*The Stability of Political Preferences: Comparisons of Symbolic and Nonsymbolic Attitudes**

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It has often been argued that political attitudes vary along a continuum from highly symbolic to nonsymbolic and that symbolic political attitudes are more stable across the life cycle than are nonsymbolic political attitudes. The evidence used to support this contention shows that the over-time consistency of attitude reports is high for symbolic attitudes, such as political party affiliation, and low for nonsymbolic attitudes, such as attitudes toward specific government policies. This paper reports three investigations that decomposed test-retest correlations between attitude reports into components due to attitude change and attitude measurement unreliability. Data from 1956–60, 1972–76, and 1980 National Election Panel Studies (NES) revealed that reports of symbolic attitudes were more consistent over time because they contained less random measurement error, not because these attitudes were more persistent over time. The differences in measurement precision across attitude object categories appear to be due to differences in the format of the survey questions used to measure them. It therefore seems that the persistence and potency of political attitudes vary more across citizens than they do across attitude object categories. All of these findings suggest the need for revision of conventional wisdom about the viability of some central assertions of democratic theory.

Introduction

Since the earliest empirical research on political judgment, political scientists have assumed that citizens form broad political orientations in early childhood and that these orientations are primarily affective attachments to social groups and value perspectives. Furthermore, these orientations are presumed to persist relatively unchanged throughout adulthood and to shape citizens' attitudes on new concrete political issues that become the subject of public debate (e.g., Campbell et al. 1960; Dawson and Prewitt 1969, 43; Easton and Dennis 1969, 9; Greenberg 1970; Hess and Torney 1967, 7; Marsh 1971; Schonfeld 1971; Searing, Schwartz, and Lind 1973, 421; Searing, Wright, and Rabinowitz 1976). The notion that core value orientations are formed early in life and shape later-formed attitudes is consistent with a great deal of psychological theory

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(e.g., Katz 1960; Rokeach 1968, 1973) and with empirical research on the long-term persistence of political orientations (Alwin, Cohen, and Newcomb 1992; Alwin and Krosnick 1991b).

Probably the most systematic and extensive statement of this perspective in political science is symbolic politics theory (Lau, Brown, and Sears 1978; Sears 1988; Sears and Citrin 1985; Sears and Kinder 1970). According to this theory, political attitudes range along a continuum from highly symbolic to nonsymbolic. Symbolic attitudes are thought to develop early in life through preadult socialization in response to the behavioral and attitudinal norms exhibited in a child's environment. As a result, positive and negative affect presumably become attached to a variety of diffuse political symbols. Encountering these symbols during adulthood is thought to evoke the associated affect, which in turn is assumed to have a powerful impact on political cognition and behavior.

Symbolic attitudes are thought to differ from nonsymbolic attitudes in three primary ways (Sears 1975, 121, 1983, 83–84; Sears and Whitney 1973b, 263). First, symbolic attitudes are thought to be acquired early in life, presumably through conditioning-like processes, and these attitudes presumably have strong affective components and little informational content. In contrast, nonsymbolic attitudes are assumed to be formed during adulthood primarily as the result of information integration. As a result, symbolic attitudes are thought to be highly stable over time during adulthood and resistant to persuasion, whereas nonsymbolic attitudes presumably change easily in response to persuasive arguments and to changes in the objective political world. Second, people's reports of symbolic attitudes in surveys are assumed to be highly consistent across different question wordings, whereas reports of nonsymbolic attitudes can presumably change easily as the result of question wording changes. And third, symbolic attitudes are assumed to be powerful determinants of attitudes toward newly encountered objects, whereas nonsymbolic attitudes are thought to have relatively little impact on the formation of other attitudes. Symbolic attitude objects are also thought to divide people along social and demographic lines of cleavage, to be highly salient in the political environment, to retain stable meaning over long periods of time, to be frequent subjects of public discussion, and to be extensively connected to other cognitive elements in citizens' minds (Sears 1983). Given these criteria, symbolic politics theory proposes a hierarchy of political attitude objects ranging from highly symbolic to nonsymbolic: (1) political party identification, (2) liberal-conservative ideological orientation, (3) attitudes toward social groups, (4) attitudes on racial policy issues, (5) attitudes on nonracial policy issues, and (6) attitudes regarding political efficacy and trust in government (Sears 1983).

Although some aspects of this ordering are unique to symbolic politics theory, one has been widely accepted for many years. Since the publication of

The American Voter (Campbell et al. 1960), many political scientists have believed that identification with political parties is the single most potent and persistent political orientation. According to this widely shared view, party identification is typically inherited from parents early in life, is highly resistant to change thereafter, and is a powerful organizer of individuals' political sympathies throughout life (e.g., Campbell et al. 1960; Converse 1964; Converse and Markus 1979; Markus 1982).

The only assertion of symbolic politics theory's ordering of political attitudes that is widely disputed among political scientists involves political efficacy and trust. Many scholars have suggested that political efficacy and trust form early in childhood, persist through adulthood, and shape many later-formed political attitudes (e.g., Markus 1979, 338; Searing, Schwartz, and Lind 1973, 422). Thus, although the basic assumptions of symbolic politics theory's perspective are widely shared, its ordering of attitudes from highly symbolic to nonsymbolic is not universally agreed upon.

Three sets of empirical findings have been cited as justifications for symbolic politics theory's ordering of political attitudes. The first is evidence that children are able to articulate their attitudes toward objects high in the hierarchy at very young ages, thus suggesting early acquisition (Greenstein 1965; Harding et al. 1969; Hess and Torney 1967; Katz 1976; Simmons and Rosenberg 1971; Tudor 1971). Second, attitudes higher in the hierarchy tend to be strongly correlated with newly formed attitudes, thus suggesting that the symbolic attitudes may have shaped the newly formed attitudes (Kinder and Sears 1981; Lau et al. 1978; Sears and Citrin 1985; Sears et al. 1980; Sears et al. 1978). And third, attitudes higher in the hierarchy evidence stronger test-retest correlations over long periods of time, thus suggesting greater stability over time and greater resistance to change (Sears 1983; Converse 1964; Converse and Markus 1979; Markus 1982).[1]

The focus of this paper is the latter body of evidence, regarding over-time consistency of attitude reports. Although this evidence is consistent with symbolic politics theory's hierarchical ordering of attitudes, its interpretation is equivocal. It is well-known that zero-order test-retest correlations are not pure measures of attitude stability (see, e.g., Alwin 1973, 1989). These correlations

[1] Also consistent with this claim is evidence that according to recall questions, people claim that such symbolic attitudes as party identification have rarely changed during their lives (e.g., Campbell et al. 1960, 150). However, the validity of such recall questions has been discredited (Niemi, Katz, and Newman 1980). Furthermore, although Markus (1986) asserted that poor recall of changes in policy attitudes attests to the weakness of those attitudes, Niemi, Katz, and Newman (1980) documented just as poor or poorer levels of accuracy in questions measuring recall of change in party identification. Thus, data from recall questions do not actually support the symbolic-nonsymbolic distinction.

reflect both the amount of attitude change that takes place during a given time period and the amount of random measurement error in reports of attitudes. Differences between attitude object categories in terms of over-time correlations may therefore reflect differences in the extent of either or both processes because both attenuate such correlations.

