Separation of Powers and Appointee Ideology

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The traditional view of appointments to executive agencies is that the president has virtual carte blanche in the selection of personnel for his "team." Yet many formal models of appointment suggest that presidents must accommodate the policy preferences of senators when making nominations. Several empirical studies have confirmed that legislative preferences are a significant determinant of the ideology of appointees, but these studies have focused on appointments to the federal judiciary; the research has not addressed appointments to executive agencies. Appointments to executive agencies from 1936 to 1996 are examined, by employing a special sample of appointees to those positions—those who have served in Congress at some point in their careers. For these "bridging" individuals, it is possible to analyze strictly comparable measures of ideology for the appointees, their nominating presidents, and the senators who voted to confirm them. A linear regression analysis provides significant support for the hypothesis that appointee ideology is affected by variation in the ideological tilt in Congress.

1. Introduction

Scholars of executive politics have examined appointments in substantial detail from two primary perspectives. The first camp views appointment as an exercise in presidential power. From this perspective, the policy preferences of a nominee are irrelevant to confirmation. Moe (1985, 1987) has long contended that presidents dominate the appointment process and has argued that senators are constrained from opposing a nominee on policy grounds because "a candidate's ideology is not a legitimate basis for voting no" (Moe, 1987: 251). Deering (1987: 116) echoed that thought:

Although some nominees may be rejected because of their policy views, almost no senator will state publicly that policy views are a basis for rejection. Moreover, the use or nonuse of policy questions would be unlikely to change the basis of votes.... At bottom... the only consensual grounds for rejection are those dealing with a nominee's personal integrity.

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In a similar vein, King and Riddlesperger (1996) found that conflict between the president and the Senate over cabinet nominees is much less a function of the institutional conflict between the legislative and executive branches, and is more a function of qualification questions.

A second camp of executive scholars has come to see appointment as a more complex exercise in bargaining between policy-motivated actors over specific policy outcomes. Mackenzie (1981:169) has argued, in direct contrast to Moe, that policy concerns are the dominant factor in most confirmation decisions, and that Senate concern over the policy implications of nominees extends even to cabinet positions:

Even when Senators cite other reasons as the basis for opposition to a nominee... often that is just a disguise for their displeasure with his political philosophy or his views on important policy issues.

By extension, presidents must anticipate the preferences of the Senate in order to get their nominees confirmed, and a potential nominee’s policy preferences are central to explaining the appointment outcome. A number of formal models of appointment have been proposed that depict the nomination and confirmation as rooted in strategic interaction and ideological policy preferences of presidents and senators.

In the following sections, I lay out an empirical test for the hypotheses that (1) nomination choices by presidents and confirmation reactions by senators are rooted primarily in the ideology of appointees, and that (2) presidents and senators anticipate one another’s policy preferences and bargaining positions in their nomination and confirmation behavior. My findings reinforce Mackenzie’s perspective on appointment, at least for executive branch positions. Appointee ideology is systematically related to the policy preferences and bargaining positions of presidents and senators.

2. Formal Depictions of Bargaining Over Appointee Ideology

Several formal models of strategic nomination and confirmation based on the policy preferences of the president, the Senate, and nominees have been proposed in recent years (Hammond and Hill, 1993; Moraski and Shipp, 1999; Nokken and Sala, 2000; Snyder and Weingast, 2000; Chang, 2001). In those models, presidents observe a range of nominees the Senate would find acceptable and strategically nominate the most ideologically compatible person who can still win confirmation by the Senate.

There are several alternative formalizations of what kinds of nominees the Senate will find acceptable. The primary distinction between most formal models of appointment boils down to differences over how to characterize the “reversion policy”—the policy outcome if the position cannot be filled. For appointees to multimember panels such as the National Labor Relations Board (NLRB) or the Supreme Court, who cannot be unilaterally removed by the president, it has been suggested that the reversion policy is rooted in the policy preferences of the sitting
Figure 1. P, president’s ideal policy; Hm, House median ideal policy; Sm, Senate median ideal policy; Hvo, ideal policy of pivotal veto override legislator in House; Svo, ideal policy of pivotal veto override legislator in Senate. For generality, Figure 1 depicts an outlier president on the liberal side of the House and Senate medians. Presidents have been outliers, relative to the override pivots in all but three Congresses since 1937, as discussed in footnote 6.

Panel members (Hammond and Hill, 1993; Moraski and Shipan, 1999; Nokken and Sala, 2000; Snyder and Weingast, 2000; Chang, 2001). Appointments to multimember panels are beyond the scope of this project.

For executive branch appointees, such as the Secretary of Commerce, the reversion policy is simpler, owing to the unilateral removal and recess appointment powers of the president. As Nokken and Sala (2000:104) note:

If the president were free to make recess appointments to these [independent commission] slots—as he does for other executive branch positions—he could end-run an intransigent Senate by filling vacancies during recesses with hand-picked appointees who, presumably, would do his bidding [italics added].

That is, the president may always obtain his most preferred executive branch nominee, if he is willing to do so by making a recess appointee or designating a person of his choosing as “acting” or “interim” official.

