THE VARIABLE EFFECTS OF EDUCATION ON LIBERAL ATTITUDES: A COMPARATIVE-HISTORICAL ANALYSIS OF ANTI-SEMITISM USING PUBLIC OPINION SURVEY DATA*

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The effects of education on liberal values are shown to be not universal, but rather, to vary systematically cross-nationally. These effects are interpreted as a form of socialization—not as psychodynamic or class effects—and they are investigated in a comparative-historical perspective, using attitudes on anti-Semitism as the dependent variable. Selznick and Steinberg's hypothesis that education's liberalizing effects represent the influence of the "official," Enlightenment culture in the United States is expanded and applied cross-nationally in the United States, West Germany, Austria, and France. It is argued that this effect varies according to two determinants of Enlightenment culture: the length of time a country has had a liberal-democratic regime form, and the degree of religious heterogeneity in the country.

THE EFFECTS OF EDUCATION

The positive relationship between higher levels of educational attainment and social and political liberalism (especially tolerance) has been one of the most stable and consistent findings in empirical social research of contemporary American society. This relationship has been established from at least the 1950s to the 1970s for attitudes on civil liberties by Stouffer (1955), Davis (1975) and Nunn and others (1978); for democratic values by Almond and Verba (1963, 1981); for race relations by Hyman and Sheatsley (1964), Greeley and Sheatsley (1971), and Taylor et al. (1978); and for anti-Semitism by Selznick and Steinberg (1969) and Martire and Clark (1982; also see Lipset and Schneider, 1978). Others, like Lipset (1981) and Inglehart (1977), have shown that education's effect on political liberalism can also be found in a large number of Western democracies outside of North America (also see Olsen and Baden, 1974); and Sallen (1977) has found education's effect on anti-Semitism in West Germany (also see Silbermann, 1981, 1982; and Weil, 1980). In fact, this relationship has appeared to be so universal that, in a comprehensive review of the data at the end of the 1970s, Hyman and Wright (1979:60) concluded, "Many measurements on thousands of adults aged 25 to 72, drawn from 38 national sample surveys conducted from 1949 to 1975 . . . establish that education produces large and lasting good [by which they mean liberal] effects in the realm of values."

To be sure, there have been some attempts in the last decade to argue that the relationship between education and liberal attitudes is merely an artifact of the instruments used in opinion surveys, or merely reflects a different response style by the better educated. Jackman (1973) has shown that the effect of education on at least one scale of anti-Semitism was actually an effect on a response set rather than on prejudice (but its effect on the *items* remains); Jackman (1978) has argued that the better educated's apparent liberalism is superficial, extending only to abstract principles, and not to specific policies; Jackman and Muha (1984) suggest that liberal responses are actually sophisticated ideological utterances meant to justify the status quo and disguise vested interests; and Sullivan and his associates (1982) were able to account for education's effect on political tolerance with a redefined dependent variable and some fifty control variables in a

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LISREL model (cf. Weil, 1984, for a critique of their dependent variable). However, these studies tend either to suggest that the better educated merely know the "right" (liberal) answers, or else to interpose a very large number of ideological and psychological variables between education and the dependent variable without showing that some other variable is a better predictor of liberal values. The effect of these research strategies, one may argue, is either to raise the standards of liberalism sufficiently above the level where education makes a difference or else to specify in more detail the processes which lead to liberal responses—mainly cognitive, affective and ideological. Inasmuch as they have not otherwise challenged the consistency of the relationship, or suggested other major social groups which are more liberal, one wonders whether the original findings did not tap something after all. The processes and reasons which constrain the better educated to give more liberal responses may not be a admirable as once thought, but something may be better than nothing: one presumes that the recipients of apparently liberal opinions (even if somewhat hypocritical) prefer such constraints to heartfelt illiberal responses. And more pointedly, as Weiss (1984:114–17; see also Weiss, 1977–78) argues with regard to anti-Semitism in Austria, there are strong a priori reasons for believing that, to the extent that the better educated are opinion leaders or bearers and shapers of "official" ideologies, their willingness to make liberal statements in public does affect the legitimacy of those views—even if they have not internalized these views to the same extent in private.

On the other hand, there have been more studies than has generally been recognized which have simply failed to find the education–liberalism relationship—although there is now a growing awareness of this fact. Among the oldest is Stember's (1961) study, which provides a comprehensive review of then extant opinion surveys, and which demonstrates that education's effect on liberal values is *by no means* universal. Rather, using attitudes on Jews and blacks as dependent variables, Stember shows systematic variations in education's effects, although he does not provide a compelling account of the reasons for this variation. Nor are these results isolated, although many researchers have tended to dismiss them as anomalous when they occur: thus, Almond and Verba (1963) found a lack of correlation between education and certain forms of affective attachment to democratic values in West Germany and Italy; Muller et al. (1980) found a lack of correlation between education and support for the freedom of assembly in West Germany and Austria; and Marin (1979, 1983) reports that education has no effect, and sometimes a reversed effect, on certain anti-Semitic opinions in Austria—a result which has been reproduced in Czech, Polish, Rumanian, and Hungarian samples (Radio Free Europe/Radio Liberty, 1980:17–18). There has also been a growing recognition of education's variable effects in recent years, although there have not always been convincing attempts to relate the results systematically to theoretical explanations. Jackman and Muha (1984:759) demonstrate the weakness or lack of relationship over a number of items although they acknowledge that their proffered explanation goes "well beyond the data presented"; McClosky and Brill (1983:420–22) note the lack of relationship in some cases, but also do not develop a detailed explanation; and the present writer found in earlier research on political tolerance in West Germany that education had no effect on a number of important measures of tolerance there (Weil, 1981, 1982), but again, was forced to go somewhat beyond the data to present a plausible explanation.

This accumulation of methodologically straightforward findings seems sufficient for us to reject the conclusion that education has a universally liberalizing effect on values. On the other hand, its effect is sufficiently widespread for us to consider it a norm under certain circumstances. This conclusion implies two things: first, that we must reconsider the old debate concerning the proper interpretation of the education–liberalism correlation when it does appear; and second, that it is now incumbent on us to begin to investigate systematically the conditions under which the correlation emerges. The eventual results of the second investigation will, in turn, also aid our understanding of the correlation itself, and this investigation may also have implications for policy decisions on the best way for educational institutions legitimately to instill certain values.

