everyone who takes position  $A_i$  on issue  $A$  takes position  $B_j$  on issue  $B$  and vice versa) and nonconsistency as "unpredictability" (statistical independence), rather than "disagreement" (bivariate uniformity), as Weissberg prefers. Under these definitions, certain correlations—Kendall's tau- $b$ , Goodman and Kruskal's tau,

Pearson's  $r$ —measure bivariate consistency quite nicely (Luskin, 1987b).

Most of the correlations in the literature, to be sure, do not. The most popular, Goodman and Kruskal's gamma, is a particularly unfortunate choice. Gamma is notoriously inflated, its magnitude generally wafting high above that of any other correlation computed on the same data (note Figure 2 and Table 2 in Weisberg, 1974). More to the immediate point, gamma nears or equals 1.0 for configurations far from perfectly consistent qua correspondent, or in any other intuitive sense.<sup>10</sup> Another deficiency is that nearly all the correlations reported have been bivariate and therefore limited as measures of multiitem consistency. Multiple correlations would be better, squared multiple correlations better still.

Depending on the definition of consistency, these failings may be extrinsic—more appropriate correlations can be used—but aggregateness and sampling bias aside, the most fundamental objection to correlations is to the second half of the underlying assumption. The consistency-sophistication connection is weak. Too many things besides sophistication affect consistency. It is true that consistency reflects:

(1) Constraint. Most constraint is more naturalized than native, rooted more in mass and interpersonal communication than in individual ratiocination. Information comes structured, in sentences and paragraphs, not unconnected words, and we imbibe the structure with the rest. This in turn means that a great deal of constraint is shared. The distribution of positions on issue  $B$  that people see as “logically consistent” with position

<sup>10</sup>One example, for two trichotomous items, will suffice. Let  $X$ 's denote nonempty cells.

	$A_1$	$A_2$	$A_3$
$B_1$	$X$	$X$	$X$
$B_2$	$X$	0	0
$B_3$	$X$	0	0

A person who takes position  $A_1$  on Item  $A$  may take any position on Item  $B$ , and a person who takes position  $B_1$  on Item  $B$  may take any position on Item  $A$ . Perfectly consistent? Few of us should say so. Yet gamma equals 1.0. For other passing criticisms of gamma, see Coveyou and Piereson (1977, p. 97n) and Darcy (1980). Balch (1979) argues that gamma under (!) states consistency.

$A_i$  on issue  $A$  is rarely uniform. Frequently—perhaps typically—it is unimodal. Hence constraint contributes to statistical association.

(2) Size and range. Filters notwithstanding, respondents will sometimes hazard opinions of objects they cognize dimly if at all. We may assume the scale locations of such cognitively ungrounded responses to be largely random, and that their presence therefore depresses the level of observed consistency. They are not completely random because respondents may react to superficial or incidental cues—words here and there—engendering consistency among items sharing cues (Schuman and Presser, 1981). Where such cues are sufficiently salient and work sufficiently in the “same” direction to produce a great deal of this shallow consistency, they may even reverse the intrinsically negative relationship between noncognition and consistency. Normally, though, larger and wider-ranging PBSs, which generate fewer non- or minimally cognitive responses, should make for greater consistency.

So far, so valid. But consistency also reflects:

(3) Constraint-sharing. Even socially diffused constraint may be heterogeneous. The distribution of positions on issue  $B$  “logically” associated with position  $A_i$  on issue  $A$  may not be concentrated in a single narrow band. And not all constraint is socially diffused. Some is a homebrew of individual inventions and variations, which may or may not be shared by other individuals thinking independently on similar lines. Thus even if all objects were cognized and all cognitions constrained, different people would inevitably make different connections. How far this occurs probably varies with the items, the population, and the historical context, but to the extent that it occurs, consistency suffers.<sup>11</sup>

(4) Question form. To a major degree, the correlations depend on the form and content of the items being correlated. Take form. Is there a filter question? How porous? As a general proposition, more contentless opinions should vary more randomly, and more rigorous screening should therefore make for greater consistency, although this admittedly requires a heavy dose of *ceteris paribus*. Tangential cues may conceivably make low-cognizers’ responses more consistent than high-cognizers’ (Schuman and Presser, 1981), which suggests an interaction between filtering and phrasing. In addition, phrasing may have some main effects. Tangential cues may either raise or lower the level of consistency,

<sup>11</sup> A point most commonly raised as an excuse for scrawny correlations: the correlations are low because constraint is largely idiosyncratic (e.g., Lane, 1973; Bennett, 1975; Darcy and Aigner, 1980).

while unclarity should lower it. Finally, the number of response categories may matter. Too many or too few may damage reliability and hence consistency. Although the evidence permits little disentangling of effects, changes in filters, question wordings, and response categories appear to have been responsible for most of the 1960–64 increase in correlations among NES Election Study items (Bishop, Oldendick, and Tuchfarber, 1978; Bishop, Oldendick, Tuchfarber, and Bennett, 1978; Bishop, Tuchfarber, Oldendick, and Bennett, 1979; Sullivan, Piereson, and Marcus, 1978, 1979; Brunk, 1978; anticipated by RePass, 1976, and Bennett, 1977). Bishop, Oldendick, Tuchfarber, and Bennett (1979) and Bishop, Oldendick, and Tuchfarber (1980) provide further evidence of filter effects.

(5) Question content. Salient question referents are more widely and heavily cognized than obscure ones, concrete ones more widely and heavily cognized than abstract ones. Thus Pierce and Lovrich (1980) find higher correlations among specific attitudes than between those attitudes and more general orientations or among the latter (see also Converse, 1964; Pierce, 1975). As a corollary, group-related items, which are both more salient and more concrete than most, should be more consistent than most. In Nie, Verba, and Petrocik's (1976) compilation of correlations from eight national (NES and NORC) surveys from 1956 to 1973, the one pair of items plainly referring to the same social group—"black welfare" and "integration"—produced the highest correlation in every survey, averaging .19 higher than whichever pair came in second (my calculation, from p. 125).<sup>12</sup> Along similar lines, cognitions of similar referents are likelier to be interconnected than cognitions of disparate ones. This is what underlies and lends force to RePass's (1976) complaint that the items in Miller et al. (1976) do not really tap different attitudes, and to Bishop, Tuchfarber, Oldendick, and Bennett's (1979) objection to Nie and Rabjohn's (1979) examination of civil liberties items. The narrower the domain from which items are drawn, the worse—in particular, the more overstated—the measurement of sophistication.

(6) External association. Cognitions may co-occur without being collocated in thought. One kind of nonpsychological association is "ideology by proxy" (Campbell et al., 1960)—consistency from cue taking. People guided by the same reference group(s) or figure(s)—the president, the Democratic or Republican party, a labor union or trade association, a black or Jewish organization—are apt to form many of the same opinions.

<sup>12</sup>In six of the eight surveys, the next highest correlation belonged to "black welfare," and "welfare," the second of which may also carry racial connotations, even without explicitly mentioning blacks.

Few people toe any party or factional line absolutely, but since cue taking saves effort, most do engage in some follow-the-leadering of this sort. A still more basic mechanism is more directly experiential. Partially overlapping sets of people share the experiences of being black or white, of living in the inner city, or the suburbs, or the South, of holding a white-collar or a factory job, and so forth. With those experiences come reinforcement contingencies—incentives to do and believe certain things and not others. Thus people in similar circumstances come to hold similar constellations of political attitudes, without necessarily reasoning their way from attitude to attitude. To put it another way, much—I should say most—consistency is spurious, arising from the attitudes' dependence on common experiences or reference points, rather than from any psychological dependence on one another.