The reversion policy for executive branch appointees may be further identified by considering a second set of formal models of the statutory process that limits the policy choices of any appointee. To illustrate, consider a simple unidimensional array of ideal preferences, as in Figure 1. Hammond and Miller (1987) have shown that there is a nonempty set of policy outcomes, denoted as the “core,” which cannot be defeated through legislation.1 Thus any confirmed appointee may implement any policy within the core without fear that a law will be passed to change policy.

Hammond and Knott (1996) have shown in a formal proof that any confirmed appointee may establish policy at the boundary of the core closest to the president, and congressional opponents of the appointee’s policy will be unable to attract sufficient legislative support for a bill that

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1. Borrowing the “pivot” term and logic from Krehbiel (1998), the actors determining the location and extent of the core are the president, P, the pivotal veto-override member in the House, Hvo, and Senate, Svo, and the median members of the House and Senate, Hm and Sm, respectively. See Hammond and Miller (1987), and Hammond and Knott (1996). The results for this set of models is not limited to a single dimension.
replaces the appointee’s policy with a policy closer to the congressional medians. This result is similar in spirit to other models of U.S. policy making, all of which might be called separation of powers (SOP) models (Hill, 1985; Ferejohn and Shiplan, 1990; Eskridge and Ferejohn, 1992; Woolley, 1993).

One way to use SOP models to define the reversion policy for executive branch appointments is to consider the oversight costs incurred by presidents and legislators when forcing policy changes on confirmed appointees. A president wishing to limit his oversight costs will nominate someone no further from his ideal than the pivotal veto override legislator. Illustrating with the example ideal locations in Figure 1, the president cannot obtain a worse policy outcome than $H_{vo}$, even if all appointment negotiations break down, because he can unilaterally name a recess appointee willing to implement $H_{vo}$ without fear of a statutory reversal. Of course, nominating someone whose ideal policy outcome lay to the right of $H_{vo}$ would be less preferred by the president because the steps needed to monitor and force the appointee to move policy left to $H_{vo}$ are costly to him. Similarly a median senator wishing to limit her oversight costs will confirm only those nominees at least as close as the pivotal veto override legislator. The median senator cannot obtain a worse outcome than $H_{vo}$, even if the president makes a recess appointment, because a veto-proof congressional coalition can reverse any policy implemented closer to the president than $H_{vo}$. Yet the monitoring and effort of statutory reversals are costly for the median senator, making any appointee with an ideal point to the left of $H_{vo}$ less attractive. In a single dimension, the only overlap between these two ranges of ideologically acceptable appointees is a single point—the ideal policy of the pivotal veto override legislator.

Thus if the SOP dynamics described above and the possibility of recess appointment define the reversion policy for executive branch appointments, then presidential nominees should have ideal policy preferences matching the ideal of the pivotal veto override legislator at the time of their confirmation. We may denote this location as the “SOP appointment prediction.”

3. Empirical Tests of Bargaining Over Appointee Ideology

So far, direct tests of ideological bargaining over appointments have been difficult to conduct because useful measures of appointee ideology or subsequent policy choices have been difficult to establish. Several scholars have developed ideology measures for presidents, senators, and representatives in a common space (Poole, 1998b; Groseclose et al., 1999). But extending those measures to include comparable ideology scores for appointees has been more problematic. Moraski and Shiplan (1999) ignored the measurement issue, and conflated adjusted Americans for Democratic Action (ADA) scores for presidents and legislators with Segal-Cover scores (Segal and Cover, 1989) for Supreme Court
appointees. But as Bailey and Chang (2001) demonstrate, the Moraski and Shshan results may be flawed by the incomparability of these two measures. As a solution, Bailey and Chang (2001) have established truly comparable ideology measures for appointees to the Supreme Court, based on a “bridging” approach used in previous articles to estimate presidents, senators, and representatives in the same issue space (Zupan, 1992; McCarty and Poole 1995; Poole, 1998b). But no published work has extended this approach to generate ideology measures for executive branch appointees, in part because suitable “bridging” individuals have not been identified and in part because there are no existing measures of ideology or policy decisions for executive appointees.

In order to provide a direct test of the formal appointment model, I propose to rely on a particular measure of policy preferences and to examine a particular set of appointments in which the preferences of senators, presidents, and executive branch appointees are strictly comparable. A useful sample of executive branch appointees may be identified by looking at those who have served in Congress. These individuals serve as the bridges between appointees and other actors. As a measure of ideology, Poole’s “common-space” scores are available for every president and member of Congress since 1937 (Poole, 1998b). I make the assumption that the ideology of an appointee is accurately reflected by his or her common-space score while in Congress.

These data are uniquely suited as measures of appointee ideology because the scores have been shown to be very stable indicators of ideology (Poole and Romer, 1993; Poole and Rosenthal, 1997; Poole, 1998a), and the scores are strictly comparable across individuals, institutions, and time. Therefore it is possible to examine the ideology of appointees, their nominating presidents, and the senators who approved their nomination, all in the exact same metric, in order to determine the political influences that led the president to nominate and the Senate to confirm candidate A rather than other potential candidates B or C. Members of Congress subsequently appointed have been examined previously in a tangential literature, and the analysis suggests that loyalty to the president’s agenda significantly improves a member of Congress’ chance of being appointed (Palmer and Vogel, 1995).