INTERPRETATIONS OF THE EFFECTS OF EDUCATION

Two rival interpretations have been especially important in accounting for the usual effect of education: (a) a psychodynamic theory, that better-educated people tend to be personally more secure and are therefore better able to tolerate diversity (see Adorno et al., 1950; Lipset, 1981; cf. Kohn, 1969); and (b) a socialization or cognitive theory, that better-educated people are able to escape the common, folk culture, with its strains of intolerance, by their access to the less-prejudiced "enlightenment" culture (see Stember, 1961; Selznick and

Steinberg, 1969; Lipset and Raab, 1978). A third interpretation is also sometimes considered, namely that education is a measure of social status, and that the correlation merely demonstrates class interests. However, this last interpretation has not stood up well in multivariate analysis inasmuch as most investigators have found that education retains an important effect even after economic class level is taken into account. However, in their article cited above, Jackman and Muha (1984) now find that education fails to have an effect on expressions of class (racial, sexual, etc.) interests, especially after imposition of controls for class, but that it retains an important effect on ideological "general principles," even net of these controls; and they argue that the better educated simply give more adequate ideological accounts for their interests. This interpretation assumes that the "general principles" contain hidden group interests—that is, that the two dimensions are importantly related—but Jackman and Muha do not empirically demonstrate this. Without such a demonstration, their findings remain consistent with a range of earlier attempts to develop typologies among the dependent variables and to argue that education has an effect on some but not on others—for instance, Lipset's (1981) distinction between economic and social liberalism and his observation that education may have little independent effect on the former, and the present writer's attempt to develop an analogous distinction between conflict of interests and prejudice in intergroup relations, and his prediction that education can be expected to affect the latter but not the former aspects (Weil, 1983). Much research is still needed to develop such typologies, and important results can be expected here: for the present, however, one must say that Jackman and Muha's findings do not clearly support the interpretation of education's effects as class effects since (1) education has little or no effect on items which seem to measure group interests, and (2) *when* education has an effect, the dependent variable does not seem clearly to measure group (including class) interests.

There are a number of possible strategies for attempting empirically to distinguish between the remaining two interpretations and for weighing their relative influence. One, of course, is to examine carefully just what the schools teach and what the students learn—perhaps by distinguishing among the various "tracks" or majors or different types of school. Selznick and Steinberg (1969) have shown that those who majored in social sciences and humanities are less likely to be anti-Semitic than those who majored in the natural sciences or went to professional schools, and Lipset (1976) found the same pattern with regard to student protest. Selznick and Steinberg argued that this finding supports the socialization interpretation, but they also observed that certain personality types might be *predisposed* to seek out certain types of education: more liberal students might be drawn to more liberal majors; and the less authoritarian might even seek more education. In fact, Plant (1965) claims to have found just this in a study of college students. The problem of self-selection seems difficult to avoid in this research design.

There is perhaps a second problem with this research strategy. Selznick and Steinberg argue that education socializes students into the dominant or "official" cultural of a country, and they argue that the official culture of the United States has been liberal or enlightened for a long time: such a country's official culture may simply have remained too stable to provide sufficient grounds for testing these theories. A more adequate research design, it may be suggested, would be a comparative and historical study that compares countries with liberal official cultures to countries with illiberal official cultures: (a) the variation in what is learned in schools may well be greater between countries over time than within countries at one time; and (b) it is unlikely that very many students will select the country or historical period whose characteristics best fit their psychological predispositions (although emigration may occur for reasons which are less strictly psychological).

Now, a number of the studies mentioned earlier (Almond and Verba, 1963; Muller et al., 1980; Weil 1981, 1982) *do* show that the correlation between education and tolerance varies systematically among such countries, and perhaps also in the same countries over time (Stember, 1961). This being the case, it would seem that the psychodynamic interpretation can only be maintained by being subsumed under the socialization interpretation, which posits cross-cultural variation. If this is true, the next step is to investigate the social and historical factors that influence the values adopted by the better educated.

THE EFFECTS OF EDUCATION ON ANTI-SEMITISM

The remainder of this paper will focus on the effects of education on one measure of (il)liberalism, anti-Semitism, and further, on *political* anti-Semitism, which has been the most important form in this century.[1] The re-

[1] It is possible to distinguish here between two principle forms, traditional and modern anti-Semitism. The older, traditional form stemmed from cer-

sults summarized in the previous sections suggest a way in which we might account for the variable effect of education on anti-Semitism. In West Germany and Austria, both of which were fascist before 1945, education has no effect, little effect, or only recent effect on certain measures of political tolerance and anti-Semitism. These findings, if confirmed, should indicate something about the effect of a regime type or political culture on the values transmitted by the educational system. In order for education to instill "enlightened" outlooks, one could argue, such orientations must have been institutionalized for some time in the educational system; and if educational institutions tend to transmit the dominant political cultural values of the regime, then "enlightened" views among the better educated are at least partly dependent on there having been a liberal regime in place for some time.

A similar argument might be made regarding cultural pluralism. Halpern (1956), for instance, has argued that certain elements of religious and ethnic heterogeneity in the American population contributed to lower levels of anti-Semitism here than in European countries, and Lipset (1963) has similarly argued that the plurality of nonestablished churches in the United States helped support other forms of liberalism. We need not decide here whether or not such heterogeneity directly promotes tolerance, but students of conflict resolution in plural societies point out that it is generally the elites who must take the lead if open conflict is to be avoided and good relations are to be maintained (e.g., Lijphart, 1977). Thus, it seems reasonable to hypothesize that in heterogeneous societies in which the communal leaders try to promote accommodation among population segments, educational institutions are likely to attempt to instill tolerant attitudes, which will tend to counteract directly experienced intergroup tensions to some extent and thereby differentiate the better from the less well educated.[2] In homogeneous societies, since there are not the same intergroup tensions to be counteracted, exposure to schooling may have little effect on levels of tolerance.

Suppose we consider four countries and classify them crudely as follows: postwar America has a long tradition of liberal democratic government and a religiously heterogeneous population; West Germany has a short liberal democratic tradition, but a religiously heterogeneous population with fairly good relations among denominations;[3] France is an example of a country with a religiously homogeneous population (secularism aside), but one with a long history of liberal democratic regimes—with the last major break precipitated from outside; and Austria has a religiously homogeneous population and a short liberal democratic history. If regime type and religious heterogeneity are hypothesized to have independent effects on the education–tolerance correlation, then the strength of this correlation can be predicted as shown in Figure 1: in America, which is liberal democratic and heterogeneous, the correlation should be strong; in Austria, which is homogeneous and has a short history of liberal democracy, the correlation should be weak; in West Germany and France, which are mixed cases, the correlation should be moderate.

