Advance Access publication March 24, 2016

Political Analysis (2016) 24:172–188
doi:10.1093/pan/mpw003

Anchors Away: A New Approach for Estimating Ideal Points Comparable across Time and Chambers

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Existing methods for estimating ideal points of legislators that are comparable across time and chambers make restrictive assumptions regarding how legislators' ideal points can move over time, either by fixing some legislators' ideal points or by constraining their movement over time. These assumptions are clearly contradictory to some theories of congressional responsiveness to election dynamics and changes in constituency. Instead of using legislators as anchors, our approach relies on matching roll calls in one chamber and session with roll calls or cosponsorship decisions on identical bills introduced in a different chamber or session. By using these "bridge decisions" to achieve comparability, we can remove any assumptions about the movement of legislators' ideal points. We produce these estimates for both chambers from the 102nd (1991–92) to 111th (2009–11) Congresses, and we show that our estimates provide interesting insights into the nature of legislative behavior change.

1 Introduction

The ideal point estimation techniques developed in the past two decades have added significantly to our understanding of the ideology of members of Congress, partisan cleavages, and the dimensionality of the issue space. The micro-foundation of these estimation techniques in the spatial model has also given researchers the confidence that these estimates, based on explicit and defensible assumptions, are suitable for quantitative analyses of legislators' behavior. However, in contrast to the theoretically grounded assumptions needed to generate ideal points within a single session of Congress, the assumptions normally made to allow for comparability of ideal points across time and chambers are not well grounded in theory, and in most cases are clearly contradictory to theories of congressional responsiveness to electoral dynamics and changes in constituency. This is particularly troubling because most uses of ideal point estimates for empirical testing or even simple description involve comparisons over time.

Existing methods for estimating ideal points across time and chambers using roll-call votes make restrictive assumptions regarding how legislators' ideal points can move over time, either by fixing some legislators' ideal points or by constraining their movement, usually by restricting them to linear movement in one direction.[1] These restrictions present two problems. First, they preclude the finding of the sort of abrupt behavior changes we would expect from members of Congress responding to electoral dynamics (e.g., a primary challenge) or changes in constituency

Authors' note: We thank Josh Clinton, Christian Grose, Alexander Hirsch, Michael Peress, Adam Ramey, Steve Rogers, Larry Rothenberg, Jungkun Seo, and the coeditor and the three anonymous reviewers for insightful suggestions. Replication data are available on the Political Analysis Dataverse at http://dx.doi.org/10.7910/DVN/4IOIWV.

[1] One exception is Treier (2011). Although there are no restrictions on the movement of legislators' ideal points, the ADA, the ACU, and the president are treated as if they are members of Congress, and their ideal points are assumed to be constant.

(e.g., redistricting). Second, they obscure the effect of national trends that involve many members moving in the same direction (e.g., in response to a national security crisis), because the shift in ideal points coincides with a shift in the scale on which the ideal points are measured. As a consequence, existing roll-call-based ideal point estimation techniques are not adequate for testing theories of legislative behavior change because the findings (or lack thereof) are an artifact of the estimation technique. Moreover, existing estimates muddle our understanding of Congress. By depending on legislators as anchors, who may themselves be drifting, they present us with a description of Congress that has too many moving parts. Neither the members' ideal points nor the meaning of the scales can be said to be truly stationary.

The approach presented in this article does not depend on the use of legislators or any other actors as anchors, as existing methods do. Instead, we make the agendas comparable across time and chambers by matching roll calls in one chamber and session with roll calls or cosponsorship decisions on identical bills in a different chamber or session, making use of the fact that many bills are introduced in identical form in both chambers and over multiple sessions, a subset of which receive a floor vote. By identifying the bills common to more than one chamber or session to use as anchors, we can remove any assumptions about the movement of legislators' ideal points. Furthermore, the meaning of these ideal points is easier to interpret because the scale on which they lie is stationary.

This article demonstrates the utility of this approach by presenting ideal point estimates for both chambers of the 102nd–111th Congresses (1991–2010). We show that these estimates provide interesting insights into the nature of legislative behavior change in response to electoral dynamics that could not be discovered using traditional methods. After categorizing and describing existing strategies for estimating ideal points comparable across chambers and time, we introduce our method of using identically introduced bills. In presenting the results of our estimation, we use several examples of ways our ideal point estimates are uniquely suited to illustrate legislative behavior change, by comparing the pace of polarization in the House and the Senate, following the ideological trajectories of individual senators, looking at changes in ideological behavior among members who served in both the House and the Senate, and examining the abruptness of ideological shifts among party switchers. In doing so, we find evidence that, in comparison to the prominent Common Space DW-NOMINATE scores, the magnitude and pace of polarization is actually being understated, and we also present findings that undermine the assumption that members of Congress "die in their ideological boots."

2 Existing Methods

The techniques used to make estimates comparable across time and chamber can be separated into three categories: those that hold all legislators' ideal points fixed over their careers, those that hold some fixed and allow others to float, and those that allow legislators' ideal points to move along a trendline. We discuss methods falling into each of these categories as well as their susceptibility to two major problems: the inability to account for abrupt behavior changes and the obscuring of shifts in ideal points that move many members in the same direction.

2.1 *Fixed Career Method*

One assumption that ensures comparability is that legislators maintain the same ideal point for their entire career; even members who serve in both chambers are assumed to have the same ideal point in the House and the Senate. This is the assumption that underlies Poole and Rosenthal's Common Space scores.[2] Although this method places the most restriction on legislators' movements, these estimates are commonly used to compare levels of polarization in the House and the Senate and to calculate gridlock intervals. Obviously, the assumption that ideal points are fixed

[2]These are the only ideal point estimates in the NOMINATE family that are comparable across chambers. For Poole's explanation of the differences between D-NOMINATE, DW-NOMINATE, W-NOMINATE, and Common Space scores, see http://voteview.com/page2a.htm.

means that these estimates are useless for studying changes in individual behavior, since the estimated ideal point of each legislator is simply an average of their voting behavior over their entire career. Neither can these ideal points capture shifts in ideology that move many members in the same direction since they can only measure a member's ideology relative to the other members serving at the same time. As Bailey (2007) points out, this deficiency leads to the substantively questionable result that pro-segregationist members of Congress from the 1950s have ideal point estimates in the same neighborhood as moderate Democrats of the 1990s.

Related to the "fixed career" estimation strategy, the "shift and stretch" method of Groseclose, Levitt, and Snyder (1999) assumes that each legislator has an unobserved constant ideal point throughout their career, but that this ideal point is measured with error. The task of making session- and chamber-specific ideology scores comparable is accomplished by minimizing these errors.[3] Although allowing for movement across time, the fundamental assumption of time-invariant ideal points militates against uncovering changes in legislators' voting behavior between sessions, and the authors readily admit that their method cannot pick up on universal shifts in members' preferences because this would coincide with a shift in the ideological scale (47).

An additional approach to mention here is used by Bonica (2014) to create CFscores. This method eschews using the roll-call decisions of legislators but instead uses the campaign contribution decisions of donors to estimate ideal points. But it also makes the familiar assumption that candidates for office have a fixed ideological position, as well as the donors. In order to validate the estimation technique, Bonica relaxes the assumption of fixed career ideal points, and shows that very little session-to-session variation occurs, but to do so, he must keep the assumption that donors have a fixed ideal point.