The analyses presented in later sections confirm that, while this sample of appointees is possibly unrepresentative of all appointees, the political patterns probably extend to the full range of appointees—not just those who have also served in Congress at some point in their careers. A linear regression model of appointee ideology is presented, based on the political factors that obtain at the time of the nomination, which confirms the

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2. Groseclose, Levitt, and Snyder (1999) caution against standard interest group ratings, because scales shift and deform over time, making comparison of scores from different years invalid. But they ratify D-NOMINATE and other such “adjusted” ideology scores, and point out that all such scores are very highly correlated.
primary hypothesized appointment dynamics with respect to the ideology of nominees. Appointments in this sample are clearly motivated and constrained by ideological considerations, as opposed to being driven purely by personal relationships and partisan patronage. In addition, legislative policy preferences play a significant role in determining the appointment outcome in a manner consistent with an SOP model.

4. Common-Space Ideology Scores and a Bridging Sample

In order to examine the ideology of presidential appointees, I compiled a dataset consisting of every domestic executive branch appointment of every member of Congress who had served in one of the 75th through 105th Congresses.3 Multiple appointments of a single individual were treated as distinct but not independent observations.4 All told, 95 executive branch appointments between 1938 and 1996 have involved a former or future member of these Congresses. These appointments encompassed a wide range of policy spheres and responsibility levels, from secretary of agriculture to special assistant to the president. They serve as a bridging sample from the legislative and presidential ideological space to the executive appointee ideological space.

The bridging sample contrasts with previous representative samples of appointees, in which less than 6% had previously served in Congress (Stanley et al., 1967; Fisher, 1987; Cohen, 1988). Former members of Congress (as opposed to those individuals who went on to be elected to Congress only after their service as appointees) comprise more than four-fifths of my sample (83.7%). In addition, the sample is more heavily weighted in favor of prestigious positions than random chance would allow. There is normally a roughly 4:1 ratio of subcabinet to cabinet positions (Fisher, 1987), while the ratio in these data is closer to 2:1. Palmer and Vogel’s list of former members of Congress subsequently appointed between 1961 and 1992, almost all of whom are included in my data, also has a 2:1 ratio of subcabinet to cabinet positions. Finally, the duration from nomination to confirmation in this sample (only about

3. Data compiled by the author is based on an extensive search of the Biographical Directory of the U.S. Congress, Biographical Roster of Members of Congress (data compiled by ICPSR for all members of Congress who have ever served), Journal of the Executive Proceedings of the Senate, Public Papers of the Presidents, and the gracious provision of data by Harvey Palmer, Ron Vogel, Nolan McCarty, and Rose Razaghian. Only those appointive positions requiring the normal presidential nomination and Senate confirmation procedure were included as observations. Senate-confirmed appointees were excluded if they were appointed prior to 1938 (presidential approval polls are unavailable prior to that time), or if their brief tenure in Congress provided a voting record insufficient to estimate their ideology, or if their policy relevance is questionable (U.S. marshalls, U.S. attorneys, postmasters, military appointments, ambassadors, and special commissions or presidential task forces).

4. Standard errors were clustered on individuals to eliminate the assumption that errors for multiple appointments of the same individuals are independent.
2.7 weeks, on average) is much shorter than the 9 weeks that is typical (Deering, 1987).

However, these appointees appear broadly representative of all appointees in many other respects. Ninety-six percent of the sample were of the same party affiliation as their nominating president. The 20th century presidential average of same-party appointments is approximately 90% (Ragsdale, 1995). The median age at appointment is 46 years in this sample, and is 47 or 48 years in previous representative samples (Stanley et al., 1967; Fisher, 1987). Women comprise 4.4% of the sample, and most of the women were appointed after 1965. Previous representative surveys have indicated a 5.9% female cohort for post-1965 appointees (Fisher, 1987) and a 0.7% female cohort prior to that (Stanley et al., 1967). In the bridging sample, 79.6% of appointees went to public school prior to college, compared to 59% in a previous sample (Stanley et al., 1967). In the bridging sample, 31.6% went to a “top 18” university, 23.2% went to an Ivy League university, and 16.8% went to a “big 3” university.5 In Stanley, Mann, and Doig’s (1967) representative sample, the figures are 40.2%, 25.1%, and 19.5%, respectively. Finally, the median tenure in the appointed position is 24 months, which is similar to the post-New Deal pattern of 28–30 months (Stanley et al., 1967; Cohen, 1988), and 67.1% of appointees were appointed only once (in Stanley, Mann, and Doig’s representative sample, 63% of appointees were appointed only once). All of this suggests that the sample is reasonably representative of most executive branch appointees.

As a measure of presidential, senatorial, and appointee ideology, Poole’s common-space scores are particularly well suited for analysis of appointment politics and are publicly available (Poole, 1998b). The first dimension scores mirror the more traditional D-NOMINATE scores of Poole and Rosenthal (1991, 1997), indicating relative conservatism-liberalism, with positive scores indicating conservative ideology and negative scores indicating liberal ideology. While there is certainly significant texture to one’s ideology that is not captured by a single left-right measure, the primary component of political competition between the parties in the New Deal era has been between conservatives and liberals. Indeed, Poole and Rosenthal suggest that left-right competition along the primary liberal-conservative economic dimension encompasses virtually all of the political competition in modern American politics (Poole and Rosenthal, 1997).

