Constructing and Repairing our Bridges: Statistical Considerations When Placing Agents into Legislative Preference Space*

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Abstract

A statistical test of hypotheses regarding the strategic interaction between legislators and third-party agents, such as lobbyists, bureaucrats, or experts, requires some "bridging" method to place each type of actor into preference spaces that are comparable. Current solutions to the bridging problem either attempt to place both legislators and agents into an arbitrary preference space entirely disconnected from the institutional setting, or they attempt to place agents into a legislative roll-call preference space mistakenly as if agents were themselves legislators. I propose a new method that leverages the observed behavioral hypotheses to identify a set of agent-specific bridging parameters that place agents directly into legislative roll-call preference space as agents, rather than counterfactually as legislators. I apply my method to test whether members of Congress condition their questioning of witnesses in committee hearings on preference similarity within the legislator-witness dyad, as a test of lobbying models for strategic information transmission.

1 Introduction

When conducting statistical tests of hypotheses derived from spatial institutional theory, correct measurement of policy preferences among political actors is necessary and fundamental (Clinton, 2012; Poole and Rosenthal, 1997). Policy preferences are typically unobserved, and so the analyst must recover the underlying latent preference scales for a political actor using statistical scaling methods and the observed choices the actor makes in her role within her institutional setting.

Political science has well-established scaling procedures for measuring the latent preferences of legislators using roll-call ideal point scaling (Clinton, Jackman, and Rivers, 2004a; Poole and Rosenthal, 1997), and these measures in and of themselves are informative when testing hypotheses that compare legislators to each other (e.g., Burden, Caldeira, and Groseclose, 2000; Clinton, Jackman, and Rivers, 2004b). Often, though, researchers wish to use estimates of spatial preferences in order to test hypotheses about strategic interactions between legislators and non-legislative agents as they interact within political institutions (Clinton, 2012), as, for example, between legislators and lobbyists (Esterling, 2007; McKay, 2008), or legislators and bureaucrats (Bonica, Chen, and Johnson, 2015), and these third-party agents do not themselves cast roll-call votes.[1]

Since legislators and agents make qualitatively different choices in their respective roles, the latent preference scales recovered for each group typically are not comparable. In the typical case, latent scales do not have a ratio measure – the values on one scale have no intrinsic meaning relative to the other scale – and so the relationship between the scales is not identified (Clausen, 1967; Groseclose, Levitt, and Snyder, 1999; Jessee, 2016). Comparing preferences among legislators and agents therefore requires a data source that can "bridge" across separate, institution-specific measured preference scales (Bailey, 2007;

[1]Modeling interactions between actors from different institutions and domains extends beyond the legislature, for example Bonica and Sen's (2017) work placing judges and attorneys into common space using contribution data, and Ho and Quinn's (2008) work scaling newspapers and political actors based on positions taken in editorials on Supreme Court cases.

Shor, Berry, and McCarty, 2010). The bridge in effect is a functional transformation from one measure space to another, so that the scales are comparable in a way that is implied and required by spatial institutional theories.

As I detail below, current approaches to bridging rely on an identification assumption that a single underlying dimension, such as left–right ideology, structures and constrains preferences for all actors, both legislators and agents, at all times and in all places. Under this assumption the problem for bridging is reduced to sorting legislators and agents along the single dimension. The assumption that there is a unitary structure for preferences across domains and institutional settings is very strong however, particularly among elite actors (Tausanovitch and Warshaw, 2017). Agents and legislators self-select into the different roles; their observed interaction is conditioned by their institutional role, incentives and occupation; and the degree to which ideology constrains preferences will vary across policy topics and over time. For these reasons, when constrained to a single dimension the estimation procedure will likely not recover the underlying preference scales that govern behavior within the observed interaction; instead the preferences will be recovered with error and hence inappropriate for use in statistical tests (Jessee, 2016; Lewis and Tausanovitch, 2015).

To design a valid test of hypotheses, it is necessary for the analyst to recover the preferences of both legislators and agents as they were operative in the observed dyadic interaction, an interaction that is always embedded within a specific institutional setting. Bridging methods must be flexible enough to accommodate the disparate preference structures that arise within dyadic interactions, and to model the preferences of both parties as they were operative in, and arose as a result of, the dyadic interaction.

I propose a method that leverages the hypotheses regarding observed behavioral outcomes to identify a set of agent-specific bridging parameters, parameters that allow the preference dimension for agents to rotate and scale separately from that of legislators. The method recovers legislator-specific and agent-specific preferences that are comparable to each other and relevant to the observed interaction that occurs within the institutional setting. My method uses left-right ideology as a reasonable initial (null) assumption that agents and legislators interact within the left–right dimension, and then recovers posterior estimates of agents' preferences that reveal the structure of preferences that governed the observed interaction. In this approach, if there is a single dimension that structures preferences, the agent-specific parameters will test to zero and the posterior single dimension will be a result rather than an assumption of the analysis. If the posterior agent preferences depart from the left-right dimension, the departure can shed light on the substance of the legislator-agent interaction.

I apply my method to test whether members of Congress condition their questioning of witnesses in committee hearings on preference similarity within the legislator-witness dyad, as a test of lobbying models for strategic information transmission (Austen-Smith, 1992, 1993; Hall and Deardorff, 2006; Krehbiel, 1992). To construct the bridge, I use responses to a battery of ideology attitude measures from a survey that I administered to the witnesses as well as to a set of former members of the U.S. Congress. Using a flexible Bayesian model, I estimate a posterior distribution for each agent's preferences and show how to incorporate these distributions into tests of institutional hypotheses. In the case of health care financing, I recover a quality–cost dimension that structures the expert witnesses' preferences that is largely orthogonal to left–right ideology but relevant to and governs legislators' information seeking behavior. I show that the posterior preference estimates that reflect the rotation described by the agent-specific bridging parameters have better construct validity than preference estimates that constrain all actors' preferences to a single dimension.

2 Challenges in Bridge Engineering

To fix ideas and to simplify the argument, I assume that the legislatively-relevant preferences of agents and legislators each can be described by a single, latent dimension, although the two dimensions need not coincide.[2] Label legislators' *legislatively-relevant* preferences L_l and agents' legislatively-relevant preferences L_a. In addition, I take legislators' L_l preferences as indicated by their observed roll-call votes, and hence recovered correctly through well-established scaling procedures such as `IDEAL` (Clinton et al., 2004a) or `NOMINATE` (Poole and Rosenthal, 1991).[3] Roll-call votes measure legislators' office-induced or operational preferences (Burden et al., 2000; Rhode, 1991), preferences which summarize the full vector of relevant influences on each legislator's public behavior including party, constituency, interest group and donor pressure along with her personal beliefs. Furthermore, the roll-call revealed preferences capture the public ideological reputation that members cultivate for electoral purposes (Cox and Poole, 2002; Dougan and Munger, 1989; Poole and Rosenthal, 1997). For these reasons it is legislators' operational preferences measured in roll-call vote scaling that are relevant to most institutional tests (Burden et al., 2000) rather than legislators' personal or "true" ideology.[4]

With these two simplifications, the statistical task is to bridge or place third-party agents into a comparable legislative preference space in order to model the interaction between legislator and agent using both L_l and L_a. Since agents are not legislators, they do not have the opportunity to vote on legislation, and so roll-call preferences do not and indeed cannot exist for them. The *fundamental problem of bridging* is that legislatively-relevant agent preferences L_a are missing for all agents (see Jessee, 2016).

[2] This assumption is not necessary and multiple dimensions are potentially recoverable using the model I describe below and with an appropriate design.

[3] Roll-call votes are the votes that legislators cast publicly for or against specific proposals. See the appendix for a brief summary of prominent scaling methods used to recover preferences from roll-calls.

[4] In the application below I also examine the relationship between members' personal ideology revealed in their survey responses to their office-induced preferences recovered from roll-call voting and I show how to include covariates that capture external influences on roll-call voting such as party when imputing agents' legislative preference scores.

Bridging in essence is solving a missing data problem using a statistical model, such as one implemented via Bayes rule that derives the posterior probabilities for each agent's preferences using a likelihood for the observed data and prior beliefs (Jackman, 2000a,b; Tanner and Wong, 1987). The agent- and legislator-specific preference dimensions need not coincide but the parameters governing the functional relationship between the two dimensions must be identified in the missing data imputation procedure.

To fill in the missing agent preference data, the analyst must construct a bridge to link an agent-specific preference space L_a with the roll-call preference space L_l. Figure 1 diagrams the current approach to bridging. In the figure, solid boxes indicate observed variables, circles indicate latent or unobserved variables, and the dashed box indicates the partially observed data $L = [L_l, L_a]'$, where L_l is observed for all legislators and L_a missing for all agents.

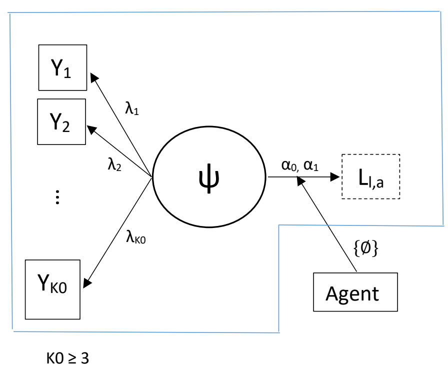

Figure 1: The standard approach to bridging, in which the agent-specific parameters are not identified.

The typical bridging model relies on data generated from outside of the institutional context in order to place both types of actors into a single but arbitrary preference space ψ, using item response theory (IRT) modeling assumptions with a conformable matrix of difficulty and discrimination parameters $\boldsymbol{\lambda} = [\boldsymbol{\lambda_1}, \boldsymbol{\lambda_2}, \ldots, \boldsymbol{\lambda_{K_0}}]$ (Clinton et al., 2004a).[5]

[5]An IRT model regresses a set of items that measure a latent variable on the latent variable itself, where the intercept for each equation is referred to as a "difficulty" parameter and the slope is referred to as the "discrimination" parameter. Identification in an IRT model usually requires three or more

Most commonly, bridging models rely on survey items measuring policy preferences administered to both types of actors (see Ansolabehere, Snyder, and Stewart, 2001; Battista, Peress, and Richman, 2012; Saiegh, 2009, 2015; Shor and McCarty, 2011; Shor and Rogowski, 2016). Alternatively, Bonica (2013) jointly scales PAC ideology and the ideology of other elites from contribution-giving patterns; Barberá (2015) uses Twitter follower data to jointly scale political elites into a common space. Label the K_0 indicators of personal ideology $\mathbf{Y}$. For identification under the IRT assumptions, $K_0 \geq 3$ and often at least one of the $\boldsymbol{\lambda}$ parameters must be fixed (Rabe-Hesketh, Skrondal, and Pickels, 2004).

In IRT modeling, for ψ to be a valid measure of personal ideology, the items in $\mathbf{Y}$ must have good convergent validity, meaning those items that have a higher correspondence to the underlying ψ dimension and that have larger discrimination parameters within $\boldsymbol{\lambda}$. For example, the survey-based preference items should all load on the same scale across populations.

To connect L_a to L_l, the bridging model of figure 1 uses a linear assumption to map the arbitrary preference space ψ onto the legislative preference space L (Jessee, 2016; Shor et al., 2010), using a linear equation to model both legislator preferences L_l and agent preferences L_a:

$$L_{l,a} = \alpha_0 + \alpha_1 \psi_i + \alpha_2 Agent_i + \alpha_3 Agent_i \psi_i, \tag{1}$$

where $Agent_i = 0$ if the observation is a legislator and 1 if the observation is an agent. L_l is observed for all legislators and so the bridging parameters $\boldsymbol{\alpha_l} = [\alpha_0, \alpha_1]'$ are identified for legislators as in ordinary regression. Since L_a is missing for all agents, the $\boldsymbol{\alpha_a} = [\alpha_2, \alpha_3]'$ parameters are not identified in this equation (and so $\boldsymbol{\alpha_a} \equiv \varnothing$) which is a formal statement of the fundamental problem of bridging, (see Jessee, 2016, 1110, equation 2).

The current approach to imputing the missing agent preference data is to set $[\alpha_2, \alpha_3]' = \mathbf{0}$, and then to predict L_a using the point estimates for $\boldsymbol{\alpha_l}$ and the model's estimates for

items since the latent variable must be estimated along with the difficulty and discrimination parameters (Bollen, 1989).

agents' ψ, that is, to impose the assumption that legislators and agents maintain an identical functional relationship to ψ as they interact on a specific topic (Jessee, 2016). This regression approach implicitly assumes that the analytic task is to counterfactually place agents into legislative space as if the agents themselves were legislators. In the strategic interaction, however, the agent interacts with legislators as non-legislative agents. The bridging procedure needs only to solve a measurement problem, not a counterfactual problem, – that is, to impute the missing agent preferences in a scale and direction that correctly measures the preferences of agents that arise within the strategic interaction. In the general case, the agents' preference dimension will differ by some rotation from the legislative dimension, and so making use of the counterfactual scores will introduce measurement error and bias hypothesis tests toward zero.[6]

Alternatively, some analysts propose to rely instead only on the estimated preference space ψ to create a single common scale across institutions and actors. This approach is very appropriate for example in public opinion research (Adams, Engstrom, Joeston, Stone, Rogowski, and Shor, 2017; Alemán, Micozzi, Pinto, and Saiegh, 2017; Bafumi and Herron, 2010; Hare, Armstrong, Bakker, Carroll, and Poole, 2015; Malhotra and Jessee, 2014; Tausanovitch and Warshaw, 2013), where the hypotheses are defined within the survey itself. For behaviors within institutions, however, using survey or other extra-institutional data will recover a latent preference space ψ that is entirely disconnected from the context of institutions, where preferences are conditioned by such things as the actors' occupation, role and incentives (Tausanovitch and Warshaw, 2017), and so lacks construct validity (Cook and Campbell, 1979; Hill, 2001).

As with any statistical procedure for missing data, identification in bridge construction must rely on assumptions. Fundamentally, all of these approaches for the U.S. case assume

[6] In some applications, the analyst observes the outside agent's choices directly within the legislative preference space, and this creates another opportunity to jointly scale (For example, Bailey, 2007; McKay, 2008; Treier, 2011). As I discuss in the appendix, this approach does not solve the fundamental problem of bridging since this method only identifies a mixture of the two preference distributions, a mixture which changes in response to the relative number of each type of actor (Jessee, 2016).

(either implicitly or explicitly) that there is a single ideological dimension that structures preferences across policies, and the task for bridging is only to place agents and legislators correctly relative to each other along this single dimension. The assumption that there is a single dimension that structures all of US politics in all domains, institutions, contexts and policy topics is too strong in practice, however (Jessee, 2016; Tausanovitch and Warshaw, 2017). While agent and legislator alike will possess personal ideologies that can be measured in a survey, in practice, policy discourse within a specific institutional setting will not be constrained by personal ideology. In particular, those who are steeped in policy expertise are often less ideologically rigid in their work since expertise can make elites aware of competing trade offs (Tetlock, 1986), such as quality versus cost and access in health care financing. More generally, while ideology as a single dimension can structure preferences across a range of policies (Ansolabehere, Rodden, and Snyder, 2008; Clinton et al., 2004a; Poole and Rosenthal, 1991), the degree to which ideology can structure within a policy topic varies (Feldman and Johnson, 2014; Treier and Hillygus, 2009).[7] For both of these reasons it is implausible to believe that single dimension structures all reasoning across contexts, institutions, behaviors and interactions.