2.2 *Fix and Float Methods*

A less restrictive approach to making estimates comparable is to fix the ideal points of some legislators, while allowing all other legislators to "float." A classic example of this involves fixing the ideal points of two extremists, say Senators Ted Kennedy and Jesse Helms, to -1 and 1, and estimating a separate ideal point for each legislator for each session relative to the career ideal points of these two legislators. The "fix and float" method is also used by Poole and Rosenthal in computing NOMINATE scores for legislators who switch parties within a session: a pre-switch and post-switch ideal point is estimated for the party-switcher, while all other members' ideal points are held constant. Treier's (2010) overlapping constraints method makes use of this technique to make senators' ideal points comparable across time, by fixing members' ideal points in the first 4 years of the term, but estimating a separate ideal point for the last 2 years. Because of the staggering of elections in the Senate, it is always the case that one-third of senators are held fixed while the other two-thirds are allowed the change. Although "fix and float" methods usually are only applicable to over-time comparisons, cross-chamber comparability can be achieved by fixing the ideal points of actors who take positions on roll calls in both chambers, such as the president and interest groups, as in Treier (2011).

Estimates produced by the "fix and float" method seem well suited for capturing abrupt changes in legislative voting behavior since no restrictions are placed on the movement of the ideal points of the floating legislators. However, two caveats are in order. First, much hinges on the assumption that the fixed legislators' ideal points are actually constant. Treier (2010) finds that even such firm ideologues as Kennedy and Byrd have shifted their ideological positions in response to electoral cycles—a cautionary example that should make researchers wary of establishing comparability from a small number of fixed legislators. A second difficulty is establishing a baseline with which to compare changes in legislators' ideal points. For example, if only legislators suspected of changing ideal points (e.g., party-switchers) are allowed to float, while all others are fixed, then

[3]Groseclose, Levitt, and Snyder (1999) are concerned with generating comparable ADA scores. Ansolabehere, Snyder, and Stewart (2001) apply the "shift and stretch" technique to ideal point estimates constructed with the Heckman and Snyder (1997) method, and Ensley, Tofias, and de Marchi (2014) apply the technique to Poole and Rosenthal's (1997) static W-NOMINATE scores.

the finding of a significant change in the ideal point of the floaters lacks a relevant reference point because the change in all other members' ideal points is by assumption required to be zero.[4]

Furthermore, the "fix and float" method cannot properly account for universal shifts in members' ideal points. Since these shifts in preference can only be registered as changes in the ideal points of the floating members, the cause of the movement of ideal points may be erroneously attributed to the characteristic that separates the floating legislators from the fixed legislators. Consider the following example in which we wish to determine the effect of redistricting on legislators' voting behavior. We estimate legislators' ideal points pre- and post-redistricting, and to ensure comparability across time, we fix the ideal points of the legislators whose district boundaries remained the same. Suppose that in the true data-generating process, redistricting has no effect, but there is a national trend that pulls all legislators two units to the right. We would estimate the non-redistricted members as having an ideal point at the average of their pre- and post-redistricting ideal point (i.e., one unit to the right of their initial ideal point), while redistricted members' second-period ideal points are estimated to be two units to the right of their first-period ideal points. Therefore, what appears to be an effect of redistricting is simply an artifact of the estimation technique.

2.3 *Trendline Methods*

A final category of estimation techniques restricts legislators' movements across time to a specific functional form. The most common restriction, used in Poole and Rosenthal's D-NOMINATE and DW-NOMINATE scores, allows for only a linear time trend in legislators' ideal points. Poole and Rosenthal dismiss estimations with higher-order polynomials as not worthwhile given the low gains in classification success at the cost of computational time (1997, 28–29). Bailey (2007) also makes use of time trends, allowing members serving 7–14 years to move with a linear time trend, those serving 15–20 years to move with a quadratic trend, and those serving more than 20 years to move with a polynomial trend of degree five.

One might think that the inclusion of polynomial trends would allow these estimates to capture abrupt changes in legislative behavior. However, as Treier (2010) points out, neither a linear nor a quadratic trend can imitate the kind of oscillating behavior we might associate with Senate election cycles. Figures 1 illustrates this point further by depicting situations in which legislators are commonly hypothesized to change their voting behavior, such as redistricting, moving from the House to the Senate, a primary challenge, retirement, or senatorial election cycles. In Figure 1, the solid lines represent the true ideal points of these hypothesized members. The dotted lines are the best-fitting linear trendlines, and the dashed lines are fitted trendlines based on polynomials of degree five. Although the linear trendlines are obviously inadequate to capture these types of movements, what is especially surprising is that even complex trendlines are not able to mimic these hypothesized movements. Except in the case of the retiring member, the high-ordered trendlines greatly understate the effect of the "treatment." It is no wonder, then, that Poole and Rosenthal (1997) find minimal benefits to upping the degree of the polynomial. Furthermore, even though polynomial trends are more suited to picking up on gradual rather than sudden changes in ideal points, trendline methods, like fixed career methods, still fail to capture universal shifts in legislators' ideal points because they can only account for relative changes.

Although Martin and Quinn's (2002) dynamic ideal point estimation technique does not rely on trendlines, we address it here because it has a flavor similar to trendline methods. Specifically, in Martin and Quinn's estimation, the current period's ideal point is modeled as a random walk from the previous period's ideal point, which means that the expectation of

[4]This is the shortcoming that Clinton, Jackman, and Rivers (2004b) identify in McCarty, Poole, and Rosenthal's (2001) analysis of party-switchers. While their criticism is conceptually right, their reanalysis of the effect of Jeffords's party-switching sidesteps the issue of cross-time comparability altogether by simply assuming that the distributions of pre-switch and post-switch ideal points both have mean zero and unit variance—in essence implying that no anchors are needed to make the estimates comparable.

