Expertise, Networks, and Interpersonal Influence in Congress

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Legislators often must vote on complex issues that they do not fully understand. I show that legislators cope with incomplete information by taking cues from trusted peers who possess expertise that they themselves lack. With a matched differences-in-differences design that exploits midsession committee assignments as expertise-increasing events, I estimate that this behavior accounts for a substantial proportion of all congressional voting decisions. These cues cross party lines and remain relevant in the face of mounting partisan polarization. My findings highlight the salience of expertise to legislators and the role that ties between legislators play in allowing Congress to reach informed collective decisions.

An enormous literature explores the effects of legislator ideology, electoral imperatives, and party influence on vote choice (Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002; Levitt 1996; Mayhew 1974). However, the vast array of issues confronted by Congress, the technical complexity of legislative remedies, and the scarcity of legislators' time often conspire to make these factors insufficient guides for action (Kingdon 1989; Matthews and Stimson 1975). This is true not only for issues in which only a fraction of legislators have any interest, such as agriculture or water policy, but also for highly technical legislation concerning issues of universal interest, such as tax policy and national defense. How can legislators map from their ideologies and constituency interests to voting decisions in the face of such limited information?

I show that legislators leverage their peers to cope with this problem. They use the voting decisions of trusted legislators with relevant expertise as informative signals for how they would vote if they were more fully informed. As a result, legislators who acquire expertise in a given area also gain a degree of interpersonal influence. Peers who share their tastes and trust their judgment, but who do not possess expertise in that area, take cues from those legislators about how to vote.

The idea that legislators decide how to vote following the behavior of better-informed peers has deep roots in political science. Using interview evidence, Matthews and Stimson (1975) argued that members of Congress coped with uncertainty about the consequences of voting one way or the other by using the voting intentions of trusted, expert colleagues to decide how to vote. Researchers have relied on such a relationship between expertise and legislative influence to develop theories of legislative behavior (Bianco 1997), motivate empirical studies (Box-Steffensmeier, Ryan, and Sokhey 2015), and interpret otherwise puzzling empirical results (Minozzi and Volden 2013; Stratmann 2000). But, surprisingly, political science still lacks a well-designed empirical test on how expertise and ties between legislators influence voting behavior. Writing only a year after Matthews and Stimson, Born (1976) failed to find evidence of this behavior in an observational empirical study.[1] Nor has political science determined whether such a relationship existed in insular Congresses of the mid-twentieth century but was subsequently swept away by mounting

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1. Matthews and Stimson (1975) showed that legislators are more likely to support proposals that many of their peers support, but the same features of the bill that make it appealing to a large coalition might well make it appealing to an individual legislator, regardless of how his colleagues vote. Born (1976) finds that the unity of a state delegation does not change when a member of that delegation joins or leaves a committee, but the timing of these assignments is correlated with major shifts in the distribution of political power (and hence the agenda) within Congress, and in any event it is unclear whether fully informed members of a state delegation would vote as a bloc.

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polarization (Poole and Rosenthal 2001) and diminishing social contact between legislators (Ornstein and Mann 2006), although Zelizer (2019) finds evidence of cue-taking in a state legislature.

Testing this relationship is more important than ever because it enriches and links two more recent but hitherto separate debates on the sources of power in Congress. Informational theories of lawmaking provide provocative and appealing explanations for features of legislative organization (Gilligan and Krehbiel 1987; Krehbiel 1992), party pressure (Minozzi and Volden 2013), interest group influence (Grossman and Helpman 2001), and presidential power (Howell, Jackman, and Rogowski 2013), but the need to cope with incomplete information is invoked as an axiom from which empirical predictions are derived rather than a proposition to be tested. While it is indisputable that lawmakers are uncertain about the consequences of their policy choices, the degree to which this uncertainty actually influences their voting decisions is not yet understood. Many instead emphasize the role of ideology, constituents, and partisan conflict in determining congressional behavior. Demonstrating that nonexpert legislators look to expert peers for guidance would provide direct evidence that legislators actively adopt strategies to cope with incomplete information and would support the hypotheses that expertise is an important source of power for congressional committees, political parties, interest groups, and presidents.

It would also elaborate on informational theories by highlighting the role of the legislative network in the diffusion of information. This would advance the study of social networks in political science generally and the study of the congressional network in particular. Fowler (2006), Kirkland (2011), and Tam Cho and Fowler (2010) claim that the position of legislators in the congressional network influences legislative outcomes, but skeptics, such as Rogowski and Sinclair (2012), counter that these analyses do not account for the possibility that their measures of connectedness are tightly correlated with ideological congruence and similar electoral constraints.[2] Well-designed observational studies, such as Masket (2008) and Rogowski and Sinclair (2012), reach contradictory conclusions about the role of social closeness on its own in influencing voting behavior. I contend that instead of viewing the network as an important determinant of behavior on its own, we should see it as a thoroughfare through which information diffuses between legislators. When legislators are called on to vote on a question that they do not understand, they take cues from experts who are nearby in the legislative network. This network is indeed important for legislative outcomes, because it allows Congress to reach informed collective decisions even though individual legislators are uninformed on many issues. The usefulness of the network for transmitting cues also suggests that network structure may be a determinant of legislative effectiveness and plays a key role in the dynamics of partisan polarization.

One of the main reasons that political science still lacks a convincing test of the relationship between expertise, the legislative network, and voting behavior is that establishing this relationship is hard. The specter of homophily frustrates many otherwise promising approaches. If a legislator votes with a trusted, expert peer, it may be for reasons that have nothing to do with that peer's expertise. Perhaps they have similar ideologies or similar electorates and would have voted together even if they had both been experts or both been ignorant. Perhaps the party pressured both of them into voting the same way.

To overcome these difficulties, instead of trying to study variation in the structure of the network, as many previous studies have done, I take the network as given and study how one legislator's acquisition of expertise changes the behavior of his neighbors in the network. I exploit a natural experiment wherein legislators are assigned to committees midsession because of the death, resignation, or transfer of the seat's previous occupant. Once assigned to the committee, legislators acquire expertise in their new committee's jurisdiction. I find that peers who have frequently cosponsor that legislator's bills in the past—that is, those who are close to the legislator in the legislative network studied by Fowler (2006) and others—respond by voting with the legislator on bills from his new committee's jurisdiction more often after the assignment than they did before. I estimate that this kind of cue-taking accounts for between 5.4% and 31.5% of all voting decisions.

My results also show that this is a remarkably robust phenomenon. My sample encompasses the period from 1979 to 2015. Congress changed dramatically during this time in ways that would seem on their face to undermine cue-taking. Partisan polarization has sharply increased, and party competition has intensified (Lee 2009; Mann and Ornstein 2016; Poole and Rosenthal 2001; Theriault 2008). Members of Congress now spend less time in Washington socializing with their colleagues (Ornstein and Mann 2006). Yet I find that these changes have not compromised the influence expert legislators enjoy over their peers. I find that legislators take cues from members of the opposite party, in both the specialized, hierarchical House and the generalist, collegial Senate, and that cue-taking survived into the present day in spite of mounting partisan polarization. Taken together, these results show that taking cues from trusted, expert peers is not some folkway of the textbook Congress—an artifact of an unusual period that has since gone extinct. Rather, it is a fundamental mechanism for coping with the pervasive problem of incomplete information.

2. Caldeira and Patterson (1987) found that ideological similarity and constituency similarity both predict friendship between legislators.

