POLITICAL ALIENATION, COHORT SIZE, AND THE EASTERLIN HYPOTHESIS*

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Easterlin argues that his cohort crowding explanation of temporal variability in fertility trends applies to divorce, suicide, crime, and political alienation. Using two commonly employed survey items held to measure political alienation, we show that Easterlin's argument does not account for temporal variability in alienation between 1952 and 1980 in the United States. In addition, we find that a period basis, as distinguished from an age, cohort, or more elaborate basis, suffices to describe swings in alienation for the years under consideration. The size of young adult cohorts at each point in time, a key variable in Easterlin's argument, is correlated with the alienation of the entire adult population at each point in time. Although this result is consistent with the notion of a cohort crowding effect, it is not the one Easterlin predicted. The result is also, we argue, spurious. The issues associated with the rise of political alienation in the 1960s were primarily political and social, not economic, as would be required by a generic cohort crowding hypothesis.

INTRODUCTION

In his 1978 presidential address to the Population Association of America and his 1980 book, Richard Easterlin argues that his explanation of swings in fertility also applies to temporal variability in divorce, suicide, crime, and political alienation. We present evidence refuting this extension of Easterlin's theory to political alienation. If his theory does not explain trends in political alienation, what does? The question requires extensive analysis in its own right. As an essential step in that direction, and because there are competing perspectives on the locus of political alienation, we consider the general question of the appropriate accounting categories with which to describe temporal variability in political alienation. Our analysis indicates that a period basis, as distinguished from an age, cohort, or more elaborate basis, is most appropriate.

Easterlin argues that relative cohort size and income ambitions acquired during socialization affect the economic aspirations and feelings of well-being of young adults, which in turn affect many aspects of their lives. Relative cohort size determines the amount of competition at each stage in life, which determines the ability to earn. Members of unusually large cohorts compete within the home for limited family resources, go to crowded schools where they receive less individual attention, and face stiff competition in the job market, all of which can limit their earnings. During socialization, members of larger cohorts develop high economic aspirations based on the success of their fathers, who themselves are members of relatively smaller cohorts and the beneficiaries of reduced competition. Because of their relative economic disadvantage and an inability to cope with their thwarted ambitions, larger cohorts of young adults experience lower fertility and higher rates of unemployment, divorce, suicide, crime, and political alienation (Easterlin 1978, 1980).

In essence, Easterlin's theory consists of two independent arguments, the first concerned with the cumulative consequences of cohort size for a wide range of behaviors and the second with the social psychology of economic ambition formation, retention, and dominance over other goals. The cohort size argument is by far the more interesting of the two, not only because it provides the basis for a structural explanation of the causes of temporal variability in fertility and other behavior, but also because it can be correct even if the socialization argument is invalid. For this reason we focus on Easterlin's relative cohort size argument and on the more basic conceptual question of how to describe trends in political alienation.

REVIEW

There is evidence to support Easterlin's argument for a cohort size effect on earnings. Welch

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(1979) and Smith and Welch (1981) found that members of large cohorts are likely to be penalized throughout their careers because they must compete with a relatively large number of people at each career phase. Freeman (1979a) found that age-specific earnings of male workers are influenced by the age structure (and hence relative cohort size) of the work force.

For reproductive, social, and political behavior and attitudes, the evidence of cohort size effects is mixed. Fertility trends are more closely related to temporal variables that cut across cohorts (i.e., period variables) than to variables that distinguish between cohorts (Brass 1974; Pullum 1980; Smith 1981).[1] Yet temporal variability in divorce (Preston and McDonald 1979), suicide (O'Connell 1975; Ahlburg and Schapiro 1984) and crime rates (Schapiro and Ahlburg 1986) is suggestively related to cohort size.

With respect to political alienation, Easterlin argues that members of large cohorts suffer economically, which disillusions and alienates them from social and political affairs. For this reason, he suggests that trends in political alienation are—at least in part—a function of cohort variability in economic well-being and presents data he takes to be evidence of this. Easterlin's is only one of several possible explanations of an upward trend in alienation since the 1960s. House and Mason (1975) provide evidence to support the view that temporal variation in feelings of alienation is driven by actual historical events that affect all cohorts (e.g., the Vietnam War, the Watergate affair). Lipset and Schneider (1983) suggest that the upward trend in political alienation beginning in the 1960s reflects increased dissatisfaction of the population with a perceived lack of success of political leaders in dealing with new major issues. Wilensky suggests that alienation may be a function of the aging process: since "morale" varies with age, so might feelings of alienation.[2] According to this argument, as the population grows older, aggregate levels of alienation may increase. Thus, in addition to Easterlin's cohort explanation, there are also period- and age-based explanations.

In sum, although there is evidence of an inverse link between earnings and cohort size for men, the consequences of this are equivocal. For political alienation, there may be a cohort component, as well as other components, of temporal variation. The extension of Easterlin's argument to political alienation cannot be dismissed out of hand, but there are alternatives with which it must compete.

EASTERLIN'S EVIDENCE

Easterlin (1978, 1980) presents percentaged responses of young men to two survey items measured at several points in time.[3] The two items, known in the political alienation literature as NO SAY and COMPLEX, have been asked regularly in the Michigan National Election Surveys.[4] Easterlin notes that before 1960, young people had low alienation scores, whereas during the 1960s their scores rose dramatically. He attributes this change to the increased size of the baby boom cohorts. This is the extent of Easterlin's empirical analysis.

Although the claimed association between political alienation and cohort size may exist, Easterlin does not empirically relate cohort size to levels of alienation. In addition, he uses age groups (18–24, 25–34) that span more years than the period intervals he employs, as well as unevenly spaced period intervals, all of which would obscure cohort comparisons if an actual empirical analysis were to be carried out.

A further problem is that Easterlin's choice of alienation items mixes content domains. Whereas NO SAY taps an individual's perceptions of government responsiveness in relation to that person's positions on politically salient issues, COMPLEX reflects the individual's sense of understanding or competence regarding political matters (Mason, House, and Martin 1985). In addition, the time trends in percent agreeing with NO SAY and COMPLEX are different (Mason et al. 1985).

Finally, and most fundamentally, Easterlin's inspection of the data is plagued by a half-table fallacy. His argument requires that swings in political alienation be due specifically to the alienation of young adults. By excluding older age groups, he cannot show that members of younger age groups (representing the large baby boom cohorts) became increasingly alienated during the 1960s and 1970s, while members of older age groups (representing the small depression-era cohorts) did not.

Because Easterlin does not test his hypothe-

[1] Shapiro (1986) finds relative cohort size effects on fertility but fails to account for the prior contrary findings of the research cited above.

[2] This view was advanced by Harold Wilensky in 1981, in personal correspondence with William M. Mason.

[3] Easterlin (1978) differs slightly from Easterlin (1980). In his 1978 presidential address Easterlin presents data for males 18–24 and 25–34. In his 1980 book, which is oriented toward a lay audience, Easterlin drops the 25–34 year olds, presumably for clarity of presentation.