In order to relax the assumption of a single underlying dimension, the bridging method must be able to identify the $\boldsymbol{\alpha}_{\mathbf{a}}$ coefficients in equation 1. Doing so will allow the analyst to test hypotheses regarding the strategic interaction between agent and legislator using the preferences that were relevant in practice to the dyadic relationship. If in practice only a single dimension constrains all preferences, then $\boldsymbol{\alpha}_{\mathbf{a}} = [0, 0]'$ and unidimensionality will be a result of the model rather than only an assumption. If unidimensionality is false, the more flexible model can accommodate a divergence between L_l and L_a and recover the preferences in a way that is useful for testing institutionally-contextual behavior.

[7]For example, in the application below I examine discourse in committee hearings regarding the Medicare program, which is a social insurance program and often involves complex and technical choices regarding physician prospective payments, disease management, telemedicine, and rural health care, specific topics where preferences are largely unrelated to ideology among experts.

3 Bridge Construction and Repair

This paper offers a solution to the fundamental problem of bridging. I propose to use the information contained within the relevant institutional theory and the substantive hypotheses regarding behavioral outcomes in order to separately identify the agent-specific bridging parameters. The agent-specific bridging parameters are identified when the agents' preference measures are themselves nested as hypotheses within a set of outcome equations. The agent-specific bridging parameters accommodate any linear rotation and so the model places agents in legislative preference space as agents, rather than mistakenly as legislators, while maintaining an identified relationship with legislator preferences L_l.

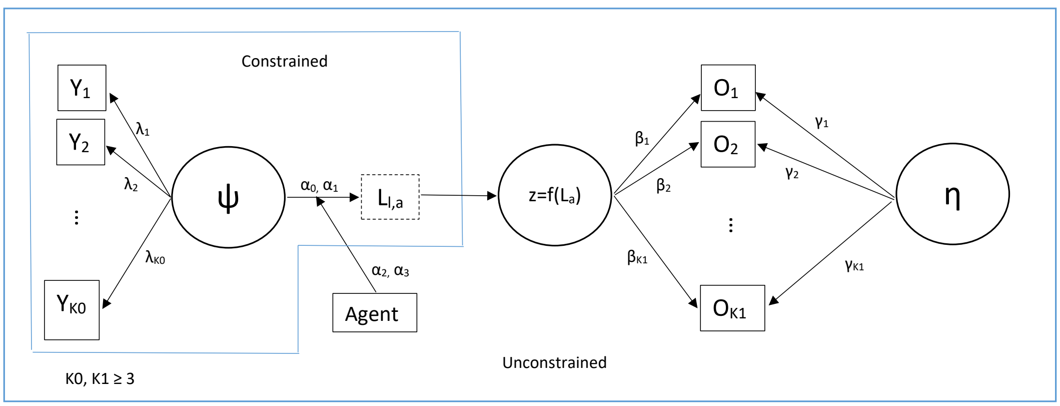

Figure 2: The full model, in which the agent-specific parameters are identified. The unconstrained agents' preference estimates L_a can be compared to the constrained (regression-based) preferences derived from the center box.

My proposed model is diagrammed in figure 2. What I will define as constrained agent preferences L_a^0 are derived from the model in the inner box, which corresponds to the regression approach, and the unconstrained preferences L_a^1 are derived from the full model.

In the model of figure 2, the agent-specific rotation parameters $\boldsymbol{\alpha_a}$ are identified since the estimates for $z = f(L_a)$ are themselves embedded in $K_1 \geq 3$ repeated outcome equations $\mathbf{O}$ as a random effect, measured for each agent, where β is a conformable matrix of outcome hypothesis structural parameters and constants, and z is a function of the missing L_a.[8] The hypotheses contain statements about the legislator preferences L_l, which are observed using roll-call scaling scores, and the agent preferences L_a, which are missing. The function z is application-specific. For example, in the application below, institutional theory predicts that agents will be less informative to legislators the greater the *distance* between their preferences and so the application can use either linear or quadratic Euclidean distance functions for z.[9]

By embedding agent preferences as a parameter in the outcome equations, simultaneous to the imputation in the bridging equation, the model is able to update the agent preferences in legislative preference space using information revealed in the hypothesis tests of the legislator-agent interaction. The imputed posterior distribution over the updated agent preferences *augments* the missing data in the bridging equation, and the posterior distributions of the agent bridging parameters $\boldsymbol{\alpha_a}$ are identified and well-defined with the augmented data (Jackman, 2000a; Tanner and Wong, 1987).[10] To construct the constrained preferences L_a^0, the model uses the structural relationship defined by ψ and the initial $\boldsymbol{\alpha_l} \equiv \boldsymbol{\alpha_a}$ assumption used in the regression method. To update to the unconstrained posteriors L_a^1, the model uses the behavioral hypotheses and z, and allows $\boldsymbol{\alpha_a}$ to differ from $\boldsymbol{\alpha_l}$.

In this method, the posterior agent preferences are estimated using theory and behavioral data; the agent preferences and the outcome hypothesis tests jointly inform each other (Tanner and Wong, 1987). Information from the imputed agent preferences update the hypothesis tests, and at the same time, fitting the hypotheses to data refines and improves estimates of the agent preferences.

[8] Note that the hypotheses only require dependence to update posterior distributions over agent preferences; these do not need to be causal hypotheses. If confounding is relevant to the institutional theory, this remains true under either method.

[9] In the simulation below I use the identity function for z.

[10] As I show in the simulation, the agent-specific parameters remain identified even when the hypotheses are false, provided there are three or more hypotheses, but that the model shows poor mixing and lower fit compared to a model where the hypotheses are true.

To identify z, the model in figure 2 must include an appropriate set of random effects $\boldsymbol{\eta}$ to capture endogenous dependence among the outcomes, dependence which is unrelated to the hypotheses. For example, in the application below I model outcomes that result from interactions within agent and legislator dyads, so I include agent-, legislator-, and dyad-specific random effects in addition to the linear terms including z in the outcome equations. These random effects can be scaled with a conformable matrix of $\boldsymbol{\gamma}$ parameters.

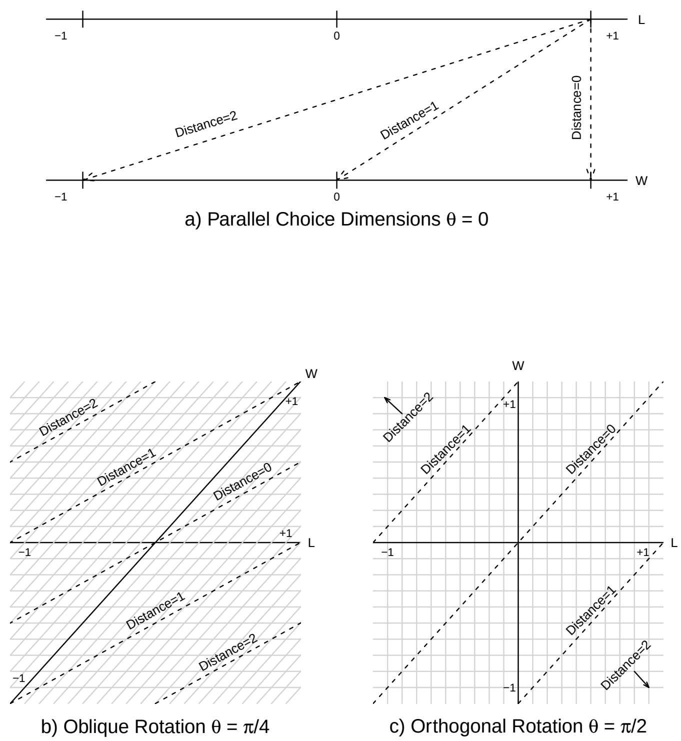

Figure 3: Agent-specific preferences can rotate separately from legislators' roll-call preferences in the full model.

Mathematically, estimating the $\boldsymbol{\alpha_a}$ parameters allows the posterior agent preference dimension L_a to change basis away from L_l through a shift in location and a rotation in direction in a way that accommodates the separate preference dimensions of legislators and agents. In the application below, the hypotheses are stated in terms of the distance in preferences between legislator and agent, and the model is able to define and measure distance relative to the origin in both L_l and L_a, not in the original basis, but in the changed basis that is an endogenous result of the model. The distance measures for different rotations are illustrated in figure 3, where the top panel is for the unidimensional or *parallel* case, the bottom left for the *oblique* case, and the bottom right for the *orthogonal* case.[11] The rotation between the legislator preference dimension L_l and posterior agent preference dimension L_a is captured by the angle θ governing the direction of the relationship through a Cartesian coordinate space, and one can retrieve θ post-estimation via the cosine rule and the vectors of constrained and unconstrained preferences (Binmore and Davies, 2001, 18),

$$\theta = \cos^{-1} \frac{\langle \mathbf{L_a^0}, \mathbf{L_a^1} \rangle}{||\mathbf{L_a^0}|| \, ||\mathbf{L_a^1}||}. \tag{2}$$

The angle $\theta = 0$ is the parallel case, $\theta = \pi/2$ is orthogonal, and $0 < \theta < \pi/2$ is oblique.[12] In contrast, the regression method must constrain the rotation to always and often implausibly equal the parallel case with $\theta \equiv 0$.

The model in figure 2 lends itself to computational Bayesian estimation, where the priors and the likelihood are application-specific. Equation 3 writes out the posterior joint distribution of the parameters, given data, decomposed into conditional distributions, and this equation shows how the missing data $z = f(L_a)$ and the β parameters are mutually informative (Tanner and Wong, 1987),[13]

$$\begin{aligned} p(L_a^1, \psi, \alpha, \beta, \eta | \mathbf{Y}, L_l, Agent, \mathbf{O}) = \int_Z & p(L_a^1 | \psi, \alpha, Y, L_l, Agent) \times \\ & p(\psi | \alpha, Y, L_l) p(\alpha | Agent) p(\beta | z, \eta) p(\eta) p(z) dz, \end{aligned} \tag{3}$$

[11] An obtuse rotation is permissible in the model but often implausible substantively, and generally would represent an over fit to a specific sample. In the model below, I show how to impose soft constraints on prior beliefs to ensure a posterior rotation does not exceed the orthogonal direction.

[12] The transformation coordinates are $X(L_a^1) = \cos(\theta) L_a^0, Y(L_a^1) = \sin(\theta) L_a^0$ (Binmore and Davies, 2001, 183)

[13] All covariates and ancillary parameters are application-specific and so suppressed from equation 3 and figures 1 and 2.

where $z \in Z$. In effect, L_a appears on both the left- and the right-hand side of the model, so equation 3 must marginalize over the missing data distribution for z to estimate β, which in turn updates L_a.

Equation 4 shows the posterior joint distribution for the missing data L_a and model parameters that results from the regression approach to bridging, where the missing data L_a are modeled only as an outcome of the bridge equation and the bridge parameters condition only on the complete data.

$$p(L_a^0, \psi, \alpha_l, |\mathbf{Y}, L_l) = p(L_a^0|\psi, \alpha_l, Y, L_l)p(\psi|\alpha_l, Y, L_l)p(\alpha_l). \tag{4}$$

In the regression approach, the bridge parameters are set using the assumption that the relationship between ψ and preferences is the same for legislators and agents, or $\boldsymbol{\alpha_l} \equiv \boldsymbol{\alpha_a}$. To conduct a hypothesis test for the outcome equations, the regression approach constructs the z function using expected values for agent preferences L_a^0 under this constraint. In this way, the regression approach implicitly evaluates the integral in equation 3 by conditioning on the expected values of the imputed data instead of conditioning β on the L_a that are endogenous to the model. Statistically, then, the posterior for the β parameters will not be correct under this constraint since it omits the missing data distributions and instead conditions on the expected values as if they were observed data. For example, if the imputed values were exactly correct then the standard errors of the structural parameters will be too small. If the imputed values are incorrect then the parameters are biased downward from measurement error. I illustrate these identification and statistical issues in the simulation I describe next.

3.1 Simulation

In the appendix I provide details for a simulation study that examines the conditions for identification for the full model of equation 3.[14] The simulation study also examines the biases that occur in the regression approach of equation 4 in cases where the agents' true preference scale L_a rotates away from the legislator preference scale L_l. I draw sets of simulated data under three conditions: parallel preference dimensions for agent and legislator ($\theta = 0$), an oblique rotation ($\theta = \frac{\pi}{4}$) and an orthogonal rotation ($\theta = \frac{\pi}{2}$). For each data set I compare the structural parameters of the regression approach of equation 4, which relies on the unidimensional assumption for identification, to the full model that I propose in equation 3 which accommodates rotation via the agent-specific parameters in the bridging equation.

The results show that the full model, which relies on unconstrained estimates of agent preferences, retrieves the correct agent-specific rotation parameters as well as unbiased estimates for the structural parameters and the standard errors of the outcome equations for all three cases. In contrast, in the presence of rotation ($\theta > 0$), the regression approach, which relies on constrained preference estimates for agents, returns results that underestimate the parameters testing the outcome hypotheses and that the degree of bias increases with the amount of rotation θ; in addition, the regression approach overestimates the standard error of the regression. Both of these results, underestimated parameters and overestimated standard errors, are typical results in the presence of measurement error, error which in these cases is due to the undimensionality constraint. In the parallel case ($\theta = 0$) the regression approach retrieves unbiased parameter estimates but incorrect standard errors.

Finally, I consider two special cases to demonstrate the identification conditions for the full model. The first case demonstrates the necessity of embedding the agent-specific

[14] There are no general necessary and sufficient conditions for global identification. Here I rely instead on empirical identification that are necessary and sufficient for a local solution, and search the parameter space to verify the local mode is unique within reasonable values for the parameters.

preferences, via the z equation, in multiple outcome hypotheses in order to identify the agent-specific parameters in the bridging equation. The second case shows that the agent-specific parameters remain identified even if the outcome hypotheses are false (the β coefficients set to zero), but that the model struggles to converge and shows overall poor model fit. These latter results demonstrate the importance of good theory for the statistical analysis.

4 Application: Interactions between Legislators and Witnesses in Committee Hearings

I demonstrate my proposed method for placing outside agents into legislative preference space by examining the behavior of legislators in congressional committee hearings, making use of hypotheses derived from institutional theory to state expectations for the strategic interaction. In committee hearings, members of Congress publicly pose statements and questions regarding policy topics to outside agents, who in this context are referred to as "witnesses," and who typically have some form of expertise on the given topic. It has long been established in formal models of strategic information transmission that statements should be more credible to a receiver (in this case a legislator) the closer the preferences between the receiver and sender (witness) (Austen-Smith, 1992, 1993; Hall and Deardorff, 2006; Krehbiel, 1992).[15] Applying theory to this context, one should expect to see a member posing fewer questions and statements to witnesses, the farther their distance in legislatively-relevant preference space.

[15]This comparative static should apply to the public behavior of legislators irrespective of whether the legislator uses the hearing to gain new policy information. If instead legislators use the hearing to highlight their interactions with agents behind the scenes (Huitt, 1958), then the observed behavior simply reflects the legislator's prior preparation of witnesses through the work of their staff (Hall and Deardorff, 2006). Either way the dependence should hold.

4.1 Data and Model

To select the sample, I randomly drew 29 hearings from the universe of all hearings on the Medicare program that were held between 2000 and 2003 across 12 different congressional committees and subcommittees. The identity of the legislators and witnesses at these hearings are in the public record, as is the transcript. The issues include headline grabbing topics such as prescription drug benefits, comprehensive health financing reform, and the solvency of the Medicare trust fund, and also less visible issues such as prospective payment systems for health providers, competition and managed care, billing fraud, medical savings accounts, risk adjustment, coverage information for beneficiaries, prevention and disease management, telemedicine, long term care, billing relations between the VA and Medicare, demonstrations involving the military retirees and the Federal Employee Health Benefits Program, and the Indian Health Service billing practices.

