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# Comparing Interest Group Scores across Time and Chambers: Adjusted ADA Scores for the U.S. Congress

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*Interest group ratings are widely used in studies of legislative behavior. Since the set of votes used is not constant over time and across chambers, the scales underlying the scores can shift and stretch. We introduce an econometric model that corrects the problem. Specifically, we derive an index, much like an inflation index for consumer prices, that allows one to make intertemporal and interchamber comparisons of interest group ratings. The adjusted scores for the ADA show a strong liberal trend in the average member of Congress during 1947–94, followed by a conservative reversal. A nonparametric test using ADA and ACU scores demonstrates the validity of adjusted scores and the invalidity of nominal scores for intertemporal and interchamber comparisons. Using two studies (Levitt 1996; Shipan and Lowry 1997) we illustrate that the choice of adjusted versus nominal scores may greatly affect substantive conclusions of researchers.*

Without question, the 1974 House elections that brought an influx of “Watergate babies” caused the House to become more liberal. Despite this, however, both the median and mean rating of House members by the conservative interest group Americans for Constitutional Action (ACA) rose between 1974 and 1975 (Groseclose 1994). While a naive comparison of ACA scores from these two years would suggest that the House became more conservative, the perverse result is surely due instead to the ACA shifting its scales, not to a true change in House preferences.

The example highlights a fundamental difficulty facing researchers who use interest group ratings to make intertemporal or interchamber comparisons. Because the set of votes used to construct the ratings are different each year, the scales underlying interest group ratings are likely to shift and stretch across chambers and time.<sup>1</sup> Even worse, when preferences in Congress change, whether due to membership turnover or actual changes in members’ views, interest groups may respond by changing the scales to keep the average score

roughly constant. As a consequence, the shifting and stretching of scales may seriously mask changes in preferences.

The same principle also presents problems for interchamber comparisons. A senator and House member may have identical preferences or ideologies, but because an interest group uses different roll call votes in constructing its ratings for the two chambers, the two politicians may have different scores.

Unfortunately, intertemporal and interchamber comparisons of group ratings are necessary in order to test many of the predictions made by recent models in American politics. Spatial models such as those of Ferejohn and Shipan (1990), Gely and Spiller (1990), Kiewiet and McCubbins (1988), Krehbiel (1996), Segal (1997), and Shepsle and Weingast (1987) make predictions that depend on the relative preferences of House and Senate committee medians, House and Senate floor medians, and nonlegislative actors such as the president, the Supreme Court, or administrative agency heads. At a minimum, therefore, one needs a common scale on which both House and Senate preferences are measured. Also, in order to have a sample size greater than one, such studies require several years of data on a common scale as well. Intertemporal comparisons are also necessary to test various hypotheses about “ideological shirking” (is there a “last-period” shirking problem, or do members shirk more in nonelection years?) and to describe and test hypotheses about how the ideological composition of Congress has changed over time.

In this article we introduce an econometric model that corrects for shifting and stretching scales of interest group scores. Specifically, we derive an index, much like an inflation index for consumer or wholesale prices, that allows one to convert the scores so that they can be used to make intertemporal and interchamber comparisons. Next, to demonstrate the value of the converted scores, we provide three empirical applications. The most important is a demonstration of how aggregate congressional preferences compare over

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<sup>1</sup> The notion of shifting or stretching scales is best explained by an analogy to a thermometer. Suppose the tube of mercury is fixed, but one can recalibrate the numbered marks on the side of the thermometer. If, say, all the marks (and corresponding numbers) are moved  $x$  units above the original marks, we say that the scale has shifted. If one recalibrates the marks so that the distance between them increases, then we say the scale has stretched.

time and across chambers. To do this, we use converted scores of the Americans for Democratic Action (ADA).

Previous researchers have incorporated a number of partial fixes to the problems of shifting and stretching scales. Poole and Daniels (1985), for example, allow for shifts in the scale over time but do not account for differences in scales across chambers, nor do they allow for stretching or shrinking of the scales over time. Similarly, Lott and Bronars (1993) include year-fixed effects as independent variables, which corrects for shifting scales but not for stretching. Our findings indicate that the ADA scales exhibit a substantial amount of stretching over time, in addition to substantial shifts. Also, in one of our empirical applications we find that adjusted ADA scores significantly change the results, even when year-fixed effects are included in the specifications.<sup>2</sup> A third strategy for mitigating the problem of shifting scales is to focus on differences in scores across individuals rather than changes in an individual's scores over time. For example, Grier (1991) assumes that each member's preferences are fixed, and he measures a member's preference as the average of his or her ADA scores over many years. Thus, changes in Grier's variable of interest—the average score of the Banking Committee—are driven largely by membership turnover, and these changes are likely to dwarf those due to changes in the ADA scale. Similarly, Moe (1985) and Spiller and Gely (1992) focus on committee chairs, while Weingast and Moran (1983) concentrate on subcommittee membership. A large proportion of the variation in these variables is also due to turnover, so shifting and stretching scales may not pose too large a problem.

The most systematic approach to achieving intertemporal comparability of roll call voting measures is Poole and Rosenthal's (1991) D-Nominate scores.<sup>3</sup> While these have the shortcoming that House and Senate scores are not comparable, in many other respects the motivations underlying the development of D-Nominate and the conclusions drawn from analysis of D-Nominate scores parallel those presented in this article (with a few important differences discussed below).

Presumably, the relative attractiveness of adjusted interest group ratings and D-Nominate scores will vary with the particular research question at hand, an issue we consider at greater length in one of our empirical applications. One advantage of our method is that it is simple to understand and easy to implement (it is as simple as converting temperatures from Celsius to Fahrenheit). A more important advantage is that our

method can be applied to *any* interest group's ratings. This allows researchers to analyze particular issues, such as labor, civil rights, or the environment, rather than simply a generic liberal-conservative dimension.

As an example of why the second advantage is important, consider the studies mentioned above by Grier (1991), Moe (1985), Spiller and Gely (1992), and Weingast and Moran (1983). Each study focuses on a particular policy area—Federal Trade Commission decisions in Weingast and Moran, monetary policy in Grier, labor policy in Moe and Spiller and Gely. Furthermore, as noted above, the empirical specifications in these studies are designed to reduce the problems associated with changing interest group scales; yet, these specifications also prevent the researchers from examining many hypotheses they surely would like to test. For example, it is natural to test hypotheses about the relative power of chamber floors versus chamber committees, or about the relative power of the House versus the Senate. Does the House median matter more than the committee median or committee chair? Do committees matter more than subcommittees? Has the balance of power changed since the reforms of the 1970s? Do majority party members have more influence than minority members? Existing studies do not address these questions because they cannot. The problem of shifting scales means that the median or average scores of large subsets of representatives (such as whole chambers, political parties, and many committees) are incomparable over time and across chambers. What the studies need are the roll call scores of relatively narrow interest groups (labor unions, banks, environmental groups, small business associations) and a method such as ours that allows the group's scores to be compared over time and across chambers.

As we show, the use of adjusted ADA scores rather than raw scores not only substantively affects the conclusions of some influential and excellent research but also, in general, leads to more precise estimates and less sensitivity of the results to the particular choice of model specification. One of the main effects of the shifts and stretches in the ADA scales is to add considerable measurement error to the scores. In fact, we estimate that this error accounts for approximately one-fourth the total variance in the scores of a typical member. This tends to bias coefficients toward zero when the scores are used as regressors to predict other phenomena. Therefore, the use of adjusted rather than raw scores will often strengthen researchers' findings based on raw scores, as it does in one of the empirical applications we examine.

## A MODEL OF PREFERENCES AND INTEREST GROUP SCORES

Like converting temperature from Celsius to Fahrenheit, we assume that an interest group's scores (hereafter, ADA scores for brevity) can be converted from one year to another or one chamber to another by a linear transformation with two parameters: a shift and stretch factor. For instance, to convert a temperature

<sup>2</sup> Cox and McCubbins (1993) propose the use of ordinal rankings of legislators rather than interest group scores themselves. This approach is not very helpful for making intertemporal or interchamber comparisons, however. For instance, consider the median of the House in 1975 versus the median in 1995. No matter what their ideological differences, both would receive a percentile ranking of 50. Also, since in any given year there is no overlap in membership between the House and Senate, the ordinal rankings method is extremely ineffective for making interchamber comparisons.

<sup>3</sup> See also, Poole and Rosenthal (1985a, 1985b, 1997). D-Nominate stands for Dynamic NOMINAL Three-step Estimation process.

$F$ , listed in Fahrenheit, to the equivalent temperature  $C$ , listed in Celsius, one uses the formula

$$C = \frac{F - 32}{9/5}.$$

Here, 32 is the shift parameter, and 9/5 is the stretch parameter.

Next, we need a base year and a base chamber by which to set scales (similar to fixing a base year for an inflation index). We chose 1980 and the House. Define  $y_{it}$  as the *nominal* ADA score of member  $i$  in year  $t$ . This is the raw score that the ADA reports.<sup>4</sup> For House scores define  $a_i^H$  and  $b_i^H$  as the respective shift and stretch parameters, and define  $a_i^S$  and  $b_i^S$  as the parameters for Senate scores. Define the adjusted score of member  $i$  in year  $t$  as

$$\hat{y}_{it} = \frac{y_{it} - a_i^c}{b_i^c}, \quad (1)$$

where  $c$  ( $= H$  or  $S$ ) indicates the chamber of the member.

To see why interest group scales may shift and stretch, consider the following example of cut points and ideal points.<sup>5</sup> Imagine that three members of the House, 1, 2, and 3, are aligned on a liberal-conservative scale as follows:

$$|-----1-----2-----3-----|.$$

That is, 1 is more left-wing than 2, who is more left-wing than 3, and ideological distance between the three legislators is the same. Now suppose in year 1 the ADA chooses four roll call votes with cut points  $c_1$ ,  $c_2$ ,  $c_3$ , and  $c_4$ , aligned on the above liberal-conservative scale as follows:

$$|--c_1--1--c_2--2--c_3--3--c_4--|.$$

Since cut point  $c_1$  is more liberal than all three of the members, none of them would vote on the liberal side of this measure. (For instance, suppose  $c_1$  represents a measure to cut the defense budget by 75%. Although liberals might prefer this more than conservatives, it is so extreme that no members, not even extreme liberals, vote for it.) Next, on roll call  $c_2$ , 1 votes on the liberal side, while 2 and 3 vote on the conservative side. On roll call  $c_3$ , 1 and 2 vote on the liberal side, while 3 votes on the conservative side. On roll call  $c_4$ , all three members vote on the liberal side. For this year the ADA scores of the three members would be: 1, 75; 2, 50; and 3, 25.

Now suppose in year 2 the ideology of the three members stays the same, but the cut points from the

ADA's new set of roll calls change. In particular, let  $c_5$ ,  $c_6$ ,  $c_7$ , and  $c_8$  be these cut points, aligned as follows:

$$|--1---c_5---2---c_6---3-c_7-c_8--|.$$

Note that the cut points have shifted right, relative to year 1, making it easier for legislators to vote on the liberal side of the roll calls. For example, imagine that  $c_2$  represents a vote to allow partial birth abortion, while  $c_6$  represents a vote to allow abortion in the case of incest or rape. Although both roll calls involve the same issue (abortion) the substance of the legislation has changed, causing the cut point to shift. For year 2 even though the ideology of the three members is the same, their ADA scores change to 1, 100; 2, 75; 3, 50.

These scores can be converted to the year 1 scale simply by subtracting a shift parameter of 25. In general, the more right-wing are the cut points in year  $t$ , the larger will be  $a_i$ , the shift parameter.

When the ADA and other interest groups choose different cut points in different years, their scales will vary from year to year. Furthermore, as Clausen (1967, 1020–1) has noted, even if the interest groups tried to keep the mix of cut points the same from year to year, it is not clear that they could: "Unlike the survey researcher and the psychometrician, the roll call analyst is not given the opportunity to construct the set of items on which his measurements are based."