ANALYSIS

Data

The data used to analyze these hypotheses come from national opinion surveys in the four countries under consideration, and several steps were taken in order to assure the most comprehensive possible location and acquisition of data within certain cost and travel limitations (e.g., it was not possible to travel to Europe for this project) and within roughly a year's time. First, major published sources of survey results were searched for relevant items: this included compilations from survey organizations (the Gallup American and International collections, the *Harris National Surveys 1963–1976*, the French *Sondages*, the German *Allensbach Jahrbuecher fuer Demoskopie*), other compilations of survey results (Cantrill, 1951; Hastings et al. (eds.), 1975; *Polls;* the *Index to International Public Opinion*; review articles in *Public Opinion Quarterly*), and of course an extensive search of published literature, including important previous studies (e.g., Stember, 1961, 1966; Erskine, 1966; Selznick and Steinberg, 1969; Lipset and Schneider, 1978; Martire and Clark, 1982; Rosenfeld, 1982; Sallen, 1977; Silberman,

tain religious and folk stereotypes, and while it had political aspects, they were often subordinate. Modern anti-Semitism is more characteristically political, being bound up in the development of the nation-state and the concomitant enfranchisement or mobilization of the mass population—but it is also affected in important ways by the legacy of traditional anti-Semitism. For a fuller account of this distinction, see Weil (1983).

[2] When accommodation breaks down among culturally different population segments, of course, one could hypothesize the reverse, that the elites and better educated would lead the way in intolerance.

[3] This rapprochement is often remarked on as a constrast to the interfaith hostility of the pre-Nazi period, and is viewed as one of the bases for West Germany's moderate party system (see, e.g., Lepsius, 1974).

Figure 1. Determinants of Education's Effects on Anti-Semitic Attitudes[a]

		Religious Heterogeneity: High	Religious Heterogeneity: Low
Liberal-Democratic History	Long	Strong (USA)	Medium (France)
	Short	Medium (Germany)	Weak (Austria)

[a] Cell entries show (a) posited strength of the education–tolerance correlation, and (b) an example of a country that fits this pattern.

1982; Bichlbauer and Gehmacher, 1972; Marin, 1979, 1983; Bensimon and Verdes-Leroux, 1970).

Second, the Roper Center and the Harris archive at the University of North Carolina were commissioned to search their holdings for relevant items, and the archive of the American Jewish Committee in New York, which contains some survey results, was searched by the investigator. Finally, a number of survey organizations were contacted directly by mail, usually with questions about specific surveys which had already been located, but in a number of cases, on the basis of incomplete information.[4]

On the basis of this search, roughly 400 to 500 data points (i.e., questions asked at given times and places) were located that bore on the hypotheses—more when the major studies are included. Of these, about twenty questions were found (some with additional variations on the theme) that had been asked in comparable form in more than one country. Altogether, about 100 cross-tabulations of the dependent variables by education were available, but only for seven questions were such cross-tabulations possible in at least three of the four countries. The texts of six of these questions are given in Appendix A; the seventh, willingness to marry a Jew, was considered too uncertain a measure of anti-Semitism to include in the main analyses.[5]

An effort was made, within the constraints mentioned earlier, to obtain as many original data sets as possible for secondary analysis. Several of these data sets were available from survey archives, and some only directly from the survey organizations themselves; but while a number of the latter organizations did send tabulated reports on survey results, in the end, none of the original data were obtained from them.[6] Finally, many of the data sets obtained had not yet been prepared for secondary analysis and were initially unreadable by common analysis programs: these data were cleaned (multiple card punches spread, non-numeric codes recoded, etc.) and systems files were prepared.

Table 1 lists the sources of data used for

[4] It is not usually feasible simply to write to commercial survey organizations with general requests for data, since most are not organized to provide extensive searches of their archives for scholarly research; and in the present case, such a search would probably have been too costly, if agreed to at all.

[5] Attitudes toward intermarriage do not necessarily reflect hostility or prejudice toward the object group. Indeed, the major studies of anti-Semitism do not include this variable in their central scales (Selznick and Steinberg, 1969; Martire and Clark, 1982; Sallen, 1977).

[6] Again, many commercial organizations are not organized to act as archives: proprietary information must sometimes be removed, and in other cases the data have been lost or damaged or are in delicate condition on old punch cards. For these reasons, the importance of survey archives cannot be stressed too highly.

Table 1. Survey Characteristics

Survey	Source	N	Sample
	Austria		
SWS, FB 84, 1968	Marin, 1983	825	NA
IMAS, Oct. 1973	Marin, 1983	962	b
DR. FESSEL, 1976	Dr. Fessel	1500	NA
SWS, FB 210/B 170, 1982	Marin, 1983	2142	NA
	West Germany		
IfD 1041, 1960*	Roper Center Survey Archive	1868	b
IfD 2014, 1966*	Roper Center Survey Archive	1955	b
IfD 2052, 1969*	Roper Center Survey Archive	1708	b
ZA 838, 1974*	Zentralarchiv, Cologne	2084	b
	France		
IFOP, Sept. 1966	Bensimon and Verdes-Leroux, 1970; Sadoun, 1967	2527	a
IFOP 991, 1969*	Roper Center Survey Archive	1769	a
	USA		
AIPO 622, 1959*	Roper Center Survey Archive	1527	a
NORC 760, 1964*	ICPSR	1913	a
AIPO 714, 1965	Hyman and Wright, 1979:114	620	a
AIPO 776, 1969*	Roper Center Survey Archive	1634	a
Yankelovich 8225, 1981*	Roper Center Survey Archive	1041	a

Notes:
SWS = Sozialwissenschaftlichen Studiengesellschaft;
IMAS = Institut fuer MARKT-und-Sozialanalysen;
IfD = Institut fuer Demoskopie, Allensbach;
IFOP = Institut Francais d'Opinion Publique;
NORC = National Opinion Research Center;
AIPO = American Institute for Public Opinion (Gallup);
ICPSR = Inter-University Consortium for Political and Social Research
* Indicates survey obtained for secondary analysis.
a = stratified probability sample; b = quota sample.

analysis, together with selected sampling characteristics of the surveys. All surveys were conducted as in-person interviews, and the universes are the adult national populations. Some of the surveys were quota samples, with the most common quotas being age and sex, but the stratified probability samples also tend to contain similar quotas at the block level. While the organizations which originally collected the data have established records of accurately predicting verifiable results (e.g., elections), certain allowances were made in the log-linear analyses, which follow, for these and other possible variations in sampling designs. Fienberg (1978) has shown that sampling quotas do not affect log-linear models which include the quotas as predictor variables: these were included when possible. Also, the chi-square statistics in the confidence tests were divided by two to allow for clustering in the sample designs (see Davis, 1975): this practice also tends to give more conservative estimates of the substantive significance of marginal effects when the use of multiple data sets increases the sample size—and thereby the statistical significance—as is the case here.