In fact, there is good reason to suspect that measurement unreliability may be responsible for the observed differences between the over-time consistency of reports of symbolic and nonsymbolic attitudes. I argue below that the formats of the survey questions typically used to assess symbolic attitudes are likely to produce highly reliable measurements, whereas nonsymbolic attitudes are typically assessed using survey question formats that are likely to generate relatively unreliable measurements. Thus, I shall suggest, some or all of the difference between the over-time consistency of symbolic and nonsymbolic attitude reports may be due to differences in question format, not due to differences in the persistence of the attitudes themselves. After presenting this argument, I use three national survey data sets to decompose the over-time consistency of reports of a variety of types of political attitudes into components due to attitude stability and to measurement unreliability. I then examine whether symbolic attitudes are more stable or more reliably measured than nonsymbolic attitudes.

Methodological Confounds

Nearly all previous studies of the over-time consistency of political attitude reports have been based on data from the National Election Panel Studies (NES) conducted in the 1950s, 1970s, and 1980s. In these studies the formal properties of the survey questions varied across the attitude object categories in ways that may have affected measurement precision. The most obvious and potentially significant differences between the NES question formats involve the number of response options offered by a question, the proportion of those response options that are verbally labeled, and the use of branching formats. A number of studies indicate that seven-point scales are most reliable, with reliability decreasing as the number of scale points decreases or increases (Alwin and Krosnick 1991a; Andrews 1984; Bendig 1953; Birkett 1986; Champney and Marshall 1939; Finn 1972; Symonds 1924). Thus, Miller's (1956) claim that seven is a "magic number" in perception and cognition is strongly validated in this instance. Research on verbal labeling indicates that an attitude measure's reliability improves with increases in the proportion of response alternatives that are labeled with words (as opposed to simply being labeled with numbers, see Alwin and Krosnick 1991a; Madden 1960; Peters and McCormick 1966; Zaller 1988). And work by psychologists suggests that decomposing decision tasks into simpler subtasks increases the precision of the final result (Armstrong, Denniston, and Gordon 1975; Krosnick and Berent 1990). Therefore, the NES branching approach, which first involves measuring attitude direction and then measuring attitude extremity, may enhance the reliability of attitude reports relative to nonbranching questions that ask respondents to report direction and extremity in a single response.

These findings have interesting implications for the NES measures of political attitudes. The most highly symbolic attitudes—political party identification and ideological orientations—have typically been measured on seven-point response scales that have all points labeled with words and employ a branching format. Party identification is measured by asking respondents whether they consider themselves to be Republicans, Democrats, or independents. People who report identifying with a party are then asked whether they do so strongly or weakly. People who say they are independents are asked whether they leaned toward one party or the other. As a result, respondents were segmented into seven groups along a continuum ranging from strong Republican to strong Democrat. This branching approach presumably makes it very easy for respondents to understand the meanings conveyed by the response alternatives, so their choices may be highly reliable as a result. Liberal and conservative self-ratings are also acquired in these surveys by asking respondents to place themselves on a fully labeled seven-point scale: 1 = extremely liberal; 2 = liberal; 3 = slightly liberal; 4 = moderate, middle of the road; 5 = slightly conservative; 6 = conservative; 7 = extremely conservative. Here again, the meanings of the response alternatives are quite clear and are likely to facilitate high reliability.

As we descend the symbolic hierarchy, the structure of questions changes in ways that signal a possible increase in inherent unreliability. For example, attitudes toward social groups have typically been measured in the NESs using nonbranching 0–100 feeling thermometers with verbal labels on only some of the points. Respondents are told: "If you don't feel particularly warm or cold toward a group, then you should place them in the middle of the thermometer, at the 50 degree mark. If you have a warm feeling toward the group, or feel favorably toward them, you would give them a score somewhere between 50 and 100 degrees, depending on how warm your feeling is toward that group. On the other hand, if you don't feel very favorably toward a group—that is, if you don't care too much for them—then you would place them somewhere between 0 and 50 degrees." Respondents are then given a show card displaying a thermometer with verbal labels as follows: 100 = very warm or favorable feeling; 85 = good, warm, or favorable feeling; 70 = fairly warm or favorable feeling; 60 = a bit more warm or favorable than cold feeling; 50 = no feeling at all; 40 = a bit more cold or unfavorable feeling; 30 = fairly cold or unfavorable feeling; 15 = quite cold or unfavorable feeling; 0 = very cold or unfavorable feeling. Thus, this scale offers many more than seven response alternatives, few of which are verbally anchored, so the thermometers are likely to be less reliable than the party identification and ideology questions.

Descending the symbolic hierarchy a step further, there appears to be even more inherent ambiguity in the question formats employed. The racial and non-racial policy attitude questions in the NESs most often involve seven-point scales, so reliability should be helped somewhat. However, only the endpoints of the scales are labeled with words, and no branching is involved. Thus, even fewer verbal labels are provided than the thermometers offered. If the loss due to labeling outstrips the gain due to number of response alternatives, random measurement error may again be increased, particularly in the midrange of the scale, where no verbal labels are offered at all.

Finally, most of the political efficacy and trust items in the NESs asked respondents to choose between just two response alternatives. Both response choices were verbally labeled, which presumably minimized ambiguity in the meanings of the alternatives. However, dichotomous items are likely to be less reliable than politimous items because random error in reports by respondents with attitudes near the midpoint of the attitude continuum causes these reports to oscillate. If these respondents were given the opportunity to express slight leanings in one direction or the other, their responses would be substantially more reliable (see, e.g., Andrews 1984; Finn 1972; Krosnick 1984). Therefore, these dichotomous items may be especially unreliable.

Furthermore, most of the dichotomous measures of efficacy and trust in government offered respondents a statement and asked them whether they agreed or disagreed with it. It is well-known that agree-disagree questions are influenced by acquiescence response bias, the tendency to agree with any statement, regardless of its content (Lenski and Leggett 1960; Schuman and Presser 1981). And this response bias appears to affect answers to the NES measures of efficacy and trust (see, e.g., Wright 1975).

Relatively few respondents typically acquiesce on any given item (Lenski and Leggett 1960; Schuman and Presser 1981), and recent research indicates that the tendency to acquiesce is an unstable property of individuals (Hui and Triandis 1985). That is, a person who evidences acquiescence response bias during one interview will not necessarily evidence it during a subsequent interview months or years later. It therefore seems appropriate to think of acquiescence response bias as contributing measurement error to an assessment. Furthermore, because this error is unstable over time, it seems appropriate to think of this process as increasing unreliability and therefore attenuating test-retest correlations, just as any source of random measurement error would do. Therefore, the use of agree-disagree response choices in most of the NES measures of efficacy and trust should further decrease their inherent reliability relative to the other attitude object categories. Because attitudes toward government policies sometimes have been measured in the NESs using nonbranching agree-disagree questions, their reliabilities are also presumably compromised as a result of acquiescence response bias (see Jackson 1979).

In sum, differences between attitude object categories in terms of the over-time consistency of reports in the NESs may be due partly or completely to differences in reliability produced by response format variation rather than to variations in the stability of the attitude objects. In order to determine whether highly symbolic attitudes are indeed more stable over time than nonsymbolic attitudes, it is necessary to decompose over-time correlations between attitude reports into a component due to attitude change and a component due to attitude measurement reliability.