This example explains why ACA scores made the House appear to become more conservative from 1974 to 1975. Although ideal points of the members shifted left (due to the entry of the Watergate babies), the cut points used by the ACA shifted left even more, and this caused the scores to show a spurious increase in conservatism.

While one problem is that the mix of cut points can *shift* from year to year, another is that their *dispersion* can change from year to year. For instance, suppose in year 3 that cut points  $c_9$ ,  $c_{10}$ ,  $c_{11}$ , and  $c_{12}$  are aligned as follows:

$$|----1-c_9--c_{10}-2-c_{11}-c_{12}-3----|.$$

That is, instead of shifting left or right, they clump toward the center. Then, ADA scores in this year would be 1, 100; 2, 50; and 3, 0.

Although ideology has remained constant, year 3 shows an increase in the standard deviation of the scores. That is, ADA scores in this year spuriously show an increase in polarization of the members. The scores can be converted to year 1 scores by subtracting a shift parameter of  $a_3 = -50$  and dividing by a stretch parameter of  $b_3 = 2$ . In general, when cut points are less dispersed, the stretch parameter will be larger.

The example is highly stylized and simplistic, and perhaps most bothersome, it assumes that ideal points do not change from year to year. In our actual estimation, however, we relax this assumption and allow ideal points to change from year to year. Nevertheless, the example demonstrates the measurement problem that a researcher faces when cut points change from year to year.

While congressional scholars often ignore this prob-

<sup>4</sup> The ADA began reporting scores in 1960. From 1947 to 1959, however, the ADA reported "key votes" for each chamber, and by applying the same method used by the ADA after 1959, we calculate scores for this earlier period as well.

<sup>5</sup> An ideal or "bliss" point is the policy that maximizes the utility of a legislator. It is the policy for which s/he would most prefer to vote if given the opportunity. Cut points separate legislators by ideal points. That is, all legislators with ideal points on one side of a cut point vote yes; all others vote nay.

lem, judicial scholars have been aware of it for years. In particular, they have used Baum-adjusted scores to correct for measures of ideology of the justices.<sup>6</sup> Baum's (1988) technique assumes that, while cut points may vary greatly from year to year, the median ideology of a natural Court (a set of years in which there is no turnover in membership on the Supreme Court) remains fairly constant—or at least much more constant than the cut points from the set of cases that the justices consider. After computing scores of justices by their voting records, these scores are adjusted so that the median of a natural Court maintains a constant adjusted score.

Our technique is very similar, although it basically relies on means rather than medians. Also, unlike Baum's technique, it attempts to correct for spurious increases or decreases in dispersion, such as the increase that occurs in year 3 of the above example.

Our primary task is to estimate  $a_i^c$  and  $b_i^c$  so that we can convert nominal scores to adjusted scores. To do this, we must also estimate for each member a *mean-preference parameter*,  $x_i$ , which is a weighted average of the adjusted scores of member  $i$ .<sup>7</sup>

Since  $x_i$  is a weighted average of adjusted scores, if it were the case that member  $i$ 's preferences (i.e., adjusted scores) remained constant across time, then for each year that s/he serves,  $\hat{y}_{it}$  would equal  $x_i$ . By equation 1 this would imply

$$y_{it} = a_i^c + b_i^c x_i, \quad \forall t. \quad (2)$$

Yet, we *do not* assume that individual preferences remain constant over time. To account for this we add an error term to equation 2. Specifically, we assume

$$y_{it} = a_i^c + b_i^c x_i + \varepsilon_{it}, \quad (3)$$

where  $\varepsilon_{it}$  is distributed  $N(0, \sigma^2)$ , and it is correlated neither with errors of other members nor past or future errors of the member's own score.<sup>8</sup> In the Appendix we examine various ways these assumptions can be relaxed.

Given this representation, we can estimate  $a_i^c$ 's,  $b_i^c$ 's, and  $x_i$ 's by maximizing the following likelihood function:

$$L(\bar{a}, \bar{b}, \bar{x}, \sigma; \bar{y}) = \prod_{t \in T} \prod_{c \in \{H, S\}} \prod_{i \in I_t^c} \phi\left(\frac{y_{it} - a_i^c - b_i^c x_i}{\sigma}\right) \frac{1}{\sigma}, \quad (4)$$

where  $T$  is the set of all years in the sample,  $I_t^c$  is the set of all members serving in chamber  $c$  during year  $t$ , and  $\phi(\cdot)$  is the standard normal density.

We compile the ADA scores of each senator and representative serving from 1947 to 1996. Our sample

includes 2,503 members and 25,762 ADA scores. Using these data, maximization of equation 4 gives estimates of  $a_i^c$  and  $b_i^c$ , which we list in Table 1.<sup>9</sup> The standard error of each  $a_i^c$  is approximately 1.6 for the House and 2.6 for the Senate. The standard errors of each  $b_i^c$  are approximately 0.03 for the House and 0.05 for the Senate.<sup>10</sup>

We do not provide estimates of the individual  $x_i$ 's—primarily because there are 2,503 of them, but also because they are in a sense “nuisance” parameters. From the estimates of  $a_i^c$ 's and  $b_i^c$ 's, however, one can easily compute them using the formula

$$\hat{x}_i = \frac{\sum_{t \in T_i} b_i^c (y_{it} - a_i^c)}{\sum_{t \in T_i} (b_i^c)^2}, \quad (5)$$

where  $T_i$  represents the years in which member  $i$  serves.<sup>11</sup>

The hypothesis that the scale does *not* change from year to year or from chamber to chamber is rejected at overwhelming levels of confidence.<sup>12</sup> Similarly, the possibility that the ADA changes its scales from year to year but not from chamber to chamber is also rejected at overwhelming levels.<sup>13</sup> We should note that there are 2,213 observations for members who switched chambers, 8.6% of the sample. Finally, it is possible that the ADA shifts its scales but does not compress or expand them; once again, however, this hypothesis is rejected at extraordinary levels.<sup>14</sup>

The results are not only statistically significant but

<sup>9</sup> The adjusted scores that are produced from the index of Table 1 can be downloaded from <http://wesley.stanford.edu/groseclose>.

<sup>10</sup> Estimation of standard errors requires inverting a matrix larger than  $1600 \times 1600$ , a task that exceeds the bounds of our software package. Yet, estimating subsamples of the data shows that standard errors of  $a_i^c$ 's and  $b_i^c$ 's are closely proportional to  $1/\sqrt{N_t^c}$ , where  $N_t^c$  is the number of members in the subsample serving in chamber  $c$  during year  $t$ . Using this fact and computing standard errors from a subsample of approximately one-third of the total sample, we estimate standard errors for the whole sample. Standard errors of  $a_i^c$  range from 1.35 to 2.06 for the House and from 2.28 to 3.08 for the Senate. Standard errors of  $b_i^c$  range from .023 to .039 for the House and from .045 to .066 for the Senate.

<sup>11</sup> Note that to compute  $x_i$  one needs values of  $a_i^c$  and  $b_i^c$ . To compute  $a_i^c$ 's and  $b_i^c$ 's, however, one needs estimates of  $x_i$ 's. The apparent infinite regress can be skirted by estimating all three sets of parameters simultaneously, which our maximum-likelihood technique does. Our method is similar to the singular value decomposition technique that Aldrich and McKelvey (1977) apply to seven-point-scale survey data. It is also similar to the work of Poole (1996), who generalizes the Aldrich-McKelvey model and applies it to survey data and W-Nominate scores (a static version of D-Nominate scores).

<sup>12</sup> The value of the unconstrained log likelihood function is  $-97,900.26$ , while the value of the constrained function (where the scales are not allowed to vary) is  $-101,672.55$ . The likelihood ratio test gives a value of  $2(101,672.55 - 97,900.26)$ . Under the null, this is distributed  $\chi_{194}^2$  ( $194 = 49 \text{ years} \times 2 \text{ chambers} \times 2 \text{ parameters for each year} - 2 \text{ base parameters}$ ). It is significant at the  $p \ll 10^{-6}$  level. The significance is approximately as great as a  $t$ -statistic of 103.17.

<sup>13</sup> Specifically, the value of the unconstrained log likelihood function is  $-97,900.26$ , and the value of the constrained function is  $-98,405.19$ . The likelihood ratio statistic is  $2(98,405.19 - 97,900.26)$ . Under the null it is distributed  $\chi_{98}^2$ . It is significant at the  $p \ll 10^{-6}$  level. Its significance is approximately as great as a  $t$ -statistic of 30.98.

<sup>14</sup> Specifically, the value of the unconstrained log likelihood function is  $-97,900.26$ , and the value of the constrained function is  $-98,716.50$ . The likelihood ratio statistic is  $2(98,716.50 - 97,900.26)$ .

<sup>6</sup> Judicial scholars use slightly different terms. What we call ideal points and cut points, they call  $i$ -points and  $j$ -points.

<sup>7</sup> The formal definition of  $x_i$  is listed in equation 5. It is derived by taking first-order conditions of the likelihood function, equation 4.

<sup>8</sup> We also assume the errors are homoskedastic. An alternative, heteroskedastic model assumes  $y_{it} = a_i^c + b_i^c(x_i + \varepsilon_{it})$ , where in this case the variance of the error is  $(b_i^c \sigma)^2$ . The maximum likelihood estimator for the homoskedastic model is also a consistent estimator for this model. That is, even if the heteroskedastic model is true, the method we use is robust.

**TABLE 1. Index for Converting ADA Scores**

Year	House		Senate		Year	House		Senate	
	$a_t^H$	$b_t^H$	$a_t^S$	$b_t^S$		$a_t^H$	$b_t^H$	$a_t^S$	$b_t^S$
1947	13.69	1.127	25.18	1.141	1973	-2.72	1.022	2.16	1.057
1948	14.94	0.994	26.99	1.067	1974	0.28	0.901	2.62	1.065
1949	3.35	1.244	10.57	1.331	1975	-4.82	1.076	1.26	1.072
1950	1.87	1.171	18.03	1.053	1976	-5.90	0.963	-0.15	0.970
1951	2.45	1.151	12.36	1.204	1977	-4.20	0.921	0.51	0.999
1952	1.67	1.218	16.57	1.185	1978	0.80	0.769	5.91	0.802
1953	13.25	1.039	16.35	1.005	1979	0.75	0.940	7.63	0.781
1954	12.61	1.047	14.73	1.079	1980	0.00	1.000	12.86	0.896
1955	17.39	0.982	3.17	1.102	1981	-2.10	0.961	-0.57	1.810
1956	26.73	0.982	19.78	1.138	1982	0.63	0.987	5.71	1.070
1957	22.23	0.890	26.19	0.835	1983	1.91	1.031	6.41	1.004
1958	7.86	1.139	13.99	1.128	1984	1.34	0.991	5.50	1.119
1959	6.33	1.195	1.01	1.250	1985	2.40	0.955	-2.51	1.121
1960	6.11	1.280	5.14	1.284	1986	0.47	1.013	1.10	1.116
1961	-4.08	1.388	5.08	1.347	1987	3.59	1.033	8.35	1.060
1963	-0.12	1.232	5.04	1.164	1988	7.88	0.982	1.41	1.120
1964	-0.12	1.232	5.04	1.164	1989	0.55	1.051	-0.80	1.069
1965	-11.18	1.219	-0.18	1.225	1990	3.21	0.985	2.80	1.070
1966	-12.52	1.228	-3.27	1.222	1991	-1.04	0.992	2.01	1.107
1967	-4.95	1.080	-0.08	1.034	1992	7.31	0.971	6.59	1.117
1968	-7.12	1.140	0.44	0.938	1993	0.68	1.025	7.68	1.021
1969	-7.00	1.044	1.89	1.194	1994	-1.16	0.997	4.77	1.105
1970	-0.59	0.984	0.91	1.082	1995	-4.31	1.076	-4.13	1.280
1971	-3.44	1.010	1.02	1.140	1996	-1.29	1.004	0.59	1.156
1972	-9.02	1.031	-1.48	1.007					