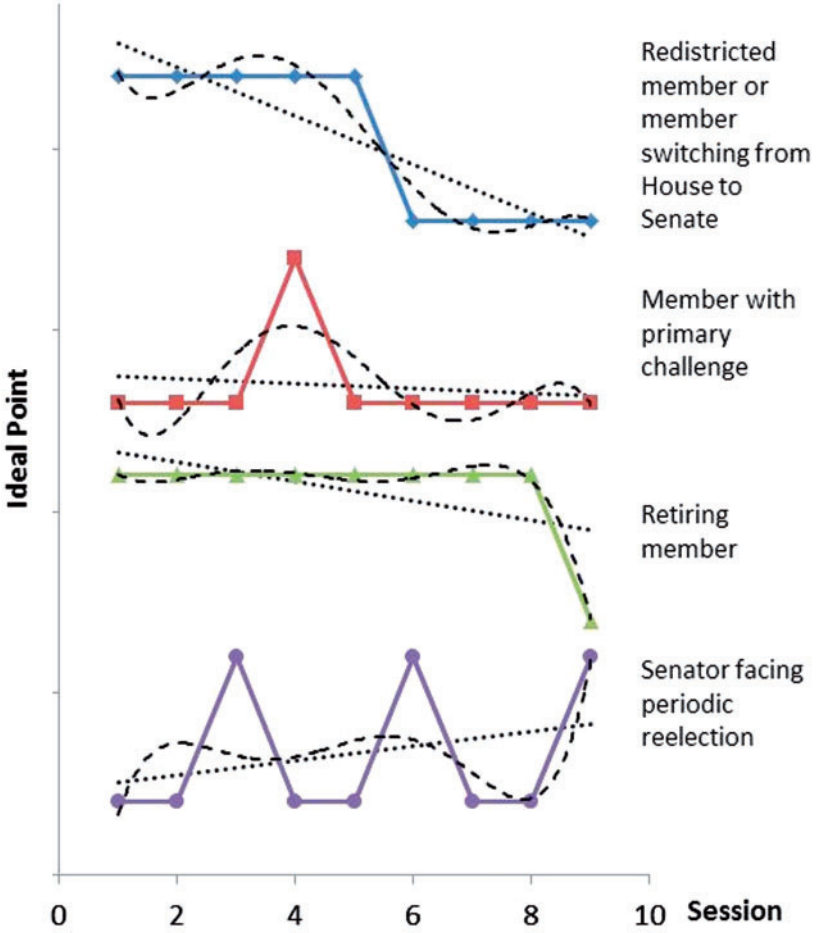

Fig. 1 Changes in legislators' ideal points with a linear trend (dotted) and polynomial trend of degree five (dashed).

one's ideal point in the current period is one's ideal point in the previous period. However, the amount of movement detected over time depends entirely on the choice of the prior distribution of the evolution variance parameters which determine the amount of smoothing, as the authors readily admit (2002, 147).[5] Thus, while we can use this method to see whether there is some solid evidence showing that people really change their ideological positions over time by assuming very small evolution variance parameter, it is not appropriate to measure how big such movements are.

3 Our Approach

The common denominator of existing methods for making roll-call-based ideal points comparable across time and chambers is the focus on the actors as anchors.[6] The problems of accounting for abrupt changes in legislative behavior and identifying universal shifts in members' ideal points

[5] In criticizing Martin and Quinn's (2002) scores for not controlling for agenda change, Bailey (2013) introduces various sources of bridging information to deal with agenda change. However, Bailey's method faces the same problem as Martin and Quinn (2002) do, in that the evolution variance parameter determines the extent to which ideal points can move.

[6] For methods that do not rely exclusively on the roll-call record for comparability, see Shor and McCarty (2011) and Battista, Peress, and Richman (2013). Both exploit surveys of legislative candidates that ask the same questions over time and across chambers.

cannot simply be solved by more computational power, higher-order polynomials, or cleverer fix and float arrangements. Instead, the focus must shift from fixing the actors to fixing the agendas. Recent analyses have incorporated bridge votes, usually votes on identical versions of conference reports, as a means to make House and Senate estimates comparable (Bailey 2007, 2013; Treier 2011), yet the lack of votes that bridge across time means that the sole use of roll-call votes as anchors is not enough to allow for the dropping of all restrictions on the movement of legislators' ideal points.[7]

Our approach connects the agendas over time by including legislators' decisions to cosponsor legislation. We make use of the fact that many bills are introduced in identical form in both chambers and in multiple sessions of Congress, a subset of which receive a floor vote in at least one session of a chamber. By matching the roll-call votes on bills in a particular chamber and session with cosponsorship decisions on identical bills introduced in another session or chamber, we can achieve comparability of agendas and do away with using legislators as anchors.

Although Mayhew (1974) once claimed that cosponsorship is a costless and therefore inherently symbolic activity, more recent studies have considered cosponsorship to be indicative of legislators' preferences and have employed cosponsorship data to characterize social networks in Congress and identify influential members (Fowler 2006), to estimate status quo positions of roll-call votes (Peress 2013), and to test theories of position-taking and party effects (Kessler and Krehbiel 1996; Krehbiel 1995). Moreover, Aleman et al. (2009) and Talbert and Potoski (2002) both use cosponsorship data to estimate ideal points of members of Congress and find that cosponsorship-based estimates correlate roughly with roll-call-based measures, although neither of these studies exploits identical, multiply introduced bills as a way to make comparisons across time and institutions as we do in this analysis, nor do they attempt to scale cosponsorship and roll-call decisions together.

The question of whether cosponsorship data can be treated in the same way as roll-call data is a challenging one. Bernhard and Sulkin (2013) give support for the notion that cosponsorship decisions ought to be taken seriously—members of Congress rarely renege (only about 1.5% of cosponsors vote no on final passage), and reneging members are subject to punishment. On the other hand, Harward and Moffett (2010) point out the paradox that roll-call support on final passage usually far exceeds the number of cosponsors, and Desposato, Kearney, and Crisp (2011) caution against using cosponsorship data to estimate ideal points because of the difficulty of determining the meaning of non-cosponsorship.

We deal with this challenge by interpreting the decision to cosponsor (or sponsor) a bill as equivalent to voting "yes," but we treat non-cosponsorship as missing information.[8] This means that the cosponsorship decisions on a bill in a particular chamber and session are not by themselves informative because the estimation procedure would treat the decisions the same as a unanimous vote. But if an identical bill receives a non-unanimous floor vote in another chamber or session, then the locations of the bill's cosponsors also help determine where the cutline falls (and vice versa) and hence how the ideal point estimates of each chamber session fit on the same scale. By matching roll calls with cosponsorship decisions, we can estimate comparable ideal points even while making minimal assumptions about what non-cosponsorship means.

[7] In addition to votes on conference reports, Bailey (2013) uses some inter-branch position-taking devices, such as amicus brief filings by members of Congress, and comments in the Congressional Record or cosponsorship decisions on bills related to court cases to help anchor the ideal point space, but these only help with cross-time comparability if the positions are taken in a different session than the court case. (Position-taking by justices on prior court cases also contributes to cross-time anchoring of the judicial branch that could have a carry-over effect on congressional ideal points.) Therefore, a substantial amount of the over-time anchoring is achieved through bridge actors, not bridge items, that is, by assuming a smoothing parameter that relates a member's ideal point to their ideal point in the prior period. Bailey admits that the choice of smoothing parameter is somewhat arbitrarily chosen such that the ideal points move across time "but not too dramatically" (827).

[8] For alternative approaches to modeling cosponsorship decisions, see Aleman et al. (2009), Desposato, Kearney, and Crisp (2011), Peress (2013), and Woon (2008).

3.1 *Data*

We apply our method to the 101st–112th Congresses.[9] We identified identical bills[10] introduced in the House and the Senate of the same Congress by browsing the Bill Summary and Status reports on the Library of Congress's THOMAS Web site (http://thomas.loc.gov), and collecting the "related bills" information. We identified identical bills introduced over multiple sessions in the same chamber by comparing bill descriptions in the Congressional Bills Project files (Adler and Wilkerson 2001–2004). To make data collection more manageable, we confined our focus to bills only (no resolutions or amendments), and only those bills that received a non-unanimous roll-call vote at any time during the period. This results in many "bridge votes" (i.e., roll-call votes on identical bills in both chambers, usually conference reports) and "bridge decisions" (i.e., combinations of roll-call and cosponsorship decisions that span more than one chamber or session), the totals of which are presented in Table 1 by Congress and the chamber.