Previous Studies

A number of previous investigators attempted to decompose correlations thusly. For example, Converse and Markus (1979) and Green and Palmquist (1990) used single-indicator structural equation modeling to accomplish a decomposition in the case of party identification. They found, as would be expected, that its stability appeared to be substantially higher after correction for unreliability. McPherson, Welch, and Clark (1977) applied multiple-indicator structural equation modeling to the NES measures of efficacy and also found quite high stabilities after removing the impact of unreliability. However, neither McPherson, Welch, and Clark (1977) nor Converse and Markus (1979) nor Green and Palmquist (1990) implemented the same procedure for more than a single category of political attitude, so their findings cannot be used to test the assertions of symbolic politics theory.

Achen (1975) went a step further by using structural equation modeling to disentangle the stability and unreliability of both party identification and policy attitudes. Achen claimed that after correcting for unreliability, these two classes of attitudes were found to be equally stable. However, a careful reading of his parameter estimates reveals that party identification did indeed appear to be more stable than racial attitudes, which in turn appeared to be more stable than nonracial policy attitudes (1975, 1225). But because Achen did not test the statistical significance of these apparent differences, it is impossible to assess their robustness.

Sears and Gahart (1980, described in Sears 1983) went much further by examining party identification, ideological orientation, racial tolerance, women's liberation, political trust, and political efficacy. In order to estimate attitude stability, they conducted multiple-indicator structural equation analyses in which sets of items were treated as measures of the same single underlying attitude. As expected, the stabilities of party identification, ideological orientation, racial tolerance, and women's liberation were found to be relatively high, whereas political trust and efficacy were relatively low.

Although the method used to generate these stability estimates is an improvement over simply examining zero-order correlations, it is potentially problematic. The validity of parameter estimates generated by multiple-indicator structural equation models hinges importantly on the validity of the measurement model. If a set of indicators are assumed to be measures of a single latent construct, whereas they actually measure multiple partially independent constructs, the resulting parameter estimates will most likely be attenuated. Of most relevance to Sears and Gahart's (1980) study, considerable research has shown that the NES items that measure political trust and efficacy reflect complex multidimensional factor structures and that correlations between these items partly reflect methodological confounds (Acock, Clarke, and Stewart 1985; Balch 1974; Craig and Maggiotto 1982; Craig and Niemi 1988; Feldman 1983; House and Mason 1975; Mason, House, and Martin 1985; McPherson, Welch, and Clark 1977). This multidimensionality may therefore be responsible for Sears and Gahart's finding that trust and efficacy have lower stabilities than the other attitudes that they examined.

The Present Investigation

In this paper I report three sets of analyses that more thoroughly explored whether attitude stability varies across attitude object categories according to the symbolic and nonsymbolic hierarchy once measurement error differences are taken into account. I reanalyzed the national survey data examined by Achen (1975), Converse and Markus (1979), McPherson, Welch, and Clark (1977), and Sears and Gahart (1980), and I examined one new data set as well. In all three sets of analyses, I used single-indicator structural equation modeling procedures to decompose test-retest correlations into their two constituents, a method that overcomes the problems inherent in Sears and Gahart's (1980) analyses. Where possible, I also examined the association between response format and item reliability.

Data and Analysis Method

Surveys

I examined the 1972–76, 1956–60, and 1980 National Election Panel Studies. The sizes of the nationally representative samples for these studies were 1,320, 769, and 1,132 adults, respectively. For the 1972–76 study, interviews were conducted before and after the 1972 presidential election, again before the 1974 midterm election, and again before and after the 1976 presidential election. For the 1980 study, respondents were interviewed first in January and February, again in June, and a third time in September and October. Finally, for the 1956–60 study, respondents were interviewed before and after the 1956 presidential election, again after the 1958 midterm election, and again in 1960 before and after the presidential election. Taken together, these three surveys permitted examination of attitude change that occurred both between elections and within a single election campaign.

Measures

The analytic method employed here requires that attitudes be measured on three occasions. In the 1972–76 panel, respondents were asked in each year 30 identical questions that measured all of the six categories of attitudes addressed by symbolic politics theory: (1) political party identification; (2) liberal-conservative ideological orientation; (3) attitudes toward social groups, such as labor unions, the military, and the police; (4) attitudes on racial policy issues, such as school busing; (5) attitudes on nonracial domestic policy issues, such as federally guaranteed employment and protecting the rights of people accused of committing crimes; and (6) political efficacy and trust. In the 1980 surveys, respondents were asked in each year nine identical questions that measured attitudes in three of these categories: (1) political party identification, (2) liberal-conservative ideological orientation, and (3) attitudes on nonracial domestic policy issues. In the 1956–60 surveys, respondents were asked in each year a total of seven identical questions that measured party identification, racial policy attitudes, and nonracial policy attitudes.[2]

[2]The first-wave item numbers for the 1972–76 NES attitude measures are party ID: 140 (party ID seven-point scale), 719 (Democrats thermometer), 721 (Republicans thermometer); ideology: 652 (liberal-conservative seven-point scale), 709 (liberals thermometer), 724 (conservatives thermometer); groups: 112 (civil rights leaders push), 707 (big business thermometer), 722 (labor unions thermometer), 717 (the military thermometer), 714 (policemen thermometer), 718 (whites thermometer), 720 (blacks thermometer); racial policy: 202 (busing); nonracial policy: 232 (women's role seven-point scale), 621 (rights of accused seven-point scale), 1067 (guaranteed job seven-point scale); efficacy and trust: 570 (government wastes money), 571 (how much trust government), 572 (government run by big interests), 173 (people running government are smart), 574 (people running government are crooked), 269 (no say), 270 (voting only way to have input), 272 (politics seems complicated), 273 (Congress loses touch), 274 (parties only interested in votes), 576 (parties make government pay attention), 578 (how much attention do members of Congress pay).

The first-wave item numbers for the 1980 NES items analyzed here are party ID: 801 (party ID seven-point scale), 439 (Democrats thermometer), 440 (Republicans thermometer); ideology: 944 (liberal-conservative seven-point scale); nonracial policy: 1114 (government spending), 1189 (get along with Russia), 1284 (build nuclear power plants), 1278 (relax environmental regulations), 1282 (allow gas and oil prices to rise).

The first-wave item numbers for the 1956–60 NES items are party ID: 88 (party ID seven-point scale); racial policy: 74 (school integration); nonracial policy: 32 (guaranteed job), 35 (isolationism), 41 (foreign economic aid), 53 (federal aid to cities and towns), 56 (send soldiers to fight communism).

For three additional attitude measures in the 1972–76 NES, two in the 1980 NES, and two in the 1956–60 NES, either LISREL was unable to estimate the structural parameters for one age-group, or one age-group's parameter estimates were far out of range. These problems probably occurred because of random fluctuation in the zero-order correlations between the attitude measures due to small sample sizes and sampling error. In order to maintain comparability of age-groups across item categories, these items were not included in any of the analyses reported below. However, when the analyses were repeated deleting only those single data points that were not estimable, the results were equivalent to those reported here (and in fact supported my conclusions even more strongly).

Placement of items into attitude object categories was based solely on the object being addressed by the question, and I leaned toward classifying items in the most symbolic category that was suitable. Therefore, although one might view Republicans and Democrats as social groups, I classified questions addressing them as measuring party identification. On the basis of similar reasoning, attitudes toward liberals and conservatives were classified as ideology, and attitudes toward blacks and whites were classified as social groups. And a question asking whether civil rights leaders are trying to push too fast was classified as an attitude toward civil rights leaders rather than a policy attitude because it did not ask directly about what federal government policy ought to be.