Note: To convert a nominal ADA score to an adjusted score, use the formula  $Adjusted\ Score = (Nominal\ Score - a_t^c)/b_t^c$ , where  $a_t^c$  ( $b_t^c$ ) is the shift (stretch) parameter for converting scores in year  $t$  and chamber  $c$ . The ADA did not release ratings in 1962, and it made each member's 1963 score identical to his or her 1964 score.

also very significant substantively. For example, suppose one House member received a score of 92 in 1964 from the ADA, while another received a score of 58 in 1978. Although it is tempting to conclude that the first member was more liberal, our results show that the two members were approximately the same ideologically, since each member's adjusted score was approximately 75.<sup>15</sup>

A final indication of the substantive significance of changing ADA scales involves the estimates of the variance of the error term,  $\sigma^2$ . When all  $a_t^c$ 's are constrained to zero and all  $b_t^c$ 's are constrained to one—that is, when scales are allowed neither to shift nor stretch—the estimate of  $\sigma^2$  is 156.9. In this case, each  $x_i$  equals the average nominal score of the member. Accordingly, 156.9 is also the individual variance of each member's set of nominal scores. In contrast, when the  $a_t^c$ 's and  $b_t^c$ 's are allowed to vary, the estimate of  $\sigma^2$  is 117.0. This means that approximately one-fourth ( $[156.9 - 117.0]/156.9 \approx .254$ ) of the total variance of an individual's nominal scores is due to scale shifts. This also indicates the superior reliability of adjusted scores over nominal scores, since the variance of the former is only 75% of the variance of the latter.

Under the null it is distributed  $\chi_{98}^2$ . It is significant at the  $p \ll 10^{-6}$  level. Its significance is approximately as great as a  $t$ -statistic of 43.18.

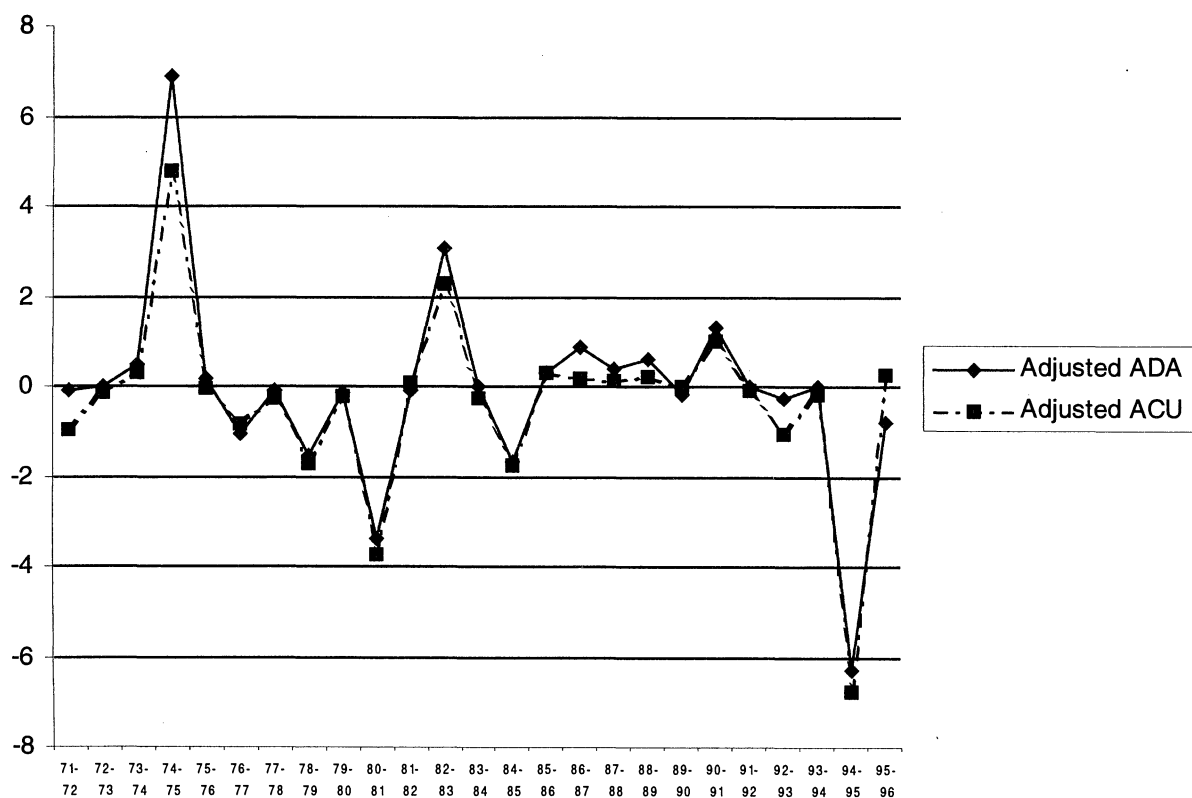
<sup>15</sup> Using the  $a_t^c$ 's and  $b_t^c$ 's from Table 1, note that  $75 \approx (92 - (-.12))/1.232 \approx (58 - .80)/.769$ .

## A CHECK ON THE VALIDITY OF THE METHOD

Next, we perform a test that shows the validity of adjusted scores and the invalidity of nominal scores. Besides ADA scores, we have also converted scores from the American Conservative Union (ACU), the chief opponent of the ADA for the 1971–96 period.<sup>16</sup> The ADA and ACU scores are designed to measure the same ideological dimension, albeit from opposite sides of the dimension. Evidence that they do measure the same dimension is the very high (negative) correlation between the scores in any *single* year and chamber. For the 1971–96 sample, correlation coefficients between the scores are usually greater than .90, and in all but one chamber-year pair the correlation is at least .80. (The exception is the 1977 Senate, for which the coefficient is .72.) As the following analysis shows, however, the high correlation significantly decreases when one uses the scores to make comparisons across years.

Our test follows Campbell and Fiske's (1959) notion of "convergent validity." One assumption of the notion is that two measures of the same concept, if they are both valid, should be highly correlated. Our claim is that both adjusted ADA and ACU scores are valid for

<sup>16</sup> The ACU began recording scores in 1971. We thank Jason Mycoff for providing the scores to us. See Mycoff 1998 for more details on ACU scores.

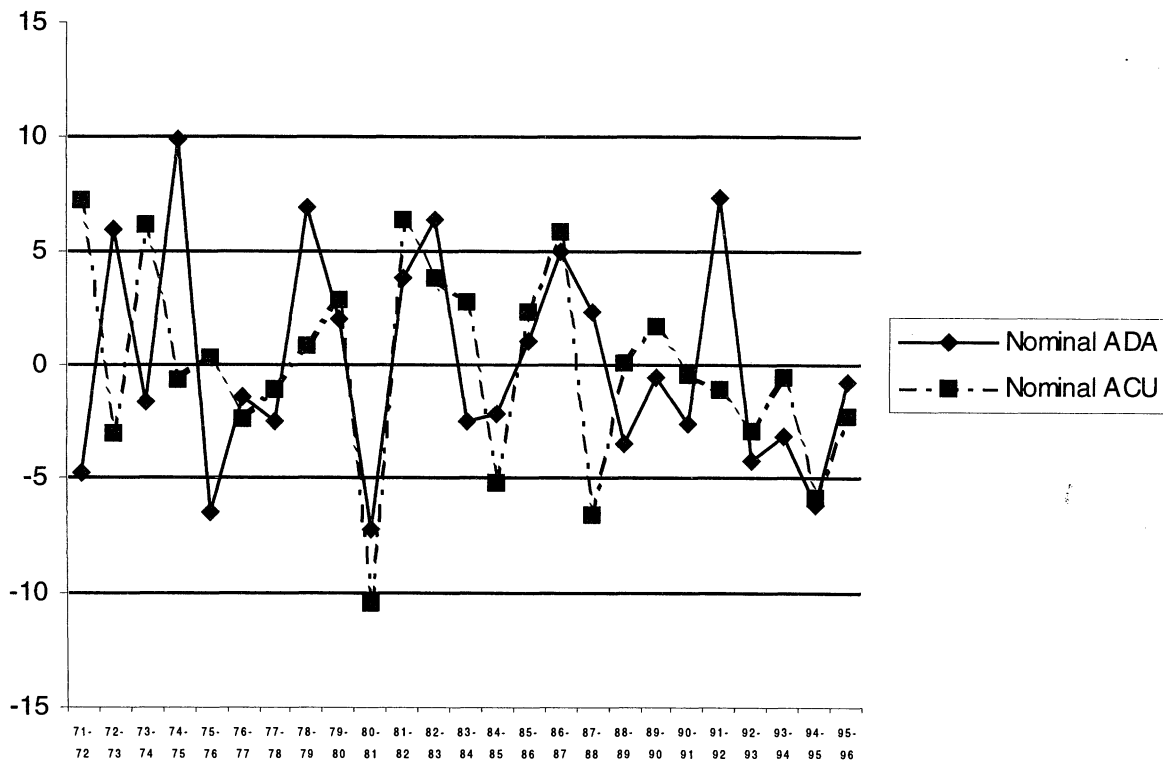
**FIGURE 1. First Differences in House Means, Adjusted ADA and ACU Scores**

measuring conservative/liberal ideology across time and chambers, but *nominal* ADA and ACU scores are not.

For each year and chamber we compute the mean adjusted and nominal score for the two interest groups. Next, to test how well the scores compare across time, we compute first differences of the means between adjacent years. Figure 1 lists these differences for adjusted ADA and adjusted ACU scores in the House. (Similar results occur with the Senate.) Figure 2 lists the differences for nominal scores. In both figures we list the negative of ACU changes, so that liberal changes are represented by upward movements with both scores. Most striking about the figures is the strong agreement between adjusted scores and the relative disagreement between nominal scores. In fact, in many years nominal ADA scores and nominal ACU scores do not even agree on the *direction* in which ideology changed. For instance, between 1971 and 1972, nominal ADA scores show that the House became five points more conservative, while nominal ACU scores show that the House became seven points more liberal. Meanwhile, adjusted scores agree with each other: Neither shows significant ideological change. Furthermore, that adjusted scores show such little change is consistent with a well-documented finding in the congressional literature: Ideological change is due almost entirely to turnover and very little to individual changes of heart (Brady and Sinclair 1984;

Poole and Rosenthal 1991; Shaffer 1987; Stone 1980). Since the two years are part of the same Congress—and thus turnover was essentially nil—one should expect little ideological change.

The validity of adjusted scores and the invalidity of nominal scores is further confirmed by comparing correlation coefficients of the four methods. By considering each possible pair of the four measures we can generate six correlation coefficients. The first column of Table 2 lists these for House data. Note that the correlation between the two adjusted scores is the highest, while the correlation between the two nominal scores is the lowest. The probability that this would occur by chance alone is  $\frac{1}{6} \times \frac{1}{5} \approx 0.03$ . Thus, the exercise provides a nonparametric test that is statistically significant. In fact, when we consider both chambers, we obtain an even greater significance. Column 2 of the table lists the correlations for the Senate. Again, the two adjusted scores correlate the highest, and the two nominal scores correlate the lowest. The probability that this could happen in both chambers by chance is  $(\frac{1}{6} \times \frac{1}{5})^2 \approx 0.001$ . Column 3 presents the results when we stack House and Senate data. Column 4 presents the results when we compute differences between chamber means instead of differences between adjacent years of the same chamber. Again, adjusted scores correlate the best, and nominal scores the worst, further demonstrating the validity of adjusted scores and the invalidity of nominal scores.

