LABOR UNIONS AND UNEQUAL REPRESENTATION*

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ABSTRACT

While research has found that lawmakers are more responsive to the views of the affluent than to the less well-off, it is not well understood what institutions may limit unequal responsiveness. We argue that labor unions can reduce the bias of legislators despite high economic inequality, expensive campaigns and comparatively low union density. We provide robust evidence for this argument from the contemporary U.S. House of Representatives. Our extensive dataset includes a novel measure of district-level union strength, drawn from 350,000 administrative records, and income-specific measures of constituency preferences matched to 23 roll-call votes, based on 278,000 survey respondents. Exploiting within-district variation in preference polarization, within-state variation in union strength and rich data on confounds, we can rule out a host of alternative explanations. Additional evidence shows that unions moderate responsiveness during an exogenous economic shock. Our findings suggest that unequal representation is not an unavoidable feature of democratic capitalism.

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I. INTRODUCTION

Over the last 15 years or so, political scientists have paid increasing attention to the link between economic inequality and political representation. In contrast to the principle of political equality that is central to the ideal of democratic governance, this vibrant strand of research has repeatedly found disparities in political representation by income. Specifically, elected officials and policy outcomes are more responsive to the views of affluent citizens than to middle-income and low-income citizens, and sometimes they are not responsive to low-income citizens at all.[1] As is summarized in a recent discussion of the literature (Bartels 2016: 235), evidence of unequal representation has been found for legislators, party platforms, national policy and state policy.[2] There is also evidence that legislative staffers in Congress have biased estimates of constituent opinions to the advantage of corporate and business interest (Hertel-Fernandez et al. 2017). While scholarship has initially focused mostly on the political system of the United States, recent comparative work has revealed similar patterns of unequal representation across a larger range of political institutions (Bartels 2017; Elsässer et al. 2017; Lupu and Warner 2017; Rosset 2013), including proportional electoral systems and multi-party government that had been previously associated with kinder and gentler (and presumably more equal) representation (Lijphart 1999). Given these results, scholars are asking why the less-affluent are underrepresented politically and whether there are institutions or organizations that can dampen unequal responsiveness in the democratic process despite high levels of economic inequality.

In this paper, we argue that labor unions can systematically shape the extent of unequal representation by elected representatives, with the effect of significantly reducing unequal responsiveness even in a context of expensive electoral campaigns, comparatively low union density, and dramatic trade shocks hurting especially those in the lower part of the income distribution. The argument is based on the credible and visible (for elites) threat of unions to mobilize citizens to vote, contact their representative, donate money, participate in meetings or volunteer in a political campaign, which shapes the selection, incentives and information of legislators. Existing research has documented that union membership is associated with lower income differentials in political participation and political knowledge (Leighley and Nagler 2007; Rosenfeld 2014). Moreover, unions tend to take positions favored by less-affluent citizens (Gilens 2012), and they are one of the few organizations in national politics

[1] For example, see Bartels (2008: ch. 9); Bartels (2016: ch. 8); Bhatti and Erikson (2011); Flavin (2012); Gilens (2005); Gilens (2012); Gilens and Page (2014); Rigby and Wright (2011); Rigby and Wright (2013).

[2] But see Enns (2015); Erikson (2015) for a different interpretation.

that advocate on the behalf of non-managerial workers, spending a substantial amount of resources in the process (Schlozman et al. 2012).[3] There is also evidence that some unions (sometimes) take costly strike action in the interest of others (Ahlquist and Levy 2013).[4]

However, it is not clear from existing research whether unions actually cause meaningful reductions in the pro-affluent bias of national politicians. Several scholars of representation suggest that unions have become too weak, too narrow or too fragmented to have a significant egalitarian political impact in national policymaking (Gilens 2012: 175; Hacker and Pierson 2010: 143),[5] and to date there is very little direct evidence of the impact of unions on unequal responsiveness.[6] Even if there is an empirical relationship between union strength and more equal responsiveness by politicians, it is possible that it is spuriously driven by the same fundamental determinants. Based on differences in social capital (Putnam 1993), for instance, workers in some electoral districts may be better at solving collective action problems than others. As a result, they will be more likely to unionize their workplace in the first place and, independently, politicians will be more responsive to them. Another concern is that the activity of unions may influence "parties and policy, but policy and institutions also affect unionization rates"(Ahlquist 2017: 427). Accounts of winner-take-all politics (Hacker and Pierson 2010) also emphasize a dynamic perspective. Consistent with this, state-level governments biased toward the affluent (Flavin 2012) may drive biased representation in the congressional delegation by strategically enacting policies that make it more difficult to organize unions. In particular, right-to-work and collective bargaining laws hamper unionization efforts, and recent research demonstrates that these laws have profound political effects (Feigenbaum et al. 2018; Flavin and Hartney 2015).