All dependent variables were dichotomized into anti-Semitic and non-anti-Semitic attitudes, with "Don't Know" responses included with the latter.[7] Other major variables used were coded as follows. Education was broken into three categories—in the United States by level of certification: less than a high school degree; a high school degree; and a college degree or higher. Analogous categories were sought for the European cases: in West Germany, less than a Mittlere Reife, a Mittlere Reife, and Abitur or university; in Austria, less than "abgeschlossene Schulbildung," an abgeschlossene Schulbildung, and Matura or university; and in France, primaire or less, primaire superieur or technique commercial, and secondaire or superieur. Cohort was defined with respect to the year in which the respondent became 21, an arbitrary age of political or social maturity, and meaningful historical periods were sought to create roughly fifteen- or twenty-year cohorts: before 1918; 1919 to 1933; 1934 to 1945; 1946 to 1964; and 1965 and after.

[7] Nonreponse is, of course, negatively correlated with educational level, but it was felt that an explicitly illiberal response was substantively significant as against liberal or noncommittal responses.

Validation of Anti-Semitism Items

Dimensional analyses were performed on the dependent variables in order to assess their face validity. However, in studies like the present one, it is generally not possible to perform such analyses with a degree of comparability that is ideally desirable—for if it was difficult to find comparable individual questions, it was even more difficult to find a *set* of questions to compare cross-nationally. Indeed, no data at all were obtainable for secondary analysis for Austria, and there were hardly any other questions that seemed to measure liberal values in the single French survey obtained. And while extensive surveys were available for the United States and West Germany, there were important lacunae in the sets of questions from survey to survey.

Two main questions are addressed in this section: first, whether the items selected for further analysis are good representative measures of a range of anti-Semitic beliefs; and second, whether these anti-Semitism items relate to other measures of liberal values. A number of principal-components analyses were conducted, and a selection of results is shown in Table 2.

Four of the items listed in Appendix A were available on all three major American and West German surveys, and these were factor analyzed together with a fifth, whether Jews have too much power in business (results not shown, but available from the author on request). Using criteria of a scree test and the eigenvalues, it appeared that it was possible to limit the dimensionality to one factor. However, in all three surveys, the question of whether Jews are being punished for rejecting Jesus loaded most weakly, and it loaded very strongly on a second factor—which was always just above or just below an eigenvalue of 1.0. It would appear from this result that a distinction would be warranted between certain religious or traditional forms of anti-Semitism and more modern or political forms (see above, footnote 1).

In order to test this possibility further, a question on willingness to vote for a Jew and three additional scales were added to the analysis (liberal democratic values, xenophobic or ethnocentric orientations, and Christian traditionalism: see Appendix B for scale construction), and the results are shown in Table 2. This analysis is not as strictly comparable between countries as the first, because two of the variables were not available in the West German survey and because the scales are not constructed with identical items in the two countries. However, the results are consistent with each other and with the first factor analysis: a Christian religious dimension and two political factors always appear. The fomer dimension is defined by the Christian anti-Semitism variable and (in the U.S.) the Christian-Traditionalism scale; but the two political factors do not appear to be fully distinct since there is a good deal of overlap in variable loadings, and in the United States, some shifting of items between them over time. Thus, the distinction between traditional-religious and modern-political forms of anti-Semitism is further supported in the present analysis. The non-anti-Semitism scales provide additional validation at this point, for they load appropriately on their respective factors: religious illiberalism on the religious anti-Semitism factor, and xenophobic and political illiberalism on a political anti-Semitism factor.

Of the political anti-Semitism items, the question whether Jews have too much power loads least in common with the liberal democratic and ethnocentric scales in the 1974 West German survey just discussed. However, this question was also available on a 1969 survey.[8] This survey was taken at the height of postwar neofascist electoral success in West Germany, and it contains a number of items measuring attitudes toward past and present Nazism, nationalism, ethnocentrism, and willingness to repress political opposition (the Communist party, in this case). Several factor analyses were performed on this survey (not shown in Table 2). In these analyses, the anti-Semitism item loads most strongly with the questions on Nazism and neofascism, and also to an extent with the opinion that foreign "guest" workers are harmful; the item on banning the Communist party loads moderately with anti-Semitism in some analyses, but not in others; and the mild *voelkisch* nationalism always constitutes a factor separate from anti-Semitism. In these analyses, then, the opinion that Jews have too much power *is* related to certain forms of political illiberalism and ethnocentrism.

Two further dimensional analyses were possible with the data sets available, but in both cases the range of items which seemed to measure additional forms of liberalism was limited to questions parallel to the anti-Semitism items. Thus, the Gallup organization has for many years asked about willingness to vote for presidential candidates in the respondents' own party: three principal-components analyses are shown in Table 2 for 1959 and 1969 surveys

[8] The tripartite categorization of the responses on this variable on the 1969 survey made it unsuitable for comparisons with the dichotomized variables in the later log-linear models, but it did not affect its use in factor analyses.

Table 2. Validation of Anti-Semitism Items: Principal-Components Analyses

	U.S. 1964			U.S. 1981			Germany 1974		
	1	2	3	1	2	3	1	2	3
Jews Too Much Power	.77	.09	−.12	.87	.04	.02	.90	.00	.11
Jews Cause Trouble	.76	.03	.09	.40	.48	−.10	.44	.58	.16
Jews Less National	.62	.12	.36	.55	.34	.17	.37	.62	−.22
Not Vote for Jew	.14	.93	.02	.25	.64	−.04	—	—	—
Liberal Democratic Values	.12	.37	.48	−.14	.71	.35	−.05	.78	.24
Xenophobia Scale	.50	.18	.33	.18	.63	.19	−.03	.81	.10
Christian Traditionalism	−.04	.07	.77	−.04	.26	.73	—	—	—
Jews Rejected Jesus	.22	−.09	.67	.15	−.03	.83	.12	.14	.94
Eigenvalue	2.45	.89	1.15	.84	2.38	1.26	.99	2.32	.88

	USA 1959				USA 1969	
	1	2	1	2	1	2
Vote for a Jew	.72	−.31	.81	.07	.78	.00
Vote for a Catholic	.67	.08	.71	−.06	.79	−.05
Vote for a Black	.67	−.03	.67	−.22	.70	−.16
Vote for an Atheist	.55	.17	—	—	—	—
Vote for a Woman	.36	.54	.07	−.98	.09	−.99
Vote for a Baptist	.20	−.79	—	—	—	—
Eigenvalue	1.88	1.05	1.68	.96	1.79	.94

	France 1969	
	1	2
Unions Too Much Power	.78	−.04
Communists Too Much Power	.72	.15
Catholics Too Much Power	−.52	.01
Jews Too Much Power	.12	.67
Bankers and Financiers Too Much Power	.05	.64
Freemasons Too Much Power	−.08	.60
Eigenvalue	1.49	1.15