**FIGURE 2. First Differences in House Means, Nominal ADA and ACU Scores**

## EMPIRICAL APPLICATIONS

Next, we perform three applications to demonstrate the value of our method. The first uses adjusted scores to compare aggregate congressional ideology over time and across chambers. Of course, this exercise is inappropriate if one uses nominal scores. The second examines Levitt's (1997) research on factors that affect senators' voting decisions. The latter application analyzes how researchers' results change when adjusted ADA scores are used instead of nominal scores. It is ideal for our analysis since it uses ADA scores to make comparisons across time *and* across chambers. A final application highlights an advantage of our method over D-Nominate: It can measure preferences on particular

issue dimensions, such as environmental or labor policy, and it is not restricted to measuring only general liberal/conservative preferences. For the application we use Shipan and Lowry's (1997) computation of adjusted scores of the League of Conservation Voters (LCV). With these scores we track environmental preferences over time, and we compare them with changes in conservative/liberal preferences.

### Application I: Tracking House and Senate Preferences over Time

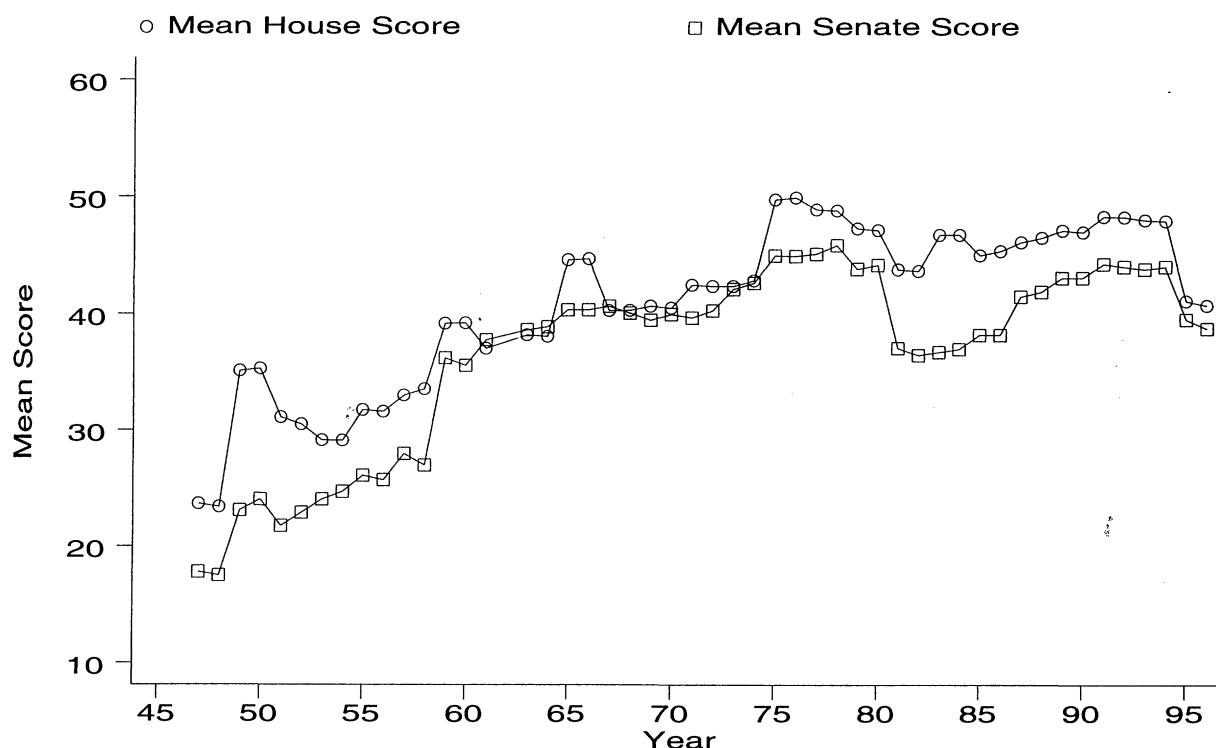
After converting nominal ADA scores to adjusted scores, we compute the median and mean score of each

**TABLE 2. Changes in Mean Ideology: Correlations between ADA and ACU Measures**

	Differences between Years			Differences between Chambers
	House	Senate	Both Chambers	
Adjusted ADA – Adjusted ACU	.97	.96	.97	.97
Adjusted ADA – Nominal ACU	.41	.51	.46	.53
Adjusted ADA – Nominal ADA	.57	.56	.56	.71
Adjusted ACU – Nominal ADA	.57	.60	.59	.70
Adjusted ACU – Nominal ACU	.44	.53	.49	.60
Nominal ADA – Nominal ACU	.27	.31	.29	.44

*Note:* To construct the first column, we compute four statistics for each year of the 1971–96 sample: the House mean measured by (1) adjusted ADA, (2) adjusted ACU, (3) nominal ADA, and (4) nominal ACU scores. (We multiply ACU scores by  $-1$  to make higher values represent more liberal ideology with both scores.) Next, we compute first differences of the four measures across adjacent years. We illustrate these in figures 1 and 2. The first differences give four vectors of length 25 (1996–71). By considering pairs of the four vectors we compute six correlations. We do the same exercise for column 2, but we use Senate means instead of House means. Column 3 uses both chambers, that is, it constructs four first-difference vectors of length 50. Column 4 uses differences between chamber means instead of differences between adjacent years. It constructs four vectors of length 26 (1996–71 + 1).

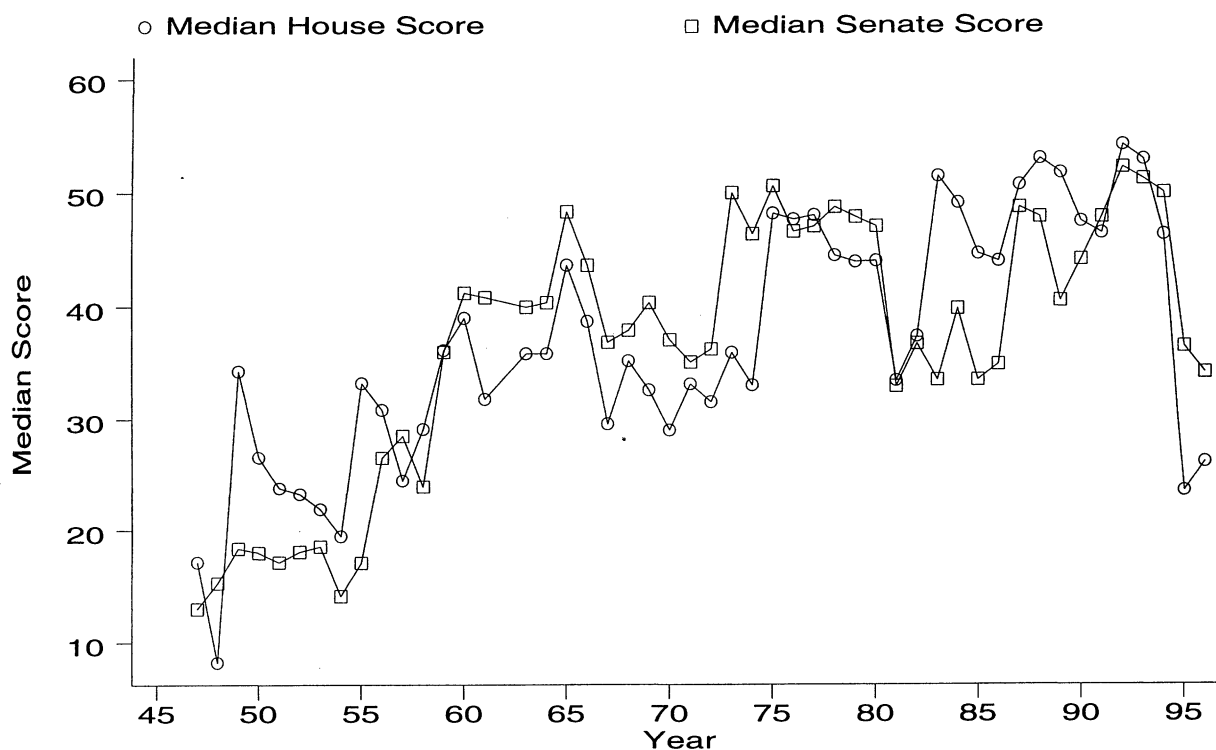


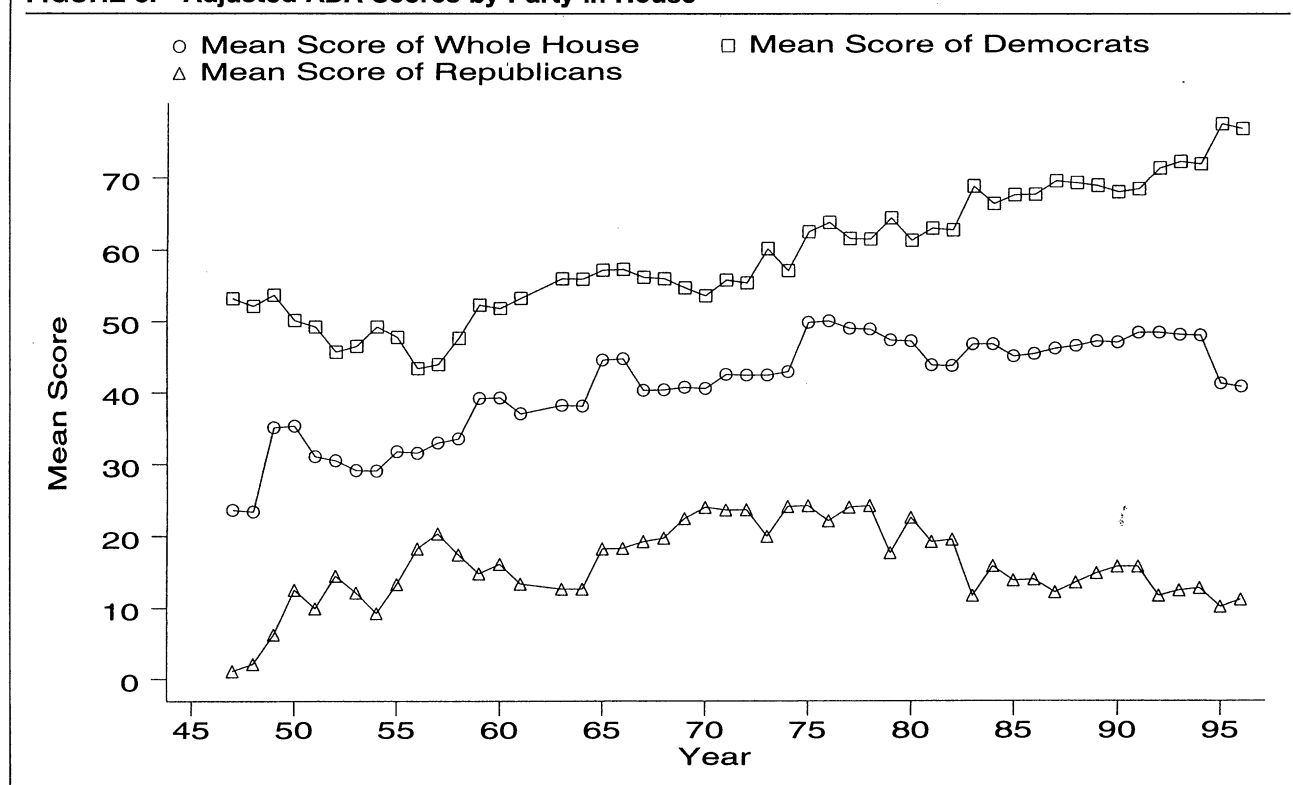
**FIGURE 3. Mean Adjusted ADA Scores by Chamber**

chamber over the years 1947 to 1996. We illustrate these in figures 3 and 4.