Some of the attitude measures allowed respondents to indicate that they did not know what their attitude was on an issue or that they had not thought enough about an issue to form an attitude. Respondents who gave either of these answers to a given question at any wave were eliminated from the analyzed sample for that question. This led to the omission of approximately 10% of respondents for almost all of the items and 35% for liberal-conservative self-placements.

Analysis

In order to assess the stability of these attitudes and the reliability of the attitude measures, I estimated the parameters of a model that is composed of two structural equations:

$$X_t = \tau_t + \varepsilon_t \tag{1}$$

$$\tau_t = \beta_{t,t-1}\tau_{t-1} + \zeta_t \tag{2}$$

Equation (1) specifies the measurement model, which describes the relation between the latent attitude being measured and verbal reports of it. It decomposes variance in reports (X_t, where $t = 1, 2, 3$) into two components, one due to the latent attitude (τ_t) and the other due to random measurement error (ε_t). Equation (2) specifies the structural model, the relations among the latent attitudes. The term $\beta_{t,t-1}$ is the stability of the latent attitude during the period from $t - 1$ to t, and ζ_t represents sources of change over time in the latent attitude. This model has been discussed extensively by Heise (1969), Wheaton et al. (1977), and Wiley and Wiley (1970).

As it appears in equations (1) and (2), the model is underidentified. There are six independent pieces of information available (σ_{12}, σ_{32}, σ_{31}, σ_1^2, σ_2^2, and σ_3^2) with which to estimate eight parameters (β_{21}, β_{32}, $\sigma_{\varepsilon 1}^2$, $\sigma_{\varepsilon 2}^2$, $\sigma_{\varepsilon 3}^2$, $\sigma_{\tau 1}^2$, $\sigma_{\zeta 2}^2$, and $\sigma_{\zeta 3}^2$). In order to identify the model, reliability is assumed to remain constant across waves, $(\sigma_1^2 - \sigma_{\varepsilon 1}^2)/\sigma_1^2 = (\sigma_2^2 - \sigma_{\varepsilon 2}^2)/\sigma_2^2 = (\sigma_3^2 - \sigma_{\varepsilon 3}^2)/\sigma_3^2$ (Heise 1969).[3]

[3] An alternative approach to identifying the model described in equations (1) and (2) was proposed by Wiley and Wiley (1970). All the analyses reported below were recomputed using their

Given this constraint, the model is just-identified, so the parameter estimates will always fit the observed data perfectly. Maximum likelihood estimates of these parameters were produced for each individual attitude measure using LISREL VI (Joreskog and Sorbom 1986).

The current investigation was conducted in the context of a larger research project that explored the relation of age to political attitude structure (Alwin, Cohen, and Newcomb 1992; Alwin and Krosnick 1991a, 1991b; Krosnick and Alwin 1989). For the purposes of this project, each NES sample was split into seven subgroups based on age, which allowed us to explore the impact of aging on susceptibility to political attitude change (see Krosnick and Alwin 1989). The parameter estimates generated for these analyses were then used in the present investigation. This approach generated seven estimates of the stability and reliability of each attitude measure examined and thus afforded greater statistical power to detect differences between attitude object categories than would have been the case had only one pair of estimates per measure for the full survey samples been used.

One might imagine that cutting by one-seventh the sample size on which each estimate is based would substantially increase its imprecision. However, the averages of the age-subgroups' estimates for each item were consistently extremely close to the full sample estimates for that item, so this approach did not alter the patterns of results shown below. Furthermore, when the analyses reported below were repeated using only the full sample parameter estimates, equivalent results were produced, though levels of statistical significance were reduced (see Alwin and Krosnick 1991a). Because the ability to detect statistically real differences in attitude stability between attitude object categories was of paramount importance in the present investigation, the age-subgroup approach seemed most sensible.

The sample subgroups were specified on the basis of respondents' ages at the time of the first interview: 18–25, 26–33, 34–41, 42–49, 50–57, 58–65, and 66–83. The total numbers for these groups in the 1972–76 surveys were 208, 227, 190, 204, 167, 147, and 161, respectively. For the 1980 NES, the age-group total sizes were 127, 163, 114, 68, 93, 87, and 88, respectively. And for the 1956–60 NES, the age-group total sizes were 94, 228, 241, 187, 138, 101, and 99, respectively. These numbers do not sum to the total Ns for the panels because the ages of some respondents (16 in the 1972–76 data set, 29 in the 1980 data set, and 44 in the 1956–60 data set) were not ascertained or fell outside of the 18–83 range.

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identifying assumption instead, and the results were comparable to those reported in the text. However, the standardized stability coefficients produced by the Heise model allow more reasonable comparison across object categories.

The parameter estimates generated for each of the seven age-subgroups were analyzed in two ways. First, in order to gauge the stability of latent attitudes over time, I examined the estimates of β_{21} and β_{32}. Second, in order to gauge the reliability of the attitude measures, I computed the reliability of each attitude measure by the following formula: $\alpha_t = (\sigma_t^2 - \sigma_{\varepsilon t}^2)/\sigma_t^2$. In order to compare the conclusions reached by this analysis to those that would have been reached had the distinction between attitude instability and attitude measurement unreliability been ignored, I examined Pearson product-moment test-retest correlations between the attitude measures at each wave: r_{12} and r_{23}.

To analyze the parameter estimates, separate repeated measures ANOVAs were conducted for the zero-order correlations, stability coefficients, and reliability coefficients derived from each data set. These analyses treated the seven age-groups as subjects and the attitude items as repeated measures. Thus, the statistical tests of differences between attitude object categories reported below take into account the nonindependence of the data points due to age effects.[4]

Results

Zero-Order Correlations

The first column of Table 1 displays the average zero-order correlations for the six attitude object categories in the 1972–76 NES. An ANOVA revealed statistically significant differences among these categories ($F(5, 408) = 53.84$, $p < .0001$), and the ordering of the object categories corresponds very closely to symbolic politics theory's assertions. Party identification, ideological orientation, and attitudes on racial policy issues have the largest average correlations; attitudes toward social groups and on nonracial policy issues have somewhat lower correlations; and political efficacy and trust has the lowest correlation. Contrasts

[4] Statistical tests in this analysis involve treating zero-order correlations, stability estimates, and reliability estimates as dependent variables within the framework of a type of meta-analysis. This approach has a number of disadvantages. First, the standard errors of the parameter estimates are ignored in these analyses. However, these standard errors are reflected somewhat by the variability between the seven age-groups. That is, variability between the seven age-groups partly reflects the imprecision of the coefficient estimates. This may make my tests overly conservative, but using the seven age-groups also adds degrees of freedom, which increases the probability that real differences between categories can be detected. Second, the covariances between the parameter estimates for the various items are not formally estimated and directly incorporated into the analyses. However, the repeated measures ANOVA takes these associations into account to a degree. Also, because these covariances are very likely to be positive, omitting them most likely makes my tests overly conservative. Thus, although the statistical tests reported here are not necessarily strictly formally correct, no sufficiently powerful, correct tests are available. Furthermore, the tests reported here simply confirm what is readily apparent by mere inspection of the mean parameter estimates. Therefore, I view these tests as somewhat crude but nonetheless informative indicators of the magnitudes and reliabilities of differences.