Hence correlations tell little about sophistication. They are only aggregate measures, computed on a biased subset of the sample, and, as we now see, deeply encrusted with extraneous covariation. It is not that high correlations are sufficient but unnecessary to infer high sophistication (Coveyou and Piereson, 1977; Wyckoff, 1980). If anything, correlations between attitudes tend to be "too high." Idiosyncratic constraint deflates them, but external association and effective sampling bias inflate them, and the first is unlikely to counterbalance the second and third. Yet neither is it that high correlations are necessary but insufficient to infer high sophistication (Herzon, 1980). They are far from necessary, far from sufficient.

*Correlations with "ideological" self-locations.* These remarks apply even to correlations with "ideological" self-locations (as in Miller et al., 1976; Klingemann, 1972; Sani, 1974; Stimson, 1975; Holm and Robinson, 1978). Large values mean only that respondents' self-locations are "consistent" with their responses to other items, not that they consciously think of those items' referents as "liberal" or "conservative" or that their conceptions of such terms are such as to facilitate or indicate much cognitive organization. "Liberal" may signify no more than "people like us," conservative no more than "the rich" (Converse, 1964, 1975b; Ladd, 1972; Butler and Stokes, 1969; Sani, 1974; Inglehart and Klingemann, 1979; Levitin and Miller, 1979). Yet a person for whom "liberal" and "conservative" carry only this limited cargo has merely to perceive, in addition, that the Republicans represent "the rich" or that low-cost housing benefits "people like us" to add his bit of cognitive-but-not-very covariance to the correlation between liberal-conservative self-location and party preference or attitude toward government provision of housing.

*Entropy measures.* The *entropy* of the items' joint distribution is an inverse measure of consistency, qua nonuniformity. The distribution has zero entropy when every respondent falls in the same combination of categories and maximum entropy when the respondents are spread evenly among all possible combinations of categories (see Theil, 1972). Thus Darcy and Aigner (1980), pursuing constraint, propose and employ several entropy-based statistics:  $A$ (distributional) gauges the patterning due to the "variances" of the marginals,  $A$ (internal) the patterning due to the items' dependence on one another, and  $A$ (external) the patterning due to their mutual dependence on other, specified variables.  $A$ (residual) is literally a residual, defined as  $1 - A$ (distributional) -  $A$ (internal) -  $A$ (external). (See also Darcy, 1980.)

It should be obvious from even these thumbnail definitions that only  $A$ (internal) and  $A$ (external) can plausibly be claimed to be measuring constraint. But even these statistics are technically unattractive. Neither has any proportional-reduction-of-entropy interpretation.  $A$ (internal) cannot equal 1.<sup>13</sup> More conventional entropy-based measures of association (see Hays, 1963; Coombs, Dawes, and Tversky, 1970) make more sense (Luskin, 1987b).

But the problem with using these statistics to measure constraint goes beyond technical eccentricity. Where the external variables are sociodemographic,  $A$ (external) is partly a function of constraint that varies by group. As Converse (1964) observes, the cognitions we mortise-and-tenon frequently have common origins, in the experiences associated with given social locations. People who live in an inner-city ghetto and believe  $X$  and  $Y$  as a result of that experience will, to the extent that they eventually put  $X$  and  $Y$  together, find reason to believe that one subsumes or implies the other. Some constraint, i.e., is rationalized external association. But response patterns may also be similar within and different between social groups for reasons outside the individual psyche, and we simply cannot sift rationalized from purely external association. I personally believe that the latter predominates, but any patterning by group is in the end an inscrutable mix.

$A$ (internal) is at least as opaque. Despite the separation of  $A$ (external), it is defined solely in terms of the items' joint and marginal distributions, not their joint conditional distributions within groups, and is thus not really partial. The numerators of  $A$ (internal) and  $A$ (external), which express entropy reduction, can sum to more than the total entropy. In

<sup>13</sup>For perfectly associated variables (only one nonzero cell in each row and each column, in the bivariate case),  $A$ (internal) = .5 (see n. 14).



spirit, in short,  $A(\text{internal})$  is no different from a matrix of zero-order correlations, and subject to the same biases and confoundings.<sup>14</sup>

Of course, one might devise a partial entropy statistic, or use some other partial statistic. A matrix of partial correlations, for instance, would gauge the association between items within groups. But even within groups the association between items need not be psychological. For one reason, we shall never be able to identify, measure, and introduce all the external variables that could account for apparently internal association. But suppose we somehow managed the feat. Even then, we should have reservations about even a partial version of  $A(\text{internal})$  as a measure of constraint. It would still be aggregate, still be biased up by missing data. And we should still be in doubt as to the nature of both the association removed, some of which might be due to constraint, and the (presumably minor) association remaining, much of which might still be due to such extrapsychic factors as questions sharing incidental cues.

*“Corrected” correlations.* Another approach is to scour the items of measurement error, in the hope of uncovering the correlations between the “true” attitudes beneath. Achen (1975), Stipak (1977), and Erikson (1979) apply simple corrections for attenuation. Herzon (1980) conducts an exploratory factor analysis, the solution obliquely rotated, and cites the correlations between factors. More inventively, Conover and Feldman (1984) perform an obliquely rotated  $Q$  factor analysis and take the correlations between factors (which they believe reflect schemata) as a measure of organization. Norpoth and Lodge (1985) estimate a confirmatory factor model, including the correlations between factors.

<sup>14</sup>To see more concretely just how short a distance this sort of analysis can take us, we may imagine a population divided into two equally large groups, each unanimously choosing the opposite combination of positions on two binary issue items:

		Group 1		Group 2		Whole Population	
		Item A		Item A		Item A	
		$A_1$	$A_2$	$A_1$	$A_2$	$A_1$	$A_2$
Item B	$B_1$	50	0	0	0	50	0
	$B_2$	0	0	0	50	0	50

For this configuration, caricaturing but capturing the reality of consistency due largely to shared dependence on social location,  $A(\text{internal}) = A(\text{external}) = .5$ . But what does that say about constraint? The pattern of perfect agreement within and perfect disagreement between groups may reflect similarities and differences in true constraint, or it may simply reflect similarities and differences in references and experiences. We cannot tell.

Whether these corrections paint a truer picture, however, depends on the primary locus of error: in the questions or in the respondents? This remains a subject of debate (Converse, 1970, 1974, 1980; Achen, 1975; Pierce and Rose, 1974; Converse and Markus, 1979; Achen, 1983; Norpoth and Lodge, 1985), but as Kinder (1983) observes, the errors-in-the-questions position must confront some embarrassing side evidence.<sup>15</sup> Item: political elites show much greater attitudinal stability than mass publics (Putnam, Leonardi, and Nanetti, 1979; Converse and Pierce, 1986, pp. 247–51). Are the questions put to political elites so much more precisely worded than those put to mass publics? It is much easier to believe that political elites simply have more crystallized opinions about more political issues. Item: party identification is far more stable than policy preferences (Converse and Markus, 1979). Are questions about party always so much better put than questions about policy? It is much easier to believe that the major parties are simply the objects of more fully developed cognition than are policy issues. Item: the distribution of policy preferences varies—sometimes considerably—with question wording (Schuman and Presser, 1981) and order (Bishop, Oldendick, and Tuchfarber, 1982). Are informed respondents tracking every nuance of phrasing with the zeal of Talmudic scholars? It is much easier to believe that uninformed respondents are being swayed by tangential cues. Other bits of evidence support the errors-in-the-respondents position more directly. Item: the stability of policy attitudes varies with interest in the policy area (Converse, 1964; Schuman and Presser, 1981)—exactly what we should expect if stability is a function of cognition, and cognition a function of interest. Item: significant percentages of respondents obligingly give opinions of fictional politicians (“Thomas Walker”) and groups (“Wallonians,” “Danireans,” “Pireneans”) and of real legislative proposals (the Monetary Control Bill, the Agricultural Trade Act) so obscure they might as well be fictional (Hartley, 1946; Kolson and Green, 1970; Schuman and Presser, 1981). This willingness to respond to unrecognized objects is the essence of respondent error. How much more willing must people be to opine about objects of which they have some genuine but hazy cognition?