Along with revealing some noteworthy trends, the

hills and valleys of figures 3 and 4 provide perspective on some key elections of recent U.S. history. First, they confirm the conventional wisdom that the 1994 elec-

**FIGURE 4. Median Adjusted ADA Scores by Chamber**

**FIGURE 5. Adjusted ADA Scores by Party in House**

tions caused a huge conservative shift in the House. In fact, if anything, they show that the conventional wisdom may understate the change. Only one election in the entire postwar history of the United States caused greater change to the median score of the chamber (the 1948 House elections), and the degree of the 1994–95 change was almost double the Watergate change: While in 1975 the House median became 15 points more liberal, in 1995 it became 24 points more conservative.

Next, although the liberal shift in the House due to Watergate was very significant, about a half dozen other shifts in modern congresses were at least as significant. For instance, the shift in the Senate in the wake of Reagan's victory in 1980 was just as large, even though this usually receives much less attention than the Watergate change.

The twists and turns of congressional preferences closely follow partisan changes in membership. For example, the large liberal House swings of 1948, 1958, 1966, and 1974 occurred when Democrats gained, respectively, 75, 49, 47, and 49 seats. The relationship between average preference and partisan balance is not perfect, however. For example, in 1964 Democrats gained 37 seats in the House, while in 1966 they lost 47. Although on balance they lost ten seats, the House became more liberal over the period. The reason is explained in Figure 5, which shows the means of party caucuses as well as whole chamber means. During the 1964–66 period the parties themselves moved. While Democrats made a small shift leftward, Republicans—because the new members tended to be much more liberal than the old—made a *large* shift leftward.

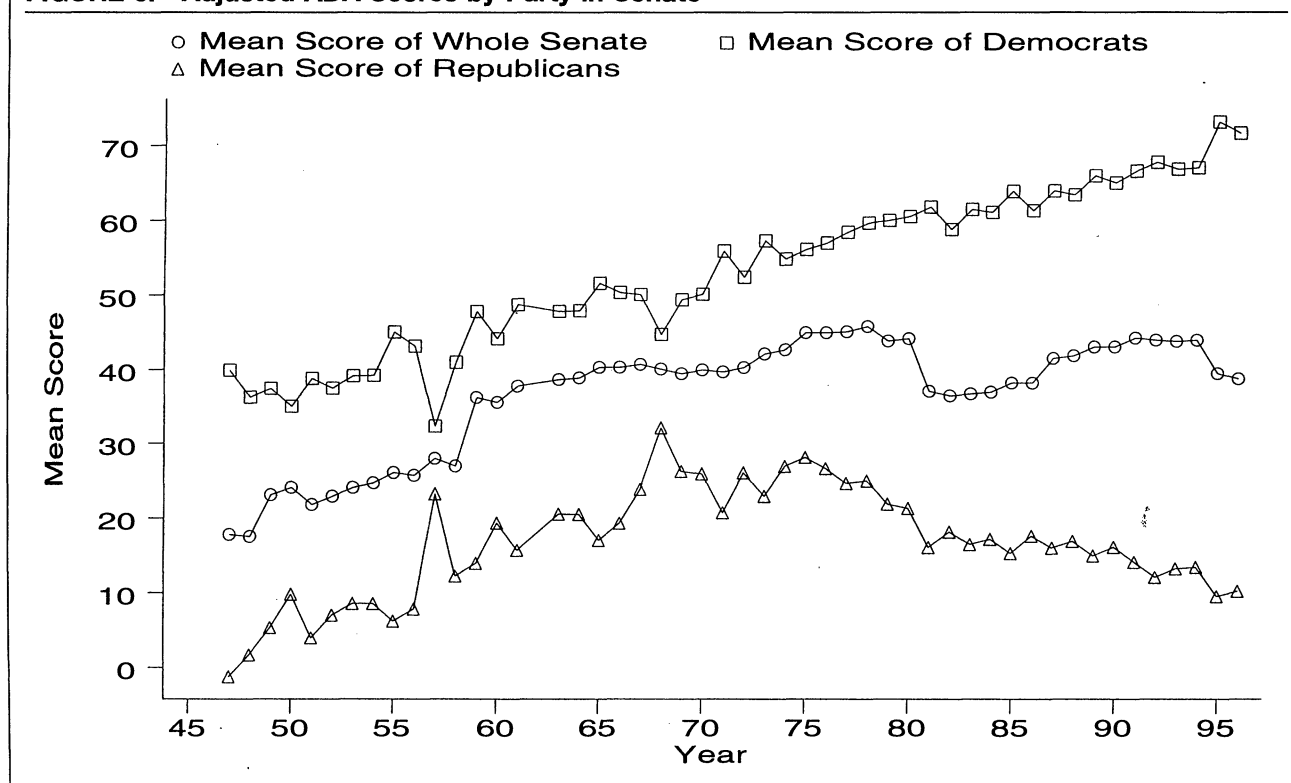
Similarly, the liberal swing in the 1948 House was also due to more than just turnover. Here again, both parties moved in the liberal direction. Although parties often follow whole-chamber ideological changes after sweeping elections, this is not always true. For instance, when the average House and Senate member moved substantially leftward in 1974, the Republicans did not budge. Even more striking, when the House and Senate moved rightward after the 1980 and 1994 elections, the Democrats did more than simply hold their ground: They made positive movements leftward.

Next, figures

5 and 6 add some perspective to the changes in whole-chamber ideology. Most significant, they show how substantial was the liberal trend between the late 1940s and early 1990s: The average member of the whole House and Senate in 1994 was about as liberal as the average Democrat in the early 1950s. They also show how substantial was the conservative reversal in 1995. The House median returned to a level of conservatism similar to that in the 1950s.<sup>17</sup>

Party movements in figures 5 and 6 reveal another

<sup>17</sup> Although we do not present the results, we have also analyzed scores from the ACA for 1960–81 and scores produced from the linear factor model of Heckman and Snyder (1997) for 1935–90. The results are remarkably similar to those obtained using the ADA scores. Similar liberal trends, similar interchamber differences, and similar peaks and valleys in the graphs are revealed. Also, the results show similar trends in party means and medians and similar trends in party-region means and medians. The main difference is that the liberal trend in the ACA scores is not quite as large as the liberal trend shown by ADA and factor scores.

**FIGURE 6. Adjusted ADA Scores by Party in Senate**

general trend: Parties of recent congresses have become more polarized. This reinforces findings of Poole and Rosenthal (1984), who note that the polarization has been increasing since 1960.<sup>18</sup>

Figures 3 and 4 also provide perspective on inter-chamber differences of preference. For the period we study, the Senate has usually been more conservative than the House. Moreover, during 1947–60 and 1980–94 the difference between chambers was substantial: The average senator was usually five or more adjusted points more conservative than the average representative.

The conservative tendency for the Senate has not always been so strong, however, and at times during the 1960s the Senate was more liberal than the House. Furthermore, if one looks at medians instead of means (as shown in Figure 4), the Senate had a general tendency to be more liberal than the House during the 1960s and 1970s. This confirms Ripley's (1969) claim that the Senate was more liberal than the House during the early 1960s. Finally, it is also worth noting that, consistent with conventional wisdom, during the 104th Congress (1995–96), the Senate was substantially more liberal (approximately 5 points) than the House.

In contrast, as Figure 7 shows, few of these trends and interchamber differences are apparent from nominal scores. For instance, nominal scores show no

tendency for the Senate to appear more conservative than the House, and the overall liberal trends are not as pronounced. Moreover, the nominal scores spuriously exhibit much more year-to-year variation than actually occurs in congressional voting patterns.

### Application II: Levitt's AER Study

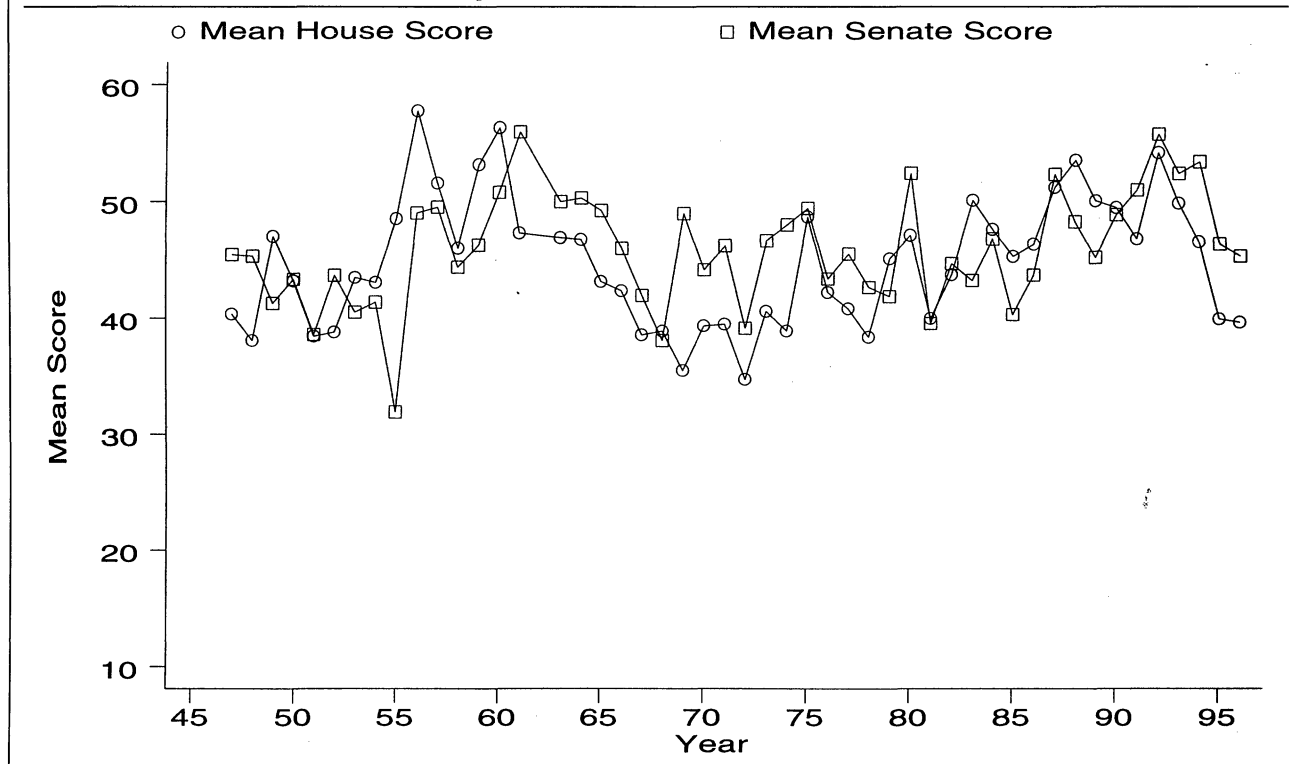
**Background and Theory.** Levitt (1996) attempts to estimate the relative importance of various factors that influence voting patterns of senators. He hypothesizes senator voting to be a function of (1) the overall preferences of the state electorate, (2) the preferences of a particular senator's "supporters" within the state electorate, (3) the national party line, and (4) the senator's own ideology. The problem is formalized by assuming that the senator chooses a voting profile to minimize a weighted average of the squared distances from the ideal points of the four different sets of interests listed:

$$U_{it} = -[\alpha(V_{it} - S_{it})^2 + \beta(V_{it} - C_{it})^2 + \gamma(V_{it} - P_{it})^2 + (1 - \alpha - \beta - \gamma)(V_{it} - Z_{it})^2],$$

where  $i$  indexes senators, and  $t$  corresponds to years.  $V_{it}$  is a senator's voting profile in a given year.  $S_{it}$ ,  $C_{it}$ , and  $P_{it}$  are, respectively, the ideal points of state voters, the senator's support constituency, and the senator's party.<sup>19</sup>  $Z_{it}$  is the senator's ideological ideal point, assumed to be constant over time. The senator's ide-

<sup>18</sup> Other work by Poole and Rosenthal (1991) suggests that this is only a modern, relatively short-term phenomenon. When one takes a broader view, parties have been becoming less polarized since the Civil War. Although Poole and Rosenthal's study also shows a slight increase in polarization beginning in 1960, the increase is very minor when compared to the major decrease since 1865.