It is likely that health care financing is a hard case for the unidimensional assumption, since the topic is relatively technical and, as of the early 2000s, there remained broad (but not complete) bipartisan support for Medicare and social insurance as an entitlement (Hacker and Skocpol, 1997; Patashnik and Zelizer, 2001). Indeed, at the time Medicare was known as one of the "third rails" of American politics that partisans from both sides of the aisle sought to support (Campbell, 2003). Thus, preferences in elite policy discourse over specific Medicare policy elements are unlikely to be rigidly constrained by a single underlying, fixed left-right ideology dimension. And indeed as I show below, the posterior preferences of witnesses, L_a, are defined along a quality–cost dimension that is largely orthogonal to both the ideologically-defined survey preference space ψ as well as to the legislative roll call preference space L_l. I also show, though, that in this topic space the ideological preferences of the legislators remain meaningful within the dyadic interaction. That is, legislators' ideology remains relevant in this context, even though the elite discourse over the program design does not find itself confined to a rigid, left-right ideological dimension.

4.1.1 Behavioral Outcomes

The statistical model requires hypotheses over repeated outcomes in order to identify the agent-specific bridging parameters. In this application, the complexity of language allows me to identify multiple outcomes of interest in the discourse of a legislative hearing.

I focus on the relationship between preference distance and the propensity for members to direct *information-seeking* questions and statements to witnesses within a hearing. The primary outcome is the count of legislators' *falsifiable sentences*, or questions and statements that engage the witness in policy analytical discourse. One might expect, however, that preference distance can matter irrespective of the role of information in policy making, for example, if through homophily members direct more sentences to witnesses who are sociologically similar. If homophily is the underlying explanation, then legislators' behavior regarding falsifiable sentences should be similar to their behavior regarding non-falsifiable *opinion* and *anecdotal* sentences, that is, those questions and statements that do not engage in policy analytical discourse. The comparison across the three question types can reveal if members engage in analytical discourse in the same manner or in a different manner as non-analytical discourse, as a kind of placebo test to isolate the effect of information-seeking.

To construct the main outcomes variables, I created coding rules to mutually exclusively and exhaustively code sentences in the written transcripts of the hearings into the three sentences types (see the appendix for details). I then count up the number of each of these three types of sentences that legislators make within each legislator-witness dyad. Table 1 shows the descriptives for the 669 dyads in the sample. In all, across all of the 29 hearings there are 67 lobbyists and 87 members. In the sample I exclude all committee members who do not ask any questions or make statements to any of the witnesses at the given hearing and so are likely not to have been present. That is, these counts are only for members who showed up at the relevant hearing and who expressed at least one sentence to any of the witnesses present at the hearing.

Table 1: Member-Lobbyist Dyadic Data

	Mean	SD	N
Falsifiable Count	1.2	3.1	669
Opinion Count	0.5	1.6	669
Anecdotal Count	0.4	1.7	669

Note that under my coding, at these hearings members are most likely to make policy-relevant systematic inquiries. Most members ask zero questions to most witnesses, and so note that these counts have low means and high standard deviations. As I show below, I account for this over dispersion in the count variables in the statistical model by including legislator-specific, witness-specific, and dyad-specific random effects.

4.1.2 Bridging Data: Joint Scaling Witnesses and Former Members of Congress

For this application, I use first dimension *DWNominate* scores (Poole and Rosenthal, 1991) as the measure of legislators' publicly-expressed legislative preferences L_l.[16] To bridge the witnesses into legislative preference space, I made use of a survey that contains responses both from the sampled witnesses and from *former members of Congress*, who are not in my hearings sample, to estimate a personal ideology scale ψ. As I demonstrate below, the personal ideology attitudes measured in this battery are very strong correlates with congressional roll call voting-based measures of legislative preference.

Note here that I use survey responses from former members of Congress who did not attend my sampled hearings rather than the members from my sampled hearings, most of whom at the time of the data collection were current incumbents. This approach is preferable for two reasons. First, current members would likely assign staff to complete and return the survey which would likely introduce measurement error in the latent measure of personal ideology ψ. Former members are either retirees or they are employed where it is unlikely that staff support would fill out surveys relevant to their past legisla-

[16]The results should be similar if one substitutes roll-call scales from Bailey (2007) or Clinton et al. (2004a) (see Carroll, Lewis, Lo, Poole, and Rosenthal, 2009).

tive work.[17] Second, like the witnesses, former members are typically private citizens, not elected officials, and so former members are more likely to fill out the responses based on their own, personal ideology rather than on an office-induced ideology that is conditioned by preferences of constituents, party, donors or groups.

In the fall of 2005 I mailed paper surveys to 199 former members of the U.S. Congress, tracking down their current address using an Internet search. Among those who received surveys, 77 former members returned a completed survey (11 were returned as undeliverable) for a 39 percent response rate.[18] The survey contained a consent cover letter and a second page containing only five questions designed to measure personal ideology,[19] each measured on a five point scale (strongly agree = 1 to strongly disagree = 5).

- [**Markets**] The protection of consumer interests is best insured by a vigorous competition among sellers rather than by federal government regulation on behalf of consumers
- [**Companies**] There is too much power concentrated in the hands of a few large companies for the good of the country
- [**HelpPoor**] One of the most important roles of government is to help those who cannot help themselves, such as the poor, the disadvantaged, and the unemployed
- [**Access**] All Americans should have access to quality medical care regardless of ability to pay
- [**Incomes**] The differences in income among occupations should be reduced

The labels in square brackets were not included in the survey question wording. Descriptively, these items load on a single factor (the first eigenvalue 2.12, the second 0.37). The factor loading on the Markets indicator is negative and the rest positive.

[17] Further, many of the responses had handwritten notes and comments from the responding former member him or herself.

[18] Note the response rate and representativeness of the sample are not important for this application since I am only using these responses to enable a transformation between two scales, which in a mathematical sense is defined for any two or more members under the linear transformation assumption. The sample of former members has good coverage in any case. Among the former members, 51 were Democrats, 26 Republicans; 18 served in the Senate. The most liberal (minimum) $DWNominate$ score is -0.85 and the most conservative (maximum) is 0.69. This gives good coverage of the $DWNominate$ dimension. By comparison, in the 109th House, the most liberal member scored -0.743 and the most

Table 2: Ideology Indicators for Former Members and Lobbyists

	Mean	SD	N
Markets	2.9	1.3	142
Companies	2.9	1.2	139
HelpPoor	1.7	0.7	144
Access	1.7	0.8	144
Incomes	3.4	1.3	142
FMC DWNominate	-0.1	0.3	77

I also administered a survey with the same set of ideology questions to the witnesses in my sample, and then appended responses to the former member sample in order to bridge both types of actors into a personal ideology preference space ψ. To complete this bridging dataset, I merged in the most recent $DWNominate$ scores for each former member, that is, the score from the Congress just prior to the member separating from the institution.[20] In this rectangular dataset, I have responses to the ideology survey questions from everyone in this sample, both former members and witnesses, but the first dimension $DWNominate$ scores are missing for every witness. The descriptives of the survey responses are in table 2.

4.1.3 Statistical Model

The full statistical model is diagrammed in figure 2. The appendix gives a formal statement of the model. In short, the statistical model is composed of two linked submodels. Submodel A is a measurement model that jointly places former members and witnesses into a personal ideology space ψ and bridges these scores into legislative common space L_l and L_a. Submodel B contains the outcome equations of substantive interest. In my proposed full model that yields posterior estimates of agent preferences, both submodels

conservative, 0.998, with only 8 members exceeding 0.69.

[19]These questions come from the study Heinz, Laumann, Nelson, and Salisbury (1999), response sheet P, items a, d, e, i, n.

[20]$DWNominate$ scores are constant (Poole and Rosenthal, 1991).

are estimated as a set of simultaneous equations. To implement the regression approach, I estimate these two submodels separately, first estimating (constrained) agent preferences under the unidimensional assumption with Submodel A and then using the set of expected values for the constrained agent preferences to construct z in the outcome equations of Submodel B.[21]

4.2 Results

I estimate the model in `OpenBUGS` using Bayesian MCMC methods (Spiegelhalter, Thomas, Best, and Gilks, 1996). Here I first discuss the measurement model, then the regression results, then the full model results.

4.2.1 Constructing the Bridge using the Unidimensional Assumption

In this section I show the results of the measurement model (represented in figure 1) estimated separately from the outcome equations. The measurement submodel uses the responses to the five question survey to estimate ψ_i for the former members and the witnesses who are in the sample.

The first two columns of results in table 3 show the factor coefficients ($\boldsymbol{\lambda}$) that result from the model for ψ based on the survey items alone, for now omitting the bridge equation for the $DWNominate$ score. The factor coefficients correspond to the discrimination parameters in an IRT model (the model also estimates cut offs between the ordered response categories that correspond to the IRT difficulty parameters, not reported). Note first that the estimated discrimination parameters for each survey item is large and precisely estimated. These scores indicate a high inter-item correlation and hence strongly suggest the convergent validity of the items to measure the survey-based personal ideology.

Comparing the discrimination parameters from the pooled sample (including both witnesses and former members) to the unpooled (former member only) results in appendix

[21] In the Bayesian approach I use in the application, it is trivial to combine these two steps within one model, and doing so returns identical results.

table 5, I find no evidence of rotation in the ψ ideology dimension between the two groups. This is because, unlike policy preference or roll-call vote preference survey items that are found in the literature (see Lewis and Tausanovitch, 2015), the items I use to recover ψ were designed to measure personal ideology in a way that is invariant across groups.

Table 3: Bridge Model for Witness Prior Preferences

	ψ Model Only		Without Party		With Party	
	Mean	SD	Mean	SD	Mean	SD
Bridge Equation						
Ideology (ψ)			0.25	0.03	0.10	0.02
Witness			NI		NI	
Ideology X Witness			NI		NI	
Party					0.47	0.05
Constant			-0.07	0.03	-0.22	0.02
σ			0.20	0.03	0.14	0.01
Item Discrimination Parameters						
Markets	1		1		1	
Companies	1.64	0.34	1.58	0.30	1.56	0.32
Help Poor	2.14	0.55	2.08	0.46	2.15	0.50
Access	1.92	0.43	2.01	0.38	2.04	0.43
Incomes	1.66	0.36	1.56	0.29	1.62	0.33

Notes: NI = Not Identified

The middle two columns show the bridge results omitting party as a covariate for the imputation of L_a. Note that the α_1 coefficient that provides the bridge or transformation from the survey-preference space ψ to $DWNominate$ is 0.25 and the posterior density is far to the right of zero. In addition, this coefficient is substantively large in that the estimated standard deviation for $DWNominate$ scores in the sample of former members item is 0.33, and hence a one standard deviation change in ψ is associated with about two-thirds of a standard deviation change in the estimated $DWNominate$ score, which demonstrates a strong relationship between personal ideology as measured in the survey scale and $DWNominate$. The σ parameter, which is the residual variance, is 0.20, meaning the explained variance in the bridging equation is 0.63.

The final two columns show the results of the bridge model including a single covariate, political party ($Republican = 1$, $Democrat = 0$). To implement this regression, I first recorded the known party identification of each member. Witnesses, however, do not have an attached party identification nor for this sample do the groups they represent.[22] Instead, I use a data-driven approach to classify witnesses by party where I regress estimates for witness's personal ideolgy ψ on a classification for the type of organizations that the witness works for.[23] In this descriptive regression, I examine which employer types have witnesses that are statistically different on ψ compared to those who work at the baseline category of for-profit trade associations, organizations that are firmly in the Republican constituency. In this analysis, I find that professional and voluntary associations, firms, law firms, partisan think tanks, and industry coalitions are statistically similar to for-profit trade associations, while labor unions, universities, non-profit NGOs, state and federal agencies, not-for-profit trade associations, nonpartisan think tanks, and hospitals are statistically more liberal. Witnesses in the first set I label as in the Republican constituency, and the latter set in the Democratic constituency.

The final two columns of table 3 show that party accounts for much of the relationship between personal ideology and roll-call preferences. Here I do not need to resolve whether this finding isolates the effect of party on roll-call voting (Krehbiel, 2000, see the appendix for a longer discussion); I only need to examine whether including party as a covariate affects the construct validity of any estimates of L_a.

4.2.2 Regression using Unidimensional-Constrained Preference Estimates

To illustrate the regression approach diagrammed in figure 1, I use the measurement models (with and without party identification) to estimate the expected values of the unidimensional-constrained witness preferences L_a^0 to construct the distance measure from legislators' $DWNominate$ scores. Since these agent preference estimates make use of

[22] In the larger survey I sent to the witnesses, 55 percent reported that their groups are never involved in electoral politics, and 72 percent indicated that their group does not make campaign contributions.

[23] This regression is on the full sample of 165 witnesses who returned surveys as a part of the larger project, not just the ones in the sample of hearings that is the focus of this paper.

the bridging parameters identified only from the former member data, they represent the beliefs over witness preferences under the unidimensional constraint – that is, what witnesses' $DWNominate$ scores would have been, had they themselves been members of Congress during the committee hearing.

I estimate the regression for the outcome equations as seemingly unrelated regressions, where the legislators, witnesses and dyads are conditionally independent given η_1, η_2, and η_3. In the appendix, I report the results for the regression specifications that assume either a linear or a quadratic distance measure, and either with or without including the party covariate in the bridging equation, for a total of four model specifications. Overall, the results across the specifications show that the outcome hypotheses are not confirmed using this measurement strategy. The effects are estimated as small and not significant, and exhibit considerable variability across specifications.

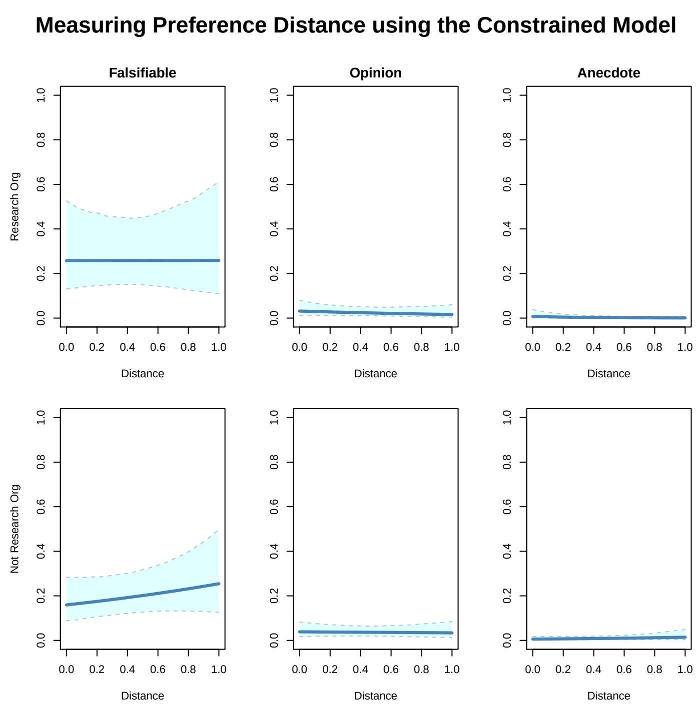

Figure 4: Relationship between preference distance using the priors (the absolute value of legislators' roll-call preferences L_l minus agents' prior preference estimates L_a^0) and expected number of questions at a hearing, indicating a lack of construct validity.