The common-space scores are static interval level measures of ideology available for each president and member of Congress since 1937, and are strictly comparable across individuals and across time (Poole, 1998b). That is, it is legitimate to claim that a legislator who served

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5. Stanley, Mann, and Doig (1967) declared Yale, Harvard, and Princeton as the “big 3” universities. They present a list of the “top 18” universities in Table D12.
in the Senate in 1947 and had a score of .455 is more conservative than a legislator who served in the 1972 House and had a score of .402, and it is equally legitimate to claim that a president serving in 1972, whose score is .288, is more liberal than either of the two legislators. A few other measures of presidential and legislative ideology share this property, but they are very highly correlated with common-space scores (e.g., Groseclose et al., 1999). In practice, Poole’s ideology scores tend to lie in the interval (−0.5, +0.5), and presidential ideology is consistently more extreme than the pivotal veto-override legislator.6

Poole and others have demonstrated that legislators exhibit ideological consistency throughout their congressional service, even in the face of changing constituencies, even in the absence of any constituency pressure, and even when they enter a new institutional setting (Poole and Romer, 1993; Poole and Rosenthal, 1997; Poole, 1998a). As they put it, “not only [do] representatives die in their ideological boots, but...they do not change them when they run for the Senate” (Poole and Rosenthal, 1997:76).

While ideological consistency across broader institutional divides has not been previously examined, it turns out that ideology, as measured by common-space scores, is relatively consistent across even the most diverse institutional settings. For example, Segal-Cover scores of Supreme Court ideology (Segal and Cover, 1989; Segal et al., 1995) correlate almost perfectly with common-space scores, at least for the four justices for whom a comparison is possible (Pearson = −.946), and common-space scores predict liberal economic voting on the Court even better than Segal-Cover scores (Pearson = −.678 versus .651). For the federal appeals courts judges for whom a comparison of ideology measures is possible, the fit between Songer’s judicial economic liberalism (Humphries and Songer, 1999) and common-space scores is good, but not great, largely because John Danaher proved a much more liberal judge than legislator (Pearson = −.594 with Danaher, −.793 without Danaher).

Of course, these are only preliminary demonstrations for a research endeavor in itself. However, congressional and judicial service are polar opposites, in terms of constituent pressures, so if ideology is even partially consistent across these institutional settings, the correspondence between congressional ideology and ideology in executive branch positions must be at least as strong. In any event, changes in ideology would have to be

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6. Eisenhower is an exception because his estimated ideology (.279) was more liberal than the estimated ideology of the 67th most liberal senator for the 83rd–85th Congresses (between .279 and .318, depending on the Congress). Eisenhower was therefore free to nominate a candidate sharing his ideal policy preference without concern that the nominee would fail confirmation, and without concern that a veto-proof liberal majority coalition could enact legislation mandating more liberal policy decisions from the appointee. The target policy during this time was therefore Eisenhower’s ideal policy, and that definition is used for all subsequent operationalizations.
systematic in order to cause problems for the empirical analysis of SOP constraints on appointments, which is extremely unlikely. A sample selection analysis, discussed in footnote 11, confirms that sample selection does not bias the results in this study.

5. Design for a Direct Test of Strategic Policy-Oriented Constraints on Appointments

The point prediction of the SOP model of appointments is, of course, virtually impossible to support exactly. How could the ideology of all, or even very many, appointees be located precisely at the veto-proof congressional majority pivot? Nevertheless, if suitable controls are introduced, there may be evidence that SOP constraints are indeed a component of the appointment choice established by the president and the Senate. To that end, I propose a linear regression model of appointee ideology, which is a function of several plausible factors.

The SOP appointment prediction identified by the formal model in the introductory section serves as the primary independent variable in this model. For each appointment, the ideology of the pivotal veto override legislator was measured at the time of the confirmation. This is the location of the nominee closest to the president who can still win confirmation by the Senate. A statistically significant positive relationship between this independent variable and the ideology of the confirmed nominee is confirmation that the president and Senate rationally respond to one another’s bargaining position, each trying to achieve a policy outcome through the appointment which is closest to their respective ideals. Because the dependent and independent variables are measured on exactly the same scale, perfect covariation will be evidenced by a regression coefficient equal to 1.0. Perfect independence between appointee ideology and SOP appointment prediction (the null hypothesis) will be evidenced by a regression coefficient equal to 0.0.