[4] NO SAY: People like me don't have any say about what the government does (agree/disagree). COMPLEX: Sometimes politics and government seem so complicated that a person like me can't really understand what's going on (agree/disagree).

sis, it is unclear whether or in what way cohort size is related to political alienation. In the present analysis, we use a longer time series of National Election Survey data to ask: (1) Is cohort size, measured variously, positively related to levels of alienation? (2) Are members of the large baby boom cohorts substantially more alienated than members of earlier cohorts? and (3) Is the concept of cohort relevant to the study of political alienation?

DATA

To analyze trends in political alienation we use the Michigan National Election Surveys (NESs), as did Easterlin. These surveys constitute the single most important source of survey data with which to study political alienation. During the past three decades the NESs have introduced and replicated more than twenty-five alienation items. Although these items have been used frequently in research, construction of one or more dependent variables based on them remains far from automatic for several reasons (Mason et al. 1985). (1) Although most of the items have considerable face validity as indicators of political alienation broadly construed, some have much less. (2) The items vary in their connotations and referents. (3) Although the items have been developed over time, their construction has not been closely linked to theories of political alienation. (4) The items have been clustered, and indexes computed, with labels such as "political efficacy," "political trust," "government responsiveness," and "political interest," to use NES nomenclature. However, not all of these clusters, and the indexes based on them, are unidimensional. For this reason Mason et al. (1985, p. 146) conclude that "these indexes, which include the widely used NES political efficacy and political trust scales, . . . obscure important differences in the way their constituent items relate to other alienation items and to their causes." Thus, past practice provides less guidance than might be expected. (5) The number of items has grown over time. Therefore, the longer the time series the analyst wishes to consider, the smaller the pool of available items. A further complication is that there are gaps in the time series for a number of items. That is, an item could appear in one survey, disappear from the next, and reappear in the following one.

The choice of dependent variable(s) in the present context is further complicated by a need to maintain substantial consistency with Easterlin's intent. As noted above, his strategy is to begin the time series with 1952—the starting point of the NES series—and to plot the percent in agreement with NO SAY and COMPLEX. These items are members of the NES "political efficacy" cluster that includes two other items also administered beginning in 1952—VOTING and DON'T CARE.[5]

Easterlin's choice of the "political efficacy" cluster from which to select items is appropriate. This cluster has received considerable attention (e.g., Campbell et al. 1960; Converse 1972; House and Mason 1975; McPherson et al. 1977). In treating NO SAY and COMPLEX in an undifferentiated fashion, however, Easterlin ignores the findings of past researchers concerning the four items. The principal result is that the items are not all indicators of a single dimension. More recent research (Mason et al. 1985) reaffirms this conclusion and extends its temporal validity. In fact, the only subset of the four items that does seem unidimensional consists of NO SAY and DON'T CARE. These two items are similarly responsive to individual positions on politically relevant issues, are similarly dependent on sociodemographically defined position, and have similar time patterns in percent agreement. In all of these respects COMPLEX differs, as does VOTING.[6]

On the basis of past research, and in order to maintain essential consistency with Easterlin's examination of data, we present results based on examination of data for NO SAY and DON'T CARE. Because the cumulative research of House and Mason (1975) and Mason et al. (1985) supports the unidimensionality of NO SAY and DON'T CARE, we form an index (NSDC) based on these two items in order to reduce the amount of information to be presented below.[7]

In addition to the NSDC index, we retain the COMPLEX item for separate analysis and also consider one further item—CAMPAIGN—from the NES "political interest" cluster.[8] The results obtained with these items are not presented below, since, by the criteria to be invoked in our examination of the NSDC index, the COMPLEX and CAMPAIGN findings are not even superficially consistent with Easterlin's argu-

[5] VOTING: Voting is the only way that people like me can have any say about how the government runs things (agree/disagree). DON'T CARE: I don't think public officials care much what people like me think (agree-/disagree).

[6] Because it taps more than one dimension, Mason et al. (1985) recommend that VOTING be dropped from future National Election Surveys.

[7] The NO SAY/DON'T CARE (NSDC) index = 1 if the respondent agrees with both items, = 0 if the respondent disagrees with both items, and = .5 if the respondent agrees with one of the items.

[8] CAMPAIGN: Some people don't pay much attention to the political campaigns. How about you? Would you say that you have been very much interested, somewhat interested, or not much interested in following the political campaigns this year?

ment. These results nevertheless deserve note in the present context because, although COMPLEX and CAMPAIGN differ in meaning from NO SAY and DON'T CARE as well as from each other, they appear to tap other aspects of political alienation. Moreover, these items, along with NO SAY and DON'T CARE, exhaust the list of those asked continuously in presidential election years since 1952, once items that fail the validity tests of Mason et al. (1985) are excluded from consideration.

By requiring that the time series begin with 1952 we limit the pool of alienation items. In our view it is more useful to maximize the time span of analysis than the number of alienation items. Starting with 1952 is consistent with Easterlin's choice, but more importantly, maximizes the time spanned by the analysis. Easterlin's hypothesis concerns long swings in cohort size. Thus, the ideal series length would include multiple complete "generational" cycles. However, a political alienation series of this length will long be unattainable.

For NO SAY, DON'T CARE, COMPLEX, and CAMPAIGN, data are available not only at unbroken four-year intervals for presidential election years from 1952 forward, but also at unbroken two-year intervals for congressional election years from 1964 forward. The results to be presented here are based solely on the presidential election years from 1952 to 1980. Inclusion of the congressional election years complicates the analysis with little substantive gain.

The validity of Easterlin's argument should be most apparent for men, on whom he focuses, and among men, the argument should apply most clearly to whites. For this reason, we select only white males from the NESs.[9] To avoid the previously noted "half-table" fallacy, the ages selected include those from 21 to 68 years old at the time of each survey.

We operationalize the dependent variable as the mean NSDC score for each age group in each period in order to reduce the magnitude of the data structure to be analyzed. Table 1 presents these mean alienation scores and their base *N*s. Because the span of each age group is equal to the interperiod interval, the diagonals of the table define synthetic cohorts. This format is consistent with the multiple cross-section design described by Fienberg and Mason (1985). Although the synthetic cohorts do not in general contain the same individuals from period to period, the sample size and design of the surveys permit cohort inferences.

Easterlin (1978, 1980) does not operationalize cohort size, nor does his theory imply a specific operationalization of the concept. One reasonable choice, used here, is the percentaged age-distribution of the population for each year of observation. Table 2 presents cohort size so measured for U.S. working-age white males for presidential election years between 1952 and 1980. Because it is a floating measure, this operationalization of cohort size is sensitive to shifts in the age structure over time—as seems appropriate. Other measures of cohort size can be derived from the basic information provided in Table 2, and we consider some of these alternatives in our analysis.[10]

ANALYSIS

To test Easterlin's hypothesis, we use the GLIM statistical software package (Royal Statistical Society 1985) to carry out weighted least squares regressions with the NSDC cell means as the dependent variable and the base *N*s of Table 1 as the weight variable (i.e., GLIM weight).[11] The various regressors employed, and regressions computed, are described below.