Sources: See Table 1.

which compare willingness to vote for a Jew to willingness to vote for a Catholic, a black, an atheist, a woman, and a Baptist (two of these latter items were only available in the 1959 survey). The results show that, while it is sometimes possible to compress all items into one factor, a second factor centering on the female and Baptist candidates is always clearly defined (the criteria for dimensionality are similar to the first set of analyses described). The anti-Semitism item, on the other hand, loads with the items for Catholics, blacks, and atheists. One might argue that a distinction emerges here between groups which have often been considered peripheral or disadvantaged minorities and groups which have often not been considered separate from the broad majority.[9]

The final principal-components analysis was conducted on a French survey from 1969 for items concerning which groups have too much power in society. The factor structure here is quite clear: the anti-Semitism item loads with items on bankers and financiers and freemasons, while the other factor is defined by items on unions, Communists, and (negatively) Catholics. One could argue that the factor containing the anti-Semitism item concerns groups that are perceived as exerting their influence behind the scenes and, indeed, have often been the subjects of conspiracy theories (see Lipset and Raab, 1979, for a similar interpretation for the United States). The latter factor, on the other hand, might be interpreted as a percep-

[9] Judging from social movements since the achievement of female suffrage, it is perhaps only in the last two decades (i.e., since the women's movement has gained momentum in pressing for greater equality in the public sphere) that women have more widely been considered underprivileged and disadvantaged.

tion of the open opposition between the French establishment, as represented by the Catholics, and the working class, as represented by the unions and the Communists.

The results so far would seem to indicate (1) that the anti-Semitism items tested here do, in fact, form a cluster of opinions; but that (2) it is possible to make an empirical as well as theoretical distinction between political aspects and traditional religious aspects of anti-Semitism; and (3) in these analyses, questions on anti-Semitism are moderately strongly related to other measures of political, ethnic, and religious illiberalism.

One final test can be made of this last point simply by reviewing the correlations between the anti-Semitism items used and the other available measures of illiberalism. A summary of such correlations is shown in Table 3. The other measures of illiberalism used here include the three scales used above for the United States; an F-scale, an anti-black scale, and an anomie scale for the 1964 American survey; opinions on the rights of blacks (and minorities in general) and homosexuals, prayer in school, and a self-classification of political ideology for the 1981 American survey; the two scales used in Table 2 for the 1974 West German survey; and the items used in Table 2 for the 1969 West German survey, plus support for a grand coalition form of government, which has been interpreted as a deviation from full adherence to liberal democratic values (Dahrendorf, 1969; Lijphart, 1977). As indicated at the beginning of this section, no other independent measures of illiberalism were available in French or Austrian surveys except the French items in Table 2 discussed above. The results in Table 3 show the same moderately strong relationships we saw in the principal-components analyses: the mean gamma for all tests is .32, the mean gammas for each anti-Semitism item in each country vary between .19 and .53, and the mean gammas for each anti-Semitism item over all countries vary between .30 and .39.

Taken together, these tests provide fairly strong support for the validity of the items listed in Appendix A as measures of anti-Semitism, and moderate support for the interpretation that these particular measures of anti-Semitism are related to other forms of illiberalism.

Cross-National Comparisons

In order to test the central hypothesis that the effect of education on liberal attitudes varies systematically across countries, a series of bivariate and multivariate tables were computed for the dependent variables listed in Appendix A, and the multivariate tables were then analyzed with log-linear models.

Correlations (gammas) for the bivariate tables are given in Table 4. A rapid inspection of this table shows that the education–tolerance correlation does seem to vary by country for several of the measures, but not for others. This variation is smallest for the measure of religious anti-Semitism (column E), a variable which appeared in the previous section to constitute a separate dimension from the other measures of anti-Semitism. Later log-linear analyses (not shown) indicated that the variation was statistically insignificant. For the opinion that one can tell a Jew by looking (column F), the pattern of variation is not entirely in the direction predicted, for the negative effect of education is not smallest in Austria, but rather in France. Here, however, the variation is perhaps caused by a different question in the outlying case: to an open-ended question about the identity of the Jews, in France the better educated responded more than average that they are a separate race—but also that they are another religion or men like any others. These two questions will not be analyzed further.

Education's effects on the remaining mea-

Table 3. Gammas of Anti-Semitism Items with other Measures of Liberalism[a]

Mean Gammas for: Country, Year (Number of Items)	Anti-Semitism Items					
	Too Much Power	Not Loyal	Cause Trouble	Other Race	Reject Jesus	Not Vote
U.S., 1984 (8)	.48	.51	.53	.26	.39	.25
U.S., 1981 (13)	.19	.27	.21	—	.25	.39
West Germany, 1974 (2)	.25	.33	.44	.44	.27	—
West Germany, 1969 (5)	.31	—	—	—	—	—
Mean Gamma, Over Country	.31	.37	.39	.35	.30	.32
Mean Gamma, Over Items	.30	.36	.34	.30	.30	.34
Standard Deviation of Gammas Over All Items	.20	.15	.23	.09	.19	.16

[a] See text for content of other measures of liberalism.
Sources: see Table 1.

Table 4. Education's Effects on Anti-Semitism: Cross-National Comparisons[a]

		A	B	C	D	E	F
USA	1959		−0.32				
			(−0.34)				
	1964	−0.47		−0.58	−0.56	−0.39	−0.14
		(−0.51)				(−0.41)	
	1965		−0.40				
	1969		−0.60				
			(−0.60)				
	1981	−0.15		−0.32	−0.33	−0.35	
		(−0.20)				(−0.38)	
GERMANY	1960		−0.05				
			(−0.17)				
	1966	0.04					
		(0.04)					
	1974	−0.23		−0.19	−0.27	−0.39	−0.20
		(−0.21)				(−0.34)	
FRANCE	1966		−0.18	−0.06		−0.46	0.34
			(−0.18)		NA		
	1969	0.10					
AUSTRIA	1968			−0.09			
	1973		NA			NA	−0.03
	1976	0.20					
	1982				−0.11		
S.E. GERMANY	1960		−0.24				

Columns:

A. Do Jews have too much power?
B. Would vote for Jew in own party.
C. Questionable Jewish nationality.
D. Do Jews stir up trouble?
E. Religious anti-Semitism.
F. Can tell a Jew by looking (France: Jews are a separate race).