In order to ensure that covariation between legislative preferences and appointee ideology is not spurious, it is important to control for several other potentially contaminating factors. First and foremost, a scrutinizing estimation of the impact of SOP predictions on appointments should

7. It is possible to consider measures of legislative preferences alternative to the veto override pivot, such as the Senate or congressional median. In practice, all these measures are very highly correlated (partial Pearson correlation between the veto override pivot and Senate median = .92, after controlling for White House party; partial Pearson correlation between the veto override pivot and congressional median = .72, after controlling for White House party), and statistically determining which is the better predictor of appointee ideology is problematic. Employment of the veto override pivot as the primary predictor has the virtue of a more solid grounding in the published formal models of policy making. In addition, both the Senate median and the congressional median serve as (marginally) poorer predictors of appointee ideology than the veto override pivot, in a nonnested comparison, and do not contribute to the model beyond the veto override pivot, to a statistically significant degree, based on nested F-tests against their additions to the model.
feature the party of the nominating president as a control variable. As a practical matter, most of the variation in the SOP appointment prediction is rooted in party control of the White House. When a partisan change occurs in the presidency, the pivotal veto override in Congress is shifted from one ideological wing of Congress to the other. Naturally the ideological balance of appointees also shifts dramatically at that time, too. But an estimation that merely demonstrates that Democratic presidents appoint liberals and Republican presidents appoint conservatives is not very informative and fails to distinguish between the Moe and Mackenzie views of appointment. A true test of the SOP model will be whether marginal changes in legislative coalition during an administration result in marginal changes in appointment outcome during that administration. One could conduct two regressions—one for Democratic administrations, one for Republican—but a simple control for presidential party accomplishes the same thing without reducing the efficiency of the estimation for the rest of the model parameters.

Other factors play roles in shaping the ideological character of appointees, and their omission as controls might lead to an overstatement of the relationship between SOP determinants and appointee ideology. For example, presidential approval has long been considered a scarce resource employed in legislative battles to obtain presidential victory (Neudstadt, 1990). Presidential approval has been shown to be a factor in confirmation votes when there is a public dispute over a nominee (King and Riddlesperger, 1996). Perhaps presidential approval plays a role even when conflict over a vacancy is less obvious. It seems likely that congressional preferences diverge from the president’s when his popularity falls, and that appointment outcomes slip further away from the president’s ideal policy at the same time. In such circumstances, failure to control for presidential popularity may result in an estimation that overstates the influence of shifting legislative policy preferences on appointment outcomes. Presidential approval, as measured by the Gallup survey conducted just prior to the appointment, centered on its mean and reverse-coded for appointments in Democratic administrations, serves as the independent variable.\footnote{Presidential approval is the standard measure, in percent, based on the poll conducted most recently prior to the confirmation of the appointee. Centering and reverse-coding ensures a proper estimate of this effect, without artificially generating collinearity with other independent variables.} The effect is predicted to be positive. As a president becomes more popular (resulting in higher values for this variable for Republican presidents and lower values for Democratic presidents), confirmed appointees should be more conservative (higher common-space scores) for Republican presidents and more liberal (lower common-space scores) for Democratic presidents.

It has been suggested that appointment dynamics change over the course of an administration, owing to expectations about the potential
change in control of the White House. Waller (1992) proposed a formal model of nominations in which presidents nominate and the Senate confirms more reliable partisans early in their administration and more political moderates later in the administration. There is empirical confirmation that presidential nomination is dominated by partisanship early in an administration. However, later in an administration, the nomination choice has been shown to be less predictable—not necessarily less extreme (Havrilesky and Gilda, 1992). Nevertheless, Waller’s hypothesis has not been examined in the context of executive branch appointments (Havrilesky and Gilda examined Federal Reserve appointments). Inclusion of the time, in years, from the appointment until the next presidential inauguration, reverse coded from 4 to 0 for nominees of Democratic presidents, allows a test of Waller’s hypothesis and controls for the effect as well. To the extent that legislative preferences exhibit a trend during administrations, failure to control for this phenomenon might lead to an overstatement of the relationship between changes in legislative preferences and appointment outcomes. A positive coefficient for years until the next presidential inauguration indicates that appointees are more moderate as a presidential election draws near.

Divided government serves as another potentially contaminating factor in a test of the relationship between legislative ideological preferences and appointment outcomes. Several scholars have identified divided government as a significant component of appointment delay (e.g., McCarty and Razaghian, 1999; Binder and Maltzman, 2002). A shift in partisan control of Congress to the party opposing the president would tend to shift both ideological medians and veto override pivots in the direction away from the president’s ideal. As a result, the SOP model would also predict a shift in appointment outcomes away from the president’s ideal policy in such circumstances. However, presidents facing divided government may diverge from the SOP prediction in favor of the Senate median to a greater extent than an SOP model predicts, owing to the discontinuities of control of the chambers and oversight committee chairmanships. Failure to account for the possible effect of divided government on appointment outcomes will tend to overstate the effect of shifting legislative ideology on appointee ideology. For this article, the coding for unified government is based only on Senate control,\(^\text{9}\) and is interacted with a dummy for party of the appointing president (+1 for Republican presidents, −1 for Democratic presidents). The effect of this variable should be positive, as unified government leads to more conservative appointees under Republican administrations and more liberal appointees under Democratic administrations.

\(^{9}\) The definition of divided government could include the House of Representatives, though the results are not a statistical improvement, based on a likelihood ratio test.
In sum, the ideology of confirmed appointments is hypothesized to be a function\textsuperscript{10} of

SOP appointment prediction (ideology of the pivotal veto override legislator at the time of appointment)
Party of nominating president (\(-1\), Democratic; 1, Republican)
Presidential approval (percent at time of appointment) (centered on mean and reverse coded for appointees under Democratic presidents)
Presidential tenure (years from time of appointment until next presidential inauguration) (centered on mean and reverse coded for appointees under Democratic presidents)
Unified government (0, divided; 1, unified at time of appointment) (positive values for Republican presidents, negative values for Democratic presidents).