THE CHALLENGE OF STUDYING CUE-TAKING FROM EXPERTS

To motivate my research design, it is first helpful to more carefully consider why legislators take cues from experts, how cue-taking happens, and what parts of the process can be observed. Legislators want to vote for bills that are high valence (Hirsch and Shotts 2012; Ting 2009), close to their ideal points, and likely to be popular with their constituents. However, legislators need expertise in order to figure out whether a bill is high valence and to decipher the ideological location of a bill in a potentially high-dimensional ideological space (so they can compare it to their own ideal point and that of their constituents). Without expertise, legislators face considerable uncertainty as to whether they ought to vote for or against a proposal.

When legislators are confronted with a vote on a question that they do not fully understand, they have several options available. First, they can exert effort to build understanding, either by studying the issue themselves or by assigning their staff to study the issue for them. However, a legislator's time is scarce, and his staff must often focus on committee work and constituency service (Matthews and Stimson 1975). Second, he can absent himself from the vote, either by missing it or by abstaining. However, frequent abstention or absenteeism could prove an electoral liability and also forfeits a potentially valuable opportunity to make the "correct" choice from the perspective of his ideology or constituency. Moreover, the legislator sometimes has a much better option available to him. If there is a broad consensus within the legislature or within his party about the right way to vote, he can vote with the majority on the theory that what is right for most legislators is probably right for him as well (Kingdon 1989).

But legislators have a still better option, one that they can deploy even on divisive issues. Because of the division of labor within Congress institutionalized by the committee system, for any given vote, some subset of the legislature possesses the expertise to discern what is at stake. These expert legislators are promising sources of information about how to vote because they understand not only the policy implications of the question at hand but also the political ramifications (such as how interest groups, constituencies, and other legislators are likely to react to a given vote) that are so central to legislators' interests (Matthews and Stimson 1975). The behavior of these expert legislators acts as a signal to nonexperts about how they would vote were they fully informed.

It is of course an imperfect signal, because no two legislators are exactly alike. The voting decisions of some legislators are less useful than others. When voting on an energy bill, a probusiness conservative cannot infer too much from the voting decision of an environmentalist liberal because the pair have very different criteria they use to decide how to vote.[3] The most informative signals come from those with whom the legislators share some common ground as to what constitutes good legislation.

Noticing that an expert peer voted one way or the other (by watching him vote, checking the electronic tally, or even asking the doorman) provides some information about how an uninformed legislator should vote, but actually talking to the expert about his rationale provides even more. These conversations might result from deliberately seeking out the expert's advice in advance or from chance encounters. The legislator can learn more from his conversation with the expert when they have similar tastes in legislation (because then the expert is more likely to notice considerations that are relevant to the legislator) and when the legislator trusts that the expert is telling him the whole truth. Accordingly, cues from a legislator will have the largest effect on those closest to him in the legislative network insofar as network connection tracks similarity in tastes and interpersonal trust.

As a result of the numerous potential channels, directly observing cue-taking from expert peers is extremely difficult. It is hard to track which peers legislators converse with and what they talk about and even harder to measure whose votes they take into consideration when casting their own and what weights they place on them. But cue-taking from experts has one readily observable implication: if a legislator begins taking cues from one of his peers (because that peer acquires expertise that the legislator himself lacks), then his votes will agree with the expert's more often after the acquisition than they did before. The more informative those cues are for the legislature and the more they contradict the alternative heuristic of voting with the party, the larger the observed effect will be.

RESEARCH DESIGN

Identifying the effect that the acquisition of expertise has on the votes of the expert's peers requires a research design with three properties. First, it must use a treatment that causes a legislator to acquire expertise but is uncorrelated with other events that might influence how often legislators vote together. Second, it must provide a measure of which legislators are most likely to take cues from the newly minted expert. Third, it must construct a plausible counterfactual for how often the

3. Ringe, Victor, and Gross (2013) raise the possibility of negative cue-taking: awareness of ideological dissimilarity and voting against the cue-giver to exploit that dissimilarity. I find negative cue-taking implausible in the congressional context. Many motions pass or fail by lopsided majorities, so the support or opposition of an ideologically distant legislator cannot send a reliable signal in these numerous cases. Moreover, negative cue-taking ignores the role of valence (Hirsch and Shotts 2012; Hitt, Volden, and Wiseman 2017) in legislative voting decisions.

pair would have voted together had the expert not acquired expertise. To that end, I estimate the effect of expertise on the voting decisions of nearby (in the network sense) peers by using the following model:[4]

$$y_{i,j,c,t+1} - y_{i,j,c,t-1} = e_{i,c,t}\gamma + x_{i,j,t}\theta + e_{i,c,t}x_{i,j,t}\delta + z_{i,j,c,t}\beta + \varepsilon_{i,j,c,t}, \quad (1)$$

where $y_{i,j,c,t}$ is the agreement rate (the number of bills where they both voted the same way divided by the number of bills where both voted either yea or nay) between legislator i and legislator j for votes on bills in the jurisdiction of committee c during Congress t,[5] $e_{i,c,t}$ is a binary indicator for whether i acquired expertise in c's jurisdiction during t, $x_{i,j,t}$ is a binary indicator of whether j is connected to i in a directed network (i.e., a network where j is connected to i does not imply i is connected to j), $z_{i,j,c,t}$ is a vector of additional covariates that I will enumerate shortly, and $\varepsilon_{i,j,c,t}$ is a normally distributed error term. I predict that $\delta > 0$; that is, the agreement rate increases between Congresses $t-1$ and $t+1$ when i acquires expertise in c during Congress t and j is connected to i in the network. Estimating this model requires a treatment that causes i to acquire expertise, an operational definition of the network that serves as a channel for the diffusion of cues, and, since it is a difference-in-differences design, a justification of the parallel trends assumption.

Late committee assignments as the treatment

When a legislator joins a committee, he acquires expertise in that committee's jurisdiction (Gilligan and Krehbiel 1987; Salisbury and Shepsle 1981; Waxman 2009). Through regular attendance in committee hearings, markups, and meetings with interested parties, the legislator can quickly become more knowledgeable than colleagues outside of the committee.[6] Consequently, committee assignments offer an attractive opportunity to study the effects of expertise on the legislative process.

However, committee assignments are constrained by the supply of open seats, and approximately 95% of these open seats are assigned at the beginning of each Congress (Deering and Smith 1997; Frisch and Kelly 2006). This poses potential problems for using committee assignments as expertise-increasing treatments. New committee assignments are correlated with turnover, so if I used all committee assignments my sample would be disproportionately drawn from sessions with high turnover. If parties put very contentious issues on the agenda when they anticipate that turnover going into the next session will be high (because they want to take advantage of their majority while they still have it), it would inflate the agreement rates in the prior Congress for both treatment and control pairs relative to the population. The estimated effect could be attenuated because there is less room to make a difference. Alternatively, if parties put only inoffensive measures on the agenda when they anticipate the next election will be very competitive (because they do not want to put their vulnerable members at risk by forcing them to take a stand on controversial issues), it would suppress agreement rates in the prior session for both treatment and control pairs. This would bias the estimated effect away from zero. Likewise, after an election that leads to substantial turnover, the incoming majority may feel it has a mandate to pursue highly partisan policies, inflating the agreement rate of copartisan pairs in the sample relative to the population average while deflating the agreement rate of opposite-party pairs.