Table 1. Zero-Order Correlations, Attitude Stabilities, and Attitude Measure Reliabilities by Attitude Object from the 1972–76 NES

Attitude Object	Zero-Order Correlation[a]	Standardized Stability Coefficient[a]	Reliability[b]
Party identification	.60	.92	.66
Ideological orientation	.56	.93	.61
Attitudes toward social groups	.50	.95	.54
Racial policy issues	.57	.97	.61
Nonracial policy issues	.51	.95	.57
Political efficacy and trust	.38	.90	.46

[a] In these columns the party identification entry is an average of 42 coefficient estimates; the entry for ideological orientation is an average of 42 coefficient estimates; the entry for social group attitudes is an average of 98 estimates; the entry for racial policies is an average of 14 estimates; the entry for nonracial policy issues is an average of 42 estimates; and the entry for political efficacy and trust is an adjusted average of 182 estimates.

[b] In this column the party identification entry is an average of 21 coefficient estimates; the entry for ideological orientations is an average of 21 coefficient estimates; the entry for social group attitudes is an average of 49 estimates; the entry for racial policies is an average of 7 estimates; the entry for nonracial policy issues is an average of 21 estimates; and the entry for political efficacy and trust is an average of 91 estimates.

revealed that the party, ideology, and racial policy correlations are significantly larger than the social groups and nonracial policy correlations ($F(1, 408) = 21.76$, $p < .0001$), which in turn are significantly larger than the efficacy and trust correlation ($F(1, 408) = 107.06$, $p < .0001$). There are no other significant differences among the correlations.

The first column of Table 2 displays comparable average zero-order correlations for the three attitude object categories in the 1980 NES. An ANOVA again revealed statistically significant differences among the categories ($F(2, 117) = 19.04$, $p < .0001$), and the ordering of the object categories again corresponds closely to that suggested by symbolic politics theory. Contrasts revealed that the party identification mean is significantly larger than the ideology mean ($F(1, 117) = 6.37$, $p < .02$), but the ideology and policy means are not significantly different from each other ($F(1, 117) = 2.11$, n.s.).

The average zero-order correlations for the three attitude object categories in the 1956–60 NES are displayed in first column of Table 3. An ANOVA revealed statistically significant differences among the categories ($F(2, 89) = 101.16$, $p < .0001$), and the ordering of the object categories is again as expected: party identification has the highest average correlation, followed by atti-

Table 2. Zero-Order Correlations, Attitude Stabilities, and Attitude Measure Reliabilities by Attitude Object from the 1980 NES

Attitude Object	Zero-Order Correlation[a]	Standardized Stability Coefficient[a]	Reliability[b]
Party identification	.73	.93	.79
Ideological orientations	.64	.95	.74
Nonracial policy issues	.58	.97	.62

[a] In these columns the party identification entry is an average of 42 coefficient estimates; the entry for ideological orientations is an average of 14 estimates; and the entry for nonracial policy issues is an average of 70 estimates.

[b] In this column the party identification entry is an average of 21 coefficient estimates; the entry for ideological orientations is an average of 7 estimates; and the entry for nonracial policy issues is an average of 35 estimates.

Table 3. Zero-Order Correlations, Attitude Stabilities, and Attitude Measure Reliabilities by Attitude Object from the 1956–60 NES

Attitude Object	Zero-Order Correlation[a]	Standardized Stability Coefficient[a]	Reliability[b]
Party identification	.85	.95	.89
Racial policy issues	.51	.87	.61
Nonracial policy issues	.41	.89	.48

[a] In these columns the party identification entry is an average of 14 coefficient estimates; the entry for racial policy issues is an average of 14 estimates; and the entry for nonracial policy issues is an average of 70 estimates.

[b] In this column the party identification entry is an average of 7 coefficient estimates; the entry for racial policy issues is an average of 7 estimates; and the entry for nonracial policy issues is an average of 35 estimates.

tudes on racial policy and attitudes on nonracial policy issues. Contrasts revealed that the party mean is significantly larger than the racial policy mean ($F(1, 89) = 71.52$, $p < .0001$), and the racial policy mean is significantly larger than the nonracial policy mean ($F(1, 89) = 10.73$, $p < .002$). Thus, analyses of all three data sets replicated previous findings regarding zero-order correlations.

Attitude Stability

The second columns of Tables 1, 2, and 3 display the average stability estimates for each attitude object category in the 1972–76, 1980, and 1956–60 NESs, respectively. Contrary to symbolic politics theory's assertion, these means do not indicate that attitudes higher in the symbolic hierarchy are more stable than attitudes lower in the hierarchy. Furthermore, ANOVAs revealed that there are no significant differences among the stability coefficients for the 1972–76 NES ($F(5, 408) = 1.01$, n.s.), the 1980 NES ($F(2, 117) = 0.68$, n.s.), and the 1956–60 NES ($F(2, 89) = 0.39$, n.s.). Thus, there appears to be no relation between attitude object category and attitude stability in any of these data sets. Furthermore, this result in the 1956–60 NES indicates that the apparent differences in Achen's (1975) stability coefficients across attitude object categories were not statistically significant, so his interpretation of them was correct. Therefore, the relations between attitude object category and the zero-order correlations in column 1 of Tables 1, 2, and 3 are not due to differences in attitude stability.

Attitude Measure Reliability

The third columns of Tables 1, 2, and 3 display the average reliability estimate for each attitude object category in the 1972–76, 1980, and 1956–60 NESs. Here, ANOVAs did reveal significant differences among the categories for the 1972–76 NES ($F(5, 198) = 9.55$, $p < .0001$), the 1980 NES ($F(2, 54) = 7.82$, $p < .002$), and the 1956–60 NES ($F(2, 40) = 25.00$, $p < .0001$). For the 1972–76 data, contrasts revealed that the party, ideology, and racial policy reliabilities are significantly larger than the nonracial policy and social groups reliabilities ($F(1, 198) = 4.98$, $p < .03$), which in turn are significantly larger than the efficacy and trust reliability ($F(1, 198) = 15.80$, $p < .001$). No other significant differences among these average reliabilities appeared. In the 1980 data, the party and ideology means are significantly larger than the policy mean ($F(1, 54) = 10.44$, $p < .003$), and the party and ideology means are not significantly different from one another ($F(1, 54) = 0.67$, n.s.). And in the 1956–60 data, the party identification mean is significantly larger than the racial policy mean ($F(1, 40) = 13.51$, $p < .001$), and the racial policy mean is significantly larger than the nonracial policy mean ($F(1, 40) = 5.02$, $p < .04$). Thus, the ordering of the attitude object categories in each year is identical to the ordering of them produced by the zero-order correlations. It appears therefore that the differences in the zero-order correlations are due to differences in attitude measurement reliability, not attitude stability.

I suggested above that these differences in reliability might be due to differences in the formats of the response alternatives offered by the survey questions. This claim can be evaluated formally only if the same attitude is measured using two different response formats or if two different attitudes are measured using the same response format. The NES panel studies did not permit any such direct comparisons. However, these data sets did allow for three less strict comparisons, two involving party identification and the other involving ideology.

Party identification was measured in the 1970s and 1980s NESs in two ways. First, a branching format essentially asked respondents to place themselves on a fully labeled seven-point scale ranging from strong Democrat to strong Republican. Respondents were also asked to indicate their attitudes toward Republicans and toward Democrats on the 101-point feeling thermometers. Given the literature on the effects of response format reviewed above, we would expect the seven-point ratings to be more reliable than the 101-point ratings, and this is precisely what occurred. In the 1970s data, the reliability of the seven-point rating is .84, whereas the average reliability of the 101-point ratings is .59. In the 1980s data, the reliability of the seven-point party identification measure was .87, whereas the average reliability of the 101-point thermometer ratings of Republicans and Democrats was .75.