All of the effects are hypothesized to be positive. In particular, if the estimated effect of the target policy is 1.0, then changes in the SOP constraint are perfectly tracked by changes in the ideology of confirmed appointees. If, on the other hand, the estimated effect of the target policy is 0.0, then changes in the SOP constraints are unrelated to changes in the ideology of confirmed appointees.\textsuperscript{11}

6. A Null Bridging Sample
As a secondary test of the hypothesis that legislative ideology affects appointee ideology through SOP dynamics, it is possible to identify a comparable

\textsuperscript{10} It is worth mentioning that for undersecretaries and similar lower-tier executive appointments, the literature suggests that the ideology of the sitting secretary plays a role in determining the character of appointees (Mann and Doig, 1965). Unfortunately not even a single pair of upper tier-lower tier executive appointees overlapped in their tenure in a manner necessary to test this proposition.

\textsuperscript{11} This sample of appointees is possibly unrepresentative of the full range of appointments and may therefore lead to biased inferences. The problem is that “incidental truncation” allows measurement of appointee ideology only for a subsample of the larger universe of confirmed appointees, so direct examination of selection issues is not possible. If the sample selection is systematically related to the dependent variable and an independent variable, then a straightforward regression relying on the subsample will provide a biased estimate of the effect of that independent variable on the dependent variable.

However, an analysis that uses Heckman’s (1979) correction for incidental truncation demonstrates that sample selection does not contaminate the results presented in the body of this article. The process leading politicians to settle on a member of Congress to fill a vacancy is statistically independent from the process which determines the ideology of the appointment (chi-square test of dependence = 0.07, \(p = .80\)) Even when controlling the incidental truncation, the results are robust—coefficients for the ideology regression are practically identical in magnitude, and no statistical inferences vary. Because an explanation of why presidents nominate members of Congress is a research endeavor in itself, the more straightforward regression results, which ignore incidental truncation, are presented in this article. The Heckman analysis is based on the larger universe of about 2,500 executive branch appointments during the time frame of this study, thanks to data from Nolan McCarty, and feature the following instruments: presidential approval, ideological distance between president and veto override pivot, presidential tenure, and divided government (chi-square for the probit prediction equation = 413.9***).
### Table 1. OLS Regression of Appointee Ideology

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Model 1</th>
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<th>Model 2</th>
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<td></td>
<td>b (robust Se)</td>
<td>Standardized coefficients</td>
<td>b (robust Se)</td>
<td></td>
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<tr>
<td>Constant</td>
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<td>-0.029 (0.035)</td>
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<td>SOP</td>
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<td>0.483 (0.195)**</td>
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<td>SOP appointment prediction</td>
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<td>.</td>
<td>-0.253 (0.096)**</td>
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<td>SOP appointment prediction × null sample</td>
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<td>Presidential party</td>
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<td>0.136 (0.048)**</td>
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<td>Adjusted $R^2$</td>
<td>0.81</td>
<td>0.69</td>
<td></td>
<td></td>
</tr>
<tr>
<td>White test</td>
<td>1.16</td>
<td>3.67</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* ** *** $p < 0.05, 0.01, 0.001$, respectively, one-tailed test.

Dependent variable: common-space score of executive appointee; SOP appointment prediction: common-space score of pivotal veto override legislator at time of appointment; unified government at time of appointment in positive values for Republican presidents, negative values for Democratic presidents; presidential approval and presidential tenure (years until next inauguration) both centered on means and reverse coded for appointees of Democratic presidents. Robust standard errors are clustered on individuals. Model 1 sample: 95 members of Congress confirmed to executive branch appointments. Model 2 sample includes an additional 86 members of Congress appointed to executive branch positions that did not require Senate confirmation (the “null sample”).

A sample of appointees whose ideologies one would expect to be unrelated to legislative preferences. I collected additional data on the 86 members of Congress who were appointed by the president for executive branch positions that did not require Senate confirmation. Among this sample, one would predict that SOP constraints are not binding. The president may choose appointees of whatever ideology he prefers in such circumstances. That is, the estimated coefficient for SOP constraint should be 0.0 among this sample. The test presented in Table 1 presents a second regression encompassing all 181 appointees. The second regression features an additional predictor: an interactive term between the SOP appointment prediction and the null sample. The total effect of the SOP appointment prediction for the null sample is therefore the combination of the main effect for SOP and the interaction effect. I expect the combination to be statistically indistinguishable from 0.0.12

12 The test described is the most efficient approach to the problem, and therefore the least likely to result in an estimated effect for SOP appointment prediction that is indistinguishable from zero. Employing a less efficient approach (separate regressions for each sample) results in identical findings, as discussed in note 16.
7. Direct Test of Strategic Anticipation

Overall, the results in Table 1 provide statistically significant support for the view that legislative ideology affects appointee ideology in a manner consistent with an SOP conception of appointment outcomes. Of course, the party of the president is the single most important determinant of appointee ideology, and the null hypothesis for that variable is most easily rejected. However, even after controlling for the party of the president, variation in appointee ideology is significantly predicted by variation in the pivotal SOP player in Congress. More than 60% of variation in the SOP appointment predictions is realized in the ideology of appointees. As the standardized coefficients attest, the magnitude of the impact of the SOP constraint is substantively considerable. The impact of legislative coalitions is roughly 80% as large as the impact of presidential party. Furthermore, unified government does not affect appointee ideology significantly, indicating that the ideological tilt in Congress, rather than partisan control of the legislature per se, is the driving mechanism among these executive branch appointments.