Averaging across each individual's responses to NO SAY and DON'T CARE and aggregating for each age-period combination simplify and reduce the cost of the analysis. Given the use of the NSDC index, there is little point in *not* aggregating for each age-period combination, because, apart from trivial age variability within four-year age groups, our regressors do not include variables that vary across individuals within cells (e.g., individuals' positions on paticular political issues). Thus, the only substantial aggregation in the analysis concerns

[9] For an argument that Easterlin's general economic thesis is incorrect for women, see Freeman (1979b). Among men, there are well-known socialization, labor market, and other socioeconomic differences between whites and blacks that obscure the relevance of Easterlin's thesis for the experiences of blacks. The application of his argument to nonwhite, nonblacks of either sex needs to be considered on an ethnicity-specific basis. The results of such an examination are not critical to our analysis, however, since too few nonblack, nonwhites are present in the NESs to permit separate empirical consideration of them, and their inclusion in the data base with white males can have little impact on the outcome of the investigation. Thus, substantive reasons and the limitations of the data lead us to restrict our attention to white males.

[10] The alternative measures we employ are analogous to those used by Easterlin and his associates in studies of college enrollments, crime, suicide, and socioeconomic attainment. The data structures they consider are segregated by age, as distinguished from the design we employ, which includes individuals in the full age range for which labor force participation is likely. That is why our primary choice of measure can not rest on their otherwise related work.

[11] Johnston (1984, pp. 294–95) provides a textbook illustration of the setup.

Table 1. Mean Alienation Scores on the NO SAY/DON'T CARE Index for White Males, Conditional on Age and Presidential Election Year

	Presidential Election Year							
Age	1952	1956	1960	1964	1968	1972	1976	1980
21–24	.26	.19	.17	.17	.29	.42	.53	.36
	(42)	(35)	(3)	(45)	(42)	(106)	(53)	(55)
25–28	.36	.21	.18	.23	.45	.36	.44	.52
	(65)	(47)	(20)	(55)	(48)	(116)	(72)	(53)
29–32	.25	.14	.24	.29	.35	.49	.45	.52
	(78)	(71)	(34)	(47)	(30)	(82)	(50)	(44)
33–36	.31	.21	.24	.29	.33	.40	.54	.47
	(71)	(79)	(39)	(43)	(39)	(57)	(46)	(38)
37–40	.23	.28	.18	.37	.29	.37	.43	.44
	(68)	(67)	(58)	(57)	(42)	(90)	(50)	(44)
41–44	.20	.23	.21	.32	.31	.36	.35	.37
	(61)	(62)	(46)	(72)	(42)	(64)	(47)	(31)
45–48	.22	.18	.21	.25	.42	.39	.43	.50
	(69)	(68)	(39)	(51)	(49)	(74)	(41)	(22)
49–52	.20	.21	.27	.29	.28	.41	.43	.35
	(38)	(64)	(46)	(58)	(38)	(64)	(49)	(30)
53–56	.24	.28	.16	.33	.37	.34	.48	.39
	(54)	(49)	(43)	(43)	(35)	(61)	(41)	(40)
57–60	.41	.23	.27	.37	.50	.51	.32	.42
	(35)	(33)	(32)	(46)	(34)	(66)	(34)	(32)
61–64	.26	.25	.25	.42	.52	.37	.50	.45
	(37)	(44)	(26)	(32)	(32)	(47)	(50)	(33)
65–68	.37	.21	.21	.47	.59	.56	.44	.52
	(34)	(28)	(28)	(19)	(33)	(48)	(31)	(21)

Source: National Election Surveys, Presidential Election Years, 1952–1980, Center for Political Studies, Institute for Social Research, Ann Arbor, Michigan.

Note: The means are bounded by 0 (not alienated) and 1 (most alienated). NO SAY and DON'T CARE are dichotomous items. For a given individual, only three scores (0, .5, and 1) on the summative index are possible. Base *N*s are in parentheses. Total *N* is 4,627.

the dependent variable. The use of means need not bias coefficient estimates in the linear model (Haitovsky 1973; Fienberg and Mason 1985).[12] Although R^2 values are inflated when means are used, they still indicate relative degrees of fit when alternative specifications are compared.

One consequence of the use of cell means for the dependent variable is that tests of significance based on usual (weighted) least squares formulae are incorrect even under the assumption of simple random sampling, which is not met in this instance. For this and other reasons we do not report the results of tests of significance.[13] Instead, we rest our case on the

[12] It is well known that if the individual level specification has homoscedastic errors, the weighted least squares estimator, applied to cell means and using cell *N*s as weights, is unbiased.

[13] The difficulties in statistical inference encountered in this situation include the following: the aggregation to cell means loses the ability to estimate directly the individual level error variance; the dependent variable is

Table 2. Percentaged Age Distributions for U.S. White Males (aged 21 to 68)

	Presidential Election Year							
Age	1952	1956	1960	1964	1968	1972	1976	1980
21–24	9.97	8.92	9.08	10.43	11.69	12.70	13.00	13.03
25–28	10.52	9.91	8.86	8.90	9.96	10.95	11.84	12.10
29–32	10.52	10.22	9.79	8.75	8.64	9.54	10.51	11.35
33–36	10.09	10.43	10.15	9.31	8.33	8.10	8.85	9.67
37–40	9.79	9.87	10.17	9.82	8.85	7.83	7.60	8.24
41–44	9.18	9.41	9.54	9.80	9.31	8.36	7.26	7.01
45–48	8.49	8.92	9.12	9.07	9.12	8.47	7.63	6.63
49–52	7.63	7.98	8.34	8.47	8.41	8.50	7.81	7.06
53–56	6.97	7.06	7.53	7.93	7.85	7.57	7.69	7.09
57–60	6.52	6.49	6.61	6.92	7.08	6.90	6.62	6.80
61–64	5.62	5.86	5.71	5.68	5.91	6.10	6.04	5.89
65–68	4.71	4.92	5.11	4.93	4.85	4.99	5.16	5.13
Total	100.01	99.99	100.01	100.00	100.00	100.01	100.01	100.00

Source: The following publications from Series P-25 of the U.S. Bureau of the Census were used: for 1952 and 1956, No. 311 (1965); for 1960, 1964, 1968, No. 519 (1974); for 1972, 1976, No. 917 (1983); and for 1980, No. 985 (1986).

size and direction of the patterns observed in the data; these are of decisive importance.

Given the choice of NO SAY and DON'T CARE, there are several alternatives to our use of multiple regression with the summative NSDC index. Among the more appealing are those that allow for the nominality of NO SAY and DON'T CARE and the lack of independence of each individual's responses to the two items (Koch et al. 1977; Muthén 1979; Chamberlain 1980). On the basis of previous research with NO SAY and DON'T CARE and other putative alienation items in the NESs, we judge that our simpler regression approach is adequate for present purposes (House and Mason 1975; Mason and House 1976; Mason et al. 1985).

Age-Specific Trends in Alienation

Easterlin's hypothesis is that young adult members of large cohorts will feel more politically alienated than their counterparts in small cohorts. Easterlin does not have a specific hypothesis about older adults, but presumably cohort size would be less critical for older individuals, in which case we should observe greater alienation among young adults of the baby boom cohorts and greater temporal stability in the alienation of older adults of all cohorts. We first check for this possibility in the spirit of Easterlin's (1978, 1980) graphic displays—although we include older individuals as well as younger.