While more or less conceding that the “error in the items” is mostly in the respondents, Inglehart (1985) has recently argued that the respondents making the errors have genuine cognitions all the same. The errors arise, he says, when respondents are asked, on short notice, to answer questions they have never thought much about. But surely this is close to the definition of limited cognition. Whether a respondent has thought

<sup>15</sup>Except for the final item, the rest of this paragraph follows Kinder (1983).

much about a question is the very distinction we are trying to draw. The ineluctable conclusion, *pace* Inglehart, is that respondent error is largely the product of non- or minimal cognition. To remove this error is to suppress evidence of constricted size and range and thus to *worsen* the measurement of sophistication (Converse, 1980; Wyckoff, 1980).<sup>16</sup>

*Factor loadings on common factors.* In much the same vein Judd and Milburn (1980) and Jackson (1983) adopt confirmatory factor models in which both common and issue-specific factors underlie the issue items (also Judd, Krosnick, and Milburn, 1981). In both models the same items appear at three time-points, and the common factors are time-indexed. Both are estimated by LISREL—Jackson's on the NES 1956–58–60 panel, Judd and Milburn's on the NES 1972–74–76 panel. Though details differ, the simplified, two-item model diagrammed in Figure 1 illustrates both. The major substantive differences are that (a) only Jackson includes sociodemographic variables; (b) Jackson actually posits two common factors, ideological and partisan, differentiated by the sociodemographic variables he lets affect them; and (c) only Judd and Milburn include structural equations expressing the common factor at each time-point as a function of its past values.

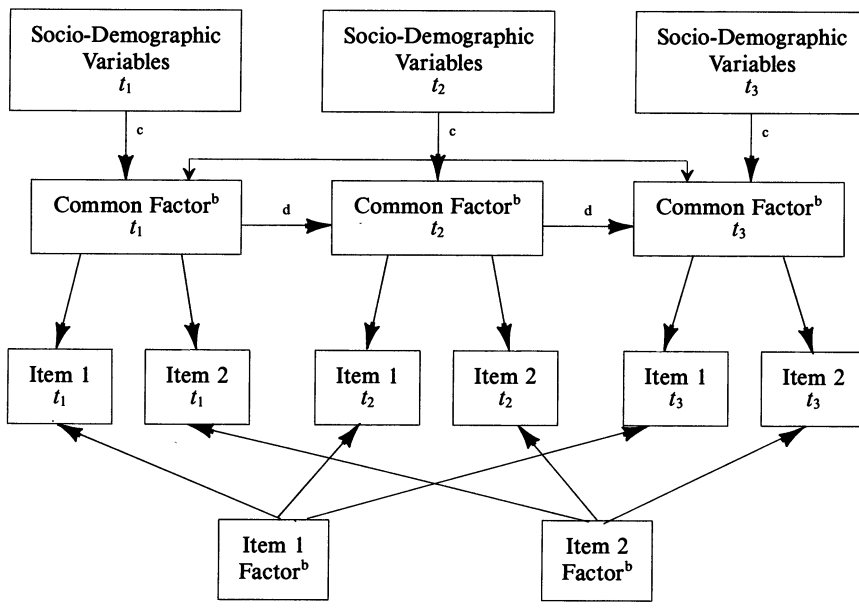
Both Judd and Milburn and Jackson interpret the loadings on their common factors (in Jackson's case, on his common ideological factor) as measures of cognitive structure. But are they, necessarily? They may instead be "spuriousness factors": mere distillations of the common experiences associated with sociodemographic variables like Jackson's. That Jackson's common factors explain considerably more variance than his issue-specific factors may suggest only that consistency is mostly a function of external association. Of course, the other shortcomings of consistency-based measures—most notably, aggregateness and effective sampling bias—cloud the measurement further.<sup>17</sup> The most revealing results, as Converse (1980) points out, are the estimated error variances, which are large, and mostly due, as I have argued above, to sparse cognition.

<sup>16</sup> Another, if a minor point, given these more fatal objections, is that even if the error resided mostly in the questions the merit of the error extraction would depend on how well its assumptions were met. The strictly random errors of corrections for attenuation and exploratory factor analyses—errors that have zero means and are uncorrelated with the attitudes they indicate, with all other attitudes, and with all other errors—seem particularly unlikely. If the errors truly lie in the questions, they are likely to push respondents systematically in the "same" direction on some question-pairs, in the opposite direction on others.

<sup>17</sup> Judd and Milburn (1980) lose roughly two-thirds of their sample to missing data (Martin, 1981). When they perform separate analyses of high- and low-involvement groups

FIGURE 1

A Simplified Confirmatory Factor Model with Both Common and Issue-Specific Factors, as in Judd and Milburn (1980) and Jackson (1983)<sup>a</sup>



NOTES: <sup>a</sup>Disturbances and error terms omitted for clarity.

<sup>b</sup>Latent variable.

<sup>c</sup>Present only in the Jackson model.

<sup>d</sup>Present only in the Judd and Milburn model.

*Vertical relations among factors.* Peffley and Hurwitz (1985) have recently tried a more structural sort of covariance structure model, specifying vertical relations among attitudes at three levels of abstractness: attitudes on narrowly defined issues depend on more general postures, which in turn depend on liberal-conservative self-placement. Figure 2 gives the idea. The model is unrealistic, of course, in omitting horizontal and upward (abstractive) relations, but such specification issues are secondary. The magnitudes of the structural parameter estimates and

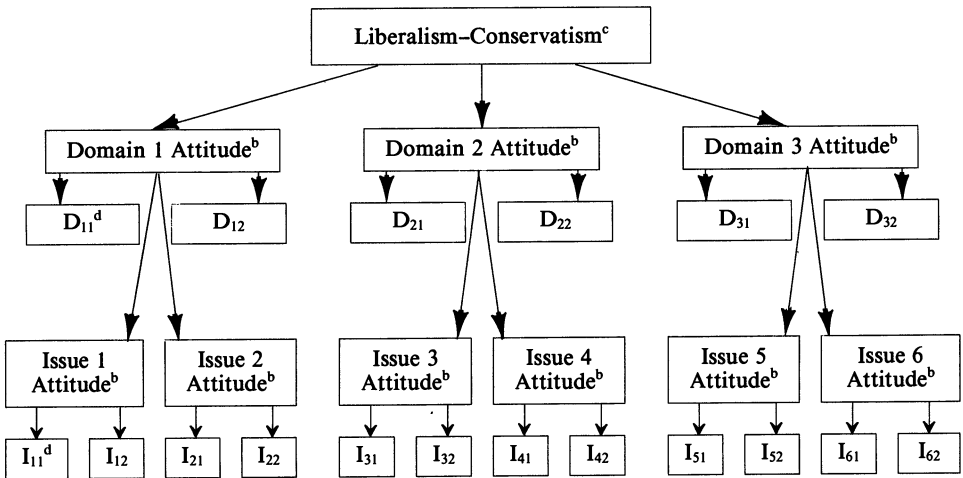
(Judd, Krosnick, and Milburn, 1981), the loss is twice as great for the latter as for the former. On other, more technical problems with the Judd and Milburn analyses, see Martin (1981).

the fit to the data, which according to Peffley and Hurwitz show a high level of constraint, still show only a high level of consistency, if that. Missing data have their usual inflationary effect, and the extrapsychic association in Jackson's and Judd and Milburn's common factors is now merely spread among the structural relations between attitudes. Again, the most revealing results are the estimated error variances, which again are large.

*Other factor analytic measures.* The same biases and confoundings undercut those simpler exploratory analyses that gauge sophistication by an orthogonally rotated solution's dimensionality, interpretability, and ability to account for the items (Bedggood, 1972; Luttbeg, 1968, 1971; Kritzer, 1978; Inglehart and Klingemann, 1976; Bennett et al., 1979; Wittkopf and Maggionto, 1983). The results may tell a good deal about aggregate lines of cleavage, but can tell very little about the individual-level cognition that is partly but only partly responsible. Consistency in, consistency out.