We assess our argument using the contemporary United States' Congress, where unequal responsiveness by elected representatives and their policy choices has been well documented (Bartels 2008, 2016; Bhatti and Erikson 2011; Gilens 2012) and the playing field of orga-

[3]There is less research on the impact of unions on preference formation, but see Ahlquist et al. (2014); Kim and Margalit (2016).

[4]For a review of the large, interdisciplinary literature on union effects, see Ahlquist (2017).

[5]Some studies of local policymaking find that union mobilization may be linked to outcomes that benefit their narrow (not generally poor) membership at the expense of the larger population (Anzia 2011, 2012). Existing studies on union effects on roll-call votes are more difficult to interpret in this respect (Becher et al. 2018; Box-Steffensmeier et al. 1997; Freeman and Medoff 1984), as they do not measure constituent preferences.

[6]To the best of our knowledge, we are only aware of two previous studies: Flavin (2016) examines cross-national variation between states (Flavin 2016) and Ellis (2013) legislative votes in the 110th House (Ellis 2013), relying on a survey-based union measure. Neither study explicitly addresses the alternative explanations considered in this paper. In addition, we study unequal responsiveness in the context of a large, exogenous economic shock.

nized interest is clearly skewed against the less-affluent (Schlozman et al. 2012). This is also an intriguing setting from a research design perspective, since it covers a large amount of district-level variation in union organization within a single-country. We focus on members of the House of Representatives during 109-112th Congress (2005-2012) because this allows us to capture within-state variation in union strength as well as within-district variation in preference polarization by income across policy issues, thus providing important leverage to rule out alternative explanations, including those mentioned above. In this setting, we have assembled an extensive dataset combining information on local unions extracted from more than 350,000 administrative records and district-level policy preferences of citizens stratified by income across 23 issues matched to corresponding roll-call votes. In order to get good estimates of political preference by income, we combine high quality income data from the American Community Survey (ACS), run by the Census Bureau, with the multiple waves of the Cooperative Congressional Election Study (CCES), which ask a large number of respondents (about 278,000) about their support for various bills voted on in the House. We estimate income percentiles from the ACS and use them to define the cut-offs for income groups in the CCES, from which we then calculate policy preferences by income group for each district and issue. To measure the district-level strength of unions, we use mandatory reports (so-called LM forms) filed by local unions to the Department of Labor. This largely neglected administrative data source allows us to construct fine-grained measures of union membership at the district level.[7] Our measurement strategy overcomes major limitations of standard survey data used to measure union strength.[8]

Our empirical analysis traces the legislative responsiveness of House members to the preferences of different income groups in their constituency conditional on district-level union strength. Given our research design, the analysis can can accommodate district and roll-call fixed effects. This rules out a large set of alternative explanations. Furthermore, we test whether the equalizing effect of unions is robust to alternative state-level and district-level moderators.

[7] As explained later, filing LM forms is a legal requirement for most unions, non-submission and incorrect submissions are penalized, and the Department of Labor conducts regular audits.

[8] The Current Population Survey is the most widely used source of union membership (Hirsch et al. 2001), but it provides no district identifiers and given its sample design, the literature on effects of unions on legislative voting has mostly focused on the state level. It is also likely to suffer from over-reporting. While the (pooled) CCES provides a fairly large sample, union membership, as a percentage of the population, is usually in the single digits and sometimes a rare event. This makes it difficult to get reliable estimates even with a few hundred respondents. For a review of data limitations in the study of (American) unions, see Southworth and Stepan-Norris (2009).

Our analysis reveals that district-level union membership dampens unequal responsiveness by national legislators. In line with previous research, House members are significantly less responsive to low-income constituents than to affluent ones. However, this gap in responsiveness is much smaller where unions are stronger, and it disappears where union members are fairly numerous (though far below levels known from countries with high union density). We show that the dramatic moderating effect of unions is not an artifact of state-level policies or state fixed effects (capturing institutions, history or culture). Neither is it driven by district-level socio-economic factors like education, race, gender, median income or employment structure. Based on additional data on unionization attempts we have collected from the National Labor Relations Board as well as county-level data on religious organizations, we can also rule out the possibility that it presents a general capacity of workers to organize (or be organized), based on social capital or some other driver of associational life. Finally, we also allow all of these confounds to be non-linear.