<sup>19</sup> The ideal point of an individual refers to that individual's most preferred policy point.

**FIGURE 7. Nominal ADA Scores by Chamber**

ology is defined to be the voting profile that the senator would adopt if s/he placed zero weight on the other three factors. Since utility functions are defined only up to an affine transformation, there is no loss of generality implied in constraining the decision weights to sum to one. In order for the estimated coefficients to be directly interpretable as weights in the utility function, however, all the ideal points and the voting profile  $V_{it}$  must be measured in the same units (in this case, ADA scores).

Maximizing the above function with respect to the senator's voting profile yields a senator's optimal voting record,  $V_{it}^*$ , which is simply a weighted average of the four ideal points:

$$V_{it}^* = \alpha S_{it} + \beta C_{it} + \gamma P_{it} + (1 - \alpha - \beta - \gamma)Z_{it}. \quad (6)$$

The foremost problem in applying equation 6 to actual data is that there is no good proxy for the senator's ideology (in particular, a senator's past voting record is not a useful proxy because it is a function of the three other factors as well as ideology). As long as all the ideal points are measured in the same units, however, there is no need to observe ideology. If senator-specific constants are included in equation 6, then the estimating equation is

$$V_{it}^* = \alpha S_{it} + \beta C_{it} + \gamma P_{it} + [(1 - \alpha - \beta - \gamma)Z_{it}] \times I_{it}, \quad (7)$$

where  $I_{it}$  equals 1 if the observation in question is for senator  $i$ , zero otherwise; that is,  $I_{it}$  is a senator-specific constant. The parameter estimates associated with the senator-specific constants have two components: the

senator's ideology and the weight the senator places on his or her own ideology in the utility function. Because estimates of the weighting parameters  $\alpha$ ,  $\beta$ , and  $\gamma$  are obtained from a regression of equation 7, the weight senators place on their own ideology ( $1 - \alpha - \beta - \gamma$ ) can be determined. Knowing that weight, parameter estimates of each senator's ideology can also be obtained. Therefore, all parameters in equation 7 are identified (in the technical sense of the word), even though senator ideology is unobserved.

**Estimating the Model.** Estimation of the model described above requires measures of the senator's voting record and the ideal points of the overall state electorate, the senator's supporters, and the party line. In order to yield valid conclusions, all those variables must be expressed in the same units. Levitt (1996) uses ADA scores as the unit of measure. As a proxy for the preferences of the overall state electorate, the mean ADA score among all House members in the senator's state is used. For support constituency preferences, the mean ADA score among all House members in the senator's state and party is used. Levitt considers two possible proxies for the party line: the mean ADA score among all other members of the senator's party and the mean ADA score among party leaders. Clearly, the results of these regressions depend critically on the assumption that ADA scores are comparable across chambers and years.

Eight different specifications of the basic model are presented in Levitt (1996). These specifications vary as to which of the two alternative party proxies are used,

**TABLE 3. Effect of ADA Adjustment on Empirical Estimates Case Study: Levitt (1996) Model of Senate Voting**

	Adjusted ADA Scores			Raw ADA Scores		
	(1) Mean Coefficient Estimate	(2) Estimated Standard Errors	(3) Range of Estimates	(4) Mean Coefficient Estimate	(5) Estimated Standard Errors	(6) Range of Estimates
Overall state preferences	.120	.045	.10-.13	.149	.054	.12-.18
Support constituency preferences	.135	.040	.10-.17	.154	.046	.07-.21
Party line	.126	.070	.02-.25	.300	.078	.12-.65
Senator's ideology	.619	.072	.52-.69	.397	.084	.14-.58

Note: Values in columns 1-3 of the table are averages across the eight specifications presented in Table 3 of Levitt (1996); values in columns 4-6 are identical specifications using raw ADA scores. The eight specifications underlying the values reported in this table differ with respect to the choice of party-line proxy, the inclusion of year dummies, and whether two-stage least squares is employed. For full details of estimation, see Levitt (1996).

whether instrumenting variables are used to correct for possible endogeneity in the proxies, and whether year dummies are included. In theory, year dummies do not belong in the specification, but they may be important omitted variables if ADA scores are not comparable over time.

Rather than reproduce the full set of results specification by specification, we summarize the results in Table 3.<sup>20</sup> Summary results are presented for estimates based on both adjusted ADA scores (columns 1-3) and raw ADA scores (columns 4-6).

There are a number of striking differences between the adjusted ADA estimates and the raw ADA estimates. First, comparing columns 1 and 4 of Table 3, the parameter estimates themselves differ substantially. In particular, raw ADA scores appear to overstate dramatically the importance of party, assigning it more than twice as much weight on average as do estimates from the adjusted scores. In fact, strong theoretical reasons indicate that this result may apply more generally to other studies. That is, there are strong reasons that raw ADA scores will *usually* cause estimation procedures to overstate the effect of party. When a researcher uses raw scores, this adds error to the measurement of ideology. Since party is highly correlated with ideology, this causes party to help proxy for ideological preferences. Accordingly, when ideology is not measured perfectly, the estimation procedure overstates the causal effect of party.<sup>21</sup>

Next, eliminating the random noise associated with shifts and stretches of ADA scores also leads to smaller standard errors on each of the four coefficient estimates (column 2 versus column 5). Standard errors fall between 10% and 20% when adjusted ADA scores are used.

More impressive, perhaps, is the increased robustness of the parameter estimates. In columns 3 and 6, the

range of the estimated parameters across the eight specifications are shown. The range is roughly twice as large for each of the categories for the raw ADAs relative to adjusted ADAs. The shifting and stretching of raw ADA scores makes the results far more sensitive to the particular model specification.

Although not formally presented in Table 2, we also performed two types of specification tests. First, we tested the hypothesis that the coefficients on the year dummies were jointly equal to zero. There is no theoretical reason to believe that year dummies should predict voting patterns. Yet, we suspect that if one fails to control for shifting scales by the ADA, then year dummies will spuriously show an effect. This is exactly what happens. When adjusted ADA scores are used, one never finds a significant effect of year dummies. When we use raw ADA scores, the year dummies enter strongly, in all cases jointly statistically significant at the .01 level.

Second, in the specifications that employ instrumental variables (the instruments used are lagged values of the relevant proxies), we tested the overidentifying restrictions that result from having more instruments than *endogenous right-hand-side variables*. For the adjusted ADA scores, the validity of the instruments is always well within conventional bounds, so the specifications employed cannot be rejected. Of the four specifications using raw ADA scores where instruments are employed, in one case the validity of the instruments is rejected at the .02 level, in another case at the .10 level.

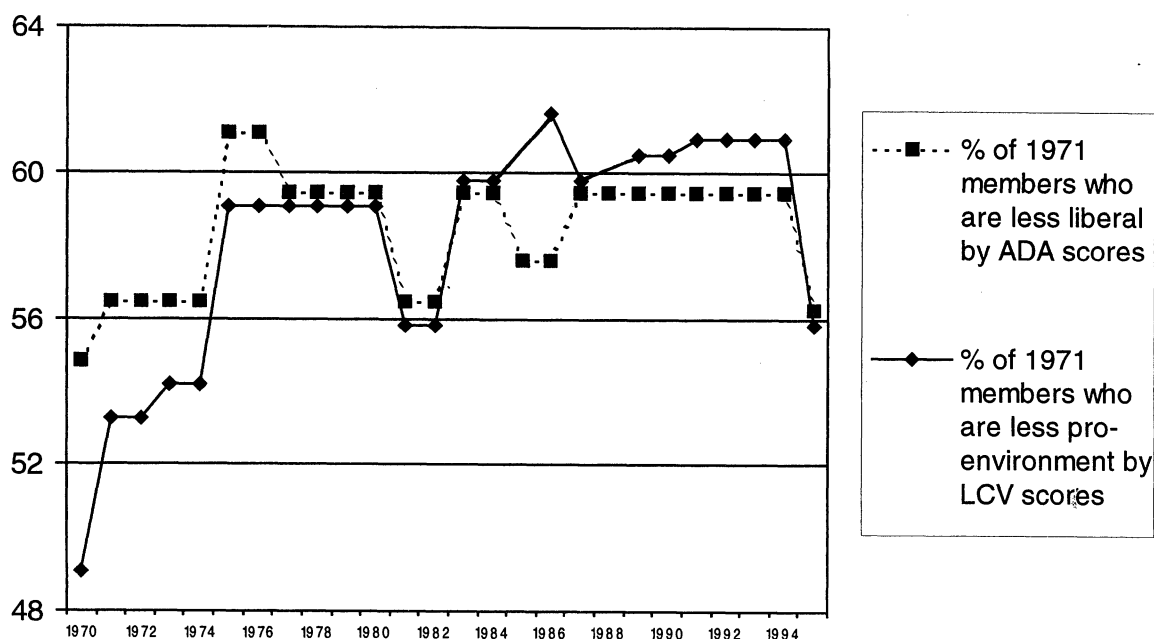
All in all, the transformation on raw ADA scores not only changes the substantive conclusions of the analysis but also reduces the standard errors of the estimates. It greatly reduces the sensitivity of the findings to the particular modeling assumptions, and it improves the performance of the model on specification tests.

### Application III: Comparing Environmental and Liberal Preferences

A main advantage of our method over D-Nominate is that it can be applied to interest group scores that tap narrow issue dimensions, such as those by the League of Conservation Voters, the National Taxpayers' Union,

<sup>20</sup> The complete set of results are available at <http://wesley.stanford.edu/groseclose>.

<sup>21</sup> See Krehbiel (1996) for a particular discussion of the spurious effects of party when one does not perfectly control for ideological preferences. See Klepper (1988a, 1988b) for discussions of the general problem of measurement error in independent variables and how this causes the effect of other independent variables to be overstated.

**FIGURE 8. ADA and LCV Means Compared to 1971 House**

Note: The solid line is constructed from mean LCV scores of the House for each year of the sample. For each mean we note the percentage of members in the 1971 House whose adjusted LCV score was less. We show these percentages in the figure. The dotted line is constructed identically, except it is based on adjusted ADA scores instead of adjusted LCV scores.

the National Farmers' Union, the National Education Association, the ACLU, the American Security Council, and others. It is not restricted only to measuring general liberal/conservative preferences, like those measured by the ADA, ACU, and first-dimension D-Nominate scores.

As a demonstration of our method's ability to tap such narrow issue dimensions, we review the work of Shipan and Lowry (1997). They use our method to compute real LCV scores for 1970–95. (The LCV began publishing House scores in 1970. Shipan has graciously provided us with the data and results of the study.) The findings of Shipan and Lowry are quite dramatic. By 1994 the mean member of the House and Senate had become as proenvironment as the mean Democrat in 1970. More interesting, for our purposes, however, is an answer to the following question: Did Congress's proenvironment preferences increase more or less than its general liberal preferences as measured by our adjusted ADA scores? While D-Nominate scores are not capable of answering this question, our method can.

In Figure 8 we compare the change in liberal preferences of the House with the change in environmental preferences. For each year of the 1970–95 period we compute the mean adjusted LCV and ADA score for the chamber. Next, for both means we record the percentage of members in the 1971 House who had lower adjusted scores by each respective measure. These percentages are shown in the figure. Proenvironment preferences indeed increased more than liberal preferences. For instance, the mean member in 1994 was more proenvironment than 61% of the 1971

members,<sup>22</sup> while the mean in 1970 was more proenvironment than 49% of the 1971 members, producing a 12% change over the twenty-four-year period. Meanwhile, liberal preferences, as measured by ADA scores, showed much less change. The mean in 1994 was more liberal than 59% of the 1971 members, and the mean in 1970 was more liberal than 55% of the 1971 members, producing a change of 4%. Thus, the LCV change was about three times as great as the ADA change.