*Group consistency.* It does not help to stratify the sample by some individual-level measure of sophistication or some related variable

FIGURE 2



NOTES: <sup>a</sup>Disturbances and error terms omitted for clarity.

<sup>b</sup>Latent variable.

<sup>c</sup>Measured by a single indicator, assumed error free.

<sup>d</sup> $D_{ij}$  denotes the  $j$ th indicator of the  $i$ th domain attitude,  $I_{ij}$  the  $j$ th indicator of the  $i$ th issue attitude.

and compute correlations or conduct factor analyses within strata (as in Converse, 1975b; Stimson, 1975; Inglehart and Klingemann, 1976; Carmines and Stimson, 1982; Norpoth and Lodge, 1985). If we take consistency's dependence on sophistication as given, the results may confirm the individual-level measure's validity or the related variable's relatedness. If instead we take the appropriateness of the variable on which we are stratifying as given, they may confirm consistency's dependence on sophistication, *ceteris paribus*. But this is only a mild causal relationship between conceptually distinct variables. Members of highly consistent strata need not be ideological; members of highly inconsistent strata need not be ignorant.

*Individual consistency.* This is not to imply that the remedy is simply to downshift to the individual level. A variety of individual-level measures have seen service. The highest-tech involve multidimensional scaling routines that generate a separate solution for each observation (Yellig and Wearing, 1974; Marcus, Tabb, and Sullivan, 1974; Jackson and Marcus, 1975; Jacoby, 1986), with sophistication mostly judged from the solutions' dimensionality, interpretability, and explanatory success. Simpler measures include the number or proportion of issues a person is on the "same" side of (Miller and Levitin, 1976; RePass, 1976; LeBlanc and Merrin, 1977; Nie and Rabjohn, 1979), the factor-loading-weighted sum of his or her standardized scores on issue items (Nie, Verba, and Petrocik, 1976; Petrocik, 1980), and the standard deviation of his or her raw or standardized scores on issue items (Barton and Parsons, 1977; Wyckoff, 1980).

At least these measures address the sophistication of individuals. They remain fatally flawed, however, by the weakness of the sophistication-consistency nexus. Who is finally to say what pairs or *n*-tuples of positions are "consistent?" Or that a given "inconsistency" is not a swatch of some idiosyncratically but tightly woven fabric of belief, or a given "consistency" not the product of environmentally associated but internally disjoint cognitions? At the individual as at the aggregate level, we cannot know what cognitive linkages underlie a particular response configuration.

### *Abstraction*

A second and generally preferable approach capitalizes on the relationship between constraint and abstraction. Constraint, in large degree, *is* abstraction. Hence by gauging a person's use of abstractions—either how abstract they are or how heavily used—we can gauge his or her

constraint. Size and range, through correlation with constraint, follow partly in tow.

*Self-locations on "ideological" dimensions.* The hard part, of course, is gauging the use of abstractions. The simplest measures distinguish respondents who locate themselves on an "ideological" dimension (left-right or liberal-conservative) from those who do not (Hikel and Segal, 1973; Holm and Robinson, 1978; Bennett et al., 1979). Binariness aside, these measures do not, however, discriminate terribly well. Self-location requires no knowledge at all, only a willingness to point to a spot on the scale. Though commonly described as "ideologues," most self-locators are anything but. Large proportions typically place other political objects incorrectly, positioning the Democrats to the right of the Republicans or the Republicans to the left of the midpoint, for example (Converse, 1964; Butler and Stokes, 1969; Erikson and Luttbeg, 1973, pp. 67-68). As measures of ignorance, moreover, these proportions should probably be doubled, since for every respondent who guesses wrong, there should be another guessing right (Converse, 1964; Butler and Stokes, 1969). But we need not rely on such internal embarrassments to show cognitively impoverished self-locations. The meanings most self-locators supply when asked to define terms like "conservative" are narrow at best (Converse, 1964, 1975b; Butler and Stokes, 1969; Sani, 1974; Inglehart and Klingemann, 1976; Klingemann, 1979c; Levitin and Miller, 1979; Conover and Feldman, 1981).

*Active use.* A second tack under this heading is to consider the abstractness of the concepts a person uses in evaluating political objects. In Converse's (1964) terminology, these are measures of *active use* (AU), the first and prototypical of which was Campbell et al.'s (1960) "levels of conceptualization" (LC). The LC was based on the open-ended questions asking what respondents liked and disliked about each of the major parties and presidential candidates. The first and highest category ("Level A" or "Ideology") housed responses that referred, at least implicitly, to some highly abstract concept(s), such as but not restricted to liberalism or conservatism;<sup>18</sup> the second ("Level B" or "Group Benefits") responses that fell perceptibly short of this synoptic standard but

<sup>18</sup> A common misconception (e.g., Coveyou and Piereson, 1977; Neuman, 1981) is that the LC and other AU measures discriminate against unorthodox abstractions, counting only references to "liberal" "conservative," or related terms as "ideological." This first of all is untrue (Campbell et al., 1960, pp. 227-28) and, second, would not matter much were it true, since unorthodox abstractions are rare (Campbell et al., 1960; Converse, 1964; Ladd, 1972; Klingemann, 1979c).

alluded to the interests of political or politically relevant groups; the third ("Level C" or "The Nature of the Times") references to isolated issues, including, in the vaguest instances, the overall goodness or badness of ill-defined "conditions" or "things" in general; and the fourth and lowest ("Level D" or "No Issue Content") the remaining, issueless, often contentless responses.

Other AU measures (e.g., Bowles and Richardson, 1967; Neuman, 1981) have been built to resemble the LC, although many have cut an important corner in working from the NES's coding of the responses ("master codes") instead of the raw transcripts (Field and Anderson, 1969; Nie, Verba, and Petrocik, 1976; Coveyou and Piereson, 1977; and, apparently, Jennings and Niemi, 1974). Though convenient, the coded responses fractionalize meaning within and homogenize meaning across responses (Smith, 1980). To draw a homey analogy, they are to the transcripts as freeze-dried coffee is to coffee. Do with them what we will, we can never quite recapture the flavor of the original. I do not mean to say that they are therefore useless—I sometimes drink instant coffee myself. But we should be aware of the cost in content and context.

AU measures may to some degree discriminate, as many authors have charged, against the sophisticated but inarticulate (e.g., Lane, 1973; Marcus, Tabb, and Sullivan, 1974; Holm and Robinson, 1978). But since political learning is partly a function of verbal ability, sophistication and inarticulateness should rarely keep company. Nor is inarticulateness much of a handicap. Campbell et al. (1960) did everything but double-backflips to avoid underrating respondents who might merely have been failing to make themselves clear. Quite deliberately, they fashioned a measure so generous that the then-novel finding of a woefully unsophisticated electorate could not possibly be dismissed as artifact. More recent measures, according a proportionally equal benefit of the doubt to the coded responses, whose cognitive backing is generally more doubtful, are more generous still. Any reference, however minimal, to any abstraction of reasonably high order ("He's too liberal for me," "They'd like to spread the wealth") earns a place in the highest category; any reference, however minimal, to any group ("They're good for Armenians") a place in the group reference category, usually the second.

By far the greater danger, in fact, lies in the opposite direction. Respondents are much likelier to be over- than under-classified, especially at the top of the scale and by measures employing the precoded responses. Over three-fourths of the occupants of Campbell et al.'s Level A were only "near-ideologues": questionable cases who in a less indulgent sorting would probably have been assigned to Level B or C instead (1960, pp. 230–34). Even the LC thus overstates the proportion of true



ideologues. Master-code-based replicas claim far higher proportions still.<sup>19</sup> If we take the Nie, Verba, and Petrocik (1976, 1981) figures seriously, for example, ideologues and near-ideologues accounted for 48 percent of the American electorate in 1968!