Figure 1 presents trends in alienation for three age categories: 21–24, 25–32, and 33–68. The figure suggests that during the years of early adulthood for the baby boom cohorts, the young were not appreciably more alienated than older adults. Moreover, the figure suggests that decreases and increases in alienation occur for young and old alike, at the same time. Figure 1 is not conclusive, however, because it omits any measure of cohort size and because there is a hint of greater temporal variability in the alienation of the young. This can also be surmised from inspection of the rows of Table 1. Appropriate parameterization, then, may sustain the Easterlin hypothesis.

Cohort Size Measured as an Age-Period-Specific Variable

How cohort size is operationalized helps determine the way in which Easterlin's hypothesis is formalized in an estimable equation. We begin

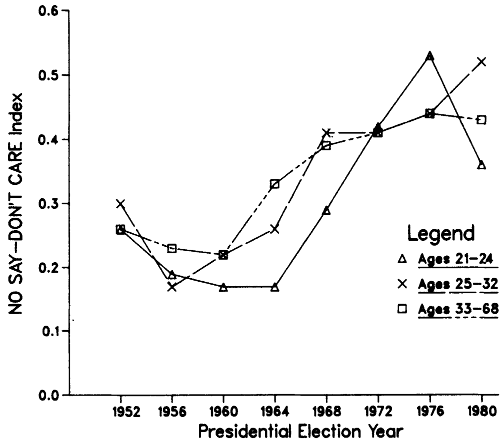

Fig. 1. Mean Alienation Scores on the NO SAY/DON'T CARE Index for White Males Conditional on Age and Presidential Election Year. *Source:* Table 1.

with the measure of cohort size specific to each age-period combination discussed earlier. With it, an appropriate representation of the Easterlin hypothesis must allow for age-cohort interaction to identify the particular effect of cohort size on the attitudes and behavior of young adults. Equation (1) includes such an interaction:

$$Y_{ij} = \alpha + \beta_i + \gamma SIZE_{ij} + \nu_{i_*} SIZE_{i_* j} + \epsilon_{ij} \qquad (1)$$

$$(i = 1, \ldots, 12; j = 1, \ldots, 8;$$
$$i_* = i \text{ for } i = 1,2,3),$$

where Y_{ij} is the mean alienation score for age i at period j; α is a constant; β_i is an age contrast for the ith age group; $SIZE_{ij}$ denotes cohort size at each point in time; $SIZE_{i_*} = SIZE_{ij}$ for i = 1,2,3, and $SIZE_{i_* j} = 0$ otherwise; the ν_{i_*} are age-specific cohort size effects for the three youngest age groups ($\nu_{i_*} = 0$ for $i > 3$); and the ϵ_{ij} are errors specific to age-period combinations. The age-specific cohort size terms are intended to distinguish "baby boom" and "baby bust" effects for young men ages 21–24, 25–28, and 29–32. Equation (1) is hierarchical, in conformity with usual arguments concerning marginality (Fox 1984). Thus, equation (1) includes all lower-order relatives of the interaction terms.[14] This means that there is a cohort size effect even for those beyond young adulthood.

nonnormal by construction; and the substance of our problem has a strong historical component. Thus, it is unclear what the formulae for the standard errors of the regression coefficients should be.

[14] From an alternative—and less productive—perspective, the age-specific size terms can be considered as main effects in the absence of lower-order age and size terms; we later consider this possibility.

To estimate equation (1), we use dummy variables for the age categories and omit the dummy for age group 65–68 (thereby setting $\beta_{12} = 0$). Regression (1) of Table 3 is the estimated form of equation (1) and is based on the data arrayed in Tables 1 and 2. If Easterlin is correct, there should be a clear age-cohort size interaction. The coefficients of regression (1) appear to be consistent with Easterlin's argument, but to gain certainty, it helps to examine the conditional age contrasts and cohort size effects. These are presented in Table 4, which shows that the age contrasts in alienation are greatest at the youngest ages for members of large cohorts (e.g., .42 > −.19; .54 > −.12; .38 > .032). The table also shows that the alienation scores of young members of large cohorts are strikingly higher than those of older respondents. The second column of Table 4 shows that the effect of cohort size on alienation, conditional on age, is positive and stronger for young ages. This follows, of course, from the pattern seen in the age contrasts, since there are only two regressor dimensions in the model. In sum, these results are consistent with Easterlin's hypothesis.

Table 3. Selected Regressions of the NO SAY/DON'T CARE Index on Age, Period, and Cohort Size for White Males

	(1)	(2)
Constant	.69	.28
Age		
21–24	−1.17	−.24
25–28	−1.20	−.23
29–32	−.53	−.10
33–36	.14	−.15
37–40	.10	−.16
41–44	.063	−.19
45–48	.063	−.17
49–52	.040	−.16
53–56	.017	−.14
57–60	.056	−.069
61–64	−.0036	−.068
65–68	.000*	.000*
Period		
1952		.000*
1956		−.053
1960		−.058
1964		.039
1968		.12
1972		.14
1976		.18
1980		.17
Cohort size	−.053	.015
Age (21–24) • Size	.12	.0013
Age (25–28) • Size	.13	.0054
Age (29–32) • Size	.070	−.0060
R^2	.31	.77
$\bar{R}^2$	.18	.70
d.f.	80	73

Source: Tables 1 and 2.

Note: $\bar{R}^2 = 1 - \{(N - 1)/(N - K)\}(1 - R^2)$, where K is the number of regressors, including the constant.

* Dummy variable omitted for purposes of estimation.

Table 4. Conditional Effects of Age and Cohort Size for Regression 1

Age	Age Contrasts	Cohort Size
21–24	(−.19, .42)	.070
25–28	(−.12, .54)	.081
29–32	(.032, .38)	.017
33–36	.14	−.053
37–40	.10	−.053
41–44	.063	−.053
45–48	.063	−.053
49–52	.040	−.053
53–56	.017	−.053
57–60	.056	−.053
61–64	−.0036	−.053
65–68	.00	−.053

Source: Table 3.

Note: Subject to the dummy variable normalization of the age polytomy, the conditional age contrasts are $\hat{\beta}_i + \hat{v}_{i_*} SIZE_{i_*j}$ for $i = i_* = 1,2,3$, and $\hat{\beta}_i$ for $i = 4, \ldots, 11$, with $\beta_{12} = 0$. For age groups 21–24 ($i_* = 1$), 25–28 ($i_* = 2$), and 29–32 ($i_* = 3$), *SIZE* is set at both 8 and 13 percent. The *SIZE* coefficients are $\hat{\gamma} + \hat{v}_{i_*}$ for $i = i_* = 1,2,3$ and $\hat{\gamma}$ for $i = 4, \ldots, 12$.

Equation (1) is by no means the only model consistent with Easterlin's hypothesis. An alternative, with which the Easterlin hypothesis must contend, is that in addition to the age and cohort (size) basis of alienation, there is a period basis. In other words, alienation may fluctuate over time and essentially uniformly across all age groups: anyone can be alienated by policies or events, not just the members of selected cohorts, or members of selected cohorts at young adult ages. Nothing in Easterlin's argument precludes this possibility, but even if it were precluded, the contrasting conceptualization of the demographic basis of fluctuations in alienation would still be worth assessing.