Thus, the timing of most committee assignments is correlated with shifts in the balance of political power and concomitant changes to the agenda. If these assignments acted as treatments, then any detected treatment effect may be contaminated by these other factors. To overcome this obstacle, I rely on a natural experiment: committee assignments that are made after the beginning of the session.[7] These midsession assignments occur for two reasons. First, the previous occupant of the seat may have died or resigned from the legislature. Second, the previous occupant of the seat may have transferred to another committee (usually because a seat on that committee opened because of the resignation, death, or transfer of its previous occupant). The timing of these midsession assignments is therefore uncorrelated with elections or significant

4. This analysis assumes no second-order cue-taking. That is, if one nonexpert takes cues from an expert, he does not himself send these cues to other nonexperts. Second-order cue-taking, if it occurs, attenuates the estimates because it would inflate agreement rates between control pairs and between treated pairs, where j is not directly connected to i on the network.

5. I define a bill as within the committee's jurisdiction if it was referred to that committee by the Speaker. Multiply referred bills count are within the jurisdiction of every committee to which they are referred.

6. This design assumes the congressman is able to acquire some valuable expertise over the course of Congress t. This is plausible. He may acquire even more expertise over subsequent Congresses, and it would be possible to study the magnitude of the cue-taking effect for longer lags. This relatively short lag constitutes a hard test, and since I find evidence for a cue-taking effect even with this short lag, I do not pursue longer lags.

7. I assume a constant treatment effect across different committees. The small number of midsession committee assignments makes it impractical to estimate a heterogeneous effect on a committee-by-committee basis. Examining which committees provide the most influential expertise is an intriguing question for future research.

Table 1. Representativeness of Midsession Assignments

	Model 1
Intercept	−1.911*
	(.190)
Tenure in chamber	.008
	(.038)
CF score distance from chamber median	.006
	(.178)
Legislative effectiveness score	−.047
	(.080)
Cosponsorships of member's bills	−.000
	(.000)

Note. Logistic regression for which a midsession assignment dummy is the outcome variable. Sample consists of all nonfreshmen legislators (because the outcome for the main analysis in this article is only defined for nonfreshmen), although the results are robust to their inclusion. Legislators who receive midsession assignments are indistinguishable from nonfreshmen who receive new assignments at the beginning of the session. CF = campaign finance. $N = 2{,}553$.
* $p < .05$.

changes in the composition of the legislature that might affect the outcome variable.

Of course, legislators who transfer onto a committee midsession are far more interested in the issues in the committee's jurisdiction than the average legislator is, and accordingly their propensity to respond to the committee assignment by acquiring expertise is much greater. Fortunately, the most important quantity of interest is the average treatment effect on the treated—the effect of expertise acquisition from the types of legislators who actually get assigned to the committee. These midsession committee joiners are in important ways similar to the legislators who are already on the committee: they too are interested enough in the committee's jurisdiction (either through their personal backgrounds or through their constituents' demands) to request assignment to the committee, and the party leaders and Committee on Committees see them as suitable choices for the assignment. Table 1 assesses whether legislators who receive midsession assignments are systematically different from nonfreshmen legislators who receive new assignments at the beginning of the session.[8] They have served the same amount of time in Congress, they are ideologically no more or less extreme by campaign finance (CF) scores from Bonica (2014), and they are no more or less effective as measured by the legislative effectiveness scores from Volden and Wiseman (2014).[9] Even more importantly for the purposes of the analysis, their bills over the three sessions before their assignments attract the same number of cosponsors as the bills of members who receive assignments at the beginning of the session.

The cosponsorship network as a moderator

I follow Fowler (2006), Kirkland (2011), Kirkland and Gross (2014), Tam Cho and Fowler (2010), and others in measuring the connection between legislators through the cosponsorship network. If j frequently cosponsors i's bills, then I say j is connected to i.[10] Thus, $x_{i,j,t} = 1$ if i and j are copartisans and j has cosponsored at least 10 of i's bills in the three Congresses preceding t or if i and j are not copartisans and j has cosponsored at least five of i's bills in the three Congresses preceding t, and $x_{i,j,t} = 0$ otherwise.[11] The lower threshold for opposite-party pairs reflects the fact that cosponsorship of another party's bill is a stronger signal of a connection and that cues from members of the opposite party may be more powerful because the information they transmit is less redundant with other heuristics, such as voting with the majority of one's party. These thresholds are exacting enough to capture meaningful relationships between legislators while permissive enough to create a sufficient variation in connection for statistical inference. By this metric, j is connected to i in 3.3% of the pairs in my sample. Online appendix A offers further discussion of my decision to dichotomize and shows the robustness of my core results to different choices of thresholds.

While the cosponsorship network has been employed elsewhere in the literature, it is particularly appropriate for studying the influence experts have on their peers. High levels of cosponsoring make prospective cues both more useful and more credible. First, cosponsoring indicates approval of the

8. If I include freshmen, there is a statistically significant relationship between midsession assignment and tenure in the chamber (meaning that many midsession assignments go to freshmen), but all other relationships remain insignificant. This does affect the analysis because my outcome is the change in agreement rate, which is only defined for nonfreshmen.

9. The CF scores and legislative effectiveness scores are missing for a number of the observations. The results in table 1 are robust to excluding them to enlarge the sample.

10. My definition of connectedness is asymmetric: if j is connected to i, that does not imply that i is connected to j.

11. Note that $x_{i,j,t}$ is not indexed by c. In other words, I do not require that the cosponsored bills fall under the jurisdiction of the committee that i will later join. This is because legislators primarily sponsor bills that will be referred to committees on which they are currently members. By construction, i is not a member of c for the Congresses preceding t, so i will in general sponsor very few bills in c's jurisdiction during that period. Using cosponsorships across all jurisdictions casts a wider net for identifying those who are connected to the committee joiner. If anything, this design decision biases against finding a cue-taking effect through attenuation bias.

sponsor's bill and implies that the sponsor and cosponsor share some common ground as to what constitutes good legislation (Harward and Moffett 2010; Kessler and Krehbiel 1996). This increases the likelihood that the sponsor will identify features of legislation that are important to the cosponsor and also makes the sponsor's vote on any given piece of legislation a more useful proxy for how the cosponsor would vote were she fully informed. Second, cosponsoring demonstrates that the cosponsor is aware of the sponsor's legislative activities. This means that the cosponsor will have opportunities to take cues from the sponsor, by either interacting with him directly or observing his behavior. Third, cosponsorship is a valuable form of legislative support (Bernhard and Sulkin 2013; Koger 2003). The sponsor has an incentive to communicate truthfully with someone who frequently cosponsors his legislation, or else he risks alienating a useful ally. Fourth, insofar as legislators are more likely to cosponsor legislation when a personal friend is the sponsor, cosponsorship levels are correlated with interpersonal friendship. This decreases the incentive for the prospective cue-givers to dissemble and the likelihood they will succeed if they try. Fifth, a legislator must sponsor many attractive bills in order to receive connections from others, and so connections in the cosponsorship network point to the most enterprising, entrepreneurial legislators (i.e., those most likely to send informative cues). These features, taken together, approximate what Matthews and Stimson (1975) seem to mean by trust.

Matched differences in differences

One could take the agreement rate of i and j during $t+1$, the Congress after the midsession assignment, as the outcome. However, the resulting estimate could be confounded by the possibility that i and j have similar ideologies or similar constituencies. By using first differences, the change in the agreement rate between $t-1$ and $t+1$, as the outcome, my design controls for legislator characteristics that vary slowly over time, such as ideology and constituency characteristics.