The roll-call/cosponsorship matrix is constructed as follows. The rows of the matrix are the members of Congress and the presidents, with a separate row for each Congress that the member served. The columns of the matrix represent the roll-call votes, with three types of entries: the unique roll-call votes for each chamber for each session, the "bridge votes" which span both chambers of a single session, and the "bridge decisions" which span multiple chambers or sessions. Cosponsorship decisions are entered directly into the column of the roll-call vote that matches the identically introduced bill from a different session or chamber. The roll-call entries take the values 1 for yea, 0 for nay, and NA otherwise. The cosponsorship decisions take the values 1 for sponsorship or cosponsorship, NA for non-cosponsorship, and NA if the bill was not introduced in the member's chamber. We also include the president's position on roll calls in the House and the Senate as recorded by *Congressional Quarterly*. The resulting matrix, the top corner of which is depicted in Figure 2, has 6,549 legislators, 12 presidents, and 20,177 items. The black squares indicate the densely filled roll-call matrices, consisting almost entirely of 1s and 0s representing yea and nay votes, and the lined squares indicate the more sparsely filled areas where the only entries are 1s representing cosponsorship.

3.2 *Method*

One advantage of our approach is that once this matrix is constructed, ideal points can be estimated using any of the techniques appropriate for the estimation of static ideal points. We use the Bayesian simulation approach of Clinton, Jackman, and Rivers (2004b), which fits roll-call data with an item response model via Markov Chain Monte Carlo (MCMC) methods. This is easily implementable by anyone with a basic knowledge of R using the package *pscl* and the command *ideal*. We estimated a one-dimensional model, running 200,000 iterations, with a burn-in of 100,000, and a thinning interval of 200.[11] The estimation time was approximately 36 h.[12]

Note that we are not making any specific assumptions about legislators who served in multiple Congresses; we estimate their ideal points for each Congress separately. For example, if a bill was introduced in the 102nd Senate and the 102nd House, and voted on in the 103rd Senate, we essentially treat it as if members of the 102nd and 103rd Senates and the 102nd House voted on the bill at the same time. Also, if a senator served in both Congresses, we are treating him/her as

[9]For replication data and code, see Asmussen and Jo (2015).

[10]In practice, we only checked if bill descriptions were identical, and in the case of small discrepancies in bill descriptions, the bill texts were compared.

[11]To assess convergence, we simulated three independent sequences with starting points randomly drawn from an overdispersed distribution. We then performed the convergence test suggested by Gelman and Rubin (1992). The test result suggests that our MCMC has converged.

[12]The package *pscl* provides an option to identify ideal points in higher dimensions with some restrictions on model parameters. Specifically, d-dimensional ideal points can be identified by fixing d+1 legislators' ideal points that can span a d-dimensional space. Since Poole and Rosenthal (2007) show that the second dimension plays an important role in the early 1990s, we also estimated "Anchors Away" ideal points in two dimensions. The analyses of the 1st dimension of the two-dimensional ideal points produce the same implications regarding polarization, chamber-switchers, and party-switchers that are presented in the next section.

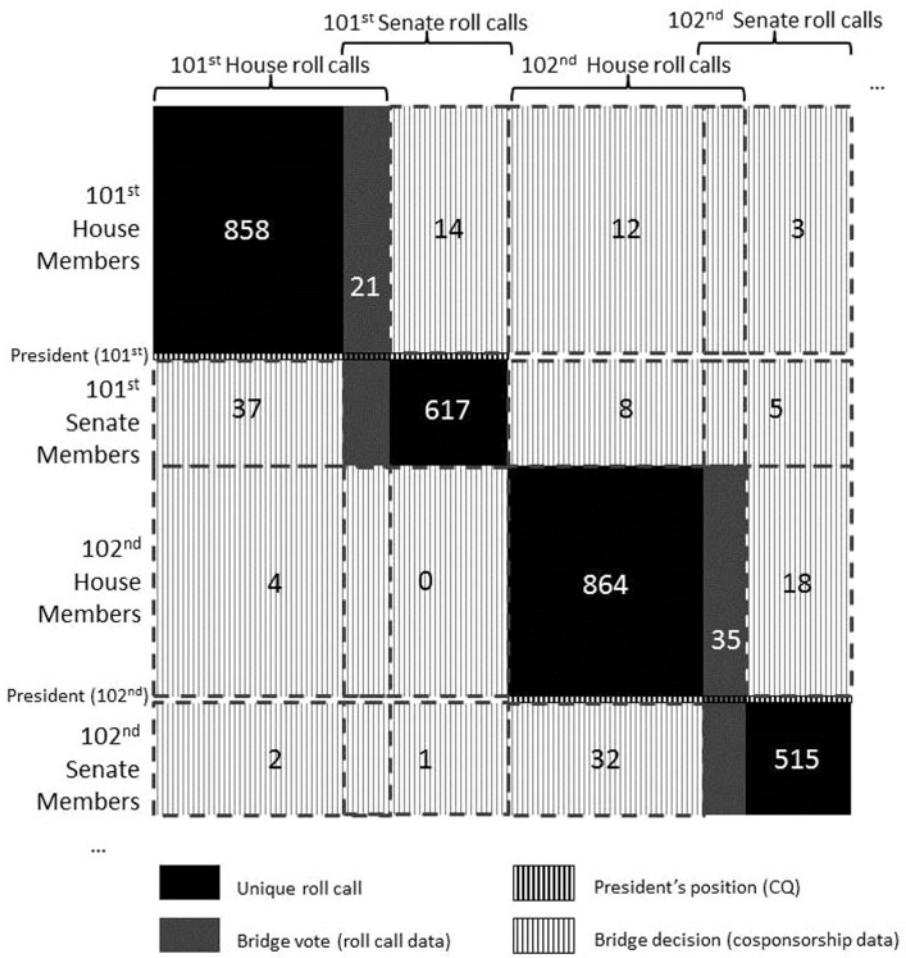

Fig. 2 Representation of the roll-call/cosponsorship matrix.

two different, independent senators. By not taking into account the fact that those two are the same person, we are losing some information that other approaches (e.g., Martin and Quinn 2002; Bailey 2013) use to smooth out individuals' ideological movements over time. However, by ignoring this information, we take the statistically conservative approach that avoids having to depend on an arbitrarily assumed smoothing parameter.

It is hard to find a general rule about how many bridge votes/decisions we need. If the bridge votes and bridge decisions are very different from each other and have a sufficiently high variance of the numbers of the "Nay" votes (i.e., they are not predominantly unanimous "hurrah" votes), a small number of them can effectively differentiate legislators. On the other hand, if the bridge votes/decisions show similar patterns of legislators' behavior, we will need many of them to estimate ideal points compatible over time and chamber. Thus, the bridge votes/decisions presented in Table 1 may or may not be enough for our purpose.