In addition, we provide evidence that that unions moderate how members respond to constituents in the context of a large economic shock that is exogenous to unionization. Since the late 1990s, the US and other developed economies have seen a dramatic and unexpected increase in important competition from developing countries, especially China, which became the world's factory. Economists have demonstrated that this "China shock" has lead to adverse effects on labor markets concentrated geographically in trade-exposed areas: declining manufacturing employment, higher unemployment, lower labor force participation, lower wages, and lower average household income (Autor et al. 2013, 2016). Crucially, short-term labor mobility was limited and wage losses are concentrated in the lower part of the income distribution. In this context, we find that the pre-existing strength of unions (again) improves the responsiveness of legislators to those especially hit hard by the shock. This finding contributes to a growing literature on the political consequences of large trade shocks (Autor et al. 2016; Feigenbaum and Hall 2015).

Altogether, our results imply that unequal responsiveness is not an unavoidable feature of democratic capitalism. The results are especially striking given that recent cross-national studies have found consistent patterns of unequal representation across different political institutions (Bartels 2017; Lupu and Warner 2017). In contrast, we find considerable heterogeneity in differential responsiveness across districts based on what may be thought of a fundamental economic institution. Against considerable scholarly skepticism, the moderating effect of unions uncovered in our analysis is large enough to swing key votes in Congress that concern the well-being of a very large population. That said, our results support the view that political efforts to (further) weaken unions, as evidenced in recent reforms in states like

Michigan and Wisconsin, are, if anything, likely to exacerbate unequal responsiveness in representation. They may also explain why unions are (still) under attack.

II. Moderating biased responsiveness in Congress?

In an era of polarized politics and high income inequality, are there institutions or organizations that reduce inequalities in political responsiveness by elected representatives? While few studies have directly assessed the impact of labor unions on unequal responsiveness in Congress or elsewhere, various strands of scholarship in political science and related fields suggest that labor unions are one of the few mass membership organization that provide collective voice to lower income persons in the political arena, with potentially important consequences for political representation (Ahlquist and Levy 2013; Bartels 2016; Freeman and Medoff 1984; Schlozman et al. 2012). Consistent with a central premise of this collective voice perspective, in the contemporary American national political arena unions tend to take positions favored by less affluent citizens. For instance, comparing public positions of national unions with mass policy preferences across several hundred policy issues, Gilens (2012: 154-161) finds that unions' positions are most strongly correlated with the preferences of the less well-off (also see Hacker and Pierson 2010; Schlozman et al. 2015).[9] Similarly, based on the *Washington Representation Study* Schlozman et al. (2012: 87) conclude that unions are one of the few organizations in national politics "that advocate on behalf of the economic interest of workers who are not professionals or managers."

However, shared preferences between the less well-off and organized labor are by no means sufficient to alter inequalities in political representation in national politics. This requires an effective political transmission mechanism. To guide the empirical analysis, we sketch key elements in a framework of union organization and political responsiveness in Congress.

Labor unions are organizations that are formed to bargain collectively on behalf of their members with employers over wages and conditions, and unions are created at the local (i.e., establishment) level (Freeman and Medoff 1984). Once formed, unions may (and often do) enter the political arena. The ability of unions to increase the political participation – broadly defined to include voting, contacting officials, attending rallies, town halls or demonstrations, and making contributions – and the political know-how of low- and middle-income citizens

[9] This fact is consistent with the argument that organized labor fosters norms of solidarity and support for the less well-off, through leadership (Ahlquist and Levy 2013; Kim and Margalit 2016) or social interactions among members (Berelson et al. 1954).

is often considered to be the key channel of political influence; importantly, unions may also increase participation among non-members with similar policy preferences through get-out-the-vote campaigns and social networks (Leighley and Nagler 2007; Rosenfeld 2014; Schlozman et al. 2012). Making contributions to favored candidates and campaigns complements the ability of unions to communicate with and mobilize members or provide campaign volunteers. Unions are among the leading contributors to political action committees (PAC), accounting for a quarter of total PAC spending in 2009 (Schlozman et al. 2012: ch. 14). In contrast to corporations and business organizations, union contributions "represent the aggregation of a large number of small individual donations" (Schlozman et al. 2012: 428).[10]

Taken together, the credible threat of political mobilization can affect policy decisions by representatives in two general ways. First, they may shape who is elected in a given electoral district. If politicians are not exchangeable, because differences in preferences and beliefs, political selection is important. In an age of elite polarization (McCarty et al. 2006), the partisan identity of a representative is often crucial for determining legislative voting (Bartels 2016; Lee et al. 2004). Since the New Deal era, unions and union members have by and large allied with the Democratic Party, given its stronger support for many of their broader policy demands (Lichtenstein 2013; Schlozman et al. 2015). Political selection may also concern other political characteristics of representatives, such as their class background or race (Butler 2014; Carnes 2013).