The majority of these respondents earn "ideological" status by using terms like "liberal" or "conservative." But as both Nie, Verba, and Petrocik (1976) and Field and Anderson (1969) come close to admitting, the cognitive content of such rhetoric is frequently slim (Smith, 1980). Field and Anderson (p. 397) would be "surprised if many Ideologues in fact possessed will-integrated belief systems along the lines defined by Campbell et al." Nie, Verba, and Petrocik concede that their "ideologues" are not "citizen-philosophers" with "elaborate and well-considered" belief systems (p. 116). But, as Smith (1981) asks, if "ideologies" do not necessarily involve "elaborate and well-considered belief systems," what do they involve? Merely, as Nie, Verba, and Petrocik teeter on the edge of saying, the use of catchwords and slogans irrespective of cognitive content? That is hardly an ideology in the Conversian sense these authors say they intend. Nor a thing we should care much about. The words are epiphenomenal; it is the cognition behind them that matters. And for most "ideologies" that cognition falls well short of ideology.

Contrary to Smith (1980), however, the AU approach retains some validity. Even the Nie, Verba, and Petrocik measure, on which Smith trains his analysis, works better than Smith allows. The test-retest reliability is low, and changes are essentially uncorrelated with changes in political interest, media consumption, and activity, as Smith shows, but Smith's own estimates of a stability/reliability model show higher reliability and almost perfect stability. Thus *changes* in this measure are almost pure measurement error and not apt to be correlated with anything. Correlations with the measure itself provide a better test, and these are substantially higher (Cassel, 1984). The LC and measures that resemble it more closely boast reliabilities and correlations with sophistication-related variables higher still (Campbell et al., 1960; Neuman, 1981; Pierce and Hagner, 1982; Hagner and Pierce, 1982; Knight, 1985).<sup>20</sup>

<sup>19</sup> Compare Field and Anderson (1969) with Campbell et al. (1960) for 1956 and Nie, Verba, and Petrocik (1976) with Klingemann (1972) and Miller and Miller (1976) for 1968 and 1972.

<sup>20</sup> Its use of the master codes aside, the Nie, Verba, and Petrocik measure makes several wayward distinctions. For example, it treats responses that describe the parties or candidates as "liberal" or "conservative" without adverting to group or issue orientations as more sophisticated than responses that neglect the labels but supply some of the group

Yet even these superior AU measures make some questionable distinctions. I see little difference, in the LC, between Level B and the specific issue portion of Level C or between the rest of Level C and Level D. Empirically, too, there seems little to differentiate Levels B and C (Hagner and Pierce, 1982; Knight, 1985; Jacoby, 1986). Hence, my own AU measure (1987a) has only three categories: the first for responses that show at least a glimmer of some high-order abstraction; the second for contentful but less abstract references to groups or issues; and the third for the remainder: responses so minimal, tangential, or bereft of meaning as to suggest a PBS of mostly wide-open spaces. Despite resorting to the coded responses (instant coffee has its advantages), this trichotomy correlates quite respectably with a set of criterion variables.

Since I shall want, as we proceed, to describe and compare several other measures I have tried, I should be more specific about this empirical test. The data are from the 1976 NES Election Study. The five criterion variables are: (i) Whether respondent locates him- or herself on a liberal-conservative scale, scored (0, 1). (ii) The number of pairs of political actors he or she orders correctly on the same sort of scale—the Democrats to the left of the Republicans and Hubert Humphrey, George McGovern, Walter Mondale, and Jimmy Carter to the left of Gerald Ford, George Wallace, Ronald Reagan, and Robert Dole. Since this makes a total of  $(4) + 1 = 17$  comparisons, the variable runs from 0 to 17. (iii) The number of times he or she correctly locates “liberals” and “conservatives” on the “government guarantee of jobs and living standard” and “reducing crime versus protecting the rights of the accused” scales, where the correct answers put “liberals” on the interventionist side and “conservatives” on the laissez-faire side of the first and “conservatives” closer to the “reducing crime” pole of the second. Here the variable counts one relative and two absolute placements, and so runs from 0 to 3. (iv) Whether the respondent knows the party holding the majority of seats in the House of Representatives both before and after the election, scored (0, 1). And (v) the interviewer’s postinterview rating of the respondent’s level of political information, on a scale from 1 to 5. These variables should all reflect sophistication, if most of them less well than an AU measure. My trichotomous AU measure’s mean correlation with them is .40 (Pearson’s  $r$ ).

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and issue orientations they subsume. It also treats issue references—directly contrary to Campbell et al.—as more abstract than group references. At a still more purely operational level, moreover, it makes some strange assignments of codes to categories. The ideology category excludes references to “communism,” “left,” “right,” and other aspects of “government activity/philosophy,” but includes references to “government spending,” to government involvement in “welfare/poverty problems,” and to “big business,” “the wealthy,” or “the powerful.”

*Recognition and understanding.* A final class of abstractness-based measures grades respondents on the meaning they are able to supply for an "ideological" term. Following Converse (1964), we shall call these measures of *recognition and understanding* (RU). Converse's original RU measure (1964) rests on a series of questions asking: first, whether the respondent considers either party more conservative; second, if the answer is no, whether people in general consider either party more conservative; third if the answer to either of these first two questions is yes, to identify the more conservative party; and, finally, if the respondent names a party, to say why that party is or is perceived as more conservative. In ascending order, the categories contain those who (1) neither see the parties as differentially conservative nor realize that other people do; (2) see the parties as differentially conservative, or realize that other people do, but cannot say what the difference entails; (3) see a difference or see that others see one but describe it incorrectly, misidentifying the Democrats as more conservative or correctly tagging the Republicans as more conservative but for some upside-down reason, such as that they want to spend more or favor bigger government; (4) correctly describe the Republicans as more conservative for a correct but narrow reason centering on one or more specific issues or groups; or (5) correctly describe the Republicans as more conservative and for a reason abstract enough to smack of ideology. Other RU measures (Butler and Stokes, 1969; Ladd, 1972; Sani, 1974; Converse, 1975b; Klingemann, 1969c) are similar.

Converse's sorting partly by accuracy may seem to contravene the dictum that sophistication is a matter of how much and how a person thinks about politics, not what. Empirically, however, there should be a strong relationship between the "how much" and "how" and some factual aspects of the "what." A politically sophisticated person may or may not believe that free enterprise is a good thing, that government spending is the root of inflation, or that effective control of handguns will lower the murder rate. These points are debatable. But he or she is unlikely to believe that Ronald Reagan is now a Democrat or that the Democrats currently hold a majority of seats in the U.S. Senate. Or that the Democratic party is more conservative than the Republican. An astronomer might as usefully believe that the earth revolves around the moon. An astronomer who did would continually find new astronomical information jarringly unexpected. Only those who care and know little about astronomy—only, on our side of the analogy, those who care and know little about politics—can maintain so obtrusively false a premise. Thus even Levitin and Miller (1979), who begin by arguing that even highly sophisticated respondents may misorder the parties

on a liberal-conservative or left-right scale, end by acknowledging that such “heterodox orderings” most often imply “limited ideological understanding” (pp. 764–65).

I am skeptical, however, of some of the distinctions RU measures typically make. It is hard to see, for instance, how the lowest three categories of Converse’s measure distinguish different degrees of meaning. They differ much more in the respondent’s willingness to admit ignorance than in any sign of sophistication. Again, it seems to me, three levels may be the most we can validly distinguish from these data. My own RU measure (1987a) therefore distinguishes (1) correct and abstract responses, (2) correct and meaningful but less abstract responses, and (3) incorrect or empty responses, together with nonresponses.