I present the results of the simplest of these models in figure 4, the absolute value distance function with no party covariate in the imputation equation. Here I set the random effects to their sample means. In this figure, the columns correspond to falsifiable, opinion and anecdotal questions, respectively, and the rows correspond to witnesses who represent research-focused (top) and non-research (bottom) organizations; research-focused organizations include think tanks, universities and foundations. The dark blue line in each frame indicates the conditional point estimate for each subgroup and each outcome, and the light shaded areas indicate 95 percent conditional confidence intervals. Note that using prior preferences, the analyst would need to conclude that the institutional hypotheses developed from strategic information transmission and sociological homophily are false under this implementation – that is, in this context, there appears to be no significant relationship between preference distance and the count of any of the three types of sentences, for each type of witness.

4.2.3 Repairing the Bridge using the Full Model

In the full model of figure 2, the structural parameters and witness preferences are jointly updated based on the data and model. In this model, the witness-specific bridging parameters $\boldsymbol{\alpha}_\mathbf{a}$ are identified, which permits a rotation of the witness preference space L_a away from the legislator roll-call preference space L_l. The results I report are from a model with a soft constraint on the priors that prevents the rotation θ from exceeding the orthogonal case, although the results from the unconstrained case where in some specifications the angle of rotation is obtuse yield very similar results (see appendix). As in the regression approach, the model assumes that dyads are conditionally independent given the random effects η_1 (dyads), η_2 (legislators) and η_3 (witnesses).

First consider the relationship between the estimated witness preferences from the full model, which yields unconstrained estimates for the preferences L_a^1, and those estimated for the regression case, which are the constrained preferences L_a^0. Figure 5 plots the relationship. In this figure, each circle represents a witness; the blue dots indicate witnesses who come from Democrat constituency groups, the red from Republican. The size of the circle is proportional to the variance of the posterior preference estimate. As figure 5 demonstrates, the rotation is virtually orthogonal ($\theta = \frac{\pi}{2.47}$) and the degree of rotation is independent of party affiliation or the degree of certainty in the estimate.

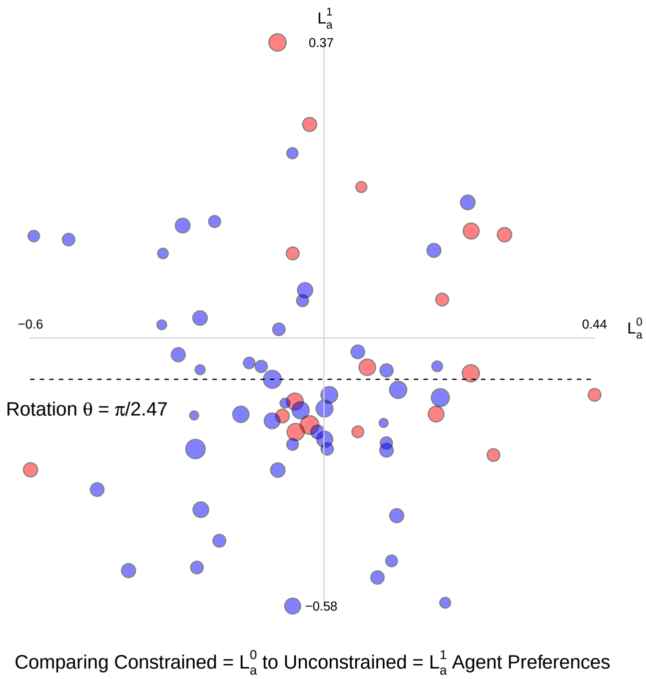

Figure 5: The relationship between agents' constrained preference estimates L_a^0 and unconstrained L_a^1. Blue dots indicate classified as in the Democratic party constituency, and red dots Republican.

Figure 6 shows a histogram of the change in the standard error of each witnesses preference estimate, where the standard error of the preferences from the constrained model is subtracted from the standard error of the posteriors from the full model, so negative changes indicate that the unconstrained preference is estimated more precisely. While the degree of change in precision varies, the historgram indicates that the unconstrained preferences are estimated typically with more precision than the constrained.

As in the regression case, I estimate the full model using both linear and quadratic

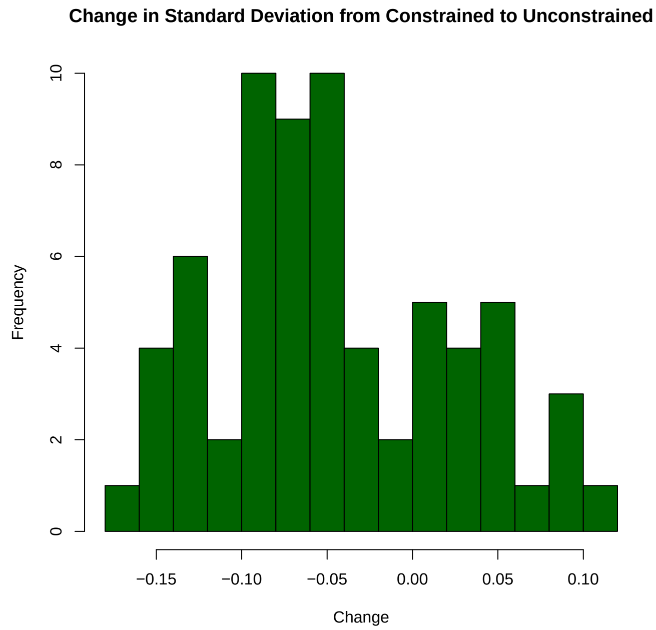

Figure 6: Agents' preferences tend to be estimated with greater precision in the full model than in the constrained (regression) model.

distance functions, and with including and excluding the party covariate from the bridging equation. The appendix reports the full set of results for all of these equations, separately for the models with and without the soft constraint. The results show a remarkable robustness in the outcome equation parameters across all eight of these specifications, where most of the coefficients testing the hypotheses are large, statistically significant, and stable across specifications. In addition, the WAIC statistics (Vehtari, Gelman, and Gabry, 2017) show very little variability across specifications compared to the regression case and lower values compared to the regression models.

For comparison between the regression and the full models, figure 7 presents a dot chart that graphically represents the parameter estimates for the outcome equations in the case of the linear distance function. The dots represent the parameter estimate where in each cluster, the top dot indicates the party variable is included in the bridge equation, the middle dot is the specification that excludes party, and the lower dot is the Bayesian model average (McElreath, 2016, 203-4). Note that in the regression case, the parameter

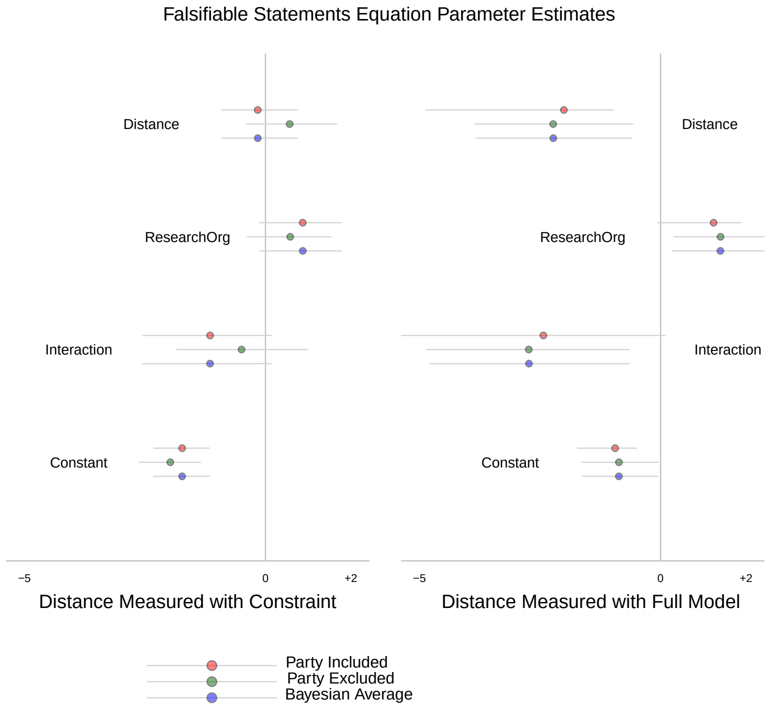

Figure 7: Coefficient estimates from the regression model (left) and the full model (right). Notice that the regression model returns small, unstable and insignificant estimates, which is characteristic of measurement error. The full model returns large and significant results across specifications.

estimates are small, not significant, and show variability across specifications. In contrast, in the full model, the results are large, significant, and remarkably similar across specifications. This contrast strongly suggests the presence of measurement error from using the constrained preference estimates for witnesses in the distance function (Kukush, Schneeweis, and Wolf, 2004), and as it turns out, in the regression case the analyst would mistakenly be led to accept the null institutional hypotheses.

Figure 8 shows the results of the outcome equation parameter estimates, and is the same set up as figure 4 above. Here one can see that, in contrast to the results in figure 4, there is a clear inverse relationship between preference distance and the number of sentences the witness attracts in the committee hearings. The most obvious and striking

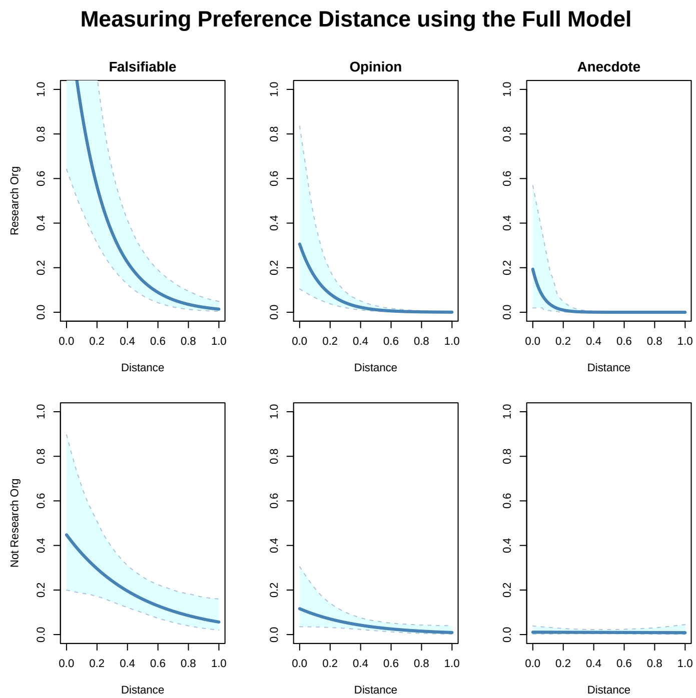

Figure 8: Relationship between preference distance using full-model posteriors (the absolute value of legislators' roll-call preferences L_l minus agents' posterior preference estimates L_a^1) and expected number of questions at a hearing, indicating good construct validity.

result is that members condition all questions, no matter the type, on preference distance. There are two possible reasons for this. First, members might simply have a psychological aversion to interacting with witnesses who do not share their ideology. This is a common phenomenon in social interactions since people are typically more comfortable interacting with those who share similar attitudes and traits.

Second, members might tend to direct questions to witnesses whose statements are most informative in sense shown in formal models of strategic information transmission (Austen-Smith, 1992, 1993). The statistical model does not directly adjudicate between these two possibilities. That said, there is some suggestive evidence from comparisons across the frames of figure 8 to suggest that the members are engaging in information strategic discourse in the hearings. First note that members condition their falsifiable sentences on the type of organization. This is consistent with information-seeking behavior in that witnesses from research organizations have invested in better quality information, and one would expect to see more questions directed to them and a greater responsiveness to preference distance. Second, note that members are more responsive to preference distance for falsifiable sentences than they are for opinion- and anecdotal-related sentences, and that is true for both types of groups.

4.3 Comparing Alternative Institutional Theory

In the full model, the witness posterior preferences rotate away from the roll call preference space, and so it might be a reasonable conjecture that ideology simply makes no contribution in this context. That is, perhaps both witnesses and legislators step outside of ideological preference space as they interact on Medicare policy. This of course would be counter to institutional theories of Congress that assume legislators' preferences are well-defined by their roll-call, ideologically-driven preferences.

To test for this, I re-estimate the model but this time excluding legislator $DWNominate$ scores from the distance equations. This is equivalent to locating all legislators at zero on the witness preference scale, or assuming that legislators simply ignore their own legislative preferences in the committee interaction, and the only relevant consideration for legislators is the extremity of the witness in the witness preference space. I estimate this model for the simplest model (linear distance specification, excluding party) for both the regression approach and the full model and report the results in the appendix.

I begin by noting that the center distance model is unstable and shows poor mixing of the posterior chains compared to the member-distance model, in a manner that is very similar to the results I observed in the simulation based on "false theory," that is, for the case when the hypotheses are false. Indeed, the model diverges without placing directional priors on the outcome parameters and in particular restricting the parameter for the distance measure to be negative. These results strongly suggest against the alternative theory that ignores members' own preferences.

It is worth noting, however, that whether measuring distance from the legislator within the dyad or from the origin of the witness preference space, the two distance measures are correlated, and indeed the correlation must be true mathematically. One way to select the better specification is by examining the WAIC statistics for each model, which show the models with members preferences are a better fit to the data. These results suggest that the legislative roll call ideology space remains relevant in committee hearings even when the witnesses' preferences are not themselves driven by ideology.

4.4 Describing the Posterior Preference Dimension

Given that the posterior preferences for witnesses rotate away from a well-defined and familiar ideological preference space, it is a reasonable question to ask if the posterior parameters measure a preference space, as opposed to a confounding space such as a degree of technical expertise or occupation or some other dimension. There are several reasons to believe the posterior dimension is based on preferences and not just a confound. First, note the the posterior dimension is measured in distance from members' ideological preferences, and I show above that members' roll call-based legislative preferences are relevant to the discourse that occurs within the dyad. It is possible, however, that there is some asymmetric preference between liberal and conservative members on a counfounding dimension, for example if the confound is expertise and one end of the ideological spectrum values expertise less than the other. I examine the correlation between a wide variety of measures of technical expertise and the posterior witness preferences and there is none.

Perhaps another confound is topic. That is, if the confound is topic, perhaps liberals ask more questions to witnesses from one topic area and conservatives from another. To test this I organized the hearings by topic and find no differences in the conditional means of witness preferences across topics. This is illustrated in figure 9, where the large center dot is the average of witness preferences and the smaller dots are the individual estimates, for topics where there are six or more witnesses. Note that the averages closely

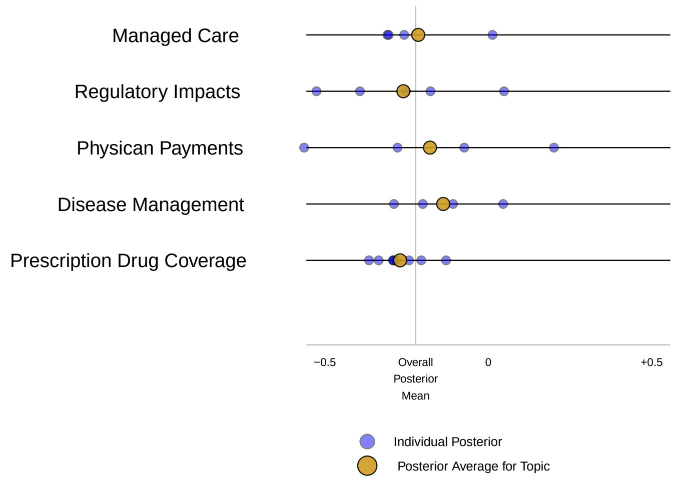

Figure 9: Distribution of agent preferences across selected topics.

cluster around the center and show little variability, while the individuals vary significantly around the average for all topics.