[a] Coefficients are gammas, based on tables with the Don't Know responses included. When available, gammas are also given (in parentheses) for tables with the Don't Know responses excluded. High education and strong anti-Semitism are coded positive, so a *negative* correlation indicates greater tolerance among the better educated.

sures of anti-Semitism in Table 4 (columns A–D), on the other hand, seem broadly to conform to the hypothesized pattern. To be sure, there are variations among these measures as well, and one might think of a number of substantive and historical reasons for this. However, the dimensional analyses in the last section provide rather narrow grounds for making substantive distinctions—at most that the question of voting is somewhat distinct from the other three items (perhaps a slightly purer measure of prejudice). Likewise, while the American data seem to suggest that education's effect rose to a strong point between the mid-1960s and the mid-1970s and then fell again (the trends shown are statistically significant), there were insufficient non-American data to investigate historical change in the other countries, and there was little change in some additional American variables not shown here. Therefore, the possible historical variation in education's effects also will not be explored in further depth here, and since most comparative data center on the period from the mid-1960s to the mid-1970s, further tests also concentrate on these years. On the whole, then, questions of historical change or substantive differences among these four measures of anti-Semitism must be left open for the present in order to concentrate on the central point that education's effect seems to vary cross-nationally as originally predicted.[10]

Log-linear models were used to test hypotheses about the anti-Semitism items shown in the first four columns (A–D) of Table 4, and to investigate the effects of additional variables on the correlation shown there: a summary of these results is given in Table 5. The primary

[10] Some of these questions were, in fact, discussed in more detail in an earlier draft of this article: a distinction between measures of intergroup conflict and measures of prejudice was further developed, and an account was suggested explaining the historical variation in the American data. However, comments of anonymous reviewers and the editor persuaded the author that he could not make his case within the bounds of a single article.

Table 5. Analysis of Cross-National Variation in Education's Effects on Political Anti-Semitism: Conditional Significance (p-levels) of Effects in Final Log-Linear Models

	A. Jews too much power[a]	B. Would not vote for Jew[a]	C. Jews less National[a]	D. Jews cause trouble[a]
1. Variation of correlation by country	.01	.01	.01	.01
2. Country variation when controlled by:				
a. Cohort	.01	.01	.05	.10
b. Size of Place	.01	.01	.01	.05
c. Church Attendance	—	.01	—	—
d. Liberal Democratic attitudes	.10	—	.01	NS
3. Difference of variation by country to variation by 2 variables, "culture" and "regime form"	NS	NS/NS	NS	NS
4. Variation of correlation:				
a. by culture	.01	NS/.01	.01	NS
b. by regime form	.10	NS/.01	.05	.01
5. Countries regrouped[b] for religious homogeneity:				
a. Variation by country	.01	NS/.01	—	.01
b. Difference of variation by country to variation by 2 variables, "culture" and "regime form"	NS	NS/.01	—	NS
c. Variation of correlation:				
—by culture	.01	NS/NS	—	NS
—by regime form	.05	NS/.10	—	.02

Notes:

"NS" indicates effect marginally not significant at appropriate modelling point; Dashes (—) indicate data not available.

[a] Tests in each column use data shown in Tables 1 and 4: U.S. 1964 or 1959–1969, West Germany 1974 or 1960, and France and Austria as indicated in Table 4. Exceptions are:

A. Tests in lines 2.a and 2.b for U.S. 1964, West Germany 1974, and France 1969 only (for comparison, p-level for these countries is .01); tests in line 2.d for U.S. 1964 and West Germany 1974 only (for comparison, p-level for these countries is .01).

B. Tests in this column for U.S. 1959 and 1969, West Germany 1960, and France 1966 only; tests in lines 2.b and 2.c for U.S. 1959 and 1969 and West Germany 1960 only (for comparison, p-level for these countries is .01). Single entry in a cell indicates tests using both U.S. 1959 and U.S. 1969; double entry indicates separate tests using U.S. 1959 and 1969, respectively.

C. Tests in lines 2.a, 2.b, and 2.d for U.S. 1964 and West Germany 1974 only (for comparison, p-level for these countries is .01).

D. Tests in lines 2.a, 2.b, and 2.d for U.S. 1964 and West Germany 1974 only (for comparison, p-level for these countries is .05).

[b] Countries "regrouped" for religious homogeneity in panel 5: religiously homogeneous regions of the U.S. (South, West North Central, and Mountain regions) grouped with France; religiously homogeneous regions of West Germany (Bavaria and Baden-Wuerttemberg) grouped with (or in place of) Austria.

hypothesis, it will be recalled, is that variation is due to the length of time a country has been liberal democratic and to the degree of religious heterogeneity. The first panel of Table 5 confirms that the differences in the correlations across countries are indeed statistically significant (at .01), and the effect parameters and percentages from the fitted models (not shown; available from the author on request) suggest that the differences are substantively as well as statistically significant and in the directions hypothesized.

The scale of liberal democratic attitudes discussed in the previous section was added to the models, and the second panel of Table 5 shows that this variable does account for some of the variation in the United States and West Germany (the only two countries for which this test was possible).[11] One might also hypothesize that the effect of liberal democratic history can be accounted for by cohort variation, since the younger cohorts, which were socialized after the regime change in the newer liberal democracies, were also better educated. Again, there is some support for this hypothesis, but it is weaker, because cohort accounts

[11] Recall that the scales shown here are not identical (see Appendix B), so further data would be desirable to reconfirm this finding.

for the variation in education's effects for only two of the four items, and then only partially. Finally, the possibility was tested that the second posited dimension—religious heterogeneity—would be better characterized as traditionalism. One might hypothesize, for instance, that since the French and Austrian populations are somewhat more rural than the American and German (U.S. Bureau of the Census, 1975: Series D 199, 213; Mitchell, 1978: Table B1), education in a traditional setting might do less to instill liberal values. Church attendance and residence in the countryside or small towns were entered into the models as indicators of traditionalism, but as shown in panel 2 of Table 5, they have virtually no impact on the variation in education's effect. On the other hand, the four anti-Semitism items being tested might not be the most appropriate to evaluate a traditionalism dimension, since earlier analyses indicated that they seem to measure more modern or political than traditional or religious aspects.

It was not possible with the data available to enter additional intervening variables, but the main hypothesis was tested further by coding each country according to the two factors specified and treating each as a cell in a two-by-two table of religious heterogeneity and regime form. The log-linear models summarized in the third panel of Table 5 show that this decomposition is possible for all of the anti-Semitism items.[12] This result supports the hypothesis, and it raises the question whether the observed variation by country is due to only one of the two variables isolated. The tests reported in the fourth panel of Table 5 show that the education–tolerance correlation varies by both factors—which is to say, it is different in the United States and West Germany than it is in France and Austria, and that it is different in the United States and France than it is in West Germany and Austria. The strength of these factors varies across the anti-Semitism items, but it is not clear whether this is systematic.