On the other hand, these results fall short of a perfect endorsement of the impact of SOP dynamics on executive branch appointments. The hypothesized perfect covariation \( b = 1.0 \) lies outside the 95% confidence interval for the estimate. Indeed, both the null and alternative hypotheses for the SOP appointment prediction may be statistically rejected. This may give pause to scholars, but I present three perspectives to suggest that the true impact of the veto override pivot on executive appointee ideology is statistically consistent with the SOP model after all.

First, in order to unequivocally reject Moe’s “deference” view and support Mackenzie’s “ideological” view of appointment, the estimation of the effect of legislative ideology presented here is a conservative one, by construction. I have included regression controls that are potentially collinear with variation in the SOP appointment prediction. Such a strategy guarantees that evidence of statistically significant impact of legislative preferences on appointment is not spuriously driven by related factors that are not, strictly speaking, elements of the SOP game. But shifts in presidential popularity, divided government, or White House control are plausible as alternative predictors of appointee ideology precisely because they affect the bargaining positions and strengths of Congress and the president. The inclusion of these factors may therefore understate the impact of legislative ideology on appointee ideology. A simpler bivariate regression between appointee ideology and the SOP appointment prediction results in a coefficient statistically indistinguishable from 1.0.\(^{13}\)

Second, one might also interpret the non-1.0 estimated coefficient for SOP appointment prediction from the standpoint of measurement error. Common-space scores are not perfect measures of ideology, and

\(^{13}\) \( b = 1.10 \), robust Se = .066.
the presence of even nonsystematic measurement error is well known to produce attenuation in regression coefficient estimates.

Third, nothing in the way the scales are constructed ensures that they will result in a linear representation of ideology. That is, there is no reason that a move from 0.1 to 0.2 is necessarily the same as a move from 0.3 to 0.4. What makes the independent variable and dependent variable comparable and the responses likely linear is that they are movements in the same vicinity (a fact that can be verified by noting that the constant in the regression is not statistically distinguishable from zero).

A number of diagnostics confirm that the assessment in Table 1 legitimately serves as the appropriate judgment about the influences of these factors. A White test, the most general and encompassing test for heterogeneity, confirms that the results are not contaminated by heteroskedasticity. Several Breusch-Pagan tests, which might indicate specific forms of heterogeneity across some plausible compartmentalizations of the data, also confirm that the results are not contaminated by heteroskedasticity. 14 Separate regressions across these categories are therefore unnecessary and inefficient, and would raise type II risks.

As model 2 in Table 1 indicates, the estimated effect for the SOP appointment prediction is reduced to nil among the null bridging sample. 15 Among those appointments, the estimated effect of the SOP appointment prediction is .23, with a standard error of .18, 16 which requires acceptance of the null hypothesis that \( b = 0 \). Thus when a bridging appointee is named to the executive branch, SOP dynamics affect the ideology of the appointee only if the Senate had a role in confirmation. This bolsters the view that legislative preferences are key determinants of appointee ideology when the legislature is engaged in an SOP game with the president.

The rest of the hypotheses are not borne out in these data of executive branch positions. Only presidential approval has an impact in the expected direction. Appointees are more conservative when Republican presidents are more popular and more liberal when Republican presidential approval slips. Because the variable is reverse-coded for Democrats, the same estimate implies that appointees are more liberal under popular Democratic

14. Several possible groupings suggested in the literature might form the basis for heterogeneity in the analysis, and a Breusch-Pagan (also called Cook-Weisberg) test rejects each as an important distinction. Thus there are no significant differences in the accuracy of the regression between executive agencies and cabinet departments, or between "inner" and "outer" cabinet posts (Cronin, 1980), or between "tier 1, tier 2, and tier 3" appointments (McCarty and Razaghian, 1999), or between former and future members of Congress, or between appointments earlier and later in an administration (Havrilesky and Gildea, 1992).

15. A less efficient approach to the same problem involves a separate regression among the null sample. In such a regression \( (n = 86) \), the estimated coefficient for the effect of SOP appointment prediction on appointment ideology is .224, with an estimated standard error of 0.356.

16. The effect and standard error calculations are based on Friedrich's prescribed method for interpreting interaction effects, and require knowledge that the covariance of the main and interaction effects is \(-.00767\).
presidents and more conservative under unpopular ones. But the impact is both statistically and substantively very close to zero. The same assessment holds for divided government and Waller’s hypothesis about years remaining in the presidential administration. In any event, the estimates of those impacts run in a direction contrary to a sensible hypothesis. An examination of the standardized coefficients bolsters the assertion of essentially nil impact for all these variables. The rejection of a tenure hypothesis complements Havrilesky and Gildea’s rejection of it among Federal Reserve appointments, though the heteroskedasticity phenomenon identified by them is not in evidence among these executive appointments, as discussed in footnote 14.