Finally, I use text analysis to describe the content of the witness preference dimension. To do this, I conduct a simple word count analysis of the written testimony separately for witnesses who have a high preference score L_a and those with a low preference score,[24] stratified by topic.[25] To illustrate the word frequencies I present word clouds in figure 10. Notice that the witnesses that are in closer proximity to conservatives focus more on costs, prices and coverage, and those closer to liberals focus more on care, requirements and beneficiaries.

Note that witnesses, who are subject matter experts, have preferences over cost versus quality, but knowing these preferences reveals nothing about their personal ideology; liberals and conservatives have reasons to be concerned with each. At the same time, legislators have preferences over cost and quality given their (office-induced) preferences. The discourse space in the committee hearing is two-dimensional and both dimensions

[24] I stemmed, removed stop words, removed words that are common to both such as Medicare, physician, plan, etc., see appendix for more detail on the text analysis.

[25] I need to stratify by topic because, while the dimension is not confounded by topic, the word distributions will by necessity be topic-specific.

(a) Closer to Liberals

(b) Closer to Conservatives

Figure 10: Content of the Witness Preference Dimension. Word counts for agents spatially closer to liberal legislators are on the left, and closer to conservative legislators on the right.

remain substantively meaningful within the hearing.

4.5 Discussion of Application

The application demonstrates the statististical method I propose and formally tests, for the first time, the basic comparative static of lobbyist-legislator models of strategic information transmission, that lobbyists are more informative to legislators the more proximate their preferences (Austen-Smith, 1993). I demonstrate how to estimate witnesses' ideological ideal point scores simultaneously with the structural parameters in an institutional outcome equation, which correctly propagates the information and estimation uncertainty in those scores through to the structural parameter estimates (Tanner and Wong, 1987). The application demonstrates that the (unconstrained) preference estimates that reflect the rotation defined by the agent-specific bridging parameters have better construct validity (Cook and Campbell, 1979; Cronbach and Meehl, 1955) than (constrained) preference estimates that constrain all actors' preferences to a single dimension. The higher construct validity of the rotated solution can be seen in a comparison between figures 4 and 8, since the relationships between preference distance and the count of sentences within a dyad are apparent in the rotated (figure 8) but not in the unidimensional (figure 4) solution.

The application also introduces a new survey dataset with former members of Congress, who are most comparable to lobbyists and other third-party agents in that they respond as non-elected citizens rather than as officeholders. Further, this method relieves the researcher from having to administer a survey to current members of Congress, who typically are not responsive to surveys.

5 Conclusion

When the political interaction of statistical interest is between legislators and outside agents, in the general case, the analyst must construct and measure two separate preference dimensions using the revealed choices that each type of actor makes, and the analyst must then recreate the correspondence between these two dimensions that reflects the actual relationship that occurred between agent and legislator. To test theories regarding actors interacting in specific institutions, it is important to place third-party agents into legislative preference space as agents, not as legislators. The fundamental problem for bridging is that agents' preferences relevant to the interaction are missing for all agents, and hence the transformation parameters between these preference spaces are not identified specifically for agents within the current regression-based approach (as noted in Jessee, 2016, 1110).

I argue that one can solve the fundamental problem of bridging, and identify agent preferences as they were expressed within the legislative interaction, through a comprehensive and theoretically-driven data augmentation strategy. In my proposed approach, the augmentation dynamically converges to a well-defined posterior distribution of agent preferences, given the behavioral data and theoretical model. The dynamically augmented preference data in turn identify the posterior distribution over agent-specific bridging parameters. In contrast, the regression-based approach augments the missing agent preferences using the expected values derived from the (unrotated) constrained model to test the institutional hypotheses. Conceptually the constrained preferences are unlikely to match the preferences as they were operative in the interaction that is embedded within an institution, and statistically the constraint introduces measurement error in the behavioral hypothesis tests.

My proposed solution updates agents' preferences based on behaviors that arise within the contextually-situated interaction. The fundamental problem of bridging applies to nearly all cross-institutional research, in any application where the researcher seeks to model the interaction between actors that come from different institutions. The method thus conceivably could generalize to other institutional interactions in future research.

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A Appendix

This appendix expands on the argument set out in the main text of the paper, and provides additional detail that space limitations do not allow in the main text. The appendix includes a summary of the scaling and bridging literature; some remarks on identification and construct validity; a detailed description of the coding and reliability tests; the statistical model; and full tables of the results that are described in the paper.

A.1 Summary of Roll-Call Scaling Methods

Much of formal institutional theory in political science takes preferences to be a fundamental or primitive attribute of political actors that guides their strategic reasoning. While preferences can be defined discretely over specific items, institutional theorists generally focus on latent and continuous preferences that determine actors' preferences across sets of observed choices. Latent preferences are not directly observable by the analyst. To recover latent preferences, the analyst can assume a functional relationship between the latent preference and the actor's choices across items and then use a scaling method to recover estimates of the latent preferences.

One set of choices legislators routinely make is over roll-call votes, which are the votes that legislators cast publicly for or against specific proposals. Roll-call scaling is based on a well-established random utility model that posits legislators vote yea on proposals that are closer to their latent preferences relative to the status quo, and nay otherwise. `NOMINATE` roll-call preference scores result from a scaling procedure that assumes a Gaussian utility function and extreme value errors (Carroll, Lewis, Lo, Poole, and Rosenthal, 2009; Poole and Rosenthal, 1991) while `IDEAL` assumes a quadratic utility function and normal errors (Clinton et al., 2004). Since roll-call scaling methods are founded on well-established theory, they are preferable to scaling methods based on cosponsorship choices, which are not well-theorized (Desposato, Kearney, and Crisp 2011; but see Alemán, Calvo, Jones, and Kaplan 2009). Roll-call votes are publicly visible and reflect the full vector of pressures a legislator faces, including pressure from constituents, party leaders, lobbyists, donors and their own preferences and convictions.

A.2 Review of the Bridging Literature

When the analyst recovers a scale based on a set of choices that a given set of actors makes within an institutional setting, the measure of that scale enables comparisons among those who make those specific choices. If actors from different institutions make different choices, then the different scales that are recovered from each set of choices are not directly comparable. In this situation, the analyist needs to identify a set of transformation parameters to make the scales of legislators and third-party agents comparable, that is, to create a "bridge" between the two preference dimensions.

Existing approaches to bridging assume a unidimensional ideological space that constrains preferences across institutions and across topics and time in order to identify the relationship between agents' and legislators' preferences. Under this assumption, the task for bridging is only to order all actors' preferences along the single dimension. Here I review the main statistical approaches to bridging in the current institutional literature and show how each method fails to resolve the fundamental problem of bridging (see also Jessee, 2016; Tausanovitch and Warshaw, 2017).

Table 4 lends an organization to the research designs that are available for use in joint scaling and bridging methods for identifying common space preferences across different institutional actors. In this table, the columns indicate the availability of data of two different types for each actor. *RC* indicates the availability of roll-call votes regarding proposed legislation (including "votes" on legislation that actors can express in a survey response); *II* indicates the availability of ideology indicators such as responses to validated questions on a survey, or to a series of preference questions, or other external data that is correlated with the actor's ideology. Within the cells, *O* indicates that the column data source is observed for the row actor group, and NA indicates that data source is missing for that group.

Table 4: Research Designs Used in the Joint Scaling and Bridging Literature

	Design A		Design B		Design C		Design D	
	RC	II	RC	II	RC	II	RC	II
Legislator	O	NA	NA	O	O	O	O	O
Agent	O	NA	NA	O	NA	O	O	O

In design A, the analyst observes the outside agent's choices over roll-call votes directly, and this creates an opportunity to jointly scale legislators and agents into a single preference space described by roll-call voting. For example, when the president takes positions on legislation in Congress, these position statements can be treated as "votes" in the same measured space as congressional roll calls (Carroll et al., 2009). Bailey (Bailey, 2007) uses president positions on legislation and on judicial decisions to create a bridge between legislators, president and the Supreme Court justices. Trier (Treier, 2011) uses the common set of ratings produced by the Americans for Democratic Action and the American Conservative Union to bridge House and Senate. McKay (McKay, 2008) adopts a nonparametric approach by placing the full set of interest groups that produce legislative ratings into roll-call space by identifying the members who receive perfect scores from the group, and noting the roll-call scores of those legislators.[26]

The fundamental problem of bridging is not solved, however, by asking agents to report roll-call like preferences. Agents do not have roll-call scores only because they did not have the opportunity to vote, but more fundamentally because they are not themselves legislators and so are not required to vote on legislation in their profession or role. Consider two approaches to joint scaling under design A. First, if the analyst places agents into roll-call space based on their revealed "votes" using the structural parameters

[26] Outside of legislative research, analysts have also used surveys to create roll-call-like responses in order to place citizens and candidates, or representatives or judges into a common survey space in public opinion research (Adams et al., 2017; Alemán et al., 2017; Bafumi and Herron, 2010; Malhotra and Jessee, 2014), as well as local jurisdictions (Tausanovitch and Warshaw, 2013).

from the legislator sample, they assume that the structural relationship between these revealed preferences are the same for agents as they are for legislators. But roll-call voting as a legislative act does not condition agents' reasoning and so assuming agents vote as if they are legislators is mistakenly treating the measurement problem as if it were a counterfactual problem.

Second, if the analyst using design A jointly scales legislators and agents based on their "votes" they simply estimate an unknown mixture of the two preference dimensions, where the mixture depends on the amount of rotation between the two preference scales and the proportion of each actor type included in the analysis. Lewis and Tausanovitch (2015) offer a formal test of this criticism by showing that unrestricted models that allow for rotation between agent and legislator preferences are nearly always preferred by model fit criteria over models that estimate the mixture.[27]

Design B proposes to jointly scale different types of political actors into a common, personal ideological preference space making use of a scaling procedure such as item-response theory modeling. This method requires that there exists responses from both sets of actors on the items being scaled. The most common data source here comes from survey-based policy preferences, for example those filled out by both incumbents and challengers (Ansolabehere et al., 2001a; Battista et al., 2012; Saiegh, 2009, 2015; Shor and McCarty, 2011; Shor and Rogowski, 2016). Other common data sources include contribution-giving patterns Bonica (2013) and Twitter following patterns Barberá (2015).

While each of these data sources and methods may have high convergent (internal) validity, and so is able to recover a valid latent scale, Tausanovitch and Warshaw (2017) note that by using data from outside of the institutional setting, the resulting scale has limited construct validity (Hill, 2001) in hypothesis tests in that reasoning about policy preferences when casting votes on roll-calls or when giving tesimony in congressional

[27] Roll call-based items are not designed to measure an ideology scale and can at best be hit and miss, where some will have a strong discrimination parameter, others weak, and so including different items will measure a different scale, and where these parameters can vary heterogeneous across populations, so including different respondents can change the measured scale. Roll calls are simply not tested as valid.

hearings, differs in comparison to when an actor is responding to survey questions, or when the actor gives or receives campaign contributions, or when the actor accumulates Twitter followers (Tausanovitch and Warshaw, 2017). To assume otherwise would require that preferences are constrained by an underlying single ideology dimension across these separate institutional settings. Of course, using ideology items to measure personal ideology is appropriate for hypothesis testing in public opinion research (Hare et al., 2015) since in that setting the hypotheses are defined in terms of the survey itself. When testing hypotheses regarding actors interacting within an institution, the hypotheses are with respect to the reasoning actors use to develop policy preferences, which is emergent in the institutional interaction being modeled.

Design C is largely the focus of this paper and proposes to make use of a common set of ideology indicators to bridge agents' preferences into roll-call preference space. The current literature recommends using a regression approach in order to connect an arbitrary preference space to an institutionally-specific roll-call preference space. This approach has the prospect to improve the construct validity over an abstract preference space defined in Design B because it connects agents' preferences to legislative preferences that are emergent from the institution being studied.

The current practice in the regression approach is to proceed in two stages. In the first stage, the method regresses legislators' roll call preferences on their estimated survey-based personal ideology preferences to identify the linear transformation from the survey space to the legislative roll call space; in the second stage the method uses the estimated structural parameters from this regression to transform the agents' preferences from the survey preference space into the legislative space. Like the direct placement of actors based on roll call-like responses, the regression method also places agents into legisltive preference space as if they were counterfacually legislators. For example, Shor et al. (2010) estimates a linear mapping from an estimated roll-call space in each state legislature to the roll-call space in the U.S. Congress by exploiting the roll-call data from state legislators who moved on to a seat in Congress serving as the bridge. Jessee (2016) states that this approach cannot solve the fundamental problem of bridging, but then offers that the solution is to constrain the structure of ideology to one or the other group of actors through a linear mapping, instead of allowing the preferences to rotate.

Finally, Design D has to date not been implemented but offers some promise as an alternative to the method I propose in this paper, but only under specific circumstances. If the analyst has responses to a set of ideology indicators from both groups, roll-call votes from legislators, and roll-call preferences from agents, she can identify the roll call-based scales for agents and legislators separately, and then use the common set of ideology indicators to identify the rotation parameters between the two roll-call scales. For this model to be identified, the ideology indicators must have internal validity when pooled across both groups, such as the survey items I administered to witnesses and former members of Congress, so that the scale estimated from the ideology indicators is unidimensional. If this identification condition is met, the analyst can retrieve agent-specific roll-call preferences. Whether this recovered preference scale has construct validity, however, is application-specific. For example, it is possible that preferences expressed in response to a set of survey questions regarding a set of bills may not be predictive of behavior within committee hearings since the committee hearing is a very different (and information-rich) institutional setting compared to responding off the top of the head to roll-call preferences on a survey.

A.3 Simulation to Demonstrate Model Identification

Here I offer a simple simulation to demonstrate the conditions for identification for the full model of figure 2. The simulation also demonstrates the consequences for parameter estimation in cases where the relevant preference dimension is rotated away from the roll call-based legislative preference dimension, for both the full model as well as the regression approach. The research design for the full model requires *nesting* the agent preference parameters as hypotheses within repeated outcome equations, since such a specification estimates the agent preferences as function of a random effect scaled to each outcome dimension. The model is not identified without this nesting because the random effect itself would not be identified. In the Bayesian framework that I use, the analyst can choose flat priors to allow the posterior preferences L_a to update via the outcome equations, or choose regularizing priors to tune the amount of institutional information that informs the posteriors and/or constrains the allowable degree of rotation.

I consider three cases in the simulation: parallel preference dimensions for agent and legislator ($\theta = 0$), an oblique rotation ($\theta = \frac{\pi}{4}$) and an orthogonal rotation ($\theta = \frac{\pi}{2}$) using the following equation set:

$$L_{l,a} = 0 + 1 \times \psi + \alpha_3 \times \psi \times Agent_{l,a} + \epsilon_L, \text{ for } l = 1 \text{ to } N_L \text{ and } a = 1 \text{ to } N_A, \tag{5a}$$
$$O_{a,k} = 0 + 1 \times L_a + \epsilon_k, \text{ for } a = 1 \text{ to } N_A \text{ and } k = 1 \text{ to } 3. \tag{5b}$$

Following the notation in figure 2, for the bridging equation (5a) I set the legislator-specific transformation parameters $\alpha_0 = 0$, $\alpha_1 = 1$. For the agent-specific transformation parameters, I set $\alpha_2 = 0$, and vary the amount of rotation by setting $\alpha_3 = 0$ for the parallel case, $\alpha_3 = -0.5$ for the oblique case, and $\alpha_3 = -1$ for the orthogonal case. For the outcome equations, I set each $\beta_{0k} = 0$ and each $\beta_{1k} = 1$.