But a first-differences analysis without further correction also suffers from potential sources of confounding. Increasing polarization, changes in the majority party status, and the introduction of new issues onto the legislative agenda might influence the change in agreement rate. Fortunately, these forces affect similar pairs of legislators similarly. Accordingly, I adopt a difference-in-differences design that compares the change in agreement rate for a treated pair of legislators (where one legislator was assigned to a committee midsession) to the change in agreement rate for a control pair of legislators (where neither was a member of the relevant committee between $t-1$ and $t+1$).

This difference-in-differences design relies on the parallel trends assumption. The change in the control pair's agreement rate must in expectation approximate how the treated pair's agreement rate would have changed if the cue-taker had not joined the committee. In order to make the parallel trends assumption credible, I match every pair of potential cue-giver and potential cue-taker to a similar control pair. I assemble a set of candidate matches by gathering all control pairs (i', j') such that i' is the same party as i, j' is the same party as j, and neither i' nor j' is a member of c (the committee that i joined during Congress t) between $t-1$ and $t+1$.

Within this set of candidate matches, I choose i' and j' to minimize the Euclidean distance between (i, j) and (i', j') on the following variables:

- The agreement rate in the Congress before the midsession assignment ($y_{i,j,c,t-1}$).
- The number of times j has cosponsored i's bills between $t-3$ and $t-1$.
- The number of times i has cosponsored bills by copartisan members of committee c between $t-3$ and $t-1$.[12]
- The number of times i has cosponsored bills by opposite-party members of committee c between $t-3$ and $t-1$.
- The number of times j has cosponsored bills by copartisan members of committee c between $t-3$ and $t-1$.
- The number of times j has cosponsored bills by opposite-party members of committee c between $t-3$ and $t-1$.

Before calculating the Euclidean distance between (i, j) and potential matches, I normalize each of these variables by their standard deviations in the population.

This matching strategy addresses a host of threats to the parallel trends assumption. Requiring that i and i' are copartisans and j and j' are copartisans ensures that increases in polarization and changes in the identity of the majority party will in expectation affect treated and control pairs similarly. Matching using cosponsorship levels ensures that the ideological similarity between the pairs is comparable, and therefore changes to the agenda will affect them similarly. Finally, matching using the number of times each legislator in the pair cosponsored committee members' bills before the treatment ensures that treated pairs and control pairs are comparably

12. "Members of the committee" are defined as those who are members of the committee at some point during Congress t.

connected to the committee before the midsession assignment. To validate that this matching makes the parallel trends assumption plausible, I present a placebo test in appendix A. I find that the same matching strategy applied to a placebo treatment (a dummy that is 1 two Congresses before the midsession assignment actually happens) leaves the effect of expertise and all of its interactions statistically insignificant.

Model specification

After assembling my data set via the preceding matching procedure, I estimate the effect of the acquisition of expertise on the change in agreement rates using the linear regression described in equation (1). In addition to the treatment (midsession assignment for i), the moderator (whether j is connected to i in the directed cosponsorship network), and their interaction, I control for the following covariates:

- Whether i and j are copartisans and the interaction of this variable with the treatment and with the moderator.
- The number of times i cosponsored bills by copartisan members of committee c between $t-3$ and $t-1$ and also the interaction of this count with the treatment.
- The number of times i cosponsored bills by opposite-party members of committee c between $t-3$ and $t-1$ and also the interaction of this count with the treatment.
- The number of times j cosponsored bills by copartisan members of committee c between $t-3$ and $t-1$ and also the interaction of this count with the treatment.
- The number of times j cosponsored bills by opposite-party members of committee c between $t-3$ and $t-1$ and also the interaction of this count with the treatment.
- Congress-committee-party-copartisan fixed effects (the four-way interaction of these variables, with Congress treated as a factor rather than as an integer). These four-way fixed effects make the comparisons within a particular Congress and committee (so that the agenda is similar across pairs) and within party distribution (so that, e.g., Democrat to Republican pairs are compared to other Democrat to Republican pairs). This controls for changes to the agenda over time and differences of the agenda across committees.
- The number of bills i and j voted on during Congress $t-1$ and the number of bills i and j voted on during Congress $t+1$. These allow for estimation of heteroskedastic-consistent standard errors that account for the fact that the variance of agreement rates is a function of their denominators.

Dependence between observations

I assemble my treated observations by pairing each legislator who joins a committee midsession with every other legislator who was not a member of that committee during Congresses $t-1$, t, and $t+1$. I then construct my sample by matching each treated pair to a similar control pair. Consequently, each late assignment contributes many observations in the data set as he or she is paired with every other member of the legislature, and each legislator potentially appears in a number of pairs within any given Congress. The fact that a single member's voting record recurs in many observations induces a correlation between error terms.[13]

To correct for this correlation, I employ the dyad cluster-robust covariance estimator from Aronow, Samii, and Assenova (2015). This allows the residual of pairs from the same Congress that share a legislator in common to be correlated. This covariance estimator is also heteroskedastic consistent, so including the number of votes taken in $t-1$ and $t+1$ in the set of control variables addresses the heteroskedasticity arising from the use of agreement rates whose denominators vary considerably across committees.

RESULTS

The sample is constructed according to the procedure described above using all midsession committee assignments from the 96th to 113th Congresses (1979–2015) in both the House of Representatives and the Senate. Appendix B describes my data sources in detail. This yields 289 midsession committee assignments, which, when paired with the appropriate set of legislators and matched to appropriate pairs, yields 152,250 observations.[14] By construction only a small minority of pairs (3.3%) have a potential cue-taker who is connected to the potential cue-giver. Together with the dyadic-clustered standard errors, this limits the power of the tests that I can conduct. Accordingly, when testing whether the cue-taking hypothesis holds in particular subsamples (such as in pairs where the legislators are members of the opposite party), I will estimate an interaction term in a single pooled regression

13. It also raises the possibility of interference between units. I argue that interference does not play a meaningful role in generating my results in online app. B.

14. A small number of pairs could not be matched to a pair with the same party split (because one of the members switched parties midsession and was hence classified as a "switcher" instead of a Republican or Democrat). Only 0.5% of treated pairs could not be matched to a control pair, and as a result the inclusion or exclusion of these unmatched pairs does not affect the conclusions of the analysis.

rather than splitting my sample into two subsamples. Additionally, my design is not powerful enough to estimate fourth or higher-order interaction effects.

Taking cues from expert peers

Table 2 presents the results of the differences-in-differences regression (with robustness to different thresholds for connection in the directed cosponsorship network presented in online app. A). Model 1 is the baseline: expertise × connected (the effect of *i* receiving a midsession assignment when *j* has frequently cosponsored *i*'s legislation in the past on the change in agreement rate) is positive, statistically significant, and substantively large. When a connected colleague joins a new committee, the model predicts a 2.9 percentage point increase in the pair's agreement rate on votes in that committee's jurisdiction. In this sample, the average level of precommittee assignment agreement between a connected pair of legislators is 73.5%. The 2.9 percentage point increase due to cue-taking from a connected committee member resolves a substantial proportion of the remaining disagreement and translates to changing the cue-taker's vote on an average of 3.9 roll calls.