A parallel comparison in the case of ideology is possible using the 1970s data. The reliability of the seven-point liberal-conservative self-ratings is .67, whereas the average reliability of ratings of liberals and conservatives on the feeling thermometers is .59. Furthermore, in the 1970s data, the average thermometer reliability of .59 for the parties and ideological groups is not significantly larger than the average reliability of the social groups attitudes measured using the thermometer (.54, see Table 1).

The judgment task required by the seven-point rating scales (i.e., placing oneself on a bipolar dimension anchored by two opposing groups) is not identical to that required by the thermometer ratings (i.e., reporting one's attitude toward each group individually). Therefore, it is impossible to be certain that the response format differences are solely responsible for these differences in reliability. Nonetheless, these results are consistent with the claim that some of the reliability differences in Tables 1 and 2 are due at least partly to variation in response format.

Discussion

Symbolic attitudes are thought to be acquired early in life through parental socialization and to remain relatively fixed throughout the rest of life. In contrast, nonsymbolic attitudes presumably shift easily in response to changes in political, economic, and social circumstances. On the basis of this reasoning, an important criterion for determining whether an attitude represents a symbolic predisposition is its stability over time: symbolic attitudes are presumably highly stable (Sears 1969; Sears and Whitney 1973a, 1973b).

The primary evidence for the symbolic nature of party identification, ideological orientations, attitudes toward social groups, and racial attitudes is evidence that survey reports of these attitudes are more consistent over time than are reports of other attitudes (Converse 1964; Converse and Markus 1979; Markus 1982; Sears 1983). This finding was replicated using test-retest correlations in the 1956–60 NES, the 1972–76 NES, and the 1980 NES. However, these differences across attitude objects were due to differential measurement reliability, not differential stability. When measurement reliability was taken into account, all six categories of attitudes examined were equally stable over time. Therefore, because stability is a necessary criterion for the identification of symbolic attitudes, this evidence challenges the claim that political party identification, ideological orientations, attitudes toward social groups, and racial policy attitudes are more symbolic than attitudes on nonracial policy issues and feelings of political efficacy and trust.

Experimental studies of the effects of item format indicate that branching questions offering seven response alternatives, all of which are verbally labeled, maximizes the reliability of attitude measures. Also, previous research indicates that responses to agree-disagree questions may be especially unreliable because of the measurement error contributed by acquiescence response bias. In the present data, the attitudes that evidenced the highest reliabilities were measured in the National Election Studies with branching response formats that included seven response alternatives that were all verbally labeled. Attitudes with moderate reliabilities were measured with nonbranching response scales that offered fewer verbal labels and/or many more response alternatives. And the attitudes with the lowest reliabilities were typically measured using dichotomous and/or agree-disagree formats. It seems therefore that the differences observed here between attitude objects in terms of measurement reliability reflect differences in response scale formats.

Although the present data afford some between-item comparisons that are consistent with this claim, it was not possible rigorously to test this hypothesis directly. Conclusive tests must be based on experimentally controlled comparisons holding item content strictly constant and varying item format. In fact, we have recently completed some such experiments (Krosnick and Berent 1990), and they offer consistent support for our assertions. We look forward to seeing more experimental evidence of this sort that attempts to validate other aspects of the present paper's findings in this fashion.[5]

Alternative Sources of Random Measurement Error

Although item format may be largely or completely responsible for the differences between attitude object categories in terms of reliability, there are at

[5]One bit of relevant experimental evidence was reported by Aldrich et al. (1982). They found that for one survey item that measures respondents' own policy attitudes, test-retest correlations were

least two other possible reasons why reports of symbolic attitudes contain less random measurement error than reports of nonsymbolic attitudes. One was suggested by Converse (1964), who argued that for any given survey attitude measure, respondents can be grouped into two classes: those who hold a preexisting attitude toward the object and those who do not. Converse claimed that because respondents in the latter category want to appear to be opinionated to their interviewers, they will sometimes choose randomly from among the response alternatives offered, thus reporting what he called nonattitudes.

If survey respondents do sometimes report nonattitudes and if these reports are indeed purely random, they would contribute to the unreliability estimates generated in the analyses reported here. Therefore, the increased amount of random error contained in reports of nonsymbolic attitudes may reflect the fact that fewer respondents have such attitudes than have symbolic attitudes. Although this explanation does not place blame on survey question format variation, neither does it vindicate the claim that symbolic attitudes are more persistent over time than nonsymbolic attitudes. It therefore represents just as significant a challenge to the traditional characterization of symbolic and nonsymbolic attitudes.

There is another possible explanation as well. Two psychological theories suggest that respondents who have attitudes on a particular issue may differ from one another in terms of the clarity of their internal attitudinal cues. According to Fazio's research (1986; Fazio et al. 1982), people are able to report some attitudes quickly and confidently due to clear and accessible internal cues, whereas other attitudes are reported more slowly, more effortfully, and less confidently because relevant internal cues are more ambiguous or less accessible (see also Krosnick 1989). And according to Sherif's social judgment theory (Sherif and Hovland 1953, 1961; Sherif, Sherif, and Nebergall 1965), a person's attitude toward an object is not always simply a single point on an evaluative continuum but rather is sometimes a range of points, what he calls the individual's *latitude of acceptance*. If asked in a survey to choose a single point on a response scale to represent his or her attitude, the individual may choose somewhat randomly from among the response alternatives that fall within the range of acceptable positions. The wider a person's latitude of acceptance, the more his or her reports of the attitude will vary randomly over time and the more random error he or she will contribute to estimates of measurement unreliability such as those computed here. Therefore, both of these theoretical perspectives suggest that some

slightly higher for a partially labeled response scale than for a fully labeled scale (1982, 410). However, when I reanalyzed their data using a simpler method, I found that the fully labeled scale produced a higher percentage of respondents giving the same response at both waves of the panel. Unfortunately, comparisons between these two forms of the question are problematic in Aldrich et al.'s data, because true attitude change appeared clearly in responses to the fully labeled question but not in responses to the partially labeled question.

of the increased unreliability of nonsymbolic attitude reports may be due to greater ambiguity in respondents' internal attitudinal cues.

Again, this explanation does not attribute variation in the reliability of attitude measurements across object categories directly to variation in survey question format, but it also does not vindicate the claim that symbolic attitudes are more persistent over time. Furthermore, social judgment theory's reasoning suggests that some of the unreliability, and perhaps some of the *differences* in reliability between object categories, can be eliminated by allowing respondents to report the boundaries of their latitudes of acceptance, rather than compelling them to choose a single point on an evaluative continuum. And finally, this explanation suggests that the impact of this unreliability induced by cue ambiguity on correlational analysis results can be minimized by integrating multiple measures of an attitude via latent variable models (see Alwin 1988).