To consider the possibility of net period variability in alienation, we extend equation (1) with a set of period contrasts:

$$Y_{ij} = \alpha + \beta_i + \gamma SIZE_{ij} + \nu_{i_*} SIZE_{i_*j} + \delta_j + \epsilon_{ij}, \qquad (2)$$

$$(i = 1, \ldots, 12; j = 1, \ldots, 8;$$

$$i_* = i \text{ for } i = 1,2,3),$$

where δ_j is a contrast for the jth period. For estimability purposes we represent periods by a dummy variable classification and omit the 1952 dummy along with the 65–68 age dummy.

In equation (2) the period classification "accounts" for temporal variability in political alienation in a content-free way. Depending on how cohort size is operationalized, however, it can also account for this temporal variability. We explore this possibility later using a period-specific cohort size measure. In the present specification, cohort size is *ij*-specific, and period-specific sums of cohort size measured in this way are constant over periods (because the sums are always 100 percent). Therefore, $SIZE_{ij}$ is orthogonal to the period classification, and there is no conceptual overlap due to the presence of both dimensions in the same equation.[15] Equation (2) thus retains the appropriateness of equation (1) as a specification for testing Easterlin's hypothesis, and in a reasonable way "accounts" for temporal variability in alienation that cuts across all ages.

Regression (2) in Table 3 presents the coefficient estimates of equation (2). Comparison of regressions (1) and (2) in Table 3 shows that the first specification, intended to reflect Easterlin's theory, is incomplete: regression (2) fits the data well—much better than regression (1). More importantly, the conditional effects of age and cohort size are minimal in regression (2) and contradict what would be expected if Easterlin's extension of his theory to political alienation were correct. This is shown more clearly in Table 5, which parallels Table 4.

Table 5. Conditional Effects of Age and Cohort Size for Regression 2

Age	Age Contrasts	Cohort Size
21–24	(−.23, −.22)	.017
25–28	(−.19, −.16)	.021
29–32	(−.15, −.18)	.0094
33–36	−.15	.015
37–40	−.16	.015
41–44	−.19	.015
45–48	−.17	.015
49–52	−.16	.015
53–56	−.14	.015
57–60	−.069	.015
61–64	−.068	.015
65–68	.00	.015

Source: Table 3.
Note: See note to Table 4 for computation of age contrasts and cohort size effects.

From Table 5 it is evident that a substantial change in cohort size, from small (8 percent) to large (13 percent), increases alienation only slightly at ages 21–24 and 25–28, and decreases alienation slightly at ages 29–32. Thus, the age-size interaction is nil.[16] Furthermore, whether cohort size is taken to be small or large (relative to the actual distributions in Table 2), the three youngest age groups have expected mean alienation scores that are no greater than those for older age groups. As for cohort size, column 2 of Table 5 shows that for the three youngest age groups the size effect is positive, which is consistent with Easterlin's theory. Nevertheless, all of the size effects are minute and much smaller than the corresponding effects in regression (1). Moreover, the size effect for age group 29–32 is actually smaller than the size effect for all older ages.

Regression (2) contains another important result: there is a clear time pattern in the NO SAY/DON'T CARE index, even controlling for age, cohort size, and the possible age-cohort size interaction. In fact, this pattern mirrors what the eye can detect over all age groups displayed in Figure 1: decline followed by sharp increase, followed by stability. This pattern is also present, for the most part, within each age group, as Table 1 shows. In sum, controlling period virtually eliminates the age-cohort size effects. Additionally, there is a clear net time pattern in alienation.

Thus far our evaluation of Easterlin's argument has depended on a specific operationalization of cohort size. Other measures might provide results at variance with those reported here. Because the alternatives are in principle limitless, we next strengthen our test of Easterlin's argument by exploiting an analytic strategy that does not depend on operationalization of *ij*-specific cohort size. As will be shown below, we again find that the data, even when modeled with a more general formulation, do not sustain Easterlin's hypothesis. We then go on to consider various alternatives for the description of variability in political alienation, including measures of cohort size that are not *ij*-specific.

To check the results thus far obtained, we rewrite equation (2) in accounting model form (see Fienberg and Mason [1985] for the use of this terminology). This is achieved by replacing cohort size with a set of contrasts for cohort membership and including a contrast that distinguishes membership in baby boom cohorts (those of 1944–59) during young adulthood (ages 21–32) from all other age-cohort combinations. The resulting equation is

$$Y_{ij} = \alpha + \beta_i + \delta_j + \lambda_k + \eta BABY\ BOOM + \epsilon_{ij} \quad (3)$$

$$(i = 1, \ldots, 12; j = 1, \ldots, 8;$$
$$k = i + j - 1),$$

[15] The same issue is present for the epistemological connection between the period classification and the $SIZE_{i_*j}$ terms. These latter are, of course, constrained interactions between age and cell-specific cohort size, but can also be thought of as constrained age-period interactions. Since equation (2) does not include explicit-age period interactions, the presence in the equation of the age-cohort size interactions (based on the use of $SIZE_{ij}$) involves no tautology.

[16] Again, our emphasis is on patterns and magnitudes of relationships rather than statistical significance.

where λ_k is a contrast for membership in the kth cohort and *BABYBOOM* is a dummy variable for young adult members of the baby boom cohorts.

The coefficients of equation (3) are estimable subject to the usual ANOVA constraints (e.g., excluding a dummy from each classification, as in equations (1) and (2)) and one additional restriction to break the age-period-cohort identity (Fienberg and Mason 1985; Heckman and Robb 1985; Mason and Smith 1985). We constrain the coefficients of age groups 61–64 and 65–68 to be equal; this seems reasonable since we are unaware of theory or evidence concerning sharp differences in political alienation for these ages. Further, since we use dummy coding, it is convenient to effect simple linear restrictions as part of the coefficient normalization. Thus, to estimate equation (3) the coefficients of age groups 61–64 and 65–68 are set to zero, as are the coefficients for 1952 and for the cohort of 1884–87.

Further constraints on the coefficients of equation (3) are not only possible but desirable. Additional restrictions define simpler models with which the performance of equation (3) can be compared. They also reduce the inherent multicollinearity of just identified age-period-cohort (and more complex) specifications. Setting η to zero results in a just identified "additive" age-period-cohort specification. Equating the coefficients for ages 33–68 simplifies the age contrasts while retaining variability across age groups consistent with an interest in the young. Setting all of the age coefficients to zero results in a period-cohort model; setting the period coefficients to zero results in an age-cohort model; and so on. Estimates of equation (3) and its simpler relatives are presented in Table 6 as regressions (3)–(10a), and it is to these regressions that we now turn.[17]

Consider first Easterlin's hypothesized positive effect of cohort size on the political alienation of young adults. If this hypothesis is correct, young adult members of the baby boom cohorts should be especially alienated because of their large size. Equation (3) provides an accounting model specification for testing this hypothesis, and regressions (10) and (10a), which differ in the extent of the restriction on the age coefficients, provide alternative estimates. In both regressions the coefficient of the dummy variable for young adult members of the baby boom cohorts is small and negative, which contradicts Easterlin's hypothesis.[18] Furthermore, the null effect of *BABY BOOM* is reinforced by the results of regression (9), the additive age-period-cohort regression, which has essentially the same coefficients as regression (10) and fits the data identically. This finding is robust with respect to choice of constraint on the age coefficients, as is shown by comparison of regressions (9a) and (10a). Thus, using the more general test provided by the accounting framework, the original finding stands. There appears to be no age-cohort size interaction in the determination of political alienation, at least when that phenomenon is measured by the NO SAY/DON'T CARE index.