Table 2. Effect of Connection, Expertise, and Copartisanship on Change in Agreement

	Model 1	Model 2
Expertise	.0047	.0094
	(.0049)	(.0053)
Connected	−.0103	−.0183
	(.0087)	(.0096)
Expertise × connected	.0285*	
	(.0103)	
Expertise × connected × copartisans		.0136
		(.0096)
Expertise × connected × not copartisans		.0452*
		(.0184)
Expertise × copartisans	−.0114*	−.0159*
	(.0055)	(.0058)
Connected × copartisans	−.0065	.0134
	(.0105)	(.0131)

Note. Model 1 gives the baseline model, and model 2 adds an interaction of the key effect of interest with whether the cue-giver and cue-taker are copartisans. Standard errors are generated by Aronow, Samii, and Assenova's (2015) dyadic standard error correction, which dramatically limits the power of the tests. Coefficients for fixed effects, the number of cosponsorships toward committee members (split out between *i* and *j* and by party), and n_1 and n_2 (the denominators of the two components of the outcome) are omitted for brevity. None of them produce a theoretically interesting result. The full table (sans fixed effects) is available in online app. C. Online app. D considers the robustness of these findings to the possibility of confounding from other sources of cues. Robust standard errors in parentheses. $N = 125{,}130$, where N is less than the total number of treated and control pairs because some pairs are reused multiple times, and these are encoded as weights to account for the fact that their error terms are necessarily identical.

* $p < .05$.

Moreover, cues cross party lines. Table 2's model 2 interacts copartisanship with the main effect. If the effect is restricted to copartisan pairs, then the expertise × connected × not copartisans effect in model 2 should be 0. I find instead that this effect is positive and statistically significant. It is in fact larger than the effect for copartisan pairs, which is not statistically significant at the $p < .05$ level (although this assumption is sensitive to the choice of the thresholds for connectedness; online app. A shows it becomes statistically significant for some thresholds, and hence one should not conclude that there is no effect among copartisan pairs).

How could it be that the cue-taking effect for opposite-party pairs is as large as or larger than the cue-taking effect for same-party pairs? And why is the total cue-taking effect for copartisan pairs statistically significant? After all, copartisan pairs are probably more ideologically similar than opposite-party pairs, and so cues sent by copartisans should be more reliable. The key is that although copartisan cues are probably more reliable, they are also more redundant with other sources of information. In the absence of any source of peer cues, the would-be cue-taker might take cues from the party leadership or vote with the majority of his party (Kingdon 1989; Matthews and Stimson 1975). The votes of a copartisan expert often point in the same direction as these simpler heuristics, and so, consistent with a skeptical perspective on cue-taking, they do not actually influence the cue-taker's behavior. The votes of opposite-party cue-givers are much more likely to contradict these simpler heuristics and, therefore, have more opportunities to change the cue-taker's behavior. Even if the cue-taker is more skeptical of cues coming from members of the opposite party, they could still be more influential.[15]

Relevantly, the interaction of midsession assignment and copartisanship has a negative and statistically significant sign. This is consistent with the account of expertise and voting behavior that I described above. Before possessing expertise, legislators tend to vote with the majority of their party unless cues from the experts they are connected to tell them to do

15. Another less interesting but still plausible possibility is that the difference is driven by a ceiling effect. Opposite-party pairs have substantially more disagreement to resolve. Copartisan pairs vote together an average of 84% of the time during Congresses before the committee assignment, while opposite-party pairs vote together only 55% of the time. Moving from 84% to 85% is plausibly just as impressive as moving from 55% to 59%. In any case, it is inappropriate to infer that there is no cue-taking effect in same-party pairs.

otherwise. Once they acquire expertise, legislators can evaluate for themselves how they should vote. In the majority of cases, they will find that they still want to vote for the majority of their party, which will lead to no change in their probability of agreeing with any given copartisan. In some cases, they will find that it is in their best interest to vote against the party, which suppresses their agreement rate with copartisans (unless those copartisans take cues from the legislator). The result is that the midsession assignment to a committee and the concomitant acquisition of expertise leads to a decrease in the agreement rate with copartisans outside of the committee unless those copartisans take cues from the legislator.

The total impact of cue-taking

The rates of cosponsorship in the legislature at large suggest that the aggregate impact of cue-taking is considerable. To estimate the total number of votes changed by cue-taking for the average legislator across all committee jurisdictions, I find the number of peers that each legislator is connected to and the number of committees on which each of these peers served for every legislator from the 96th to the 113th Congress. A connected peer gives cues for bills relating to each of the committees he serves on, so I add the total number of committee seats held by connected peers, excluding committees on which the legislator himself served. I find that on average each legislator had 45.6 sources of cues (where connected peers on multiple committees are counted multiple times) per session. Multiplying these opportunities by the point estimate of the number of roll call votes changed by each cue-taking opportunity (3.9), I estimate that each legislator changed his vote on 175.9 roll calls per session because of cue-taking. Each Congress has an average of 945.1 votes, so I estimate that cue-taking accounts for 18.4% of all voting decisions. The 95% confidence interval of this estimate is 5.4%, 31.5%. The aggregate influence of all cue-givers across all committees is far reaching indeed.

This might actually underestimate the true magnitude of the cue-taking effect. The sample consists of cue-givers who have joined the committee very recently. As their tenure on the committee increases, their expertise and the intensity of the cue-taking that follows from it may increase proportionately. Additionally, midsession joiners are by definition legislators who were not assigned to the committee at the beginning of the session. While they are the best approximations of the types of legislators who join the committee at the beginning of the session available, the legislators who are able to secure committee assignments at the beginning of the session may have an even greater propensity to acquire expertise and to send reliable cues to their colleagues.

This impressive effect size is consistent with the importance that Matthews and Stimson (1975) ascribe to cue-taking, but how does it square with common accounts that attribute upward of 80% of voting to ideology?[16] Each legislator can only be a member of a handful of the many committees, and so most of the time he is voting on issues he only dimly understands. Peer cue-taking competes with party-based heuristics (such as voting with the majority of the party or with the party's leadership) for this lion's share of the votes. None of these are necessarily substitutes for ideological or electoral concerns; rather, they provide heuristics by which the mapping from these considerations to voting decisions can be inferred. In this light, the cue-taking effect is still impressive, but it is also plausible.

Cue-taking in the House and Senate

The House and Senate are two very different institutions, and many of these differences might relate to the supply of and the demand for expertise. Compared to the Senate, the House is larger, more impersonal, and characterized by greater party centralization and more intense party competition (Oleszek 2014, 26–29); each of these might facilitate greater reliance on cues in the House than in the Senate. But, senators tend to be generalists, while representatives specialize intensely in the areas within their committees' jurisdictions (30–31). As a result, representatives may find themselves voting on issues outside of their expertise more often than senators and that the disparity between their own knowledge on the subject and those of knowledgeable colleagues is even greater. If so, the cue-taking effect should be larger in the House than in the Senate.

To examine these possibilities, I repeat my analysis from table 2 with added interactions for the House and Senate. In table 3, both coefficients are positive, but only the coefficient for the House is significant at the 5% level. The coefficient for the Senate is statistically significant at the 10% level, and its point estimate is larger in magnitude. Thus, one can provisionally infer that cue-taking exists in both chambers. The difference between the two is not statistically significant at the 10% level.

The robustness of cue-taking to recent changes in Congress

The House and the Senate alike have changed dramatically over the past several decades because of the increase in partisan polarization, the intensification of party competition, and the diminishing time legislators spend in Washington (Lee 2009, 2015; Mann and Ornstein 2016; Poole and Rosenthal 2001;

16. For example, Ringe et al. (2013) claim that the literature shows that party and ideology account for 90%–95% of congressional voting decisions.