All these measures, including mine, and Converse’s, make use of the precoded data, which is less perilous for RU than for AU measures. To qualify for the top category of an RU measure, respondents must show that they understand some “ideological” term correctly and abstractly. The problem of usage without understanding does not arise. RU measures are still generous—they too assign borderline cases upward—but less so. This is of course a plus, although the corresponding minus is that understanding does not imply centrality. Respondents who supply a seemingly abstract meaning for “liberal” or “conservative” may be effortfully retrieving the information from some dusty, rarely accessed bin of long-term memory. They may not be using the concepts to organize their PBSs, or have particularly large or well-organized PBSs. In this respect RU measures are more generous than AU measures, and for this reason may show a higher proportion of “ideologues” when “ideological” terms are not much in the air, as in Converse’s (1964) results from 1960. On balance, however, RU measures seem adequately face-valid. Against the same five criterion variables, my RU measure defeats my AU measure by a whisker, with a mean correlation of .41.

### *Information Holding*

All the measures we have considered to this point attack sophistication from the integration side. Both conceptually and operationally, the sophistication literature has given differentiation much shorter shrift. Simple measures of differentiation—counts of factual items correctly answered—dot the broader landscape (e.g., Clarke and Fredin, 1978), but not the literature consciously concerned with sophistication qua cognitive complexity. These outside measures go by other names—knowledge, information holding—and we tend not even to think of them as measuring sophistication, quite. But of course they do.

Within the sophistication literature, Neuman (1981) measures differentiation as "knowledge-in-use": the number of distinct, concrete political objects a respondent mentions during an hour-long "depth interview" composed of open-ended questions and persistent, flexible probes. I (1987a) have counted "relatively informed opinions" on 11 issues, concerning (1) federal guarantees of jobs and a "good standard of living"; (2) the balance to be struck between protecting the rights of the accused and preventing crime; (3) school busing to achieve integration; (4) federal efforts to "improve the position of blacks and other minority groups"; (5) government-provided medical and hospital insurance; (6) the best response to urban unrest (forcible suppression v.s. amelioration of causes); (7) the penalties for marijuana smoking; (8) how graduated the federal income tax should be; (9) an equal role for women "in running business, industry, and government"; (10) government power; and (11) military spending. A respondent holds a "relatively informed opinion" on a given issue if he or she, first, places him- or herself on the dimension; second, places both major parties; and, third, orders the parties correctly. On issues (7) and (9), the parties' 1976 positions were too vague and overlapping to make any ordering clearly wrong, but, for the rest, I regard responses that put the Republicans to the left of the Democrats on issues (2) or (3) or at the same scale point or to the left of the Democrats on issues (1), (4), (5), (6), (8), (10), or (11) as betraying a paucity of information.

Though aimed only at a narrower concept, Hamill, Lodge, and Blake's (1985) recent measure of "partisan schema usage" is a close relation. It differs mainly in requiring absolutely correct placements (the Democrats to the left of the midpoint, the Republicans to the right) and in not requiring self-placement. An only slightly more distant relation is these same authors' measure of "ideological schema usage," which counts correct placements of "liberals" and "conservatives."<sup>21</sup> These measures may differentially reflect ideological and partisan schema development, as Hamill, Lodge, and Blake believe, but they also both reflect sophistication.

Indeed, measures like these should gauge size and range straightforwardly and well. Less directly, they should also reflect constraint. True, Neuman's measure is confounded with loquacity. True, mine is not as discriminating as it might be (it is not much to know that the Republicans are starboard of the Democrats). But these seem relatively minor failings, and the evidence largely agrees. Neuman's measure is only modestly correlated with education and other demographic variables, but

<sup>21</sup> The same measures, differently labeled, figure in Sharp and Lodge (1985) as well.

conventional wisdom notwithstanding, education may not strongly affect sophistication (Luskin, 1987a), and the measure does bear an impressive relationship to political participation (Neuman, 1981). Both of the Hamill, Lodge, and Blake measures are strongly related to the respondent's ability to place Reagan correctly on the issues (the partisan measure, which more closely resembles mine, the more so). And my own measure does extremely well—much better than my AU and RU measures of abstractness. Its correlations with the same five criterion variables average .56.

### *Composite Measures*

A few measures are hybrids. Klingemann (1979c) folds an AU and an RU measure into a six-category index of "ideological conceptualization" (also employed by Klingemann, 1979a, 1979b; Inglehart, 1979; Jennings and Farah, 1980). The first three categories all require some "ideological" response to both tributary measures, differing only as a function of whether the response refers to "left" or "right" terms alone or to both—a dubious distinction in the AU case, where it is largely a matter of whether the respondent happens at some point to reflect his terminology. The fourth category requires some "ideological" response to at least one of the tributary measures. Only the bottom two categories are strictly "nonideological." Later, Klingemann amalgamates these last two categories into one. The unfortunate result is a variable that makes exceedingly fine and probably unreliable distinctions toward the high end of the spectrum and none at all below: four levels of "ideology" versus one of everything less.

As a general strategy, however, the combination of AU and RU measures is attractive. The usual rationale for multiitem measures applies: different items, with different frailties, may balance each other's mistakes. AU measures may accept usage without meaning; RU measures may accept meaning without usage. Thus I too have essayed a combination of AU and RU measures (1987a):  $I$  (for integration) =  $A + R$ , where  $A$  and  $R$  denote my AU and RU measures, both scored (0, 1, 2). In categorical terms, this results in a five-category measure whose categories contain (1) those in the "ideological" category of both AU and RU, (2) those in the "ideological" category of either AU or RU and the middle category of the other, (3) those in the middle category of both or in the "ideological" category of one and the bottom category of the other, (4) those in the middle category of one and the bottom category of the other, and (5) those in the bottom category of both. As expected,  $I$  outperforms both  $A$  and  $R$ , its correlations with the criterion variables averaging .48.

A still greater gain may result from combining integration-side and

differentiation-side measures. Anything that measures integration measures differentiation and vice versa, to be sure, but the weak-side measurement is very indirect. And sophistication, by definition, requires both. On this reasoning my final measure (1987a) combines my AU-RU combination  $I$  with my measure of size and range (call it  $D$ ), in the form  $S = (I + 1) (D + 1)$ . The ingredients are multiplied rather than added because sophistication is conjunctive, a matter of integration *and*, not *or*, differentiation; the additions of 1 allow sophistication to vary as a function of one component when the other is at its lowest. Unsurprisingly,  $S$  shows much higher correlations with the criterion variables than  $A$ ,  $R$ , or  $A + R$ . Surprisingly, it fares no better than  $D$ . But in absolute terms,  $S$  looks good. Its mean correlation with the criterion variables is .56. This is all the more impressive considering the relative weakness of some of the criterion variables.  $S$ 's correlation with the strongest, criterion (ii), is .73. Table 1 displays the correlations for all measures and criteria.

### Distributional Implications

As long ago as 1954, Berelson, Lazarsfeld, and McPhee suggested that most Americans were appallingly short of political information. A

TABLE 1  
Correlations between Measures and Criteria\*

	$A$	$R$	$I (= A + R)$	$D$	$S (= [I + 1] [D + 1])$
(i) Self-location on liberal-conservative scale	.35	.32	.40	.48	.44
(ii) Correct placements of political actors on same	.51	.55	.63	.69	.73
(iii) Correct placements of liberals and conservatives on issue scales	.35	.38	.43	.60	.55
(iv) Correct identification of majority party in the House of Representatives, both before and after the election	.36	.36	.42	.46	.47
(v) Interviewer's rating of respondent's level of political information	.45	.44	.53	.58	.60
Mean correlation	.40	.41	.48	.56	.56

NOTE: \*Entries are Pearson's  $r$ 's.

few years later Campbell et al. (1960), defining the somewhat larger variable we are calling political sophistication, found most Americans quite unsophisticated. Then, in the most cited work in this literature, Converse (1964) elaborated on Campbell et al.'s description of the variable, added further evidence of low sophistication in this country, and argued that the same result should obtain more generally. Inevitably, said Converse, competing demands on people's time and attention make the distribution of sophistication bottom heavy—to varying degrees perhaps, but always and everywhere. Other authors, in other traditions, have argued similarly. Converse was in fact anticipated by Downs (1957), who argued *a priori* that people rarely see incentive enough to pay the real and opportunity costs of acquiring much political information. Earlier still James Bryce (1904, pp. 250–51) and Walter Lippmann (1922) remarked much the same thing. In psychology the distinguished consistency, now schema, theorist Robert Abelson (1968) came impressionistically to the conclusion that highly ramified cognitive structures are generally confined to meaning-domains with which people are deeply involved.