Many previous investigators have noted that test-retest correlations for some NES policy attitude measures are larger than those for other NES policy attitude measures, even though all were measured using the same response format (see, e.g., Converse 1964; Converse and Markus 1979; Jennings and Niemi 1974, 1981). Analyses of the 1956–60 NES data revealed just such a result. Thus, the over-time consistency of attitude reports does vary with changes in attitude object. Although some of this variability may be attributable to differences in attitude stability, the results reported here suggest that this is unlikely, given no significant variation in attitude stability across attitude object categories. Instead, differences between the over-time consistency of policy attitude reports are probably due primarily to differences in rates of nonattitude reporting and in the ambiguity of attitudinal cues. Thus, I do not mean to suggest that item format will always account for all differences in reliability between attitude objects. Instead, it appears that differences in the test-retest correlations between attitude object categories on which the symbolic-nonsymbolic distinction rests may be largely or exclusively due to this methodological confound.

The Validity of My Estimation Method

The validity of my conclusions regarding how best to interpret variation in test-retest correlations across attitude object categories depends importantly on the validity of the statistical method used here to disentangle attitude stability from attitude measurement reliability. These findings raise the possibility that this method is inclined to misidentify real attitude change as random measurement error and thus to attribute *all* over-time inconsistency in attitude reports to unreliability. The result of such a bias would be to generate consistently high stability coefficients, just the pattern of results observed here.

Is this a real danger? I believe not, because this method has yielded substantially imperfect stability coefficients in a number of previous investigations. For example, a study of the relation of age to attitude stability found stability coefficients substantially less than 1.0 for 18–25-year-olds while the coefficients for older adults were closer to perfect (Alwin and Krosnick 1991b; Krosnick and Alwin 1989). Also, a study of attitude importance revealed that the stabilities of political attitudes that people consider to be personally unimportant are far from perfect and substantially lower than the stabilities of personally important political attitudes (Krosnick 1988a). It seems clear that the estimation method used in the analyses described above is not bound to produce nearly perfect stability coefficients.

Nonetheless, the method used here is not ideal. Although it represents an improvement over previous efforts, the structural equation model used here does make certain assumptions that may be invalid. For example, the model assumes that attitude change occurs via a simplex process (see Alwin 1988), and some skepticism has been expressed about the validity of such an assumption (Rogosa 1988). However, a careful review of the test-retest correlations revealed that nearly all of them conform to a simplex structure, so this assumption is not particularly troubling.

Another assumption made by the model is that measurement reliability remains constant across repeated interviews, which may not be the case. For example, a "socratic effect" may produce a decrease in random measurement error across waves of panel surveys (Jagodzinski, Kuhnel, and Schmidt 1987; but see Saris and Putte 1988). However, the reliabilities generated by my estimation method are essentially averages computed across the three interviews, which can be reasonably compared across attitude object categories even if the assumption of constant reliability is violated. Similarly, the model assumes that the loading of each item on the latent attitudinal factor that it reflects remains constant over time, and the resulting analytic solutions essentially average these loadings across waves if there is any variation in them. Thus, comparisons between attitude object categories are again probably not distorted as a result.

The model also assumes that there is no correlated measurement error across interviews. Violation of this assumption would lead to inappropriately low estimates of unreliability and inappropriately high estimates of stability. However, this problem would not produce inappropriate estimates of the amount of *random* measurement error in attitude reports (see Alwin 1974), so my findings in this regard do not hinge on this assumption. Furthermore, given the existing evidence of very low persistence of correlated error over time (e.g., Hui and Triandis 1985), there is almost certainly very little such error in measurements made two years apart, as was the case in the 1972–76 and 1956–60 NESs. Therefore, my findings regarding attitude stability are probably not misleading due to the assumption of no correlated measurement error.

Although there are limitations imposed by this analytic strategy's assumptions, violation of these assumptions is unlikely to bias comparisons across attitude object categories. To the extent that an assumption is unrealistic, it is likely to be equally unrealistic for all categories. Therefore, the observed differences between categories in terms of unreliability are unlikely to be due to variation across categories in terms of bias. My major conclusions are therefore probably not highly contingent upon the validities of the model's assumptions. Nonetheless, I look forward to the development of analytic techniques and the collection of data sets that require fewer assumptions to accomplish the goal of separating attitude stability from measurement unreliability.

Reconsidering the Symbolic-Nonsymbolic Distinction

The evidence reported here suggests that it might be wise to perform a careful reevaluation of the claim that political party identification, ideological orientations, attitudes toward social groups, and racial policy attitudes are symbolic attitudes, whereas nonracial policy attitudes and feelings of efficacy and trust are nonsymbolic. Part of such a reevaluation would presumably involve reviewing the two other major bodies of evidence that support the distinction. When I conducted such a review, I found relatively little solid support for it.

The first set of relevant evidence is data on the age at which political attitudes are formed. Many studies frequently are cited as having shown that symbolic attitudes are formed relatively early in childhood, whereas nonsymbolic attitudes are formed later (Greenstein 1965; Harding et al. 1969; Hess and Torney 1967; Katz 1976; Simmons and Rosenberg 1971; Tudor 1971). However, a close reading of this evidence reveals a number of limitations. First, no study that I could uncover used comparable methods to assess the age at which various types of political attitudes are formed. Instead, one method has been used to assess the age at which one type of attitude forms, whereas a different method has been used to assess when another type of attitude forms (see, e.g., Greenstein 1965, 64–75; Hess and Torney 1967, 60–92; Hyman 1959, 58–68). Consequently, direct comparisons of age of formation across attitude object categories are not justified.

Second, these studies typically assessed whether an attitude is formed by whether a child was willing to answer a question that measured the attitude. However, the answers that children provide to such questions often yield such low test-retest correlations over short time periods that they do not support the contention that what is being measured is well crystallized (e.g., Vaillancourt 1973). And finally, even if it is true that party identification is usually formed by age 10, whereas policy attitudes do not form until age 14, it is hard to imagine that this four-year difference is substantively significant in terms of the likelihood that these attitudes will persist through adulthood. Therefore, developmental research does not yet seem to offer solid support to the symbolic-nonsymbolic distinction.

The other body of evidence viewed as supporting this distinction addresses the impact of symbolic and nonsymbolic attitudes on newly formed attitudes. For example, Kinder and Sears (1981) demonstrated that racial attitudes were strongly associated with voting behavior in two mayoral elections in Los Angeles. And Sears, Hensler, and Speer (1979) demonstrated that racial attitudes and political party affiliation were strongly associated with candidate preferences in the 1972 U.S. presidential election. However, in these and other similar studies, the strengths of these associations have rarely been compared to the strength of associations between nonsymbolic attitudes and relevant newly formed attitudes. Instead, comparisons have nearly always been made between symbolic attitudes and measures of people's material self-interest in an issue. Of course, this comparison does not validate the proposed distinction between symbolic and nonsymbolic attitudes. Furthermore, one study that extensively compared the degree to which various political orientations acquired in childhood could have shaped adult stands on specific policy issues found that symbolic orientations, such as party identification, appeared to have no greater apparent impact than nonsymbolic orientations, such as political efficacy and trust (Searing, Schwartz, and Lind 1973, 426).