Table 3. Intercameral Differences in Cue-Taking

	Model 1
Expertise	.0102
	(.0053)
Connected	−.0057
	(.0079)
Expertise × connected × House	.0236*
	(.0112)
Expertise × connected × Senate	.0437
	(.0241)
Expertise × copartisans	−.0167*
	(.0058)
Connected × copartisans	−.0037
	(.0108)
Expertise × Senate	.0030
	(.0160)
Connected × Senate	−.0143
	(.0188)

Note. Same as model 1 from table 2, with additional interactions to estimate the effect by chamber. Interacted effects are both positive, and one is statistically significant at the 5% level while the other is statistically significant only at the 10% level. This is consistent with an effect in both the House and Senate, but the test is underpowered. Robust standard errors in parentheses. $N = 125{,}130$.
* $p < .05$.

Theriault 2008; Uslaner 1991). Polarization might disrupt cue-taking in a number of ways. It might make simpler heuristics, like voting with the majority of the party or with the party's leadership, more effective and therefore reduce the need for cue-taking. It might disrupt social relationships between legislators of opposite parties or otherwise cut off the flow of crosspartisan cues. It might lead to the increased application of party pressure, crowding out opportunities for cue-taking.

In table 4, I find that these changes have not dampened reliance on cue-taking. In this analysis, I interact the effect with dummy variables for whether the midsession assignment occurred before the 104th Congress or during/after the 104th Congress.[17] I split at the 104th Congress for two reasons. First, it is near the midpoint of the sample, both in terms of the number of Congresses and in terms of the number of observations on each side of the divide. Second, the 104th immediately followed the Gingrich Revolution of 1994, which marks a qualitative shift from an era of Democratic domination to one of vigorous and often acrid competition for control of both the House and the Senate. I find strong evidence of cue-taking in both periods, as both coefficients are positive and statistically significant at the 5% level. The difference between their magnitudes is not statistically significant.

The fact that the cue-taking effect survived in the face of a dramatic increase in partisan polarization, coupled with the fact that it obtains in both the House and the Senate, illustrates just how fundamental expertise is to legislative decision-making. Turning to trusted, expert peers for guidance about how to vote is not some folkway of the textbook Congress that has been washed away by the well-documented changes that have taken place since then. Nor is it peculiar to a highly specialized legislature like the House or a generalist but relatively sociable legislature like the Senate. Rather, it is a robust, enduring feature of the legislative process.

However, all of these findings condition on the cosponsorship network. That is, conditional on being connected to one of their peers, legislators still take cues from that peer today as they did decades ago. The network itself changes over time, as figure 1 shows. In particular, since 2010, connections

Table 4. Robustness of Cue-Taking to Recent Changes in Congress

	Model 1
Expertise	.0108
	(.0058)
Connected	−.0029
	(.0098)
Expertise × connected × pre-104	.0229*
	(.0107)
Expertise × connected × 104+	.0344*
	(.0149)
Expertise × copartisans	−.0166*
	(.0058)
Connected × copartisans	−.0030
	(.0104)
Expertise × pre-104	−.0016
	(.0045)
Connected × pre-104	−.0156
	(.0120)

Note. Same as model 1 from table 2, with additional interactions to estimate the effect by time period. Interacted effects are both positive and statistically significant at the 5% level. Since polarization was much higher in the second period than in the first, this shows that increasing partisan polarization has not diminished the importance of drawing expertise from connected peers. Robust standard errors in parentheses. $N = 125{,}130$.
* $p < .05$.

17. In online app. E, I show that using cut-points around the 104th Congress gives similar results. In online app. F, I show that using a binary indicator for whether the difference in the party means on the first dimension of Poole and Rosenthal's (2000) DW-NOMINATE is above or below the historical median for the chamber as a measure of polarization gives the same result.

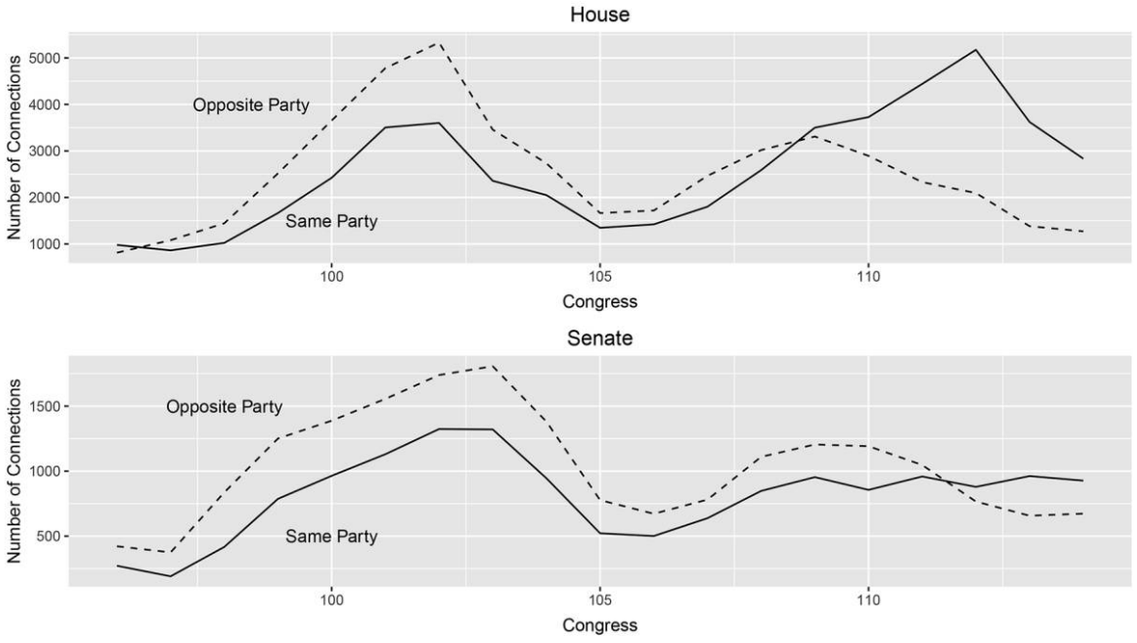

Figure 1. Change in the number of connections over time. Number of same-party and opposite-party connections in each Congress, split out by the House versus the Senate.

between opposite-party pairs have become less common. To the extent that this change in network structure is caused by polarization, this implies that polarization has not dampened cue-taking conditional on the network, but it has diminished the number of opportunities for crossparty cue-taking by making opposite-party pairs less likely to be connected in the first place.

ALTERNATIVE EXPLANATIONS

So far, I have cast midsession committee assignments as shocks to a legislator's expertise in a particular domain and interpreted concomitant changes in agreement rates as efforts to leverage that expertise. Both of these interpretations can and should be scrutinized. I consider three important objections. First, are cue-takers attempting to appropriate the cue-giver's expertise or caving to pressure from the so-called cue-giver? Second, is the cue-taker, rather than the cue-giver, the one actually changing his or her voting behavior? Third, is the important feature of committee membership instead that it gives the supposed cue-giver influence over the agenda?

Pressure

Committee members no doubt care about the fate of many of the bills that their committee reports and attempt to influence others to achieve their preferred outcomes. Indeed, deliberate and unsolicited attempts to influence other legislators may well give rise to some of the interactions through which cues are diffused. Why do others find these appeals persuasive? Is it the superior information that the presumptive cue-givers possess or social pressure from a legislative ally?