A second major conclusion to emerge from Table 6 is that only the pattern for the period coefficients is stable across the different regressions. Over time there is a slight decline followed by increase, followed by stability in the period coefficients. Further, any regression including the period classification fits the data well, even when that classification is the only regressor dimension (regression (4)).

In contrast to the period trend, the patterns of the age and cohort coefficients are unstable, and even reverse, depending on the other dimension(s) controlled. Thus, for example, in the age-period-cohort specification (regression (9)) the young are characterized as most alienated (or not distinctively alienated, as in regression (9a)). In any simpler regression, however, the young are characterized as among the least alienated. A similar reversal occurs for the cohort contrasts. When period is controlled, the youngest cohorts are least alienated. When period is not controlled, the youngest cohorts are most alienated.

No equation containing both age and cohort effects appears to be theoretically satisfactory. Regressions (7) and (7a) require a theory that postulates a monotonic increase in alienation with age, and is at the same time compatible with what is essentially a monotonic increase in alienation the more recent the birth cohort. Regressions (9) and (9a) require a theory that is

[17] These are weighted least squares regressions, as before. In a recent application of age-period-cohort modeling, Collins (1982) checked for first-order autocorrelation within age groups, using the Durbin-Watson statistic. The justification for the emphasis on age in either his case or ours is unclear. As part of our sensitivity analysis, we also checked for autocorrelation, using the within-*cohort* lagged value of NSDC as a regressor in the just identified age-period-cohort specification. A nonnegligible coefficient for the lagged NSDC index would suggest the utility of including interactions of the age, period, and cohort classifications with each other. The actual effect of the lagged NSDC index was small, and we therefore do not present results that include it as a regressor, nor do we adjust for autocorrelation or present interactive results beyond those for the age-period-cohort model (except for those pertaining to the *BABYBOOM* dummy variable).

[18] This conclusion also holds when the *BABYBOOM* dummy is redefined to exclude ages 29–32.

Table 6. Reduced, Additive, and Interactive Age-Period-Cohort Regressions of the NO SAY/DON'T CARE Index, White Males

	(3) A	(4) P	(5) C	(6) AP	(7) AC	(7a) AC	(8) PC	(9) APC	(9a) APC	(10) APC(AC)	(10a) APC(AC)
Constant	.43	.27	.37	.36	.37	.37	.37	.37	.37	.37	.37
Age											
21–24	−.092			−.12	−.50	−.19		.38	−.024	.38	−.016
25–28	−.070			−.087	−.43	−.12		.38	.017	.38	.028
29–32	−.094			−.085	−.39	−.084		.34	.023	.34	.031
33–36	−.093			−.076	−.33	.00*		.32	.00*	.31	.00*
37–40	−.11			−.10	−.33	.00*		.24	.00*	.24	.00*
41–44	−.14			−.13	−.32	.00*		.17	.00*	.17	.00*
45–48	−.13			−.11	−.27	.00*		.13	.00*	.13	.00*
49–52	−.12			−.11	−.25	.00*		.074	.00*	.073	.00*
53–56	−.11			−.11	−.21	.00*		.034	.00*	.034	.00*
57–60	−.040			−.041	−.11	.00*		.049	.00*	.049	.00*
61–64	−.051			−.054	−.076	.00*		.00*	.00*	.00*	.00*
65–68	.00*			.00*	.00*	.00*		.00*	.00*	.00*	.00*
Period											
1952		.00*		.00*			.00*	.00*	.00*	.00*	.00*
1956		−.054		−.053			−.046	−.0054	−.045	−.0052	−.044
1960		−.053		−.054			−.045	.037	−.044	.037	−.042
1964		.035		.038			.054	.18	.057	.18	.059
1968		.12		.11			.14	.31	.14	.31	.15
1972		.14		.14			.17	.38	.18	.38	.18
1976		.18		.18			.20	.45	.20	.45	.21
1980		.17		.17			.20	.49	.20	.49	.20
Cohort											
1884–1887			.00*		.00*	.00*	.00*	.00*	.00*	.00*	.00*
1888–1891			−.13		−.086	−.13	−.11	−.13	−.11	−.13	−.11
1892–1895			−.073		−.0071	−.073	−.043	−.097	−.043	−.097	−.044
1896–1899			−.095		.031	−.095	−.082	−.15	−.083	−.15	−.084
1900–1903			−.028		.11	−.028	−.042	−.15	−.044	−.15	−.046
1904–1907			−.048		.12	−.048	−.082	−.23	−.084	−.23	−.086
1908–1911			−.068		.13	−.068	−.12	−.30	−.12	−.30	−.12
1912–1915			−.023		.19	−.023	−.096	−.33	−.098	−.33	−.10
1916–1919			−.068		.19	−.068	−.14	−.41	−.14	−.41	−.14
1920–1923			−.052		.25	−.038	−.12	−.43	−.12	−.43	−.12
1924–1927			−.043		.28	−.012	−.12	−.48	−.13	−.48	−.14
1928–1931			−.063		.29	−.016	−.15	−.55	−.15	−.55	−.16
1932–1935			−.043		.32	.000	−.16	−.61	−.16	−.61	−.17
1936–1939			−.016		.35	.026	−.16	−.66	−.17	−.66	−.18
1940–1943			.063		.46	.14	−.093	−.63	−.10	−.63	−.11
1944–1947			.014		.43	.12	−.16	−.75	−.17	−.69	−.094
1948–1951			.078		.53	.22	−.11	−.72	−.11	−.65	−.015
1952–1955			.16		.62	.31	−.047	−.69	−.045	−.63	.054
1956–1959			−.0040		.50	.18	−.21	−.88	−.18	−.80	−.079
BABY BOOM										−.066	−.11
R^2	.11	.67	.23	.75	.71	.36	.77	.82	.77	.82	.78
$\bar{R}^2$	−.0079	.64	.045	.70	.59	.18	.68	.72	.68	.72	.68
d.f.	84	88	77	77	66	74	70	60	67	59	66

Source: Tables 1 and 2.

Note: BABY BOOM is a dummy variable distinguishing members of baby boom cohorts during young adulthood. It is 1 for ages 21–32 in the cohorts of 1944–59, and 0 otherwise. See note to Table 3 for additional comments.

* Dummy variable omitted for purposes of estimation.

simultaneously compatible with an inverse (or nonexistent) age-alienation relationship and a monotonic decrease in alienation the more recent the birth cohort. In the absence of well-articulated theory, the instability of the patterns of the age and cohort coefficients across regressions (7) and (9) or (7a) and (9a) renders them all implausible.

This examination of the regressions in Table 6 leaves, among the two-factor regressions, just the age-period and period-cohort regressions, whose age and cohort contrasts are also implausible. Regression (6) requires an interpretation for lower alienation among those 21–56, as compared to those 57 and older. This kind of step function has not been anticipated in the aging and other literatures, although smoother functions would not be surprising. As for regression (8), we are unaware of any reason for supposing that the oldest cohort should be most alienated. In sum, the results of regressions (6) and (8) are implausible except for the period contrasts.