There are $N_L = 100$ legislators and $N_A = 500$ agents. I draw the $N_L + N_A$ observations of the personal ideology space ψ from a standard normal distribution. I set the standard deviation σ_L of ϵ_L to 0.25 and draw the N_L observations of L_l from equation 5a assuming a Normal distribution for each ϵ. Next, I create the (unobserved) true agent preferences L_a using equation 5a, draw the N_A observations for O_1, O_2, and O_3 using the parameters in equation 5b, setting the Normally distributed ϵ_k to each have standard deviation $\sigma_k = 0.25$. After creating the outcomes I set each of the N_A observations of L_a to missing.

I estimate two versions of each model for each data set, one for the model based on the regression approach of equation 4, and one for the model that I propose in equation

3 which estimates all parameters simultaneously as a joint posterior distribution.

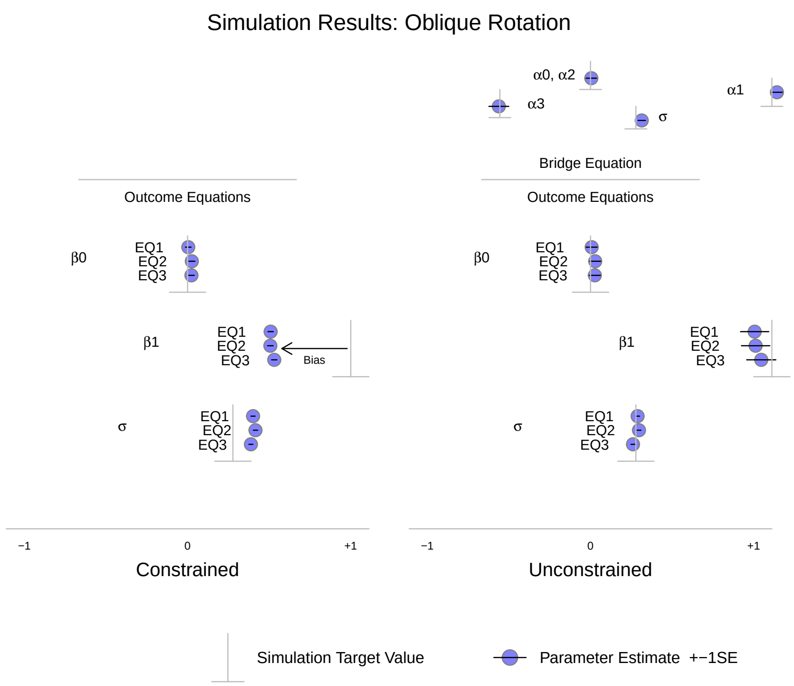

Figure 11: Simulation results for the oblique rotation case.

First consider the results for the oblique rotation case, shown in figure 11, and for the orthogonal rotation case, shown in figure 12. In each figure, the target value for each parameter is shown by a grey line. The regression-based results that make use of the constrained estimates for the agent preferences L_a^0 are in the left panel and the results that incorporate the unconstrained agent preferences L_a^1 are on the right. Note that under both rotations the regression results underestimate the outcome hypotheses, β_{1k}, and that the degree of bias increases with the amount of rotation θ. Underestimated parameters are typical in a regression with measurement error. Conversely, the full model that accommodates rotation of agent preferences yields correct estimates for the outcome parameters (with a small amount of shrinkage).[28] More importantly, the full model correctly

[28]The results remain the same when bootstrapped across iterated simulations. For example, when

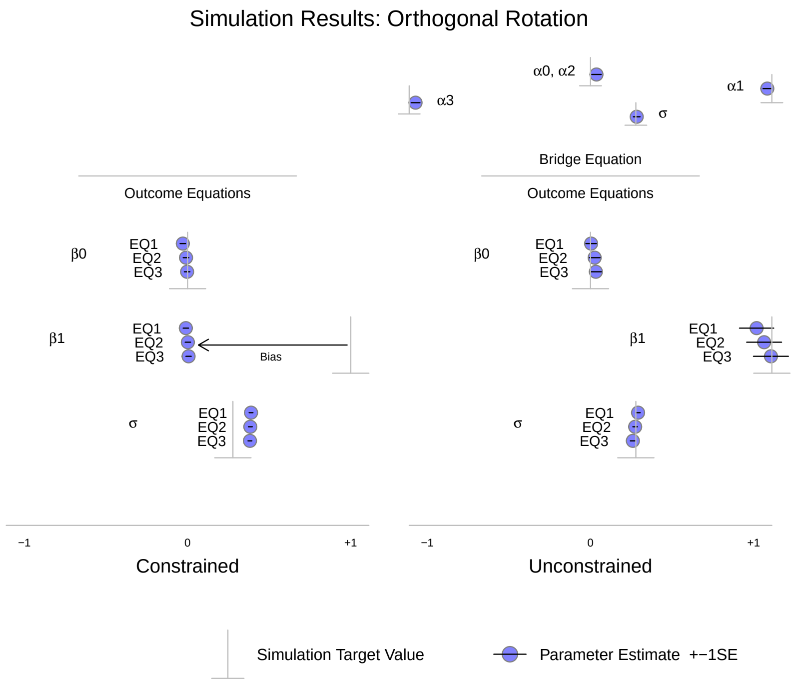

Figure 12: Simulation results for the orthogonal rotation case. Notice that the regression approach, which is identified by assuming preferences are unidimensional, increases in bias with the angle of rotation, while the full model that accomodates rotation remains unbiased.

estimates the agent-specific bridge parameters $\boldsymbol{\alpha_a}$ which demonstrates that the full model identifies these parameters and solves the fundamental problem of bridging.[29]

Next consider the case where the parallel preference dimension assumption is valid (results not shown). Here both models return the correct estimates for the outcome parameters, but the constrained (regression-based) model returns estimates of the error variance in each outcome equation (σ_K) by about 40 percent over the true value, while the full model returns the correct estimates. Both models return roughly correct t-ratios

bootstrapped across 10 simulations, the standard error of the β estimates is 0.06, so the bias in the orthogonal case is 15 times the size of the standard error.

[29] A final case that I implemented but do not report is the case of an obtuse rotation, which requires a $\theta > \frac{\pi}{2}$. This is an unlikely rotation in practice. In the simulation, however, the full model yields the correct parameter estimates for both bridging and all outcome equations.

for each parameter, however, but for different reasons. The full model estimates correct standard errors for the parameters because it is able to correctly account for the estimation uncertainty that results from using estimates instead of observed values for each L_a value, along the lines of equation 3. The regression approach yields similar t-ratios through a combination of overestimating the standard error of the regression and neglecting the uncertainty that comes from using expected values for L_a. However, two wrongs in this case do not make a right and it is not best practice to hope for correct parameter standard errors through an unknown mixture of incorrectly-estimated variances.

Finally, I consider two cases to examine identification. First, to demonstrate the necessity of multiple outcome equations to identify the agent-specific bridge parameters, I re-estimated the full model using the orthogonal case, but this time only including L_a in one outcome equation rather than three (results not shown). In this case, the posterior for L_a does not converge since it is not possible to estimate a random effect from a single equation. This demonstrates the agent-based bridge parameters are not identified unless there are repeated outcome equations. Second, I generated an alternative simulated dataset where the β coefficients are each set to zero as a way to test whether the agent-specific rotation parameters remain identified even when the hypotheses are false. In this case, the model retrieves the correct rotation parameters but the structural parameters show poor mixing and an overall poor model fit compared to the cases where the hypotheses are true. This result shows the importance of good theory for the statistical analysis.

A.4 Agent-Specific Preferences and Construct Validity

The main purpose of this paper is to identify a conceptual and practical solution to the fundamental problem of bridging (see Jessee, 2016; Lewis and Tausanovitch, 2015), a solution that will allow the analyst to impute agents' legislatively-relevant preferences without requiring overly restrictive assumptions, such as the unidimensional preference assumption. As I show in figure 2, my solution leverages agent and legislator behavior within an institutional interaction to refine and update the imputation of agent preferences.

In the simulation, I show that in a mathematical sense, agent-specific bridging parameters are identified when functions of the agents' preferences are embedded in repeated institutional hypotheses. More importantly, though, I show in the application that the unconstrained preference estimates that reflect the rotation described by the agent-specific bridging parameters have better construct validity (Cook and Campbell, 1979; Cronbach and Meehl, 1955; Hill, 2001) than preference estimates that constrain all actors' preferences to a single dimension. Operationally, the higher construct validity of the rotated solution can be seen in a comparison between figures 4 and 8, since the relationships between preference distance and the count of sentences within a dyad are apparent in the rotated (figure 8) but not in the unidimensional (figure 4) solution.

At first glance, this argument regarding the construct validity of the rotated preferences might appear circular, and even so beyond mere claims that the updated preferences allow rejection of the null hypotheses at conventional levels of significance. The circularity of the reasoning regarding construct validity is reflected in the model itself, as illustrated in figure 2. In the model, the agent preferences (L_a) update the structural parameters in the outcome equations (β) via the function z, and in turn the structural parameters update the preferences. Thus the two sides of the model are able to work jointly to find the best-fitting solution to both sets of parameters.

This circularity however is at the very core of the concept of construct validity, which requires that the very meaning of a measure of a construct is informed by the relationships implied in existing theory. In the case of my application, the hypotheses that center on the β parameters are derived from well-accepted institutional theory that preference distance matters in strategic and in social interactions, and the model makes use of this theoretical information to impute preferences that were operational within the behavioral interaction. The correlations between the preference distance and sentence counts within dyads confirms the underlying theory that generated the hypotheses, and at the same time, lend validity to the measured agent preferences as a construct. The canonical statement of construct validity comes from Cronbach and Meehl (1955), who state that construct validity is grounded in the consistency of measures of a construct with theoretical predictions, or within what they refer to as a *nomological network*. They write, "Scientifically speaking, to 'make clear what something is' means to set forth laws in which it occurs," and "the investigator who proposes to establish a test as a measure of a construct must specify his [or her] network or theory sufficiently clearly that others can accept or reject it ..." (p. 290-291). Thus, if one accepts the theory that preference distance matters within institutions, one should be willing to invest validity in the unconstrained over the constrained preference estimates (see also Cook and Campbell, 1979, 70).

The construct validity of the agent preference measures are further bolstered by the text analysis of figure 10 that identifies a cost-quality dimension structuring agent preferences, since this dimension is consistent with expert-level reasoning regarding the trade offs in health care financing. Even though the text analysis is decidedly exploratory, as Cook and Campbell (1979, 69) note, both planned hypothesis test as well as post-experimental specification can inform the meaning of a construct.

Note however that in the application the agent preferences rotate nearly orthogonal relative to witness' personal ideology in the full model, as measured by ψ. A reader who held strong beliefs in the unidimensionality of preferences would take such a rotation as evidence against construct validity. Such a reader then would need to reconcile how consistency with the unidimensional assumption leads to results that are inconsistent with virtually all of institutional theory.

Given the rotation is orthogonal in my application, it is clear that the behavioral outcomes are doing most of the heavy lifting to identify witness preferences. However, the personal ideology measurement model for ψ remains necessary in the model for two reasons. First, statistically, given the witness-specific random effect, the z function is not identified when detached from the personal ideology model, since z is also a random effect, but it becomes well-defined as a distance measure when connected to the measurement model. Second, substantively, the measurement model is necessary to identify the degree of rotation and hence reveals the nature and structure of discourse within the hearing. That is, without the connection to the personal ideology model the model could not describe the amount of rotation, and hence would not shed light on the nature of discourse within the hearing beyond the hypothesis tests themselves.

A.5 Coding

In order to identify the agent-specific transformation parameters using my proposed approach, the analyst must embed estimates of agent preferences, or functions of those preferences, within repeated hypotheses regarding behavioral outcomes. In my application, I develop a set of hypotheses regarding the different types of statements that legislators make within committee hearings.

To collect the outcome data regarding the types of sentences stated in committee hearings, a research assistant and the author hand coded the sentences (questions and statements) that legislators direct to specific witnesses at the sampled committee hearings, as recorded in committee hearing prints. To do the coding, I constructed more than 20 separate codes to mutually exclusively and exhaustively classify the types of statements and questions committee members ask of witnesses at the hearing. In addition to a set of "miscellaneous" statements, I organize these codes under the labels "falsifiable," "opinion," and "anecdotal" sentences.

Falsifiable sentences contain analytical policy information and are stated in a form that could be made into an operational research statement. Unlike opinion sentences, they are asserted as positive effects of a program or factual descriptions of the real world. Unlike anecdotal sentences, they are asserted as general and systematic, rather than local or personal. Examples of falsifiable sentences include:

- Verifiable factual statement. “Cataracts is one of the most significant causes for decreased vision.”

- Description of how a program, policy, or organization operates at a general level. “From 1966 and until the mid-1990s, claims billing errors by hospitals across the country were handled through normal external audit process[es].”

- Causal implication or argument about the effect of a current policy or program. “The SMI Trust Fund, which in balance on an annual basis, shows a rate of growth of costs which is clearly unsustainable.”

- The hypothetical future effects of a proposal. “If Medicaid payments to managed care plans are set below market rates to achieve savings, the participation of mainstream plans could be compromised.”

- Description of past actions in the policy process and intents of political actors. “Congress intended for payment reform to neither increase nor decrease overall Medicare payments to physicians.”

Opinion sentences are normative or non-falsifiable statements, or statements that are explicitly qualified as the authors own belief or opinion. In all cases, these statements are not asserted as “true” or empirically demonstrable. Examples include:

- A policy position. “Medicare beneficiaries should be provided with a range of health plan choices, and those choices should be accompanied by incentives to select the more cost effective alternatives.”

- A policy recommendation. “Delinking public health care programs from public cash assistance programs is good public policy.”

- A normative argument (fairness, ideology). “No reason has been shown why the pharmaceutical industry should be singled out from others that freely negotiate the prices of their products with the DVA and the other departments and agencies of the Federal Government."

- The speakers belief, feeling, or desire. "The proposed 16 percent reduction in the conversion factor results from a misinterpretation by HCFA of the mandate for budget neutrality contained in OBRA-89, as well as from inappropriate and demeaning assumptions about anticipated physician behavior in response to payment reform."

- A rhetorical question or political advice. "The Congress may want to create a process to adjust future conversion factors based on actual billing experience."

Anecdotal sentences reference only the speaker's immediate experience, or the immediate experience of the witness's organization with a policy or program, or only make reference to local conditions such as conditions within a specific congressional district. Examples include:

- A person's or organization's particular experience in a program. "In December our accountant received a list of more than 10,000 alleged billing errors during those five years."

- Likely effects from program or an alternative generalized from personal experience. "Dr. Russell Snow, and eye, ear, nose, and throat doctor from Caldwell, Idaho, says his colleagues are so frightened by federal enforcement provisions that many more are [going to drop Medicare patients]."

- Information about a congressional district or locality. "In my State, the hospitals that are okay are the ones that are doing cardiac."

- Statements about length of personal experience with a policy area

- Quote from well-known figure, adage, what "other people" are saying

Reliability tests for coding. A research assistant independently recoded a testbed random sample of sentences stated in Medicare committee hearings ($N = 578$) for an inter-coder reliability test, and the principle investigator re-coded a second random sample ($N = 711$) one year after completing the first round of coding to conduct an intra-coder reliability test. The Cohens Kappa reliability statistic for the intercoder reliability test is 0.57 with a 71 percent agreement rate (32 percent expected, $p < 0.0001$), and the intra-coder reliability is 0.79 with an 85 percent agreement rate (30.5 percent expected, $p < 0.0001$). While there are no established thresholds for reliability, a kappa statistic in the range of 0.75 to 0.80 is widely considered excellent agreement beyond chance, and 0.40 to 0.75 fair to good agreement beyond chance (Nuendorf, 2002, 143). All member sentences in the hearing transcripts sampled for this project were double-coded by both the research assistant and the principal investigator, with the latter resolving disagreements.