All this seems the sheerest of common sense. It also fits with what most of us must hear when we try talking politics with non-political-scientists. Yet the thesis of universally low mean sophistication has proved remarkably controversial. Some researchers claim that the American public is simply much more sophisticated (and presumably has been all along) than Campbell et al. (1960) and Converse (1964) would have us believe (Bedggood, 1972; Achen, 1975; Luttbeg, 1968; Stipak, 1977; Marcus, Tabb, and Sullivan, 1974; Jackson and Marcus, 1975; Judd and Milburn, 1980; Judd, Krosnick, and Milburn, 1981; Yellig and Wearing, 1974; Jackson, 1983; Peffley and Hurwitz, 1985). Others concede a relatively unsophisticated public in the late 1950s and early 1960s but claim that it has become much more sophisticated since (Pierce, 1970; Pomper, 1972; Bennett, 1973; Nie and Anderson, 1974; Miller and Levitin, 1976; Miller et al., 1976; Stimson, 1975). Still others claim a more upscale distribution for other publics, abroad (Klingemann, 1972; Bedggood, 1972).

Theoretical plausibility and fit with casual observation aside, the question of which results to accept is a question of which measures to believe. What, then, does the preceding review imply? The correspondence of measures and results is clear-cut. Every conclusion of widespread or greatly increased sophistication—every such conclusion without exception—rests on either a consistency-based measure of some description (correlations, common factors, or the like) or an AU measure of abstractness jerry-built on precoded data. This is hardly convincing. Measures of consistency show little beyond consistency. At first, much was made of the fact that the correlations among a number of



longtime NES items had increased dramatically. Now it appears that most or all of the increase was the artifactual result of changes in question wording and format (Brunk, 1978; Bishop, Oldendick, and Tuchfarber, 1978; Bishop, Oldendick, Tuchfarber, and Bennett, 1978; Bishop, Tuchfarber, Oldendick, and Bennett, 1979; Bishop, Tuchfarber, and Oldendick, 1978; Sullivan, Piereson, and Marcus, 1978; Sullivan et al., 1979; though cf. Petrocik, 1978, 1980; Nie and Rabjohn, 1979; and, surprisingly, Converse and Markus, 1979). But as far as sophistication is concerned, the reality of the increase is irrelevant. High or low, increasing or decreasing, measures of consistency say little about sophistication. As for the master-code-based AU measures, they are simply far too generous, and in a way that makes them sensitive to variables besides sophistication.

The most credible measures, in contrast, show little sophistication and little change. Reapplying Converse's RU measure in 1964, after the great step-change of the early 1960s had supposedly occurred, Converse, Clausen, and Miller (1965) and Pierce (1970) report percentages of "ideologues" scarcely different from what Converse (1964) found in 1960. True, Jennings and Niemi (1981), applying the same measure to their 1965 sample of high-school seniors and reapplying it to the same respondents in 1973, find a more sophisticated-looking distribution at both points and a substantial improvement from 1965 to 1973. But this is a sample of high-school almost-graduates, biased toward a higher than cross-sectional level of sophistication. The change, moreover, is probably life-cyclical, not representative of change in the public as a whole. Between the ages of 18 and 26, when people who have been students join the real world, the political learning curve is at its steepest. The sophistication of such a sample should naturally increase. Thus consider the contrast with Jennings and Niemi's piggyback sample of the students' parents. The parental distribution shows the familiar positive skew and changes trivially from 1965 to 1973, even though the parents of high-school almost-graduates are themselves an educationally and intellectually biased sample and may still be experiencing some minor life-cyclical gains.<sup>22</sup>

The LC does show some aggregate movement from Level B to Level A from 1956 to 1964 (Klingemann and Wright, cited in Converse, 1975a; Miller and Miller, 1976; Pierce and Hagner, 1982), but Level A seems to

<sup>22</sup> As a final discount, the educational and motivational bias in panel attrition—less highly educated and less subject-involved respondents are more difficult to relocate and reinterview—may produce some gain in both samples, as sketches of reinterviewed and non-reinterviewed respondents suggest (Jennings and Niemi, 1981, Table A.3, p. 398).

have shriveled again since. By Pierce and Hagner's figures, it is scarcely larger now than in 1960. But neither this recent decline nor the earlier surge reflects much real change in any event. People do not build or forget complex PBSs so quickly. The most telling evidence in all these chartings over time is that almost three-fourths of the 1956–64 increase was in the “near-ideology” substratum of Level A (see Klingemann's figures in Converse, 1975a), the home of hollow references to “ideological” terms, which reached a local peak in the Goldwater campaign of 1964. Again, the percentage of RU “ideologues”—of people who could supply some tolerably abstract and accurate meaning for such terms—scarcely budged.

Similarly, measures of political information holding (size and range) paint an unsophisticated and relatively unchanging picture. Black-humorous single-item distributions abound. After Reagan's electoral vote landslide in 1980, only 15 percent of a national sample could correctly identify the Democrats as having retained control of the House of Representatives (and some were undoubtedly guessing). Another poll in the same year showed 93 percent of the American public able to identify Mary Tyler Moore but only 5 percent able to identify Lane Kirkland (Greenstein and Feigert, 1985). In 1964, 62 percent did not know that the Soviet Union was not in NATO (Erikson, Luttbeg, and Tedin, 1980). And so on, and so on. Such tales of ignorance are well known, but by a strange sort of compartmentalization do not often get mentioned in the sophistication literature. One over-time comparison comes from Jennings and Niemi (1981), whose measure of “political” (largely historical) knowledge registers no change at all among either students or parents.

Within the sophistication literature, Neuman's (1981) count of political objects mentioned ranges from one (that's one in the whole interview) to 94. Its mean is only 26.7, with a standard deviation of 16.5. This sort of lopsided distribution conjures up Converse's image of a “continental shelf” separating the relatively sophisticated few from the relatively unsophisticated many (1964, pp. 255, 257n). My count of “informed” opinions, with a range of zero to 11, averages 5.47, which may seem creditable enough at first glance but is in perspective quite pitiful. It is very little to ask of any reasonably sophisticated respondent, of anyone who seriously follows political affairs, that he or she have an opinion and know the parties' relative positions on issues at the crux of political debate. Doing so on half the issues is not an impressive achievement. A wholly uninformed respondent would on average do as well by randomly picking a scale location for him- or herself, then

blindly guessing the parties' locations. Even a respondent who places him- or herself and correctly places the parties on eight of the 11 issues has essentially scored 73 percent correct on a particularly easy true-false exam, the sort of performance that, conventionally graded, might receive a low C. Yet 84 percent of the sample manages only a score of eight or less. And easy though it should be for anyone who "knows the material" to do so, only 3.4 percent place themselves and correctly place the parties on all 11 items.

Composite measures, too, tell a similar story. In all five countries he studies, Klingemann (1979c) finds under 5 percent in his highest category. In all five countries his single nonideological category is the most heavily populated. My own measure, with a much wider range (1 to 60), has a somewhat craggier but generally similar distribution. The lowest tenth of the scale (from 1 to 6) holds 23 percent of the sample; the lowest four-tenths (from 1 to 24) hold 75 percent. The highest four-tenths (from 37 to 60) hold only 10 percent; the highest tenth (from 55 to 60) only 2 percent.