Finally, among the one-way regressions, it is clear that only regression (4), for period contrasts, has potentially interpretable effects and fits the data well. In fact, by the $\bar{R}^2$ criterion, the one-way period regression provides a fit that is nearly as good as any of the other models that include terms in addition to period. No regression that excludes period has an $\bar{R}^2$ as large as that for the one-way period regression.

We have thus demonstrated the appropriateness of a period basis for the explanation of temporal variability in political alienation. What accounts for this variability? This is, of course, the major question. As noted earlier, several explanations have been proposed (Macke 1979; Lipset and Schneider 1983). Although the present context precludes general consideration of ebbs and flows in political alienation, we can address this issue to the extent that it intersects with the explanation offered by Easterlin. That there is such an intersection becomes apparent when cohort size is operationalized as a period, rather than an age-period, variable.

Cohort Size Measured as a Period-Specific Variable

Thus far cohort size has been operationalized as specific to each age-period combination. This is justifiable inasmuch as every individual is a member of a birth cohort, and each cohort has a relative size for all individuals in it, at all points in time. Nevertheless, it is possible to single out a particular age group (e.g., young adults) at a particular time, with the period-specific size of this age group being used as *the* measure of cohort size. If this is done, then cohort size becomes a period variable, and necessarily varies only across time, not across ages at a given point in time.[19]

A different interpretation of cohort size emerges when it is measured as the period-specific relative number of young adults. In particular, at any point in time the alienation of everybody is a function of the size of the young adult population (Macke 1979). To be sure, it is still possible for the young to be more or less alienated than the rest of the population as a function of their relative numbers, though we would not expect to find this given the above results using age-period-specific cohort size. Thus, with cohort size treated as period-specific, we can not only further assess the validity of the Easterlin hypothesis, but also consider whether the size of the young adult cohort at a given point in time accounts for temporal variability in alienation across age groups.

With a period-specific cohort size measure, equation (1) becomes

$$Y_{ij} = \alpha + \beta_i + \gamma SIZE_j + \nu_{i_*} SIZE_j + \epsilon_{ij} \qquad (1a)$$

$$(i = 1, \ldots, 12; j = 1, \ldots, 8;$$

$$i_* = i \text{ for } i = 1,2,3),$$

where the difference between equations (1) and (1a) resides in the difference between an *ij*-specific variable for cohort size and a *j*-specific variable.

We have estimated equation (1a) and a simpler, additive model that suppresses the ν_{i_*}'s, using four different measures of *j*-specific cohort size: the proportion aged 21–28; the proportion aged 21–32; the ratio of those aged 21–28 to those aged 29–68; and the ratio of those aged 21–32 to those aged 33–68, where all measures are based on the distributions presented in Table 2. In addition, the regressions group age categories 33–68 since no systematic pattern of age variability in alienation has thus far been detected for these categories. The four size measures lead to the same conclusion, although the fits are best when age 28 is taken as the upper age for young adulthood.[20] Table 7 presents the additive and interactive regressions based on the proportion aged 21–28.

[19] Cohort size has been operationalized as a period variable by Ahlburg, Crimmins, and Easterlin (1981), Ahlburg and Schapiro (1984), Schapiro (1986), and Schapiro and Ahlburg (1986), among others.

[20] These are again weighted least squares regressions. Using the period-specific strategy of measuring cohort size, Ahlburg and Schapiro (1984), Schapiro (1986), and Schapiro and Ahlburg (1986) adjust for first-order autocorrelation, apparently within age-groups over time. Our use of four-year period intervals suggests that first-order autocorrelation should be minimal, and our check for it within cohorts noted earlier suggests that it is in fact minimal.

Table 7. Additive and Interactive Regressions of the NO SAY/DON'T CARE Index on Age and SIZE, with SIZE as a Constant within Periods

	(11)	(12)
Constant	−.40	−.32
Age		
21–24	−.032	−.37
25–28	.00064	−.10
29–32	−.0016	−.42
33–68	.00*	.00*
Cohort size	.034	.031
Age (21–24) • Size		.015
Age (25–28) • Size		.0048
Age (29–32) • Size		.019
R^2	.61	.64
$\bar{R}^2$	.60	.61
d.f.	91	88

Source: Tables 1 and 2.

Note: Size is measured as the percentage aged 21–28, specific to each point in time. See note to Table 3 for additional comments.

* Dummy variable omitted for purposes of estimation.

The regressions in Table 7 verify the lack of the age-cohort size interaction seen earlier with age-period-specific cohort size. In particular, the interactive regression fits the data only marginally better than the additive regression. And although the age-specific size effects are positive, the conditional age effects do not support the Easterlin hypothesis. In particular, the minimum cohort size in the data is about 18 percent, the maximum about 25 percent. For these extremes, the fitted mean alienation levels are given in Table 8, which shows that, for the two youngest age groups, when size is at its maximum the level of alienation is trivially greater than the level for those aged 33–68. Moreover, the largest fitted mean is that for an age group (29–32) not included in this measure of cohort size. The fitted means for small cohort size are consistent with the Easterlin hypothesis, but the fitted means for large cohort size are critical, since they pertain to the young adult years of the baby boom cohorts. Clearly the conditional effects of age given large cohort size are too small to be consistent with the Easterlin hypothesis.

Although the results using period-specific cohort size do not support the Easterlin hypothesis, they are compatible with the view that the size of the entering adult cohort creates turbulence, and reactive alienation, throughout the age structure. This follows from the positive effect of cohort size in regression (11), and the not markedly worse fit of regression (11) compared to that of regression (4), which contains only the period classification. Thus, these results suggest that the role of the size of the entering adult cohort bears more scrutiny in the analysis of temporal variability in political alienation.

Cohort Size as a Constrained Age-Period-Specific Variable

We consider, finally, an extreme construction of Easterlin's hypothesis. Earlier, in the discussion of equation (1), we noted that the specification allows for a cohort size effect for individuals at all ages. This is also true, of course, for all subsequent specifications we have considered that include cohort size. However, it is possible to construe Easterlin's thesis as requiring that cohort size have *no* effect for those beyond young adulthood. One way to impose this interpretation on the modeling of alienation is to omit additive cohort size effects from regression specifications while retaining age-specific size effects. If this is done, then the interaction terms of equations (1), (2), and (1a) can be viewed as main effects (as noted in footnote 14 in connection with equation (1)). To check this possible interpretation of Easterlin's thesis we computed four regressions based on omitting the main effect of cohort size from equation (1a). Each included the constrained age classification of regressions (11) and (12) and one of the period-specific size measures modified to be zero for older ages. With this construction, cohort size is still *j*-specific, rather than *ij*-specific. In all four regressions the fit is slight for data of this kind (the R^2s range from .14 to .25) and worse than the fit of other regressions already examined. The effect of cohort size is positive, but the specification is obviously inferior on empirical grounds. Thus, the data provide little support for Easterlin's hypothesis as construed to require no cohort size effects beyond young adulthood.

Table 8. Fitted Means of the NO SAY/DON'T CARE Index Conditional on Age and Size, Using Regression 12

	Percent Aged 21–28	
	(18%)	(25%)
Age		
21–24	.14	.46
25–28	.21	.46
29–32	.16	.51
33–68	.23	.45

Source: Table 7.