A.6 Statistical Model

Here I set out the full model, and then explain how to implement the regression approach as a set of restrictions on the full model. I estimate the full model as a set of simultaneous equations in a Bayesian framework with likelihood:

Likelihood for Submodel A (Former Members & Witnesses):

$$\left.\begin{array}{rcl}
Markets_i & \sim & \text{Ordered Logit}(-1 \times \psi_i) \\
Companies_i & \sim & \text{Ordered Logit}(\lambda_{11}\psi_i) \\
HelpPoor_i & \sim & \text{Ordered Logit}(\lambda_{12}\psi_i) \\
Access_i & \sim & \text{Ordered Logit}(\lambda_{13}\psi_i) \\
Incomes_i & \sim & \text{Ordered Logit}(\lambda_{14}\psi_i) \\
L_{(a,l)_i} & \sim & \text{Normal}(\mu_{0i}, \sigma) \\
\mu_{0i} & = & \alpha_0 + \alpha_1\psi_i \\
 & & +\alpha_2 Agent_i + \alpha_3\psi_i Agent_i + \boldsymbol{\alpha_4}\mathbf{X}
\end{array}\right\} 1 \leq i \leq N_1$$

Likelihood for Submodel B (Legislator-Agent Hearing Dyads):

$$\left.\begin{array}{rcl} Distance_j & = & |L_{l_j} - L_{a_j}| \\ Falsifiable_i & \sim & \text{Poisson}(\mu_{1j}) \\ \mu_{1j} & = & \beta_{10} + \beta_{11} Distance_j + \beta_{12} ResOrg_j + \\ & & \beta_{13}(Resorg_j \times Distance_j) + \eta_{1_j} + \eta_{2_{l_j}} + \eta_{3_{a_j}} \\ Opinion_i & \sim & \text{Poisson}(\mu_{2j}) \\ \mu_{2j} & = & \beta_{20} + \beta_{21} Distance_j + \beta_{22} ResOrg_j + \\ & & \beta_{23}(Resorg_j \times Distance_j) + \gamma 1 \eta_{1_j} + \eta_{2_{l_j}} + \eta_{3_{a_j}} \\ Anecdote_i & \sim & \text{Poisson}(\mu_{3j}) \\ \mu_{1j} & = & \beta_{30} + \beta_{31} Distance_j + \beta_{32} ResOrg_j + \\ & & \beta_{33}(Resorg_j \times Distance_j) + \gamma 2 \eta_{1_j} + \eta_{2_{l_j}} + \eta_{3_{a_j}}. \end{array}\right\} 1 \leq j \leq N_2$$

where i indexes N_1 former members and witnesses (combined), j indexes N_2 legislator-witness dyads that occur within the committee hearings, L_{a_j} is imputed for each witness in the jth dyad, l_j indexes legislators in the dyad and a_j indexes witnesses.

Submodel A is the measurement model that places the witnesses who appeared at the congressional committee hearings into the relevant legislative common space. The equation set in submodel A imputes witnesses' preference score by first placing witnesses into a *survey response* ideology space defined by the set of ideological attitude survey questions, a space denoted in the likelihood by ψ, via the estimated difficulty parameters (factor coefficients) λ_{11} to λ_{14}.[30] Submodel A then identifies beliefs over the transformation from ψ to the legislative roll call space $DWNominate$ with the linear transformation given by α_0 and α_1, and covariates $\mathbf{X}$, while the posterior preferences from the full model also take into account the α_2 and α_3 structural parameters, the *Agent* indicator (1 if witness, 0 if former member of Congress), and the interaction between *Agent* and ψ.

The outcome equations are contained in submodel B. Each within-dyad question type count is modeled as over dispersed Poisson-distributed, conditional on the within-dyad distance in legislative preference space, allowing separate parameters for dyads that have a witness from a *Research organization* (= 1) and for dyads where the witness is not from a research organization (= 0). The outcome equations are estimated simultaneously

[30]For identification I constrain the difficulty parameter for the *Markets* survey item, which has an opposite ideological direction from the other items.

as seemingly unrelated regressions, and they share a common legislator-specific random effect η_2 that captures the member's propensity to ask questions and make statements of all types to witnesses, or their loquaciousness; a witness-specific random effect η_3 that captures the witness's propensity to attract questions and comments from legislators; and a dyad-specific random effect, η_1, that captures omitted variables that govern the dyadic interaction. These three random effects capture any omitted legislator-, witness-, or dyad-specific covariates, and also allows extra variance that accounts for over dispersion in the count data.

To complete the Bayesian model, I set the priors for the $\lambda_.$ parameters as distributed Uniform(0, 10) in order to ensure the correct direction labeling in the factor model. The σ prior is Uniform(0,100). ψ and each η have a standard normal prior. All other priors are unrestricted, normally distributed with mean zero and standard deviation 1000.

In order to evaluate the robustness of each modeling approach, I estimate the model under slightly different specifications to accommodate differing specifications in the literature. First, I vary the distance measure by using either an absolute value distance function as well as a quadratic distance function. The institutions literature typically assumes a quadratic distance but this is only to preserve differentiability when the preferences coincide and is not theoretically required.

Second, I also estimate the models with and without an indicator for political party in the imputation equation. One can include arbitrary covariates $\mathbf{X}$ in the imputation equation, provided one has measures of the covariates for both former members and for witnesses. Below I demonstrate the possible importance of political party as a covariate in this imputation, which requires a classification of witnesses into Democratic and Republican interest group constituencies that I describe in the text.

Finally, I estimate the full model without any constraint, and one with a soft constraint that constrains $\alpha_1 + \alpha_3 \geq 0$. This constraint prevents the angle of rotation from exceeding $\frac{\pi}{2}$, that is, to prevent an obtuse rotation that is implausible in this setting.

I estimate these multiple specifications as a way to demonstrate the robustness of the model to specification, and I report all results as well as Bayesian model averages in the appendix to demonstrate the presence or absence of variability across specifications, and I also report WAIC information statistics (Vehtari et al., 2017) for comparison. The full model estimates both A and B submodels simultaneously, so the imputation submodel and the outcomes submodel jointly inform the posterior distribution over each L_a. The regression approach estimates the two models separately and so the outcome equations do not update the preferences of agents. Overall, I find there is considerable fluctuation in the outcome equation parameter estimates under the regression approach across these specifications, but considerable stability when using the full model.

A.6.1 Estimation

I estimate the model in `OpenBUGS` using Bayesian MCMC methods (Spiegelhalter et al., 1996). I run the model until the posterior distribution of the structural estimates are stationary, and then sample from the posterior distribution to create marginal distributions of each parameter of interest. For each model, I use a 100k burn in period, and then sampled 300k iterations saving each 300 for 3 chains (3000 replicates from posterior).

A.7 Results

In this section I present the full results first for the measurement model, which returns constrained estimates for witnesses' preferences, and then the regression-based results, and then the full model results.

A.7.1 Measurement Model

Here I describe the results of the model that measures actors' personal ideological preferences ψ, which the full model and the regression approach use to construct beliefs over agents' legislative preferences. Table 5 shows the results of the measurement model using only the data from the former members' sample (i.e., for now not using the responses from witnesses). The measurement model is able to test the convergent validity of the indicators I use to estimate ψ, which is indicated by large and statistically significant discrimination parameters (factor coefficients) $\boldsymbol{\lambda}$; these are reported in the first two columns of results in table 5 and clearly indicate convergent validity. The measurement model results shown in the main text compare the discrimination parameters from the pooled (witness plus former member) sample and shows that the shows that the parameters are equivalent in the pooled and unpooled case, which shows that the responses to these items is invariant across the two groups.

The next two sets of columns in table 5 regress the personal ideology scale ψ on former members' roll-call vote $DWNominate$ scores (first dimension) in order to construct the bridge that I use in the regression approach. The middle columns regress former members' $DWNominate$ scores on their ideology (ψ) and a set of covariates which test whether there are organizational or employment-based characteristics that would affect the bridging transformation among members. Here I include covariates for party identification, whether the former member was a senator, the member's tenure in office, and whether the former member returned the survey from the DC area (likely indicating a lobbyist versus a true retiree). Note that only party identification is significantly different from zero.

The final set of columns in table 5 re-estimates the bridging model among the former members but only including party as a covariate. The fixed effect of party is large, 0.47, and the posterior is far to the right of zero. Including the party covariate reduces the mapping coefficient α_1 to 0.10, which remains to the right of zero but is roughly half of the magnitude of the estimated parameter when party is omitted (reported in the main text). That party is a statistically significant predictor of legislators' roll-call scores is no surprise. Party plays a central role in organizing congressional politics.

The literature on Congress leaves it as an open question whether it is theoretically

Table 5: Validity of Personal Ideology (ψ) as a Predictor of $DWNominate_1$

	ψ Model Only		with Covariates		without Covariates	
	Mean	SD	Mean	SD	Mean	SD
Bridge Equation						
Ideology (ψ)			0.11	0.03	0.26	0.03
DC Area			-0.01	0.04		
Chamber			-0.02	0.07		
Tenure			-0.00	0.02		
Party			0.47	0.05		
Constant			-0.21	0.03	-0.05	0.04
Sigma			0.15	0.01	0.20	0.02
Ideology Factor Loadings						
Markets	1		1		1	
Companies	1.63	0.42	1.60	0.41	1.61	0.37
Help Poor	2.86	0.89	2.65	0.76	2.53	0.67
Access	2.33	0.54	2.51	0.58	2.51	0.52
Incomes	1.83	0.44	1.86	0.44	1.75	0.38

sensible to include this covariate in the bridging model. There has been a longstanding debate in the Congress literature regarding the causal role of party affiliation, given that knowing the party identification of a U.S. member of Congress explains that vast majority of the variation in roll call votes, yielding a distinctly bimodal distribution of roll call preference scores. The bimodal distribution of roll call outcomes could be for two reasons. On the one hand members might have their roll call votes pressured away from the center by party leadership, where party has an independent and causal effect on roll call preferences, and on the other hand relatively extreme individual members might select into their party identification in a way that produces the bimodal distribution. The institutional literature on Congress has witnessed strong disagreement in the appropriateness of the use of roll-call preference scores (Clinton, 2007; Cox and Poole, 2002; Hirsch, 2010; Stiglitz and Weingast, 2010) or survey-based preference scores (Alemán et al., 2017; Ansolabehere, Snyder, and Stewart, 2001b) to test the effects of party, and indeed whether it is even possible to identify the effect of party from observational data that emerges from legislative behavior (Krehbiel, 2000).

Figure 13 illustrates the relationships between the survey preference space ψ, party identification, and the roll call preference space $DWNominate$ among the set of former members only (in a figure similar to Shor and McCarty, 2011, 535). In this figure, Republicans are indicated with filled circles, Democrats with empty circles, the unadjusted relationship between ψ and $DWNomiate$ is indicated by the solid black line, and the relationships adjusted for party are indicated by dashed lines. In the left hand panel, I include only an intercept shift for party, while the right hand panel I also allow for an interaction term. There is something of a Rorschach blot quality to the patterns in this figure. If one ignores the party classification there is a clear linear relationship between ψ and $DWNomiate$. If one considers party, the members' survey preferences remain unimodal but the roll call preferences separate into a bimodal distribution. Further, if one allows the full interaction of the right hand panel, it appears that among Republican members, personal ideology has no relationship with roll call voting, while ideology matters for Democrats. One could conclude that ideology does not matter for Republicans, although there is no suggestion in the Congress literature that would support this asymmetric relationship, and the full interaction is likely an over fit to the sample, so below I only consider the main effect of party.

In the application, I focus on bridging actors into roll-call space, and so I do not need to take a position on this question whether or not party (or constituents or groups or donors) shape legisltive preferences. I simply estimate all of my models that I report in the paper either including or excluding party as a covariate, and compare the robustness of the findings across the specifications. The results of the bridging model in themselves might be of interest to this literature and so worth reporting here.

A.7.2 Results for Regression with Priors

In this section I report the full set of results for the sentance count outcome equations from the regression approach to bridging. To estimate the models in table 6 I first extract the

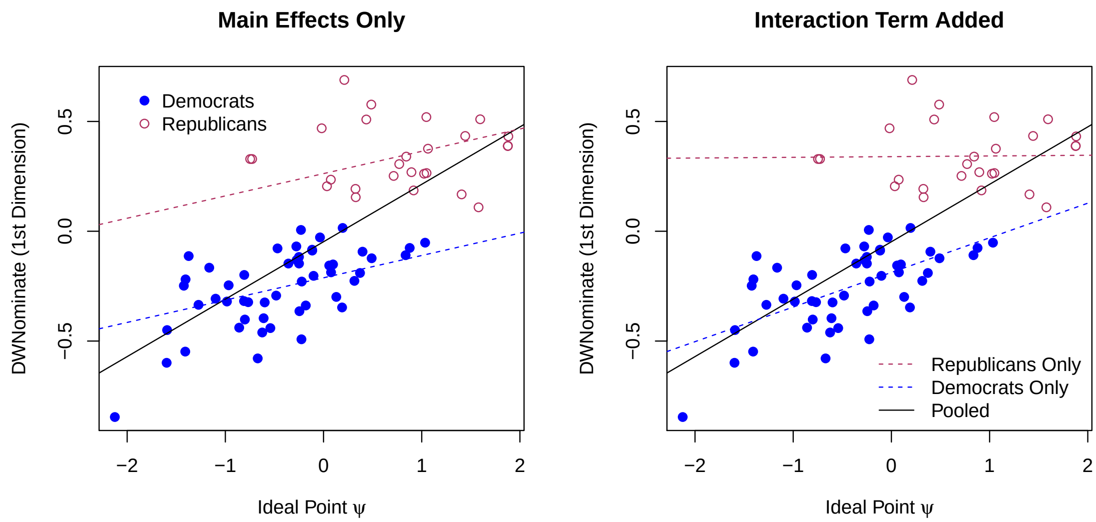

Figure 13: Estimates of the measurement model including former members only, indicating the relationship between personal ideology ψ and roll-call preferences for Republicans and Democrats. The left panel shows the best fitting line only allowing an intercept shift for party, while the right panel shows the best fitting line allowing an interaction by party.

expected values of the witness preferences based on the models reported in the second and third columns of table 3 and use these to construct the distance measures within each dyad. I then jointly regress each question count on distance, the research organiztion indicator, the interaction between distance and research organization, and a constant, using a Poisson likelihood for each count type. These three outcome models are estimated jointly and each equation includes random effects for witness, legislator and dyad (not shown). Table 6 reports the results that 1) include or exclude the party covariate in the bridging equation, and 2) use the absolute value or quadratic function to measure distance. Note that in each specification, distance is not statistically related to any of the question counts. The tables also indicate the Widely Applicable Information Criterion (WAIC) value (Vehtari et al., 2017) is lowest for linear specification with party covariate, indicating that this specification is the best fit.