This evidence suffers from a more significant limitation as well. The impacts of long-standing attitudes on newly formed attitudes have always been measured in these studies by applying ordinary least squares regression to cross-sectional data. This method assesses the *association* between symbolic attitudes and newly formed attitudes but does not assess the *causal impact* of the former on the latter. Many of the observed associations between these two types of attitudes could be spurious, resulting from common causes that were omitted from the estimated regression models, such as voting behavior in recent elections (Meier 1975), voting intentions for an upcoming election (Allsop and Weisberg 1988), retrospective evaluations of party performance (Fiorina 1981; Kinder and Kiewiet 1981), or marriage (Beck and Jennings 1975). Or the associations could reflect the impact of nonsymbolic attitudes on symbolic ones (e.g., Alt 1984; Brody 1977; Cain 1978; Clarke and Stewart 1984; Fiorina 1981; Franklin 1984; Franklin and Jackson 1983; Goldberg 1969; Jackson 1975a, 1975b; Lockerbie 1989; Luskin, McIver, and Carmines 1989; Markus 1979; Whiteley 1988; see also Campbell et al. 1960, 133–35, 165; Dobson and Meeter 1974; Dobson and St. Angelo 1975; though see Green and Palmquist 1990). Therefore, it seems best to conclude that no studies have yet demonstrated that symbolic attitudes *shape* newly formed attitudes, whereas nonsymbolic attitudes do not.

In sum, my reading of this literature suggests that the proposed distinction between symbolic and nonsymbolic political attitudes merits further, careful empirical scrutiny. Studies of the ages at which attitudes form and studies clearly identifying the *effects* of particular attitudes on others may well demonstrate that the distinction between more and less symbolic attitude object categories is a valid and useful one. It could be that symbolic attitudes are indeed formed earlier and do indeed have more impact than nonsymbolic attitudes on newly formed attitudes, even if the former are no more stable than the latter. But the findings reported here that symbolic attitudes are no more persistent than nonsymbolic attitudes call into question the assertion that attitudes toward some classes of political objects are more symbolic than attitudes toward other classes of political objects.

Reconsidering Party Identification

My findings clearly call into question the widely accepted "truism" that political party identification is the most persistent and potent political attitude (e.g., Campbell et al. 1960; Converse 1964; Converse and Markus 1979; Markus 1982). The evidence reported here clearly contradicts the claim that party identification is special among political attitudes by virtue of enhanced stability. And the present evidence challenges the claim of enhanced impact on vote choices as well. Because party identification apparently has been measured more reliably than other vote predictors, the former has had an artifactual advantage over these latter variables in predictive regression equations. Therefore, at least some of party identification's relative success at predicting candidate preferences is attributable to its superior measurement procedure. Only future research can determine whether all of its apparently enhanced potency is artifactual, but it certainly seems appropriate at this point to be cautious when ascribing to party identification an especially central role in Americans' contemporary political cognition.

This conclusion about the nature of party identification is consistent with a growing body of empirical literature on sources of change in party identification. Specifically, a number of studies have shown that citizens often adjust their party identification, and that these adjustments are guided by citizens' policy attitudes and their views of ongoing political events (e.g., Alt 1984; Brody 1977; Cain 1978; Clarke and Stewart 1984; Fiorina 1981; Franklin 1984; Franklin and Jackson 1983; Goldberg 1969; Jackson 1975a, 1975b; Lockerbie 1989; Luskin, McIver, and Carmines 1989; Markus 1979; Whiteley 1988; see also Campbell et al. 1960, 133–35, 165; Dobson and Meeter 1974; Dobson and St. Angelo 1975; though see Green and Palmquist 1990). For example, when a president implements a policy that a citizen favors, he or she is likely to identify more strongly with the president's party and less strongly with the opposing party. The portrait of party identification emerging from this research reveals that it is a more flexible attitude than has been acknowledged in the past. Thus, this work reinforces my conclusion.

It is difficult not to note a parallel between the findings and implications of the present paper in this regard and the findings of earlier papers by Bishop, Tuchfarber, and Oldendick (1978; Bishop, Oldendick, and Tuchfarber 1978), Sullivan, Piereson, and Marcus (1978), and Brunk (1978). These authors explored the validity of an important argument made in *The Changing American Voter* (Nie, Verba, and Petrocik 1979): that Americans' views on public policy issues became dramatically more constrained as the result of the 1964 presidential election campaign. This conclusion was based on comparisons of National Election Study data collected before and after 1964, and Nie, Verba, and Petrocik's (1979) interpretation of these data was widely accepted throughout the field of political science (see Kinder and Sears 1985).

As is now widely recognized, Bishop, Tuchfarber, and Oldendick (1978; Bishop, Oldendick, and Tuchfarber 1978), Sullivan, Piereson, and Marcus (1978), and Brunk (1978) all demonstrated that Nie and his colleagues' conclusion was almost certainly incorrect. Through controlled experiments that varied the wording and format of survey questions, these investigators demonstrated that the apparent increase in attitude constraint was almost certainly completely due to a change in the wording of the NES survey questions instituted in 1964. The findings reported here regarding the impact of item format on the apparent stability of political attitudes have a similar quality. Like these investigators, I have shown that a widely accepted truism in political science is most likely the result of a methodological artifact.

Democratic Theory

This conclusion has important implications for democratic theory. One view offered by some theorists asserts that democratic governments maintain stability and legitimacy because they implement policies favored by majorities of their citizens (see Dahl 1956; Pennock 1979). This presumably occurs because a majority of citizens vote for and elect representatives who share their preferences regarding government policy (a process referred to as *policy voting*). Evidence suggesting that Americans' policy attitudes are very unstable casts substantial doubt on this notion because it suggests that citizens lack policy preferences that are sufficiently crystallized to serve as bases for vote choices.

My findings provide substantial resassurance regarding the capacities of the U.S. public to perform the tasks entailed in policy voting. On average, Americans' policy preferences appear to be no less crystallized than their identifications with political parties, their ideological orientations, their attitudes toward social groups, and their feelings of political efficacy and trust. Furthermore, all of these attitudes appear to be highly stable for the U.S. electorate as a whole. Thus, according to this criterion, U.S. citizens as a group do not seem any less prepared to derive their vote choices from their policy preferences than to derive them from their party identifications. Of course, the extent of actual policy voting may not be fully predictable from levels of attitude stability alone. Nonetheless, the evidence reported here suggests that Americans as a group have at least one of the necessary criteria for policy voting: stable policy preferences (see Achen 1975 for further discussion of these issues).

Conclusion

I have no doubt that the core point of symbolic politics theory is true: some political attitudes are certainly more persistent and consequential than others. There is a great deal of empirical support for this general claim: attitudes vary in their strength, and strong attitudes are indeed more persistent and consequential than weak attitudes (see, e.g., Krosnick 1986, 1988a, 1988b; Krosnick and Abelson 1991). I suspect, however, that focusing on the attitude object may not be the most effective way to distinguish strong attitudes from weak ones. Any given political attitude is likely to be strong among some individuals and weak among others (see Krosnick 1986, 1990), and the proportion of people for whom any given attitude is strong appears not to vary substantially across the attitude object categories examined here. This discredits the notion that attitudes toward some political objects are strong among most citizens, whereas attitudes toward other political objects are strong among just a few.

An accumulating body of evidence suggests clearly that party identification is strong among some Americans, attitudes on particular policy issues are strong among others, and feelings of efficacy and trust are strong among still others. Furthermore, individuals for whom a given attitude is strong show substantial impact of that attitude on their thinking and behavior, whereas individuals for whom the attitude is weak evidence little if any impact (see, e.g., Krosnick 1990). Therefore, focusing on differences between individual citizens in terms of the strength of particular attitudes is almost certainly the more useful way to identify political attitudes that are crystallized and consequential and those that are not. In this sense, the accumulated literature attests to the heterogeneity of the U.S. electorate in terms of which political attitudes are the most persistent and potent guides of political cognition.

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