If the influence was predicated on social pressure, the effect would occur only for those who need the legislator's goodwill. If the purported cue-giver frequently cosponsors the cue-taker's legislation, then he is in a better position to pressure the cue-taker because he can threaten to withhold his cosponsorship in the future. If pressure rather than expertise explains the results, then the coefficient should be large when i frequently cosponsors j's legislation—that is, where i is connected to j (as opposed to the usual j is connected to i). Table 5 repeats the baseline analysis but interacts the interaction of interest (and lower-order terms of the interaction) with whether i is connected to j. It finds a negative, albeit statistically insignificant, effect—the opposite of what a pressure-based explanation would predict. This suggests that information and expertise provide a better explanation of the results than does social pressure.

Changes to the cue-giver's behavior

Because agreement rates are symmetric with respect to the cue-giver and cue-taker, the observed change could in principle emerge purely from changes in the committee joiner's voting behavior. Indeed, increased participation and growing expertise should influence the committee joiner's voting decisions. Alternatively, the cue-giver might come into contact with new interest groups that successfully induce him to

Table 5. Effect of Connection and Expertise When the Cue-Giver Seldom Cosponsors the Cue-Taker's Legislation

	Model 1
Expertise	.0100
	(.0053)
$\text{Connected}_{i,j}$	−.0077
	(.0083)
$\text{Connected}_{j,i}$	−.0113
	(.0084)
Expertise × $\text{connected}_{i,j}$	.0326*
	(.0111)
Expertise × $\text{connected}_{i,j}$ × $\text{connected}_{j,i}$	−.0264
	(.0163)
Expertise × copartisans	−.0167*
	(.0058)
$\text{Connected}_{i,j}$ × copartisans	−.0024
	(.0108)
Expertise × $\text{connected}_{j,i}$	.0180
	(.0097)
$\text{Connected}_{i,j}$ × $\text{connected}_{j,i}$	.0000
	(.0123)

Note. Same as model 1 from table 2, adding $\text{connected}_{j,i}$ and its interactions. It tests whether the alleged cue-taking effect is restricted to pairs where j relies on i's goodwill and hence might be especially susceptible to pressure. If that were the case, the three-way interaction should be positive and significant. It is not. Robust standard errors in parentheses. N = 152,250.

* $p < .05$.

change how he votes.[18] However, for the purposes of this study, the question is not whether legislators adjust their behavior after joining a committee but whether these adjustments explain away the observed cue-taking effect.

If the purported cue-taker's behavior is changing, then his agreement rate with other legislators ought to change in response to the cue-giver's assignment to the committee. In particular, he should vote with committee members who vote like the cue-giver more often after the assignment than before. Table 6 shows the effect of i's midsession assignment on the change in agreement rate between j and the committee member who voted most similarly to i before his assignment. Since the goal is to use the committee member as a proxy for how i would have voted if he were a member of the committee during the whole period under consideration, the analysis is restricted to matched pairs where i and the committee member agreed at least 90% of the time before the assignment. The positive and statistically significant coefficient for Expertise × connected (to i, not the committee member) shows that legislators connected to the cue-givers must be changing their behavior in response to the assignment because they begin voting more similarly to legislators who were on the committee the whole time and who were thus presumably unresponsive to i's assignment.

Changes to the agenda

Committee members accrue expertise, but committee membership confers another resource that could potentially account for the observed changes in agreement rate: the right to vote on committee business. The right to vote affords members some influence over the committee's agenda. It may be that the cue-takers are not in fact taking cues at all but rather the agenda has shifted toward issues on which they and the cue-taker agree. Given how few bills pass in any given session, it is highly unlikely that the committee joiner is able to get his or her own legislation passed by the committee and put onto the House floor. It is possible, however, that the joiner is able to influence the legislation reported out of the committee by participating in markup or by changing the ideological composition of the committee.

However, a committee member's influence over the agenda is predicated on actually being a member of the committee.

Table 6. Change in Agreement between the Cue-Taker and the Committee Member Most Similar to the Cue-Giver

	Model 1
Expertise	.0066
	(.0059)
Connected	−.0285
	(.0157)
Expertise × connected	.0425*
	(.0173)
Expertise × copartisans	−.0008
	(.0070)
Connected × copartisans	−.0040
	(.0164)

Note. Same as model 1 from table 2, except the outcome is replaced by the agreement rate between j and the member of committee c who voted most similarly to i during $t - 1$ (before the assignment). Sample is restricted to matched pairs, where i and the committee member voted together at least 90% of the time in order to ensure that the committee member's voting decisions would provide a reliable proxy for i's. Robust standard errors in parentheses. N = 72,689.

* $p < .05$.

18. It is also possible that lobbyists induce the cue-giver to try to influence the cue-taker, but insofar as this influence is predicated on expertise, this counts as cue-taking.

That influence dramatically declines when the legislator leaves the committee. Table 7 repeats the main analysis with one small but essential change: the sample consists of legislators who leave a committee midsession. These midsession departures are often the consequence of transferring to another committee, although legislators sometimes leave a committee for other reasons (such as lightening their workloads). If cue-taking is predicated on influencing an agenda, there should be a large negative effect of leaving the committee. If it is predicated on expertise, it may not, because expertise may dissipate more slowly. Table 7 shows that the agreement rate between cue-giver and cue-taker does not suddenly drop when the cue-taker leaves the committee. Although the magnitude of the estimate is not particularly informative, the fact that it is neither large (in the negative direction) nor statistically significant implies that my results cannot be explained by influence over the committee's agenda.

This analysis also provides some guidance on the nature of legislative expertise. On the one hand, expertise could be predicated on being generally knowledgeable about the domain—understanding the major policy challenges, knowing which interest groups provide reliable information, having a keen intuition for how voters will perceive features of a policy, and so on. This sort of expertise would be transferable across legislation and durable over time. On the other hand, cue-takers might not be satisfied with this broad expertise. They might instead seek to leverage a narrower form of expertise based on participating in committee deliberations, painstakingly parsing the language of the bill, and wading into the technical complexity of the issue at hand. Both of these forms of expertise are essential to the legislative process, and they are moreover interrelated in that some broad expertise is needed to acquire narrow expertise and the acquisition of narrow expertise probably builds broad expertise. The results from table 7 suggest that broad expertise is sufficient to retain influence over nonexpert peers.

Table 7. Effect of Leaving a Committee on Change in Agreement

	Model 1
Exit	.0127
	(.0069)
Connected	.0079
	(.0085)
Exit × connected	.0131
	(.0103)
Exit × copartisans	−.0083
	(.0061)
Connected × copartisans	−.0165
	(.0115)

Note. Same as model 1 from table 2, with the "expertise" variable labeled more literally as "exit." I include the same unreported covariates and fixed effects and also use the Aronow, Samii, and Assenova (2015) correction for dyadic standard errors. Robust standard errors in parentheses. N = 130,716.

CONCLUSION

Legislators are regularly confronted with voting decisions that they have neither the time nor the inclination to fully understand. Building on a literature that stretches back to Matthews and Stimson (1975), I have shown that legislators use their networks to cope with this problem by taking cues from nearby peers in the network who possess expertise that they themselves lack. These cues are pervasive. They cross party lines, seem to influence behavior in both the House and the Senate, persist in the face of mounting polarization, and explain a sizable portion of congressional roll call votes. This robust reliance on cue-taking shows that legislators are well aware that they often lack the information necessary to make informed political decisions and that looking to network neighbors who possess that information is a fundamental mechanism for coping with this problem. It also shows how the legislature can make well-informed collective decisions even when most legislators are not experts on any given question and shows a heuristic that legislators can use to vote in a manner consistent with their principles and the best interests of their constituents.