We approached this problem in two ways. First, we can confirm that the data we have are good enough by trying various choices of prior distributions over legislators' ideal points. That is, if we do not have enough data to connect different time periods and chambers, the relative ideological positions between different Congresses will be entirely driven by the choice of prior (for more explanation, see Gelman et al. 2004). To confirm that the results are from the data, not from the priors, we ran a simulation with various priors and confirm that the results are not sensitive to choice of prior. To be safe, we exclude the 101st (1989–90) and 112th (2011–12) Congresses when presenting our results, however, because they are on each end and therefore have relatively small

https://doi.org/10.1093/pan/mpw003 Published online by Cambridge University Press

Table 1 Number of bridge occurrences, 101st (1989–90) to 112th (2011–12) Congresses

			Bridge cosponsorship decisions (Congress of Bill Introduction)																							
		Bridge votes	*101 House*	*101 Senate*	*102 House*	*102 Senate*	*103 House*	*103 Senate*	*104 House*	*104 Senate*	*105 House*	*105 Senate*	*106 House*	*106 Senate*	*107 House*	*107 Senate*	*108 House*	*108 Senate*	*109 House*	*109 Senate*	*110 House*	*110 Senate*	*111 House*	*111 Senate*	*112 House*	*112 Senate*
Congress of Roll-Call Vote	101 House	21	–	37	4	2	2		2		2		1		1		1		1							
	101 Senate		14	–		1		1																		
	102 House	35	12	8	–	32	11	4	1								1		1	1	1	1	1	1	1	1
	102 Senate		3	5	18	–		1																		
	103 House	26	2	1	18	6	–	52	8	4	2	1	2	1			1		1	1	1	1	1	1	1	1
	103 Senate		1		6	4	22	–																		
	104 House	49	1	1	3	1	12	6	–	27	8	4	3	1	1											
	104 Senate		2	2	3	2	4	4	15	–	2	3	1	1	1	1	1				1		1	1	1	1
	105 House	32	1	1	2	2	6	2	15	4	–	58	27	12	7	1	2	1		1	1	1		1		1
	105 Senate						1		1	1	11	–	2	2												
	106 House	55					1	1	4	3	30	18	–	85	23	7	9	4	4	3	3	2	2	2	1	2
	106 Senate										4	2	12	–												
	107 House	30									1	1	18	3	–	42	26	8	8	4	3	3	3	3	2	3
	107 Senate													2	14	–	2	1	2	2						
	108 House	27											12	2	47	17	–	60	36	12	14	3	9	1	7	2
	108 Senate												2		5	1	11	–	1	1						
	109 House	30									2	2	4	3	10	4	37	14	–	55	27	13	10	4	4	3
	109 Senate										1	1	2	2	3	1	3	2	6	–	2	2	1	2		1
	110 House	31			1	1	1		3	1	2	1	3	5	7	7	22	11	66	40	–	111	57	32	24	14
	110 Senate																		3	6	13	–	3	2		
	111 House	20			1	1	1		1					1	3	2	9	2	16	12	70	30	–	99	50	23
	111 Senate													1		1	1	1	1	1	5	5	9	–		
	112 House	25													1	1	2		3	1	9	2	24	9	–	77
	112 Senate																				1	2	1	3	9	–

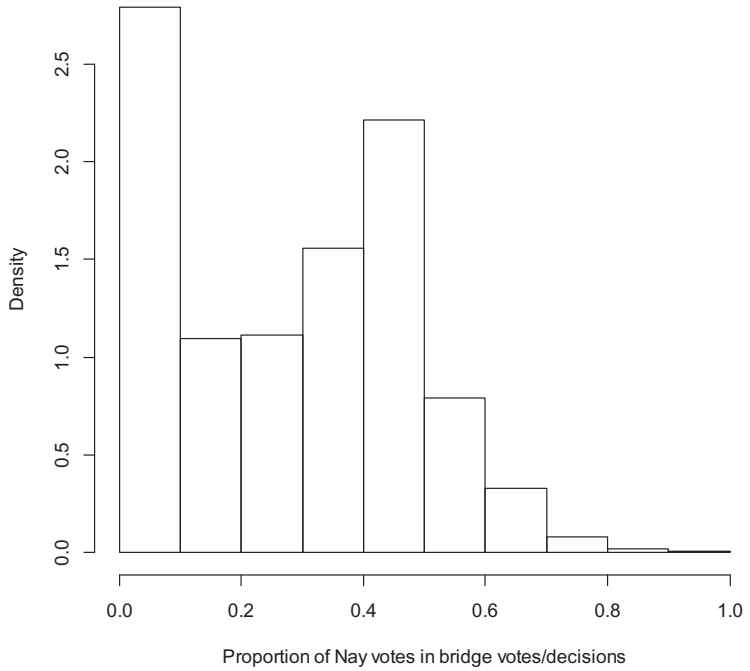

Fig. 3 Proportion of "Nay" votes in the bridge votes/decisions.

numbers of bridge votes/decisions when compared with the others. That is, the data on the first and the last Congresses are used only to help estimating legislators' ideal points in the remaining Congresses. Second, we verify that the votes used to bridge between chambers and Congresses are not severely skewed toward unanimous votes. In examining the distribution of the proportion of the "Nay" votes in our bridge votes/decisions, we find that more than 60% of the data are distributed between 0.2 and 0.8, as presented in Fig. 3. Along with the simulation results, this implies that our data are informative enough to discriminate legislators' dynamic ideological locations from each other.

Before proceeding with the estimation result, we present evidence that our bridge votes/decisions are not markedly different from other items. First, we address the item discrimination parameters for both the bridge votes/decisions and the non-bridging roll-call votes. If an item has a high discrimination parameter, it means that the item has a high ability to differentiate the legislators. If our bridge votes/decisions are not systematically different from the other roll-call votes, the item discrimination parameters will have similar distributions, which means that we are not discriminating too much or too little between different chambers and Congresses relative to within chambers and Congresses. Fortunately, our estimation results support our assumption that the ideological positions of the bridge votes/decisions are not systematically different from the other roll-call votes. Figure 4 suggests that the two groups of items have similar distributions. The two-sample t-test fails to reject the null hypothesis of no difference between the two groups at the 5% level of significance (p-value $= 0.0713$).

We also want to be careful that the set of bills comprising our bridge votes/decisions are not too polarizing or partisan compared with non-bridging bills. Comparing the ideological scores of the bills' sponsors and the partisanship displayed in roll-call votes, we find no significant differences between bridge decisions/votes and non-bridging bills.[13]

[13]The mean non-bridge bill sponsors' ideal point is 0.11, compared with 0.12 for bridge bill sponsors. The two-sample t-test fails to reject the null hypothesis of no difference between the groups ($p = 0.5391$). The mean partisanship score (absolute value of percent of Republicans voting yea minus the percent of Democrats voting yea) for non-unanimous final passage votes is 0.33 for non-bridge bills and 0.35 for bridge decision/votes. The two-sample t-test also fails to reject the null hypothesis of no difference between the groups ($p = 0.0794$).