A more case-specific approach bolsters the contention that SOP considerations are at least partially constraining for executive branch appointments. For example, Carter presided over a growing conservative tide in Congress. When the veto override pivot shifted from −.364 in the first two years of his presidency to −.317 for the last two years, the average executive appointee ideology shifted, too—from −.378 to −.362. Appointments such as moderate Martha Keys (D-KS) as assistant to the secretary of Health, Education, and Welfare (HEW) might suggest that Carter’s conservative shift in appointments is evidence of a moderating phenomenon as an administration progresses. That is, Waller’s (1992) hypothesis about recruitment dynamics may be at the root of this outcome. But the empirical results demonstrate that appointments exhibit no such consistent pattern across the range of this century.

The pattern of appointment under the Truman administration more clearly illustrates the constraint of legislative preferences, because the veto override pivot shifted dramatically back and forth during his administrations. Because he lost both the House and Senate to the Republicans in the 1946 election, Truman had to deal with the most conservative veto override pivot ever faced by a Democratic president. As a result, appointments such as the moderate Elmer Wene (D-NJ) as undersecretary of agriculture made executive branch appointments in the 1947–48 period the fourth-most conservative group, on average, of any two-year period under a Democratic president. When Truman ran against the “do-nothing” Congress in 1948, his party also recaptured both chambers, which had the effect of shifting the veto override pivot in a liberal direction by .097. His appointees in the 1949–50 period, such as the very liberal James McGrath (D-RI) as attorney general, helped shift the ideological average in the liberal direction by .157. When the Democrats lost 29 seats in the House and 5 seats in the Senate in the 1950 election, it had the effect of shifting the veto override pivot in a conservative direction by .038. Appointments in the 1951–52 period consequently tracked the movement, becoming more conservative by .074.

As noted earlier, Eisenhower’s relative liberalism meant that he played the pivotal role in any statutory reversals of executive appointee policy decisions for most of his administration. The predicted appointee ideology
thus did not shift from 1953 through 1958, even though Republicans lost both chambers in 1954. But when the pivotal veto override shifted in the liberal direction by .169, following the 1958 election, appointments such as the moderate John Allen (R-CA) as undersecretary of commerce helped the average executive branch appointee ideology also shift in the liberal direction by .216.

However, several other experiences suggest the limits of legislative impact on executive branch appointments. For example, Nixon and Ford faced a veto override pivot that steadily receded from them throughout their administrations. Yet average appointee ideology only grew more conservative. Even the crushing 1974 election, which shifted the veto override pivot in a liberal direction by .07 did not have an observable impact on the pattern of appointments. Appointments such as the conservative Richard Roudebush (R-IN) as Deputy Administrator of the VA guaranteed that the immediate post-Watergate executive appointments were more conservative than previous Nixon appointees by .01. In addition, Johnson’s appointees in his last full administration were far more conservative (.110, on average) than an SOP model predicts he could have obtained (the veto override pivot was approximately —.33 during this period—one of the most liberal veto override pivots in history). One might be tempted to ascribe this disparity to Johnson’s abysmal approval numbers in 1967 and 1968, but the empirical results rule out that explanation as a persistent phenomenon.

8. Conclusion
At first blush, the degree to which strategic interaction drives executive agency appointment outcomes falls somewhat short of what is depicted in most formal models of the process. The coefficient is only about .6—considerably less than the 1.0 assumed in formal models, even while it is also significantly more than the 0.0 implied by Moe’s perspective on appointment. The ideology of executive appointees, who serve at the president’s leisure, is partially, though not fully responsive to SOP constraints in the expected direction. It may be that more tools are available to shape the decisions of executive branch officials, so special care in selecting the “right kind” of appointee is not required. It may also be that deference rooted in phenomena other than presidential approval, unified government, or presidential tenure is at work.

Nevertheless, there is clear and compelling evidence that, even in this set of appointments where personal loyalties, conflict in unmeasured dimensions, or electoral considerations might be expected to drive outcomes, the ideology of an appointee was often the result of a tug-of-war between the president and the Senate along the primary liberal-conservative political axis. These results bring some empirical evidence to bear on precisely what kinds of dynamics determine appointee ideology for executive branch positions. As the first data and design oriented around comparable
ideology measures for all actors, the findings also provide the first assessment of the magnitude of the impact of legislative balance of power on appointee ideology. The primary dynamic on which recent formal models of appointment rest is at least partially supported by the empirical evidence, even among appointments to the president’s team. The result complements the statistically significant influence of SOP constraint on appointee ideology found for independent multimember panels (Snyder and Weingast, 2000; Chang, 2001).

Because the effect of the SOP dynamic is evident even after controlling for the president’s party, the results provide even stronger support for the phenomenon. Variation in SOP constraints during and across similar administrations has been shown to affect appointment outcomes. That is, the character of the ideological battleground in Congress plays a role in determining the ideological balance of the president’s team. This is a remarkable result, given the very high deference most observers would have expected from Congress for appointments to the president’s hierarchy. Future work may help to more carefully ascertain the remaining level of legislative deference to the president among executive branch appointees.

References