It would be interesting to conduct the same analysis on other aspects of the liberal agenda, such as labor policy, civil rights, gender issues, and civil liberties. Ultimately, such an exercise might help reveal the congruence between legislative voting behavior and voter preferences. That is, for instance, if voters' attitudes, as revealed by surveys, show more change on labor policy than gender issues, it would be interesting to see if legislators' voting behavior does the same. We leave such exercises to future work. Most important, however, this section shows that such an exercise *can* be executed with our method.

### ALTERNATIVE METHODS FOR MAKING INTERTEMPORAL COMPARISONS

As noted above, we are not the first to provide intertemporal measures of congressional preferences. In fact, our work can be seen as a second-generation model, following on the work of Poole and Daniels

<sup>22</sup> We use 1971 as a measuring stick because it is the earliest year for which we have a near complete set of scores. Other early years produce similar results.

(1985). Like us, they use interest group scores as dependent variables and estimate fixed effects for members of Congress, which they interpret, like us, as estimates of preference.<sup>23</sup> At least two key differences exist between our techniques, however. First, although Poole and Daniels assume interest group scales are constant from year to year, they allow interest groups to change their preferred positions. In terms of estimating member preferences, this has the same effect of allowing scales to shift over time. Yet, Poole and Daniels do not allow interest groups to have different preferences for different chambers. In terms of our model this has the effect of constraining  $a_i^H$  and  $a_i^S$  to be the same each year, a constraint that our joint test rejects. Since our assumptions differ about interchamber scales, it is not surprising that our results regarding interchamber comparisons of preferences are different: Poole and Daniels find that the Senate is usually more liberal than the House, while we find that the Senate is usually more conservative. In fact, in their 1959–80 sample they find that the Senate is more conservative in only one year, 1960.<sup>24</sup>

Poole and Daniels also assume that interest group scales do not compress or expand. In terms of our model this constrains each  $b_i^c$  to be 1.0. Again, the joint test that we perform rejects this hypothesis. Consequently, it is not surprising that our results differ in terms of polarization of the parties. While we find that the mean preferences of Democrats and Republicans diverged during the period, Poole and Daniels find that party differences remained approximately constant.

Poole and Rosenthal's (1991) D-Nominate scores also measure intertemporal preferences of Congress. Like our method, we consider D-Nominate as a second-generation technique springing from Poole and Daniels (1985). Consequently, we see our work more as a sibling to the D-Nominate technique than as a next-generation improvement. Accordingly, it is fair to ask: "Why not just use D-Nominate scores?"

For researchers interested in measuring aggregated liberal/conservative preferences in Congress, there is little advantage in using our scores instead of D-Nominate. D-Nominate, adjusted ADA scores, adjusted ACU scores, and first-dimension scores from Heckman and Snyder's (1997) linear factor model are all highly correlated. It therefore does not really matter which scores one uses. The primary point of our article is that *nominal* scores of the ADA (and other interest groups) do not appropriately measure intertemporal preferences. Poole and Rosenthal were aware of this, and part of their motivation for introducing D-Nominate was to create a measure that overcame this weakness. Nevertheless, researchers continue to ignore

the problem and use nominal ADA scores to measure intertemporal preferences.

There are some advantages of our method over D-Nominate. The main one, as the previous subsection demonstrates, is that our method can be and has been used on particular issue dimensions, not just on the broadly based liberal/conservative dimension.<sup>25</sup> This is important, because although a surprisingly large number of particular issue dimensions are highly correlated with one another, and thus highly correlated with liberal/conservative preferences, they are not perfectly correlated (see especially Clausen 1967, 1973; Clausen and Cheney 1970; Heckman and Snyder 1997; Wilcox and Clausen 1991).<sup>26</sup> For some issues, such as agriculture, trade, and civil rights, the correlations are often small. More important, the degree of correlation changes over time. For example, while preferences for government management (economic policy) were strongly correlated with preferences for civil liberties during the 91st–95th Congresses, they were only weakly correlated during the 83rd–88th Congresses (Wilcox and Clausen 1991).

Another advantage of our method is that, unlike D-Nominate, it does not constrain members' preferences to change linearly. (With D-Nominate, if a member's preferences change, they must change the same amount each year. With our method, a member's preferences can change in any fashion.) This is especially important for cases in which a researcher hypothesizes that preferences do not change so smoothly. For instance, suppose a researcher believes that senators tend to vote their own ideology in nonelection years, but then moderate their voting toward their constituents in election years. A researcher cannot test this with D-Nominate scores. If a senator's score changes, D-Nominate forces it to change the same amount in election years as nonelection years. Other issues that cannot be tested by D-Nominate but can be tested by our method include: (1) Is there a last year effect? That is, do senators and House members vote differently when they know they will never face reelection? (2) Do members change their voting behavior after a near-defeat in an election? (3) Do House members change their voting behavior after their district lines are redrawn? (4) Did southern senators and House members change their voting behavior after black voters were enfranchised by the 1965 Voting Rights Act? (5) Is there a socialization effect? That is, do members vote differently in their first few years in office than in later years? (6) Do members change their voting behavior

<sup>23</sup> Although their technique measures preferences along other dimensions besides the Left-Right dimension that the ADA measures, when the Poole and Daniels technique is constrained to measure only one dimension, it is similar to ours.

<sup>24</sup> We reconducted our analysis, restricting it only to the 1959–80 sample to see if this is the reason our interchamber results differ from Poole and Daniels. That does not appear to be the reason. Using the restricted sample, the Senate appears even more conservative relative to the House than the original sample. In addition, in contrast to our results, Poole and Daniels do not find as strong a liberal trend in House and Senate preferences as we do over the 1960–80 period.

<sup>25</sup> In theory Nominate (and D-Nominate) also can be computed for a particular issue dimension. To do this a researcher would simply choose a subsample of roll calls (such as all environmental roll calls or all agricultural roll calls) rather than all roll calls, as data to compute the scores. Criticisms involving the consistency of Nominate scores (see below) would be especially relevant in this case, however, since the number of roll calls would be small compared to the number of legislators. As far as we are aware, no researcher has computed Nominate scores for such a narrow issue dimension.

<sup>26</sup> In fact, there is evidence that scores from narrow interest groups such as these are especially vulnerable to scale shifts and stretches. For instance, Cox and McCubbins (1993) note that the scores of the National Taxpayers' Union, the National Farmers' Union, and the United Auto Workers fluctuate much more wildly than one should expect from true ideological changes of members of Congress.

after switching parties?<sup>27</sup> (7) Do members change their voting behavior after they become a committee chair or party leader?

Another advantage of our method over D-Nominate is that the researcher knows exactly the dimension on which s/he is measuring preferences. Specifically, it is the liberal/conservative dimension, as the ADA defines it. In contrast, D-Nominate measures preferences along the dimension that best explains the roll call data it examines. This is not necessarily the liberal/conservative dimension. In fact, since D-Nominate does not examine the substance of bills, the dimension, at least in theory, could be something else, such as region or class, or a particular preference dimension, such as agriculture, environment, or labor.

There are also methodological criticisms of D-Nominate scores that do not apply to ADA scores.<sup>28</sup> From our point of view the most important is Londregan's (1996) proof that D-Nominate and its precursor, Nominate, produce statistically *inconsistent* estimates of representatives' ideologies.<sup>29</sup> While this is not likely to be a problem in practice for large bodies such as the House and Senate, it does limit the utility of D-Nominate for studying smaller bodies, such as the Supreme Court, the California state senate (40 members), or legislatures in which party discipline is so great that the number of "independently acting" legislators is small.<sup>30</sup> For the study of such bodies, the specialized bill selection done by interest groups may be crucial in producing reliable year-by-year scores. In many cases these scores exist—for example, the ADA and a variety of other groups publish scores for the California Assembly and state Senate.

Finally, D-Nominate does not attempt to place the House and Senate on the same scale. As a consequence, interchamber comparisons are inappropriate with D-Nominate. In fact, as far as we are aware, we are the first to provide measurements that allow House-Senate comparisons.

Despite the different methods used, the intertemporal patterns in the D-Nominate scores and our adjusted ADA scores are remarkably similar. Graphs of average and median D-Nominate scores show the same pattern

of peaks and valleys as in figures 3 and 4. Also, graphs of D-Nominate scores show similar patterns of polarization in congressional parties beginning around 1960. Both scores also exhibit similar patterns when disaggregated by region and party (in the interest of space we do not present detailed results of these breakdowns). For example, both scores show that southern Democrats slowly gravitated toward northern Democrats during the 1970s and 1980s, and that southern Democrats were more liberal than northern Democrats before 1950. These similarities are reassuring. To the extent Poole and Rosenthal's method is sound, our results are supported. Likewise, to the extent our method is sound, Poole and Rosenthal's results are supported.

Still, there are some differences in our results. Probably the most substantial is that D-Nominate scores do not exhibit as strong a liberal trend in congressional preferences as do the adjusted ADA scores. Although the D-Nominate scores indicate that Congress followed a general liberal trend between 1947 and 1990, the Senate trend appears to have leveled out between 1959 and 1987. Also, the scores imply that the House actually became more conservative over the period, whereas our results indicate that both the House and Senate maintained their liberal trend. Furthermore, our results show a somewhat steeper trend over the 1947–59 period than do Poole and Rosenthal's.

These differences might be due to the different methods used to pin down congressional preferences. The task faces a problem akin to the Newtonian relative velocity problem. If an object is traveling in a certain direction relative to the rest of the universe, then its motion will be observationally equivalent to remaining still while the rest of the universe moves in the opposite direction. Without an outside reference point, there is no way to distinguish which is moving, the object or the universe. Our method for measuring congressional preferences faces a similar problem. It is possible that each member of our sample is moving, say, one adjusted ADA point per year more than our results show, while at the same time the ADA is shifting its scales one point per year more than our results show. Without an outside reference, there is no way to distinguish between this case and our actual results.

D-Nominate scores face the same problem. The ideal points of each legislator in Poole and Rosenthal's sample could be moving one unit per year more than Poole and Rosenthal record, while at the same time, roll call coordinates are also moving one unit per year more than they record. To subvert this problem, Poole and Rosenthal constrain the ideal point of each legislator so that it cannot drift too far outside the ideal points of the other legislators. Specifically, in a first stage for estimation, Poole and Rosenthal allow each legislator's score to follow a linear trend (and in a separate estimation, to follow a quadratic trend), but legislators who drift to the edge of the space are constrained not to move at all in the second stage. Effectively, this imposes the assumption that extreme members of their sample do not move. For their House

<sup>27</sup> Poole and Rosenthal (correspondence with Poole, spring 1998) actually have used their scores to examine this hypothesis. To do this they recompute their scores, treating the party switcher as two different members. The standard version of the score cannot test the hypothesis.

<sup>28</sup> Of course, there are also criticisms of ADA and other interest group scores that do not apply to D-Nominate scores, such as those noted in Fowler (1982) and Snyder (1992). The two must be weighed.

<sup>29</sup> Heckman and Snyder (1997) make a similar argument. Ladha (1991) criticizes the behavioral and informational assumptions underlying Nominate. Also, Koford (1989), Wilcox and Clausen (1991), and Snyder (1992) have criticized certain inferences drawn from analyses using Nominate, especially the claim that congressional roll call voting is just one- or two-dimensional.