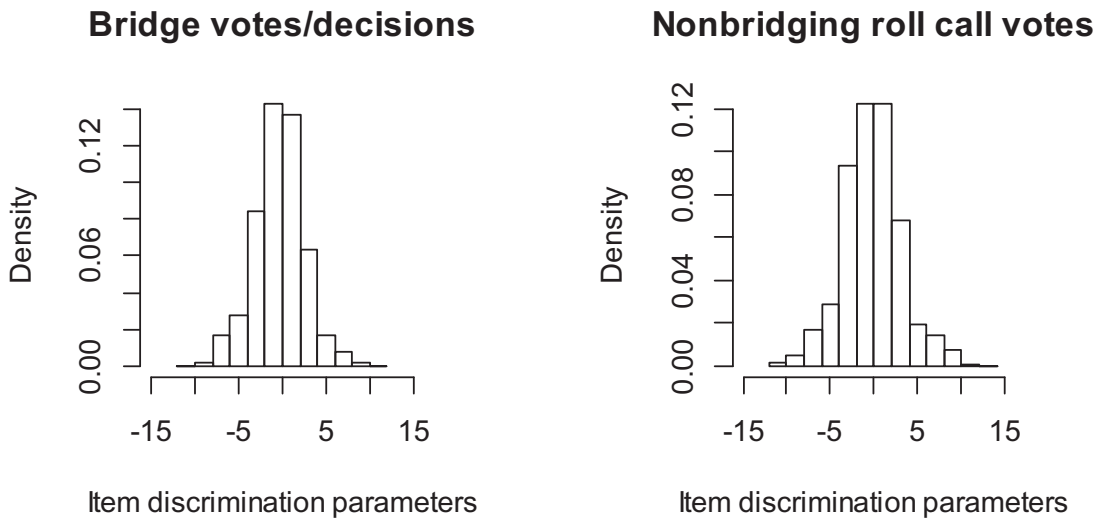

Fig. 4 Histogram of item discrimination parameters.

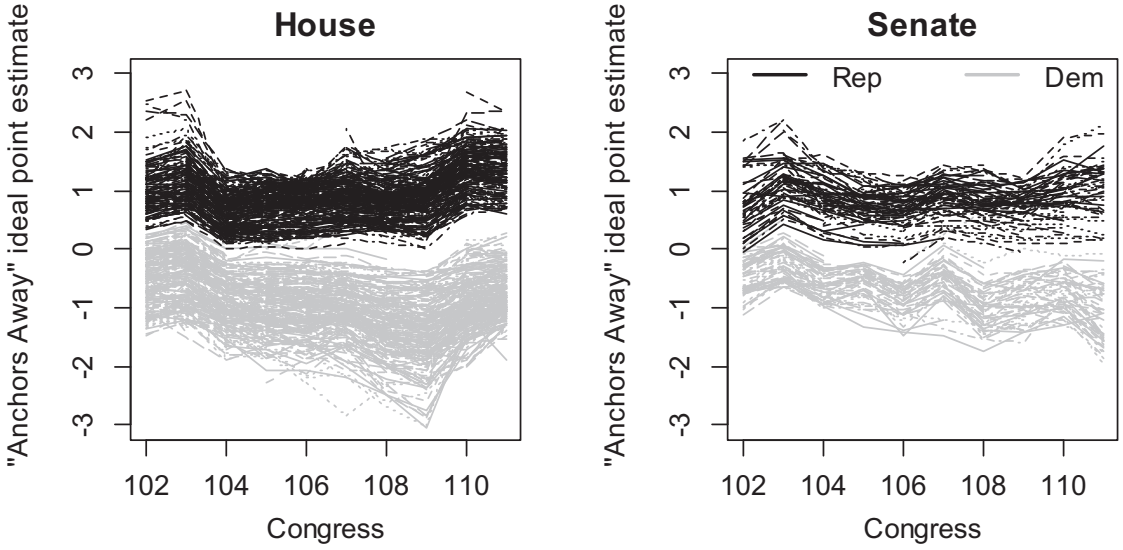

Fig. 5 Ideal point estimates for the 102nd (1991–92) to 111th (2009–10) Congresses.

4 Results

We now present the results of our ideal point estimation along with a few examples that illustrate the utility of our method for investigating changes in legislative behavior over time. Figure 5 visualizes the ideal point estimates for each legislator across ten Congresses, with each line representing the career of an individual member of the House or the Senate. Although we have plotted the House and the Senate separately, note that the chambers are directly comparable because they are estimated on the same scale using bridge votes/decisions. The figure provides some face validity for our estimates in that it confirms many generally recognized characteristics of Congress found by other estimation methods, notably, the absence of overlap between the parties, the disappearance of moderates, and the widening gap between the parties. Additionally, the figure also shows patterns that other methods cannot uncover: substantial movement of individual members (in particular, back and forth movement), and shifts in ideal points that pull a substantial number of members in the same direction.

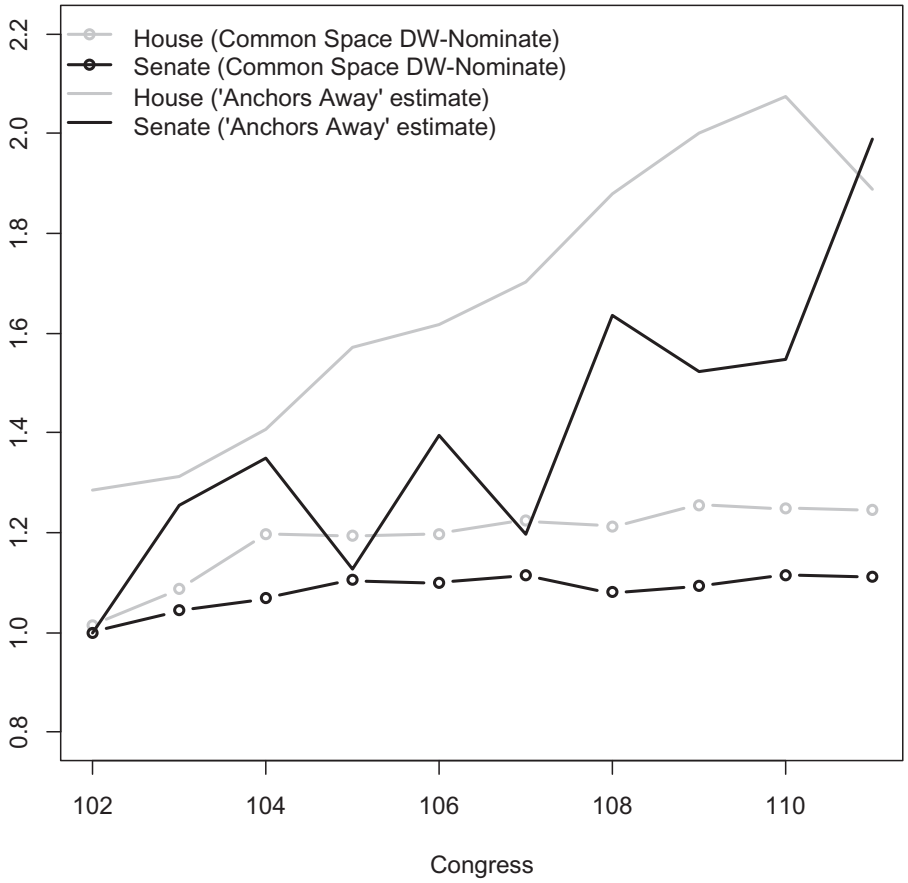

Fig. 6 Differences in the two parties' medians in each chamber for the 102nd (1991–92) to 111th (2009–10) Congresses (the difference in the 102nd Senate is used as a base).

Figure 6 plots the level of polarization in each chamber (i.e., the difference between the Republican and Democratic parties' medians). For comparison, polarization values of the Common Space DW-NOMINATE scores are also presented. Since these scores are on a different scale, we normalize the scales by setting the difference between party medians to 1 for the 102nd Senate.