Finally, as a counterpart to the test of traditionalism indicators mentioned earlier, an attempt was made to strengthen the measure of religious heterogeneity in order to reconfirm the interpretation of this dimension. To do this, the religiously homogeneous regions of West Germany (Bavaria and Baden-Wuerttemberg) were coded together with Austria—or in place of it when Austrian data were missing—and the religiously homogeneous regions of America (the South and the non-Pacific West) were coded with France. Tests of these recoded data are shown in the fifth panel of Table 5, but the results differ very little from those using the original data. France and Austria are, of course, Catholic countries, and in order to distinguish the effect of Catholic homogeneity alone, similar models were evaluated in which the homogeneous (Protestant) American regions were not combined with homogeneous (Catholic) France. Again, however, this latter recoding produced no important differences. These results do not strengthen the interpretation given that the second dimension is religious heterogeneity: the inclusion of additional cases or more explicit variables would help here.[13]

Thus, despite some indeterminateness in the finer points, the analyses indicate clearly that education's effect on anti-Semitism is variable cross-nationally. There is also evidence that this variation is systematically related to two features of the countries under consideration, their political history and the religious heterogeneity or pluralism of their populations. These analyses were conducted on data not designed for this purpose and would therefore be strengthened by additional, parallel findings in other areas and by new data designed to test these hypotheses.

DISCUSSION

It was suggested at the beginning of this article that there is a basis for skepticism regarding the usual claim that education has a universally liberalizing effect on values—even on the basis of scattered findings in the literature, whose cumulative impact has generally been neglected. Opinion survey questions on anti-Semitism in four countries were used here to evaluate the consistency of education's effects. Subject to incomplete comparability in the data, the items were validated as measures of anti-Semitism, and they were also shown to be moderately related to other measures of liberal values; education's effects were shown to vary cross-nationally, and the systematic nature of this variation was explored.

Two possible causes of such variation were

[12] That is, there is no statistically significant difference between treating the countries as four categories of a single variable or as the four cells of two two-category variables. This test can be performed for tables in which one of the four countries is not measured by using techniques for missing data with "structural zeros" as described in Bishop et al. (1975).

[13] Milton Himmelfarb, research director of the American Jewish Committee, suggested that it might be useful to include Britain in such an analysis, since it is about as homogeneous religiously as France but has a clearer liberal democratic tradition.

suggested and tested in this article, the age of a liberal-democratic regime form and the existence of religious heterogeneity, but while the results generally support the hypotheses offered, by themselves they are perhaps more suggestive than conclusive. It was argued that this indeterminacy is due in part to the fact that the data were not designed to test these particular questions. However, as noted, a reconsideration of previous, comparable findings may help put the present analyses into a fuller context. The most relevant previous findings (besides those regarding anti-Semitism in the countries considered here) seem to be these: that the better educated did not demonstrate greater affective attachment to democratic institutions in West Germany and Italy in the late 1950s, in contrast to the U.S., Britain and Mexico (Almond and Verba, 1963); that the better educated were not more supportive of the freedom to assemble in West Germany and Austria in the mid-1970s, in contrast to the U.S., Britain and the Netherlands (Muller et al., 1980); that the better educated were not less anti-Semitic in certain Eastern European countries in the early 1970s (Radio Free Europe/Radio Liberty, 1980); and that the better educated were not more tolerant of free speech in West Germany in the 1970s, in contrast to the United States (Weil, 1982; this last finding was supported by similar results in West Germany for a range of other measures of political liberalism in Weil, 1981).[14]

This considerable array of findings appears to have one overriding feature in common: the impact of education on liberal values is weaker, nonexistent, or sometimes even reversed in nonliberal democracies or countries which did not have liberal-democratic regime forms in earlier decades, compared to countries which have been liberal democratic for a long time. And in one of the studies (Weil, 1981), which investigated value change in West Germany over several decades, the extent and possible growth of education's effect on political tolerance was shown to be linked to the growth of popular adherence to democratic institutions as time passed since the Nazi regime, although the nation's schools ostensibly taught tolerance from the time of the founding of the state.

It requires only a slight expansion and modification of Selznick and Steinberg's (1969) hypothesis, that education's liberalizing effects represent the influence of the "official" culture, to encompass these results. Essentially, a more dynamic and variegated notion of "official" is needed. For instance, the present findings suggest that the "official" culture of a country cannot simply be identified with the existing regime form, because schools may tend to "lag" in their ability to socialize students according to state policy—that is, to institutionalize the regime's values—if there has been a recent change. Thus, it probably makes sense to substitute notions of dominant or predominant culture or subculture for "official," even at the cost of sacrificing parsimony. A dominant culture, however, does not necessarily derive simply from a dominant group, because a dominant culture might emerge from the *interaction* of different groups: if the second dimension in the present analysis has been correctly interpreted as religious heterogeneity, it may be an example of this principle. Once it is clear that education's effects vary systematically, and if it is accepted that culture has an influence, then the question shifts to the determination of which elements of culture predominate and how they come to do so. The present research has been able to make only a small initial step in this direction.[15]

If the arguments presented here are correct, then several conclusions seem warranted. (1) The effect of education on values is *not* universal, but rather, varies considerably cross-nationally and historically. While the better educated are more liberal in certain countries and in certain historical periods for certain values, they are not more liberal under other conditions.

(2) Since this is true, the psychodynamic interpretation of education's effects cannot be accepted as comprehensive, but at most as a subordinate explanation, valid under some cultural and historical conditions and not others. It was not possible to make this assertion on the basis of data from a single country because one could argue that students with certain psychological predispositions might seek more education than others, but they cannot be expected to choose their country or

[14] The present relevance of Stember's (1961), McClosky and Brill's (1983), and Jackman and Muha's (1984) findings of variability in education's effects on various liberalism items within one country, the United States, each at roughly one time period, are not immediately clear. One would suppose that this variation must be due to variation in the dependent variables, and it would be interesting to attempt a typology, but as noted earlier, this is not possible here.

[15] The author has begun research on the determinants of liberal and democratic values in post-authoritarian liberal democracies. The role of educational institutions in forming and transmitting these values is one of the factors being investigated, as well as the conditions under which this can vary. However, there are no empirical results to report yet, and a fuller discussion of the model is not possible here.

historical period as a result of the psychological predisposition. The interpretation that education's effects merely reflect class interests also cannot be accepted in any simple sense, because—to the extent that systematic evidence is available—the effects seem to disappear precisely when the dependent variable measures class interests, and to remain strong when the dependent variable does not measure group interests. Education's effects do indeed appear to constitute ideological utterances, but they seem less likely to represent group (class, racial, sexual, etc.) interests than legitimated and institutionalized *regime* or *societal* values, which may not be directly determined by the values of a given dominant group. The evidence indicates that education has an effect on ideological liberalism in long-term liberal democracies with heterogeneous populations, but not on certain aspects of intergroup conflict or competition even within this framework. That is, (a) the greater liberalism of the better educated seems to depend on the existence of liberal dominant values in the regime or society; but (b) even in such regimes or societies, the better educated may still not express different group interests than the less-educated members of their group.