<sup>30</sup> Studies that use votes of the Supreme Court to measure the ideology of justices include Baum (1988), Epstein et al. (1995), Segal and Cover (1989), Spaeth (1979), and Tate (1981). (See Epstein et al. for other citations.) Many of the works in this literature follow the sins of the congressional literature and do not attempt to correct for shifting scales; Baum (1988) and Epstein et al. (1995) are notable exceptions.



estimates the constraint was binding for 4.2% of the members. In contrast, we constrain every member's mean preference parameter ( $x_i$ ) to be constant across time (although *actual* adjusted scores may vary). We are comforted by the fact that Poole and Rosenthal find only minuscule movements in the estimated scores of nonconstrained legislators.<sup>31</sup> At the same time, this suggests a possible reason Poole and Rosenthal do not find as strong a liberal trend as we do. To the extent that extreme legislators have been moving faster in the liberal direction (or more slowly in the conservative direction) than moderate legislators, nonextreme legislators will appear to be moving in the conservative direction. If this is the case, then Poole and Rosenthal's method will understate a liberal trend. Of course, if the opposite is true, their method will overstate a liberal trend. Our method falls prey to similar criticisms.<sup>32</sup>

## DISCUSSION

As the evidence makes clear, the ADA and other interest groups do not keep a constant scale from year to year or from chamber to chamber. Not only is there significant idiosyncratic variation from year to year, but also the ADA's scales have trended in a liberal direction for most of the postwar period. As a consequence, the scores must be adjusted before one can make proper intertemporal or interchamber comparisons. Furthermore, as our empirical applications demonstrate, the scale adjustments matter substantively: The choice to use nominal or adjusted scores can significantly affect the conclusions in an empirical study.

Furthermore, this is a potential problem for *all* interest group ratings. Accordingly, there is much room for future work to provide conversion indexes for other interest group scores.<sup>33</sup> Such conversion indexes would pave the way for what we think are many interesting avenues for future study.

First, it would be valuable to see how many grand areas of government policymaking have changed over time. For instance, do federal budgets tend to become larger as Congress becomes more liberal? The same can be asked about particular areas of spending. For instance, do agriculture subsidies rise when adjusted National Farmer's Union scores rise? Do tax rates decrease when National Taxpayers' Union scores in-

crease? Similar ideas can be applied to measures of government regulation. Does environmental regulation respond to the median adjusted score from the LCV? Does the minimum wage respond to the median adjusted score from the AFL-CIO?

Finally, we think our scores can improve many studies that test the influence of various political institutions. These studies typically adopt a particular policy outcome as a dependent variable, then examine how the policy changes over time as the preferences of political actors within various political institutions change. This, in turn, helps reveal the influence of the institutions. Studies such as these would clearly benefit from an improved method for tracking political preferences over time.

## APPENDIX

Two key assumptions of the model allow us to link scales across time and chambers. One is that each member's mean preference parameter,  $x_i$ , is fixed throughout time. Another is that  $x_i$  does not change even if the member switches chambers.

It is important to note that we *do not* assume that members' preferences—that is, their adjusted scores—are fixed. By manipulating equations 1 and 3, one can show that adjusted scores equal  $x_i$  plus an error term. Because of the error term, adjusted scores can and do vary from year to year. Nevertheless, this implies restrictions upon the *way* preferences can change. Namely, the deviations of members' adjusted scores from their mean must be idiosyncratic. For our purposes this has two key implications: The deviations must not be correlated with time, and they must not be correlated with chamber switches.

We first consider the possibility that deviations are correlated with time. This occurs, for instance, if a member's adjusted score trends in the conservative or liberal direction. Of course, such a case for an individual member does not necessarily bias our results for *aggregate* preferences. For instance, if just as many members trend in the liberal direction as in the conservative direction (and to equal degrees), then this will not bias our measurements of mean and median preferences, such as those in figures 3 and 4.

But what if time trends among members do not cancel out and instead there is a tendency for the average member to trend in a particular direction? For instance, suppose the adage is true that humans tend to become more conservative as they age, and that this applies as well to members of Congress. Or, in contrast, suppose that members in our sample tended to follow the attitudes of voters, who seemed to become more liberal during the period we study.<sup>34</sup> Either case could bias our results. If there was a general tendency for members to become more liberal (that is, due to changes in personal ideology, not to new members replacing them), then our method will *understate* the liberal drift in Congress. Likewise, if there was a general tendency for members to

<sup>31</sup> Of course, this should be interpreted that they find only minuscule movements of nonextreme legislators relative to extreme ones.

<sup>32</sup> Another difference between our method and D-Nominate is that the latter uses every roll call vote in a Congress, while our method uses only the twenty or so that the ADA chooses. Potentially, this could cause a sample selection bias for D-Nominate, since its sample is affected by gatekeeping institutions, such as committees, the House Rules Committee, and Senate filibusters (which produce a nonrandom sample of roll calls from the *population* of potential roll calls), while the ADA can monitor its sample so that it has a fairly constant mix of issues in its votes. Yet, if obstructionists are more effective one year than another and one's primary concern is with aggregate preferences of Congress, one may actually desire the measure of preference to reflect this. Accordingly, this might be a strength rather than a weakness of D-Nominate. We are grateful to an anonymous referee for directing our attention to this.

<sup>33</sup> For this effort we are happy to provide copies of the Matlab program that we have written, which can be accessed from <http://wesley.stanford.edu/groseclose>.

<sup>34</sup> Consistent with our measures of aggregate preferences in Congress, despite the slight increase in self-identified conservatism among the general electorate, on many important social policy issues (that are reflected in roll calls selected by the ADA), including attitudes toward sexual and racial equality and tolerance for minorities such as gays and lesbians, voters' attitudes became much more liberal between the late 1940s and mid-1990s.

become more conservative, then our method will *overstate* the liberal drift.<sup>35</sup>

We conducted additional analysis that suggests, however, the potential bias from this problem, if any, is small. Instead of requiring each member to maintain the same mean preference parameter throughout his or her entire career, we estimated a different  $x_i$  for every ten years that s/he served.<sup>36</sup> This caused no significant change in the results on aggregate preferences. Similar time trends appeared, and except for a handful of early Senate years, the patterns of means and medians were virtually identical to those in figures 3 and 4.<sup>37</sup>

This is also supported by previous research. As Stone (1980) shows, it is rare for *any* member to have a significant drift in ideology. Accordingly, it is even more doubtful that individual drifts will be large enough and correlated enough to aggregate into a general tendency. This sentiment is supported by Poole and Rosenthal (1991, 228):

In the modern era, spatial positions are very stable. . . . Since the end of World War II, changes in congressional

<sup>35</sup> Another concern for bias is socialization norms among members of Congress. This may cause members' behavior to be correlated with one another, which could violate our independence assumption about the error term. The literature on socialization norms makes very little mention of any effect on ideology, however. (See, for example, Asher 1973.) Furthermore, even if there were such an effect, it would not bias our results unless it caused a general tendency for members to shift in the same direction. On this the literature has reached no consensus. We are aware of only two works that find evidence of socialization norms influencing ideology in a particular direction, and these works disagree on the direction. Poguntke (1993) finds that socialization caused a conservative movement among members of the very left-wing parties of the German parliament, while Reed and Schansberg (1993) find that socialization caused a slight, albeit statistically significant, liberal movement (i.e., toward greater federal spending) among Republicans in the U.S. Congress. (House and Senate Democrats in their study moved in opposite directions.)

<sup>36</sup> In principle, one could use time periods other than ten years for this analysis. Much longer periods make the method virtually identical to the original analysis. Much shorter periods cause large increases in the standard deviations of  $a_i^c$ 's and  $b_i^c$ 's. The latter happens because identification of  $a_i^c$ 's and  $b_i^c$ 's is better when there is more overlap in membership between any two years. In the limiting case where each member's  $x_i$  changes every Congress (or worse, every year),  $a_i^c$ 's and  $b_i^c$ 's cannot be identified at all. The reason is that for each change in a member's  $x_i$ , s/he is in effect a different person. When  $x_i$  changes every Congress, there is no overlap in membership between any two Congresses. As a consequence, parameters cannot be identified.

<sup>37</sup> The Senate means between 1947 and 1956 were four to six points greater than they were in the original analysis. The reason is as follows. The tightest estimates of  $a_i^c$  and  $b_i^c$  occurred in the House. This was true not only because there are more members in the House than the Senate but also because we chose the House as the base chamber. (Because we set  $a_{1980}^H$  to zero, for any  $a_i^c$  we are really estimating the difference between  $a_i^c$  and  $a_{1980}^H$ . As a consequence, standard errors are smaller, the more overlap there is with the 1980 House.) The next tightest estimates of  $a_i^c$ 's and  $b_i^c$ 's were Senate years that had a large overlap in membership with the House. Early Senate years tended to yield the least precise estimates, since almost none of these members served in the post-1947 House. In the original analysis this was not a large problem, since senators in the early years of our sample served in later Senates, and these had large overlaps with the House. Thus, although the direct overlap with the House in early years was small, at least there was an indirect overlap, but this virtually disappeared when we estimated a different  $x_i$  for each ten-year period. The technique effectively treated each ten-year period of service as if the member were a different person. This, in turn, drastically reduced the overlap between early and later senates, which caused the estimates of  $a_i^c$ 's and  $b_i^c$ 's for early Senates to be imprecise. Consequently, it is not surprising that these estimates differed so much from the estimates of the original analysis.

voting patterns have occurred almost entirely through the process of replacement of retiring or defeated legislators than with new members. Politically, selection is far more important than adaptation.

Next, we consider violations of the second restriction and allow deviations to be correlated with chamber switches. This occurs, for instance, if a member's voting behavior becomes more conservative or more liberal after s/he switches from House to Senate. Of course, it is natural to expect members' voting behavior to change when they make such a switch; after all, they switch constituencies. Such cases do not necessarily bias our results on aggregate preferences, however. The results are biased only if there is a *general* tendency for members to become more conservative or a *general* tendency for them to become more liberal when they switch chambers.

We conducted additional analysis that suggests, however, even if members do switch ideologies when they switch chambers, the bias to our results, if any, is small. Specifically, we reestimated  $a_i^c$ 's and  $b_i^c$ 's while relaxing the assumption that each member maintain the same  $x_i$  when s/he switches chamber. Instead, we required only that members from *representative* districts keep the same  $x_i$  when they switch from House to Senate. For all other switchers we estimated two  $x_i$ 's—one for their House years and another for their Senate years.

For this analysis we defined *representative* as follows. For each member in the sample who switched chambers we recorded the district vote for the Democratic candidate in the most recent presidential race prior to the switch. Also, we recorded the state vote for the same presidential race. If the absolute difference between the state and district vote was less than 3%, we defined the district as representative. We chose 3% as the criterion because about half the districts with House-Senate switchers (51 out of 121) satisfied this criterion.

The results for this analysis were almost identical to the original results. In no year did the Senate mean change by more than 1.5 points from the original mean, and in most years the change was less than .75 point. Next, as one might expect, the change in House means was even less. For most years the change was less than half a point, and in no year was the change more than one point. Finally, estimates of inter-chamber differences also hardly differed. In the original analysis the Senate on average was 4.46 points more conservative than the House. In the later analysis this changed only slightly, to 4.31 points more conservative. Because the changes are so slight, we are confident that the analysis is not biased by the restriction that each member maintain the same  $x_i$  when s/he switches chamber.

Furthermore, previous research finds little evidence that members change ideologies in either direction when they switch constituencies, much less that there is a general tendency for them to switch in a particular direction. One reason is that a House member with progressive ambition to the Senate should begin to vote the way the whole state prefers *before* s/he obtains the Senate seat (Rohde 1979). Another reason is that such ideological fickleness is politically costly. Politicians who significantly change their voting record lose credibility with voters (Bernhardt and Ingberman 1985). Stone's (1980) empirical research supports these theoretical accounts. He reports that House members do not change their voting behavior in response to redistricting. Glazer and Robbins (1985) find a statistically significant effect of redistricting on voting scores, but even the effect they identify is extremely small (and their estimate is the largest in the literature).

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