Regardless of estimation method, polarization has increased during the past two decades, but our estimates illustrate a different story in terms of the timing and magnitude of changes. According to the Common Space scores, polarization increased most rapidly in both chambers in the early 1990s before leveling off in the Senate and growing at a slower pace in the House. According to our estimates, however, polarization increased steadily in the House over the whole period up until the 111th Congress (2009–10), where it dipped, mostly due to House Democrats moving somewhat to the right, perhaps due to the upcoming 2010 midterm elections where House Democrats fared quite poorly. The Senate, on the other hand, was polarizing in a slower and more uneven fashion before increasing abruptly and surpassing the House in the 111th (2009–10) Congress, mostly due to Senate Democrats moving to the left, perhaps because of their sixty-seat majority.

We also see a tremendous difference in the magnitude of change in polarization between the two methods. According to our estimation, the distance between the party medians in the House has increased 46.7% from the 102nd to 111th Congresses (1991–2010), compared with a 22.8% captured by Common Space scores. Meanwhile, in the Senate, polarization has increased 98.9% according to our method, compared with an 11.1% increase using Common Space scores. It is not surprising that our estimates show greater change since the Common Space scores assume that legislators never change their ideological position throughout their whole career. With the constant

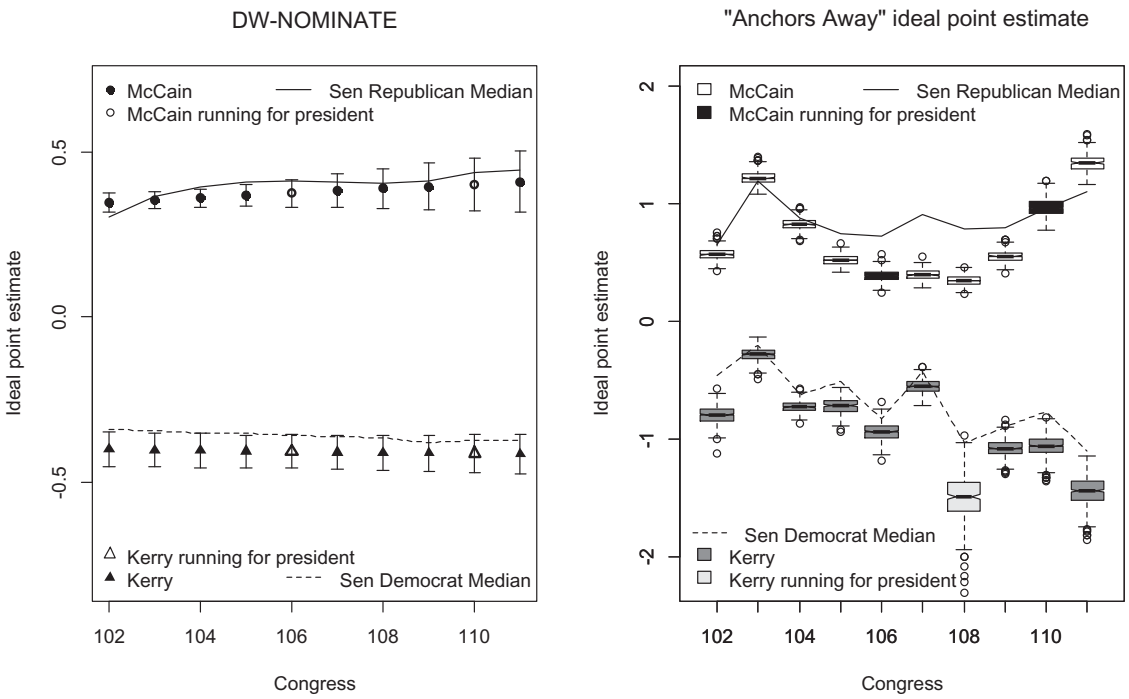

Fig. 7 Comparison of estimates for Senators John Kerry and John McCain.
Note. In the left panel, the error bars are for the 95% confidence intervals; the right panel is the box-plot of the posterior distributions of the ideal points.

ideal point assumption, adding a small number of new members can only make a mild change in the medians, considering that about 80% of seats are won by incumbents on average. However, is it true that legislators do not change their ideological position even when political circumstances change? Removing the constant ideal point assumption gives a quite different picture. Polarization has increased very rapidly, and it is not only because the newly elected members were very extreme, but because the reelected members have also changed their behavior to respond to the same political pressure as the newly elected members are facing.

In addition to overall trends, we can also use our estimates to examine the behavior of individual members over time. We illustrate this feature using two well-known senators—John Kerry (D-MA) and John McCain (R-AZ)—whose ideal point estimates are produced in Fig. 7, using both our "Anchors Away" method and DW-NOMINATE, which restricts legislators' movements to a linear time trend. Although DW-NOMINATE estimates Kerry's ideal point to be essentially stationary, despite his party's median moving to the left, our method exhibits much more movement, with Kerry to the left of the Democratic median, but following the trajectory of his party, except in the 108th (2003–4) and 111th (2009–10) Congresses, where he is estimated as substantially more liberal. Kerry's out-of-the-ordinary extremity in the 108th Congress has already been chalked up to the consequence of absenteeism and vote selection during his 2004 presidential bid (Clinton, Jackman, and Rivers 2004a), but Kerry's similar level of extremity in the latter Congress is less easily dismissed, however, as his absentee rate is more typical (13.2% as opposed to 76.4% during his presidential run).

For McCain, DW-NOMINATE shows him to be more moderate than the median Republican senator for most of his career, moving slightly to the right over time. Our estimates produce a more nuanced characterization of McCain's behavior over the time period. For the decade beginning in the mid-1990s, our method puts McCain significantly to the left of his party—a time period covering his first run for the Republican nomination in 2000, and the 2004 election in which it was widely rumored that Democratic nominee John Kerry considered choosing the moderate Republican as his running mate (see, e.g., Balz and VandeHei 2004). But in the 110th Congress (2007–8) McCain has moved to the Republican mainstream, and by the 111th (2009–10), he is far to

Table 2 OLS regression of difference in Senate and House "Anchors Away" ideal point of legislators who served in both chambers, 102nd (1991–92) to 111th (2009–10) Congresses

	Coef. (s.e.)
Difference in Two-Party Republican Vote	0.013
	(0.006)
Population (in millions)	0.0004
	(0.0096)
Republican	0.042
	(0.127)
Republican * Population	−0.014
	(0.018)
Female	−0.235
	(0.175)
Minority	−0.208
	(0.324)
Intercept	−0.014
	(0.095)
N	56
R^2	0.163

the right of the median Republican. While his move to the right in the 110th can be attributed either to higher absenteeism during his 2008 presidential run or to a deliberate attempt to court reluctant conservative voters, his unorthodox behavior in the 111th requires explanation. McCain's newfound conservatism has not escaped the notice of the media (see, e.g., Zwick 2009; Wilson 2011) or political scientists (Lauderdale 2010; Sides 2010), who point to McCain's Tea Party challenger in Arizona's 2010 Senate Republican primary or his bitterness toward Obama in the wake of the 2008 election. Our method is flexible enough to uncover these non-linear trends.