(3) Thus, one may conclude, the effect of education on values, *when* it occurs, must be interpreted as a form of socialization. On the other hand, the *variation* in education's effects may be caused not only by variation in forms of socialization, but also by variation in the type of dependent variable. In particular, to the extent that certain group interests are considered as a form of liberalism (rather than egalitarianism, group solidarity, leftism, etc.), then one may distinguish among those forms of liberalism for which education has an effect (in given societies) and those for which it does not.

However, these conclusions actually raise more questions than they answer: if educational institutions disseminate value orientations, it is by no means clear a priori what determines which values should be transmitted; and there is also no a priori reason to suppose that typologies of liberalism will not be found to vary cross-nationally and historically. Clearly, a comparative and historical approach to further analyses of survey data will help isolate these determinants and typologies. New data are indeed desirable to investigate further conditions under which education's effects on liberal values vary, but it should be kept in mind that these are also inherently historical questions, even if it was not possible to pursue trend analyses in detail here. The ongoing accumulation of newly collected variables, replicated from past surveys, is an important research activity; and one hopes that studies of the present sort help inform the collection of new data. However, if it is sometimes desirable to investigate past historical periods for conditions which no longer exist, then secondary analysis of survey data assumes greater importance in its own right.

APPENDIX A. QUESTION TEXTS: ENGLISH TRANSLATIONS

A. Do Jews Have Too Much Power?
- Do you think that Jews have too much power in the United States? (US, 1964, 1981)
- One often hears that some groups have too much influence in this country and others too little, but there is usually disagreement which groups have too much influence and which too little. Now we'd like to know whether you think these groups have too much, too little, or just the right amount of influence in the Federal Republic: Jews. (Germany, 1974)
- Do you think that the following groups in France have more influence, less influence, or a normal influence? The Jews. (France, 1969)
- Do you think the following statement is correct: "The Jews possess too much power and influence in the world again"? (Austria, 1976)

B. Would Vote For Jew In Own Party?
- There's always much discussion about the qualifications of Presidential candidates—their education, age, religion, race, and the like. If your party nominated a generally well-qualified man for President and he happened to be a Jew, would you vote for him? (US, 1959, 1965, 1969)
- Suppose your political party wanted to nominate a Jew for President of the United States, that is, a religious Jew who would go to synagogue every week the way a Christian goes to church every Sunday. Would this disturb you very much, somewhat, very little, or not at all? (US, 1964)
- Suppose your political party wanted to nominate a Jew for President of the United States, would this disturb you very much, somewhat, very little, or not at all? (US, 1981)
- Suppose your party—the party you like best—nominated a qualified man who was a Jew. Would it be all right with you to see a Jew become federal Chancellor? (Germany, 1960)
- Would you be disturbed by the following situations: having a Jew as President of the Republic. (France, 1966)

C. Questionable Jewish Loyalty/Nationality
- Jews are more loyal to Israel than to America. (Agree or disagree) (US, 1964, 1981)
- Jews are mainly loyal to Israel. They are only marginally interested in the events of the countries they live in. (Aggree or disagree) (Germany, 1974)
- Are Jews as French as other Frenchmen? (France, 1966)
- Do you think that a Jew who calls himself an Austrian would be considered a real Austrian? (Austria, 1968)

D. Jews Stir Up Trouble
- Jews are always stirring up trouble with their ideas. (Probably true, probably false) (US, 1964, 1981)
- Jews are always stirring up trouble (Unfrieden) with their ideas. (Agree or disagree) (Germany, 1974)
- Jews have stirred up lots of trouble (Unheil) in history. (Austria, 1982)

E. Religious Anti-Semitism
- Some people believe that the reason Jews have so much trouble is because God is punishing them for rejecting Jesus. Do you agree with this? (US, 1964, 1981)
- One sometimes hears that the reason the Jews have so much trouble is because God is punishing them for crucifying Jesus Christ. (Agree or disagree) (Germany, 1974)
- Do you agree with the following statements: The Jews of today are no more responsible for the death of Jesus Christ than are other people. (France, 1966)

F. Can Tell A Jew By His Looks?
- You can usually tell whether or not a person is Jewish just by the way he looks. (Agree or disagree) (US, 1964; Germany, 1974; Austria, 1973)
- Definition of a Jew: a separate race. (France, 1966)

APPENDIX B. SCALE CONSTRUCTION: NON-ANTI-SEMITISM ITEMS

1. Liberal Democracy Scales

U.S., 1964, 1981
- Would you be in favor of a law saying that groups who disagree with our form of government could not hold public meetings or make speeches, or would you be opposed to it?
- Would you be in favor of a law saying that the President must be a person who believes in God, or would you be opposed to it?

West Germany, 1974
- What Germany needs today is a strong man at the top who can make short work of all the details (Nebensaechlichkeiten). (Agree or disagree.)
- Germany wouldn't have the big problems it now has if there was more law and order here. (Agree of disagree)
- It's about time to clamp down hard on troublemakers in Germany. (Agree or disagree)
- The problem these days is that too many people are in on the decisions about what should be done. (Agree or disagree)

2. Xenophobia Scales

U.S., 1964, 1981
- Foreigners who come to live in America should give up their foreign ways and learn to be like other Americans. (Agree or disagree)
- Nothing in other countries can beat the American way of life. (Agree or disagree)
- It bothers me to see immigrants succeeding more than Americans who were born here. (Agree or disagree)

West Germany, 1974
- Every ethnic group (Volk) has certain inborn characteristics by which its members differ by nature from other ethnic groups. (Agree or disagree)
- Some peoples (Voelker) are warlike by nature, while others are peaceful by nature. (Agree or disagree)
- Even if everyone really had equal opportunities, the members of some ethnic groups would be more successful than others. (Agree or disagree)
- Some peoples are less moral by predisposition than others. (Agree or disagree)
- It depends on its people's predisposition whether a state is strong or weak. (Agree or disagree)

3. Christian Traditionalism

U.S., 1964, 1981
- Do you think that a person who doesn't accept Jesus can be saved? (Yes or no)
- What about the belief that the Devil actually exists? Are you absolutely sure or are you pretty sure that the Devil exists or are you absolutely sure or pretty sure that the Devil does not exist?

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