### **Whither Sophistication Research?**

In the grossest terms, then, the distributional evidence is clear: by anything approaching elite standards, the American public is extremely unsophisticated about politics and has not become appreciably more so over the past two-and-a-half decades. Other publics, abroad, are similarly unsophisticated. It is time to close the books on these questions and turn to others. But which? Kinder (1983), having come to the same general conclusion, suggests redirecting our efforts toward explaining more specific cognitions and cognitive structures (schemata). An earlier version of Kinder's paper bore the title, "Enough Already about Ideology!" But is it enough? In ways, we have barely scratched the surface. We know very little about why people become as politically sophisticated or unsophisticated as they do. We know almost as little about the differences their level of sophistication makes to their political behavior, although what we do know redounds to sophistication's importance. It appears to affect interest (as well as vice versa) and thus most forms of participation (Luskin, 1987a) and to condition the style and outcomes of political decision making (Knight, 1985). Let us by all means devote more attention to the processes by which people acquire certain cognitions or schemata and not others. These questions, I agree, have been seriously understudied. But other neglected questions concern sophistication. The most important—of cause and effect—have scarcely been asked.

If we are to make serious headway, we shall need the best measurement we can muster. I hope I have convinced the reader that wildly generous AU measures of abstraction and consistency-based measures of all sorts should be given a wide berth. Yet I have also argued that certain kinds of existing measures can be reasonably face-valid and perform well empirically. Carefully enough constructed AU and RU measures of abstraction may serve quite well. Simple counts of information held may serve better, combinations of measures still better. Composite AU-RU measures should be superior to their AU and RU constituents, and further combinations of AU, RU, or AU-RU measures, which incline toward integration, and information-holding measures, which incline toward differentiation, should be superior to their constituents in turn. The testimony of my own versions of these measures supports all these statements except the last. My count of informed opinions fares just as well individually as in tandem with my AU-RU composite. For theoretical reasons, I continue to prefer the combination, but the count of informed opinions comes a close second. Both on this evidence and because they are more direct than abstractness-based measures, I suspect that information holding measures represent the best *single* existing approach.

Dramatically more refined measurement awaits more specialized instruments. Some interesting possibilities lie in Szalay's "associative group analysis" of free association responses to a set of stimulus words (e.g., Szalay and Kelly, 1982; Szalay, Kelly, and Moon, 1972). Szalay and his colleagues are concerned with political culture and "ideology" in the alternative sense of group belief and thus concentrate on group distributions, but their measures begin at the individual level and can be adapted to gauge cognitive structure. To sketch some possibilities, let the stimulus phrases be a mix of "ideological" labels ("liberal," "conservative," "communist") and broad issues ("free trade," "civil rights," "pollution control"). Scoring responses to "ideological" labels by abstractness and averaging across labels yields a novel RU measure of abstractness. Scoring responses to issues by order of occurrence (which should reflect psychological proximity to the stimulus phrase)<sup>23</sup> and summing the scores for high-order abstractions across issues yields a novel AU measure of abstractness. Or to measure constraint more directly, let the responses to all stimulus words be scored by order of occurrence. The psychological connectedness of any pair of

<sup>23</sup>Szalay's weighting is 6 for the first response, 5 for the second, 4 for the third, 3 apiece for the fourth through seventh, 2 apiece for the eighth and ninth, and 1 apiece for all succeeding responses (Szalay and Brent, 1967).

stimulus words  $S_1$  and  $S_2$  can then be measured by either of two symmetric variants of Szalay's asymmetric "index of interword affinity": either the mean of  $S_1$ 's score as a response to  $S_2$  and  $S_2$ 's score as a response to  $S_1$  or the sum of the scores, on both  $S_1$  and  $S_2$ , of all responses common to both.

I see less promise in the congeries of measures developed by Scott and his colleagues (1979; elaborating on Scott, 1963, 1969, 1974), although the reason lies less in the formulas than in the data they work from. These latter come from lengthy, arid questionnaires, typically providing or asking the respondent to generate sets of objects and attributes, then asking the respondent to say which are similar and which dissimilar, and on what attributes, or to say which objects can be described by which attributes. One problem is that with objects and attributes both running well into double figures, the business of answering is tedious, and the apparent complexity of the answers given may be more a function of diligence than of cognitive structure. A more fundamental problem is that respondents will obligingly make attributions and similarity judgments ("nonattributions" and "nonsimilarities") even in the absence of preexisting cognition. For whatever reason, the intercorrelations of Scott et al.'s various measures of the same and closely related concepts are painfully low, as are the reliabilities of most of their composite measures (see their Tables 8, 9, 13, and 14).

Bolland (1985) offers formulas similar to two of Scott's, based on questionnaires asking respondents to say how relevant each of 59 "attributes" (community concerns such as "improve drug abuse programs") is to each of seven "schemata" (issue areas such as "urban planning"). This, like Scott's, is an interesting venture, but has the same problems: the task confounds complexity with diligence, induced responses ("nonrelevances") are likely, and the abysmally low convergent validity and low reliabilities of Scott's measures cast a shadow on Bolland's.<sup>24</sup>

Avoiding induced responses requires observing cognitive connections more directly. One approach, broadly similar to the LC, is to code responses to open-ended questions according to the number of dimensions they contain and the dimensions' interconnectedness (Schroder, Driver, and Streufert, 1967; Tetlock, 1983, 1984, among other studies). The resulting index, of "integrative complexity," assigns 1 for low differentiation and low integration, 3 for moderate or high differentiation and low integration, 5 for moderate or high differentiation and moderate integration,

<sup>24</sup>The correlation between Bolland's two measures is a respectable .40, but his is a highly subject-involved sample, in which both respondent fatigue and induced responses should be uncommonly rare.

and 7 for high differentiation and high integration.<sup>25</sup> Borderline cases receive 2, 4, and 6. This is somewhat finer grained than the LC and similar measures, but shares some of their difficulties. Without indulgent coding, the reliance on spontaneous revelation of structure may underrate the inarticulate or taciturn. The trichotomous sortings on differentiation and integration are still rather crude and subjective, and need well-trained, experienced coders to be reliable (Scott, Osgood, and Peterson, 1979).

In principle, at least, we can do better. The ideal instrument would cross Lane's deep-probing but subjective and unstandardized approach (1962, 1969, 1973) with the objective, standardized, but less probing questions of the NES surveys. A battery of filtered, open-ended questions and probes could ask what the government should do or refrain from doing with respect to each of a large and varied set of policy issues; what other issues are related to each given issue, and how; and what actors have a major interest in each, what those interests are, and what positions those actors take as a result. Questions like these should permit a more direct assessment of cognitive content and organization. A simple count of questions answered or objects mentioned could measure differentiation; some version of the formula for C above, or perhaps some Scott-like formula, may work for integration.

Practicability remains a question, however, for two reasons. First, so many questions aimed at cognition may make the interview seem too much like a test. Many interviewees, having little to say, may rapidly lose interest. And, second, so many questions aimed at cognition leave rather little room for others. A battery of this sort would overcrowd the omnibus NES questionnaires; only a more specialized study devoted wholly or primarily to sophistication and its causes or consequences could include it. If we want to analyze a national cross-section, we shall probably have to be less ambitious. On the other hand, that may argue for smaller-scale, more narrowly focused studies. Despite the obvious cost in external validity, this may be a road more sophistication research should be traveling.

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<sup>25</sup> These authors consider differentiation a necessary condition for integration—hence the absence of categories for low/high and moderate/high combinations. In fact, however, differentiation is necessary to integration only if integration is a matter of the number rather than the proportion of pairs connected, and only in the sense that the number of cognitions places a ceiling on the number of connections.

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