The widespread and consequential use of cues supports information-based accounts of congressional politics. Actors who possess expertise that others lack, such as committee members, political parties (Minozzi and Volden 2013), committee leaders (Box-Steffensmeier et al. 2015), presidents (Howell et al. 2013), and interest groups (Grossman and Helpman 2001), can use their superior information to influence others' voting decisions. The more reliable sources of information available to a legislator, the less influence any one source has over him. Little wonder, then, that senior legislators, with their vast networks of close colleagues, vote with their parties less frequently than their junior colleagues (Stratmann 2000). Specialization and the expertise that accompanies it can fortify the legislator against outside influences, but only if those in possession of expertise are properly positioned to send cues to their colleagues.

Cue-taking does not just support informational theories. It elaborates on them by clarifying that one's position in the legislative network confers greater or lesser access to others' expertise, as well as a greater or lesser ability to influence others with one's own expertise. This concentrates power in the

hands of those enterprising legislators who form the connections with the most peers.[19]

The research design employed advances the study of the legislative network and offers a template for the study of all kinds of social networks. Previous attempts to identify effects in the legislative network have struggled with homophily. Do networks neighbors act similarly because they are neighbors or because they are similar? My design for testing the cue-taking hypothesis leverages two important tools to overcome the problem of homophily: it studies how the application of a treatment to a single unit (committee assignment) changes behavior in a fixed network over time, and it establishes a clear expectation about how the effect should diffuse through the network (to the treated unit neighbors but to nobody else). This design can be replicated in other network settings where panel data and strong theoretical expectations are available and where randomized experiments may suffer from lack of external validity.

These findings also show that legislators, like ordinary citizens, use network-based heuristics in deciding how to vote (Huckfeldt 2001; Sokhey and Djupe 2011; Sokhey and McClurg 2012). The idea that ordinary voters often lack the information necessary to make a fully informed decision has a long history in the study of voting behavior in the mass public (Campbell et al. 1960). However, it has often been implicitly assumed that political elites, including members of Congress, do not suffer from such informational shortfalls. Research on bounded rationality, however, suggests that the need for heuristics is based on the relationship between the difficulty of the problem to be solved and the resources available to solve it (Bendor 2010; Simon 1996). Even though members of Congress, are far more attentive to politics than ordinary voters and have valuable resources, such as their staffs, to help them make decisions, the choices they must make are far more difficult. Cue-taking recognizes the difficulty of voting on legislation and that a congressman cannot hope to acquire the requisite expertise on every issue that might come before the legislature. In this way, it implies that voting in Congress and in the mass public are more similar than is commonly acknowledged.

APPENDIX A
JUSTIFICATION OF THE PARALLEL TRENDS ASSUMPTION

There are many design decisions implicit in this matching strategy, such as the choice of underlying measures, the decision to bin, and the mapping from measures to bins. To

19. Connections are concentrated in a relatively small number of legislators: 67% of connections in the cosponsorship network go to just 20% of legislators, while 35% of legislators are not the target of any connections at all.

Table A1. Placebo Test of the Joint Effect of Expertise and Connection

	Model 1
Placebo expertise	.0048
	(.0065)
Connected	−.0031
	(.0099)
Placebo expertise × connected	.0109
	(.0118)
Placebo expertise × copartisans	−.0048
	(.0069)
Connected × copartisans	−.0195
	(.0104)
Count of *i* cosponsoring copartisan committee member's bills before assignment	−.0001
	(.0001)
Placebo expertise × count of *i* cosponsoring copartisan committee member's bills before assignment	.0001
	(.0001)
Count of *i* cosponsoring opposite-party committee member's bills before assignment	.0001
	(.0002)
Placebo expertise × count of *i* cosponsoring opposite-party committee member's bills before assignment	−.0001
	(.0001)
Count of *j* cosponsoring copartisan committee member's bills before assignment	.0001*
	(.0001)
Placebo expertise × count of *j* cosponsoring copartisan committee member's bills before assignment	−.0000
	(.0001)
Count of *j* cosponsoring opposite-party committee member's bills before assignment	−.0001
	(.0001)
Placebo expertise × count of *j* cosponsoring opposite-party committee member's bills before assignment	−.0001
	(.0002)
Number of bills voted on during $t - 1$	.0002*
	(.0001)
Number of bills voted on during $t + 1$	−.0001
	(.0001)

Note. A pair is treated with placebo expertise during Congress $t - 2$ if the cue-giver will join the committee midsession during Congress *t*. Coefficients for fixed effects are omitted from the table for parsimony. Robust standard errors in parentheses. $N = 56{,}840$.
* $p < .05$.

justify these decisions, I appeal to a placebo test that shows they are jointly sufficient to make the parallel trends assumption plausible. Suppose I examine the outcomes two periods before they are actually treated (so that the outcome is $y^c_{i,j,t-1} - y^c_{i,j,t-3}$). The average treatment effect on the treated in this case should be 0, because i did not actually acquire expertise during this period. If I use the matching procedure on this placebo treatment and observe a nonzero effect, then it would suggest that differences in the main analysis were caused by underlying differences between the treated and matched pair rather than the treatment itself. However, table A1 shows that this is not the case. Placebo expertise and the interaction of placebo expertise and network connection (the main coefficient of interest) are small and statistically insignificant. Together, these suggest that the matching has achieved its intended purpose.

APPENDIX B
DATA SOURCES

I rely on two data sources. First, I use OpenGov's application programming interface to extract information about bills and votes. From these, I obtain the committees to which each bill was referred, the sponsor and cosponsors of each bill, how each legislator voted in each roll-call vote, the bill associated with each roll call vote, and miscellaneous characteristics about the legislators themselves for the 93rd–113th Congresses.[20] I use the cosponsorship data obtained from OpenGov to measure connection between legislators. To ensure that my results are not biased by truncation of the cosponsorship data (which begin in the 93rd Congress), I restrict my sample to the 96th–113th Congresses.[21] This period begins in 1979 and, thus, constitutes an out-of-sample test of Matthews and Stimson's (1975) conclusions from their interviews. I use the committee referral data and the roll call vote data to construct the agreement rates, y.

Second, I use committee assignment data compiled by Charles Stewart and Jonathan Woon (for the 103rd–114th Congresses; http://web.mit.edu/17.251/www/data_page.html#2) and Garrison Nelson (for the 80th–102nd Congresses; http://web.mit.edu/17.251/www/data_page.html#0) to construct the midsession assignment variable.[22] Stewart, Woon, and Nelson explicitly code whether each assignment was made midsession, so this is straightforward. I am also able to use the committee assignment to decipher which legislators were on each committee during each Congress and thereby exclude legislators who were already on the committee that the potential cue-giver joined from the sample.

20. OpenGov did not specify the bills to which the roll call votes pertained for the 99th and 100th Congresses (as well as a number of Senate votes), so I added these by hand on the basis of the title of the vote.

21. This is because $T_{i,j,t}$ is the number of i's bills cosponsored by j in Congresses $t-3$, $t-2$, and $t-1$.

22. There were some inconsistencies between the identification conventions used in this source and OpenGov. I resolved these inconsistencies manually.

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