DISCUSSION

For the presidential election years from 1952 through 1980, the results for NO SAY and DON'T CARE indicate that political alienation is unaffected by birth cohort membership, that there is no age-cohort interaction of the kind hypothesized by Easterlin, and that levels of alienation fluctuate over time for the populace as a whole. The results for COMPLEX and CAMPAIGN, not shown here, also support these conclusions.[21]

The failure of the age-cohort size interaction as an explanation of temporal variability in levels of alienation does not rule out the relative size of young adult cohorts as a potential ultimate causal agent in the alienation of the *entire* adult population, nor does it totally counter Easterlin's general thesis concerning the origins of alienation. Although they are related, we shall discuss these points separately, beginning with the revised crowding hypothesis.

Discontent due to crowding experienced by young adults might rapidly propagate alienation among older people. This hypothesis is compatible with Macke's (1979) findings and, seemingly, our own. We shall argue, however, that

[21] For CAMPAIGN there is the added result of an age differential, with those at ages 21–28 displaying least interest in following political campaigns. This is an additive effect with period controlled, and therefore not one that follows from Easterlin's hypothesis.

the hypothesis provides an incorrect explanation of the recent rise of alienation in the United States.

A result noted earlier, in comparing regressions (4) and (11), is nominally consistent with the crowding hypothesis. In particular, the cohort size of young adults accounts statistically for most of the temporal variability in the alienation of all age groups. This finding is measure-specific, however. It holds for the NO SAY/DON'T CARE index but not for COMPLEX or CAMPAIGN. Indeed, for CAMPAIGN the coefficient of period-specific cohort size is nil. Thus, the data are not highly consistent with the crowding hypothesis.

More fundamentally, the crowding hypothesis is based on relative economic deprivation as the source of discontent among young adults. But deprivation of this kind patently was not the major source of discontent fueling the civil rights movement, opposition to the Vietnam war, concerns over law and order, the women's and homosexual rights movements, or responses to the Watergate scandal. Although we believe that even a casual reading of the qualitative, historical record will support this conclusion, there are also quantitative, aggregate data consistent with it.

As early as 1964 alienation (as measured by NO SAY and DON'T CARE) was on the rise, peaking by 1976 (Figure 1 and regression (4)). If this rise had economic origins, we would expect economic difficulties to dominate perceptions of the country's problems during the 1964–76 period. In fact, the contrary has been found. Hibbs (1979) examined a Gallup Poll multiple response question of the form "What is the most important problem facing this country today?" for the period 1939–77. In these data respondents emphasized domestic political and social issues as well as international and defense issues during the period 1960–72. It was only in 1973 and the following years of the series ending in 1977 that respondents overwhelmingly mentioned economic problems. And, although the inflation rate soared in 1973 as a result of OPEC's quadrupling of oil prices, the unemployment rate hovered between 5 and 6 percent until the first quarter of 1975, when it soared. Thus, initially at least, the source of the economic problem was exogenous. Moreover, the increase of the unemployment rate was a consequence of federal policy aimed at reducing inflation and not the result of an economic environment ill equipped to accommodate a sudden flood of excess potential workers. Hence, for the period under consideration, by the time economic issues came to dominate respondents' concerns about the country, many of the major social issues associated with the turbulence of the 1960s and early 1970s had either become history (e.g., the Vietnam war) or had receded from public prominence. And, as we have noted, alienation increased markedly between 1960 and 1972 and much less thereafter in our series terminating with 1980.

In sum, juxtaposition of the NES data with the Gallup Poll data and federal statistics marshalled by Hibbs (1979) supports the view that the rise of political alienation beginning in the 1960s was primarily socially and politically induced, and not the result of economic pressures on young adult cohorts. The origins of recent economic problems do not appear to be rooted in cohort size, and in any case, when such problems became important to the populace, alienation was already high. For these reasons we consider the crowding hypothesis to be an invalid explanation of alienation and the statistical association between alienation and period-specific cohort size to be spurious.

There remains the question whether the evidence we have advanced suffices to discredit the extension of Easterlin's hypothesis to political alienation. It could be argued that the extension is partially correct, and that, because of data inadequacy, we were not able to demonstrate this. In particular, although the alienation of the entire population rose during the 1960s and 1970s, it could still have been the young who triggered the increase. That we did not find an age-cohort size (or age-cohort) interaction may thus be due to the sparseness of the alienation time series. By this argument, quadrennial or even biennial soundings of the alienation of the population are too infrequent. With shorter time intervals between surveys, the age-cohort size interaction would emerge. To refute this view requires empirical demonstration, for which data do not exist. Nevertheless, we consider this hypothesis to be implausible. As noted above, the origins of the recent rise in alienation are fundamentally noneconomic. In addition, the young adults most prominent in demonstrating their discontent were, initially at least, college students—hardly a group known to be deprived economically, even in Easterlin's sense. Nor were college students, in the mid to late 1960s, looking forward to entering the labor force with notably diminished prospects. Thus, we do not think that the Easterlin hypothesis can be resurrected by more frequent measurements of alienation.

That the young are often in the vanguard of change has been encoded by Ryder (1965) as a function of the process of cohort replacement. Social change is, of course, by no means an automatic consequence of cohort replacement. In the context of framing the conditions under which cohort replacement leads to social change, the cohort size hypotheses we have reviewed can be thought of as arising from attempts to determine whether the kind of role new cohorts play has a systematic basis or is instead governed by unquantifiable, unique historical circumstances. From this perspective, the error in these attempts is to focus on political alienation rather than on the events, conflicts, and issues providing substance to the shortfalls in aspirations that lead to alienation. Although young adults may have been prominently involved in the turbulence of the 1960s and early 1970s, our results suggest that neither cohort size variously measured, nor cohort membership itself—and therefore the process of cohort replacement—provides a fruitful basis for understanding political alienation. A birth cohort based interpretation of alienation must assume either that individual feelings of alienation are more strongly driven by formative experiences during socialization than they are by current or more recent experiences, or that specific cohorts dominate the economic, political, and social arenas in ways that affect only themselves. The former assumption misgauges the mutability of individuals and the potency of contemporaneous and recent experiences for individual perceptions, feelings, and behavior. The latter assumption appears to overstate the structural importance of cohort membership.

Despite considerable research on alienation, there is no widely accepted, comprehensive model of temporal variability in political alienation. In part this is due to the shortness of the time series with which to work, but it is due also to ambiguity about the meaning of survey items purporting to measure alienation, and to the unique, historical nature of many of the phenomena that "produce" alienation. Although variations in economic conditions can be, and are, measured routinely, there is no comparable dimensionality to political and social conditions that would allow indefinite repetition of reasonably comprehensive measures. Thus, aggregate time series models of alienation that include perceived as well as objective economic conditions but not political and social conditions as predictors of alienation (Macke 1979) are misspecified, a condition not easily rectified. A further problem is that the aggregation of an individual, dynamic model to a macro model of alienation suitable for time series analysis is yet to be accomplished. The establishment in this paper of the period basis of alienation removes at least one impediment to modeling alienation, but the principal task remains.

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