Table 6: Regression with Constrained Preferences

	Linear				Quadratic			
	with Party		without Party		with Party		without Party	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Outcome Equations								
Falsifiable Statement Count								
Distance	-0.15	0.37	0.47	0.44	-0.57	0.40	0.50	0.45
ResearchOrg	0.72	0.41	0.48	0.41	0.44	0.34	0.30	0.38
Interaction	-1.07	0.64	-0.46	0.65	-0.73	0.73	-0.25	0.70
Constant	-1.61	0.27	-1.84	0.30	-1.51	0.25	-1.75	0.26
Opinion Statement Count								
Distance	-0.94	0.59	-0.13	0.66	-1.47	0.66	-0.22	0.67
ResearchOrg	0.15	0.57	-0.20	0.57	-0.14	0.45	-0.36	0.50
Interaction	-1.57	1.09	-0.54	1.06	-1.60	1.26	-0.53	1.16
Constant	-2.91	0.39	-3.25	0.41	-2.92	0.34	-3.22	0.35
Anecdotal Statement Count								
Distance	-0.17	0.87	0.88	0.90	-1.07	0.93	0.49	0.89
ResearchOrg	0.70	0.78	0.19	0.78	-0.18	0.61	-0.56	0.65
Interaction	-5.42	1.70	-3.37	1.57	-6.00	2.09	-2.85	1.70
Constant	-4.79	0.61	-5.12	0.59	-4.57	0.51	-4.82	0.49
Dyad-Specific Random Effect								
Falsifiable Equation	1		1		1		1	
Opinion Equation	1.74	0.12	1.77	0.12	1.74	0.12	1.76	0.12
Anecdotal Equation	2.62	0.22	2.58	0.21	2.61	0.21	2.56	0.20
WAIC								
Falsifiable Model	994.81		1030.19		1061.32		1075.97	
Opinion Model	553.72		575.16		631.25		596.23	
Anecdotal Model	454.97		544.29		584.25		577.75	

A.7.3 Regression Results that Combine the Two-Step Model into One Step

One might reasonably wonder if the main difference between the results I observe from the "two-step" regression procedure (above) and my full model (below) is simply due to the intervening step; that is, the regression approach I describe above has a second stage while the full model has only one, and so perhaps the differences are due only to the procedure.

To evaluate this, I re-estimate the regression approach for the simulation data in a one-step procedure making use of the `cut` function available in `OpenBUGS`, which allows the model to update the structural parameters in the outcome equations based on estimates for the agent preferences, but does not simultaneously update the agent preferences based on posteriors for the structural parameters (in other words, I cut the graph in figure 2 between the L_a variable and the z variable, so that updating only flows in one direction).

This one-step method returns estimates that are exactly identical to those returned by the two-step method. In the paper I focus on the latter since the two-step method is the current practice in the bridging literature.

A.7.4 Results for Full Model

In this section, I report in tables 7 and 8 the results from the full model, which employs the rotated posterior preference measures for witnesses in the outcome equation distance functions. In contrast to the regression approach, the full model estimates the bridge equation model and the outcome models jointly and hence uses the full information in both models to improve estimation.

Table 7 reports the results of the model that includes a soft constraint $\alpha_1 + \alpha_3 \geq 0$, which prevents the rotation of the witness preferences from achieving an obtuse rotation, which is theoretically implausible and would likely reflect an over fit to the sample. There are many things to note in this table. First, note that the factor loadings for ψ are stable across the specifications and are indistinguishable from the factor loadings that

Table 7: Full Model Posteriors with Witness Parameters Soft Constraint

	Linear				Quadratic			
	with Party		without Party		with Party		without Party	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Bridge Equation								
Ideology (ψ)	0.07	0.03	0.21	0.03	0.07	0.03	0.22	0.03
Witness	-0.08	0.05	-0.13	0.07	-0.06	0.05	-0.12	0.06
Ideology X Witness	-0.05	0.03	-0.18	0.04	-0.05	0.03	-0.18	0.04
Party	0.45	0.05			0.44	0.05		
Constant	-0.21	0.03	-0.07	0.04	-0.21	0.03	-0.07	0.03
Sigma	0.19	0.02	0.25	0.03	0.19	0.02	0.23	0.02
Ideology Factor Loadings								
Markets	1		1		1		1	
Companies	1.61	0.33	1.60	0.31	1.59	0.32	1.59	0.31
Help Poor	2.13	0.52	2.14	0.48	2.13	0.53	2.10	0.46
Access	1.94	0.41	1.97	0.39	1.94	0.42	1.98	0.39
Incomes	1.63	0.35	1.56	0.30	1.63	0.35	1.56	0.31
Outcome Equations								
Falsifiable Statement Count								
Distance	-1.87	0.67	-2.07	0.76	-2.55	0.82	-2.56	0.92
ResearchOrg	1.02	0.46	1.16	0.47	0.67	0.36	0.78	0.39
Interaction	-2.26	0.96	-2.54	1.01	-2.36	1.31	-2.88	1.38
Constant	-0.88	0.35	-0.80	0.38	-1.10	0.27	-1.08	0.31
Opinion Statement Count								
Distance	-2.94	0.87	-2.54	1.12	-3.44	1.06	-2.94	1.26
ResearchOrg	0.59	0.62	0.97	0.66	0.27	0.47	0.49	0.52
Interaction	-3.08	1.55	-4.02	1.62	-4.74	2.41	-5.79	2.60
Constant	-2.02	0.46	-2.15	0.55	-2.45	0.36	-2.53	0.43
Anecdotal Statement Count								
Distance	-0.72	1.10	-0.23	1.40	-0.92	1.26	-0.31	1.39
ResearchOrg	2.39	0.93	2.88	1.00	1.20	0.69	1.53	0.76
Interaction	-13.43	3.31	-14.67	3.31	-24.15	6.99	-25.93	7.07
Constant	-4.33	0.62	-4.53	0.74	-4.45	0.51	-4.53	0.58
Dyad-Specific Random Effect								
Falsifiable Equation	1		1		1		1	
Opinion Equation	1.66	0.12	1.65	0.12	1.66	0.12	1.66	0.12
Anecdotal Equation	2.54	0.22	2.51	0.22	2.55	0.23	2.50	0.22
WAIC								
Falsifiable Model	964.93		956.34		962.03		956.93	
Opinion Model	538.13		541.88		543.99		543.30	
Anecdotal Model	425.47		421.17		424.08		420.08	

resulted in the measurement model that only includes former members. This indicates that the ideology survey measures have high convergent validity. Second, note that the model is able to estimate agent-specific bridging parameters (attached to the $Witness$ and $Ideology \times Witness$ rows) that govern the degree of rotation of the witness preference space away from the roll-call preference space. Each of these indicates an approximate orthogonal rotation. Third, note that the hypotheses regarding both preference distance, research organization, and their interaction are largely confirmed. And these results are quite stable across the four specifications. That the results are significant in this table but not in the regression approach strongly suggests that the regression approach generates preference scores for witnesses that are measured with error.

Table 8 shows the same results but without the constraint, that is, permitting the rotation of witness preferences to an obtuse angle. Note that even with this extreme rotation the results are nearly identical to those from the previous model, and also stable across specifications.

Finally, table 9 reports the results from the model testing an alternative institutional theory, one that (likely mistakenly) assumes that legislators ignore their own roll-call based preferences when interacting with witnesses in hearings. This is an implausible theory ex ante but my model allows evaluating the merits empirically. Mathematically, omitting legislators' preferences from the equation is equivalent to placing each legislator at the center (zero) of the witness preference space. I show the results for both the regression approach (first two columns) and the full model (second two columns) with each omitting the party covariate and using the absolute linear distance function.

Note that in the full model, omitting the member preferences causes the results for research organization (both direct and interactive) to be no longer statistically significant, and the results of the distance function largely attenuated relative to the standard deviation of the posterior. Finally, note that the WAIC scores for all three models are lower than the respective scores for the original full model. These results suggest that this

Table 8: Full Model Posteriors with Witness Parameters Unrestricted

	Linear				Quadratic			
	with Party		without Party		with Party		without Party	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Bridge Equation								
Ideology (ψ)	0.09	0.03	0.24	0.03	0.10	0.02	0.24	0.03
Witness	-0.09	0.06	-0.14	0.07	-0.07	0.06	-0.12	0.06
Ideology X Witness	-0.33	0.07	-0.27	0.07	-0.29	0.06	-0.27	0.06
Party	0.48	0.05			0.45	0.05		
Constant	-0.22	0.03	-0.07	0.03	-0.21	0.03	-0.07	0.03
Sigma	0.16	0.02	0.24	0.03	0.15	0.02	0.22	0.03
Ideology Factor Loadings								
Markets	1		1		1		1	
Companies	1.59	0.32	1.60	0.30	1.55	0.30	1.59	0.30
Help Poor	1.95	0.45	2.13	0.47	2.02	0.46	2.10	0.45
Access	1.96	0.41	2.03	0.40	2.04	0.42	2.01	0.39
Incomes	1.68	0.34	1.60	0.31	1.63	0.32	1.58	0.30
Outcome Equations								
Falsifiable Statement Count								
Distance	-1.86	0.59	-1.76	0.99	-2.57	0.78	-2.62	0.88
ResearchOrg	0.97	0.47	1.26	0.52	0.65	0.38	0.75	0.39
Interaction	-2.08	0.94	-2.76	1.13	-2.31	1.28	-2.72	1.38
Constant	-0.87	0.34	-0.95	0.46	-1.08	0.28	-1.07	0.29
Opinion Statement Count								
Distance	-2.40	0.85	-2.03	1.47	-3.13	1.09	-2.99	1.21
ResearchOrg	0.75	0.63	1.17	0.77	0.33	0.50	0.46	0.51
Interaction	-3.56	1.48	-4.46	1.87	-5.22	2.31	-5.71	2.53
Constant	-2.19	0.47	-2.40	0.71	-2.47	0.38	-2.51	0.41
Anecdotal Statement Count								
Distance	-0.73	1.11	0.20	1.78	-1.31	1.34	-0.44	1.36
ResearchOrg	2.41	0.95	3.15	1.12	1.15	0.74	1.45	0.74
Interaction	-14.00	3.14	-15.40	3.37	-26.17	7.22	-26.13	7.01
Constant	-4.33	0.66	-4.78	0.90	-4.39	0.55	-4.49	0.56
Dyad-Specific Random Effect								
Falsifiable Equation	1		1		1		1	
Opinion Equation	1.66	0.12	1.66	0.12	1.66	0.12	1.66	0.12
Anecdotal Equation	2.55	0.23	2.51	0.23	2.59	0.22	2.50	0.22
WAIC								
Falsifiable Model	965.10		958.67		964.94		958.12	
Opinion Model	546.49		545.27		547.00		543.17	
Anecdotal Model	428.63		427.78		424.81		421.49	

alternative specification is a worse fit to the data compared to the model that includes legislators' preferences. Further, the estimation shows poor mixing and difficulty in convergence and this is consistent with the simulation results when the hypotheses were set to false, and so this is further evidence that the alternative model is inferior to the theory that assumes legislators care about their own preferences when interacting in committee hearings with witnesses.

A.8 Text Analysis Details

The latent, agent-specific preference scale I recover in my application is orthogonal to the preference scale of legislators that is recovered using roll-call votes. In the paper I describe a text analysis procedure where I analyze the testimony of witnesses who score either high or low on the preference scale to describe the content of the preference dimension.

To conduct the text analysis, I grouped the hearings by topic, and identified the witnesses who had the highest and lowest posterior preference scores L_a^1 for the following set of topics: managed care, regulatory impacts, physician payments, disease management, and prescription drug coverage, and I pooled the testimony for those with high values together and those with low values together. I stratified by topic in order to ensure comparability across the two sets of documents and since word frequencies are governed by the content of documents.

To do the text analysis, I made use of the text analysis tools recommended in Silge and Robinson (2017).[31] I stemmed and removed common English language stop words, and then I deleted health-care specific stop words that were common to both sets of documents and hence did not distinguish between documents that score high and low on the witness preference dimension. These words are "medicare," "health," "program," "managed," "patient," "physician," and "plan."

[31] Available at https://www.tidytextmining.com/.

Table 9: Models Ignoring Members' Preferences

	Prior Prefs.		Posterior Prefs.	
	Mean	SD	Mean	SD
Bridge Equation				
Ideology (ψ)			0.25	0.03
Witness			0.31	0.14
Ideology X Witness			-0.09	0.10
Party				
Constant			-0.07	0.03
Sigma			0.21	0.02
Ideology Factor Loadings				
Markets			1	
Companies			1.57	0.30
Help Poor			2.08	0.44
Access			1.95	0.38
Incomes			1.62	0.31
Outcome Equations				
Falsifiable Statement Count				
Distance	0.23	1.48	-2.94	1.21
ResearchOrg	-0.06	0.56	-0.83	0.90
Interaction	1.66	2.25	3.45	2.64
Constant	-1.70	0.34	-0.66	0.56
Opinion Statement Count				
Distance	-0.93	1.74	-6.60	1.35
ResearchOrg	-0.83	0.67	-1.75	0.91
Interaction	2.37	2.74	4.47	2.79
Constant	-3.12	0.42	-1.09	0.69
Anecdotal Statement Count				
Distance	-0.21	2.14	-8.81	1.07
ResearchOrg	-1.90	0.88	-2.32	1.20
Interaction	3.83	3.53	3.32	4.79
Constant	-4.75	0.56	-1.73	0.86
Dyad-Specific Random Effect				
Falsifiable Equation	10		1	
Opinion Equation	1.73	0.12	1.56	0.12
Anecdotal Equation	2.58	0.21	2.27	0.20
WAIC				
Falsifiable Model	NAN		959.52	
Opinion Model	NAN		557.38	
Anecdotal Model	NAN		457.20	

A.9 Interpretation of Bayesian Model Results

Following early papers from Simon Jackman (2000a; 2000b), in recent years political science has witnessed a rapid adoption of computational Bayesian methods within the discipline (Gelman and Hill, 2006; Gill, 2015; Jackman, 2009). In large measure the discipline has been attracted to computational Bayesian methods because of the flexibility and speed to convergence compared to computational approaches for maximum likelihood estimation. And since with flat priors and/or large datasets Bayesian methods return results that closely approximate frequentist results, those who have adopted Bayesian approaches do not find the need to overcome any deep skepticism of those who have been trained exclusively in frequentist methods.

The epistemological foundations of Bayesian estimation are very different from those of the frequentist approach, however, and it is worthwhile to be explicit about the interpretation of Bayesian results. In the frequentist tradition the analyst posits the existence of parameters in the natural world that govern the data generating process, and the goal of the analysis is to identify plausible ranges of values that contain the parameters such as confidence intervals. In the Bayesian tradition, in contrast, the analyst takes no position on whether the natural world contains such parameters, and instead focuses on improving the discipline's subjective beliefs regarding the data generating process (McElreath, 2016, chapter 2). A Bayesian analysis returns correct beliefs (for the analyst and reader alike) given the model, statements of prior beliefs, and the data. In a Bayesian perspective, it is these subjective beliefs that are most relevant for scientific progress, whether the goal is to produce new knowledge or to recommend policies or other actions based on the study findings.

The goal of a Bayesian analysis is in many ways more modest than that of a frequentist approach in that the analyst needs only to return results that help to inform institutional theory rather than to claim to make statements about external truths. Applying this reasoning to my proposed model, when I state that the goal is to estimate agents' and legislators' preferences as they were operational in the institutionally-situated interaction, the Bayesian interpretation here is not that I am uncovering the actual preferences contained in the heads of the actors, but instead I am uncovering posterior estimates of preferences that are consistent with a theoretically-informed substantive model, given the data and (as in the case of my application) flat, uninformative priors.

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