Our method can also easily be used to make cross-chamber comparisons of members who served in both chambers, unlike the popular DW-NOMINATE scores, which are not comparable across chambers, and their kindred Common Space scores, which require the assumption that members serving in both chambers have the same ideal point their whole career. Our estimates urge caution in making that assumption, as we find that legislators systematically alter their behavior to fit their new constituency. Although previous research has hypothesized the legislators will become more moderate in the Senate due to the larger constituency (Hibbing 1986; Grofman, Griffin, and Berry 1995), we find no evidence of movement toward the middle. Rather, legislators' ideal points move in relation to change in partisanship of the district. To show this, we performed an OLS regression of the difference between a chamber-switching legislator's average ideal point in the Senate and in the House, using the fifty-six legislators in our data set who switched chambers from 1991 to 2010. To measure the change in the partisanship in the constituency, we used the difference between the state-level and district-level two-party vote for the Republican candidate in the presidential election closest to the legislator's elevation to the Senate. According to the results in Table 2, the greater the increase in Republican partisanship from the district to the state level, the more the legislator's Senate ideal point is to the right of their House ideal point. This result casts doubt on whether these members are adequate bridges for methods that rely on them to achieve cross-chamber comparability.

Finally, our estimates can further illuminate the circumstances of party-switching by measuring session-to-session changes before and after a legislator changes party affiliation. It is well established that a change of party affiliation is associated with a substantial change in ideal point in the direction of the new party (Nokken 2000; McCarty, Poole, and Rosenthal 2001). But is this "jump" in ideal point simply an artifact of an estimation procedure that constrains legislators' movements, allowing them to change abruptly only when they change parties? In other words, perhaps these party switchers had been drifting toward their new party all along, as they so

https://doi.org/10.1093/pan/mpw003 Published online by Cambridge University Press

Table 3 Change in "Anchors Away" ideal point for party switchers in 102nd (1991–92) to 111th (2009–10) Congresses

Party-switcher	*Party switch (t_0) occurs*	*Δideal point toward opposing party from*										*Δideal point (new party–old party)*		*Δideal point toward extreme of new party*					
		t_{-8}–t_{-7}	t_{-7}–t_{-6}	t_{-6}–t_{-5}	t_{-5}–t_{-4}	t_{-4}–t_{-3}	t_{-3}–t_{-2}	t_{-2}–t_{-3}	t_{-1}–t_{-2}	t_0–t_{-1}	*at* t_0	t_1–t_0	t_2–t_1	t_3–t_2	t_4–t_3	t_5–t_4	t_6–t_5	t_7–t_6
R. Shelby (Democrat to Republican)	Between 103 and 104									**0.46**	**0.24**	−0.02	−0.07	0.04	0.01	0.07	0.10	0.00
B. Campbell (Democrat to Republican)	During 104									**−0.29**	**0.62**	**0.09**	0.04	**0.16**	−0.28			
N. Deal (Democrat to Republican)	During 104									**−0.36**	**0.77**	**0.43**	−0.17	**0.27**	**0.29**	0.16	**0.29**	**−0.20**
J. Hayes (Democrat to Republican)	During 104								**0.09**	**−0.31**	**0.85**							
G. Laughlin (Democrat to Republican)	During 104									0.07	**1.42**							
M. Parker (Democrat to Republican)	During 104								**0.10**	**−0.13**	**0.71**	−0.08						
B. Tauzin (Democrat to Republican)	During 104								**0.16**	**−0.33**	**0.59**	**0.19**	0.02	**0.25**	**−0.22**			
M. Forbes (Republican to Democrat)	During 106								**0.11**	**−0.11**	**0.55**							
V. Goode (Democrat to Independent)	During 106									**0.16**	**0.84**	0.05						
M. Martinez (Democrat to Republican)	During 106						**0.15**	**−0.38**	−0.07	**0.62**	**0.95**							
V. Goode (Independent to Republican)	During 107										**1.02**	**−1.44**	0.05	**0.75**				
J. Jeffords (Republican to Independent)	During 107					**−0.47**	**0.27**	**0.10**	−0.01	**−0.12**	**0.49**	**0.40**	0.12					
R. Hall (Democrat to Republican)	During 108				**0.16**	**−0.38**	0.01	−0.02	−0.02	**−0.17**	**0.78**	**0.26**	**0.45**	−0.03				
R. Alexander (Democrat to Republican)	Between 108 and 109										**1.27**	**0.20**	0.07					
P. Griffith (Democrat to Republican)	During 111										**1.19**							
A. Specter (Republican to Democrat)	During 111	**−0.52**	**0.42**	**0.17**	−0.01	**−0.25**	0.04	0.07	−0.07	0.07	**1.29**							
Average switcher		**−0.52**	**0.42**	**0.17**	**0.08**	**−0.37**	**0.12**	**−0.06**	**0.04**	**−0.03**	**0.85**	**0.01**	**0.06**	**0.24**	**−0.05**	**0.11**	**0.20**	**−0.10**

Notes. Average change toward opposing party between sessions prior to party switch: −0.02.
Average change toward new party during or between session(s) of party switch: 0.85.
Average change toward extreme of new party between sessions after party switch: 0.07.

often claim, "I didn't leave the party; the party left me." Our analysis of the sixteen party switchers, presented in Table 3, suggests not. The average party-switcher exhibits a substantial change in ideal point toward their new party, while showing almost no movement on average in the sessions leading up to or coming after the change. Indeed, at the individual level, each party-switcher exhibited a statistically significant change in ideal point (indicated with bold) at the time of their switch, but only a few forecast their party switch by moving toward that party in prior sessions—Richard Shelby, Virgil Goode, and Matthew Martinez. These results bolster support for the theory that party affiliation influences legislators' behavior.

5 Conclusion

Existing approaches to estimating ideal points across chambers and time have traditionally relied on legislators who serve in more than one chamber or time period as anchors. This assumption of stationarity has seemed like a necessary evil due to the fact that legislators do not vote on the same agendas in each chamber in every session. In our review of the existing methods, we show that the approaches that assume legislators have a fixed career ideal point, or even less restrictive approaches that use trendline or fix-and-float strategies, can obscure over abrupt movements or universal shifts in legislator ideology. Furthermore, we show that this system of using the legislators as anchors is not necessary to achieve comparability since a sufficient number of identically introduced bills can be used as anchors instead, removing the need to place any restrictions on legislators' ideal points.

Our results illustrate the types of questions our estimates can speak to. Our most surprising result is that the magnitude and pace of party polarization is actually being understated by Common Space DW-NOMIANTE scores; that is, polarization is even worse than it seems! Our analysis of the ideological trajectories of Senators John McCain and John Kerry and our finding that chamber-switchers become more liberal or conservative depending on the change in district to state ideology also undermine some of the assumptions that Common Space scores are built on, namely, that members of Congress "die in their ideological boots." Finally, although not particularly surprising, we find that legislators abruptly change their voting behavior upon switching parties, with no evidence of gradual ideological movement in the Congresses leading up to or following their switch. These examples give a taste of the different types of descriptive and explanatory analyses that our ideal point estimates can be used to study in the future.

Funding

The Kyung Hee University (KHU-20130985 to J.J.).

Conflict of interest statement. None declared.

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