Second, unions' mobilization potential can shape the incentives and information of elected representatives, beyond their partisan affiliation and fixed personal traits. Policymakers' rational anticipation of public reactions plays a central role in theories of accountability and dynamic responsiveness (Arnold 1990; Stimson et al. 1995). While many individual legislative votes do not affect the reelection prospects of representatives, on potentially salient votes they can face hard choices between party as well as competing constituency preferences. On international trade agreements, for instance, Democratic representatives have faced cross-pressures between a more skeptical stance of unions and low-income opinion compared to their party (Box-Steffensmeier et al. 1997). On stimulus spending in the wake of the financial crisis, for example, Republican representatives confronted opposing pressures from less well-off constituents and partisan ideology (Mian et al. 2010).

Theories of representation also emphasize that information about constituents is essential for congressional action. Members of Congress face hundreds of voting decisions in each term, and it would be unrealistic to assume that they, especially House members, have ac-

[10]While evidence on the direct of contributions on legislative behavior is mixed, recent field-experimental results indicate that contributions help to provide access to legislators (Kalla and Broockman 2016).

cess to reliable, unbiased polling data on constituency preferences on all the issues they face (Arnold 1990; Miller and Stokes 1963). This is simply too costly. Instead, representatives – with the help of their staffers – rely on alternative methods to assess public opinion, including constituent correspondence, town halls, contacts with community leaders, or local interest groups (Hertel-Fernandez et al. 2017; Miler 2007). In this limited information context, the density of local unions can enhance the visibility and perception of constituent preferences. Underscoring the relevance of the informational problem, recent research has documented that, on average, legislators and their staff systematically mis-estimate constituent opinions (Broockman and Skovron 2018; Hertel-Fernandez et al. 2017).[11] Broockman and Skovron (2018) find that perceptions of state legislators are biased in a conservative direction. Similarly, Hertel-Fernandez et al. (2017) find that congressional staffers are biased toward conservative and business interest groups as well as personal tastes. They also present evidence that the bias is smaller in districts with higher union membership, bolstering the relevance of the informational channel of influence.[12]

Some work on unequal representation has found that legislators' biases in political responsiveness "are *not* primarily due to differences between affluent and poor constituents in turnout, political knowledge, or direct contact with elected officials" (Bartels 2016: 263). Our framework is consistent with this important result. Political contributions and elite-level information are additional mechanisms of influence, and we present direct evidence on the relevance of union contributions. Following seminal theories of congressional action (Arnold 1990; Miller and Stokes 1963), moreover, our argument emphasizes that the strength of local union underpins a credible threat of mobilization. Anticipating mobilizing efforts by unions, certain potential candidate may not enter into the race and, once elected, career-oriented politicians do not always require a full mobilization effort as long as the mobilization capacity is visible. As a result, one may not expect observed political participation to account for most of the responsiveness bias even if it is a key mechanism.

In sum, our argument implies that the district-level strength of labor unions dampens unequal responsiveness by members of Congress. While we know from previous work that politicians are considerably more responsive to the preferences of the affluent than those of the less well off, this bias should be reduced in districts with relatively higher union mem-

[11] As a caveat, note that neither study focuses on unequal responsiveness to different income groups.

[12] There is also some direct evidence that politicians respond when provided with more accurate opinion data (Butler and Nickerson 2011). Behavioral biases may lead politicians to discount constituent preferences they disagree with (Butler and Dynes 2016).

bership. Substantively, it is important to assess how far the presence of unions can move responsiveness toward the ideal of political equality.

That said, the literature is by no means unequivocal on the issue. In the end, unions may have become too weak to matter much (Gilens 2012: 175; Hacker and Pierson 2010: 143), or they end up fighting in the political arena for very specialized interests, which do not generally overlap with those of lower income persons, as is maintained by a large strand of union scholarship in economics (Freeman and Medoff 1984). Moreover, recent cross-national studies of unequal representation have unearthed the striking finding that similar income biases in representation exist across different electoral systems and other constitutional structures that are often held to be crucial for representation (Bartels 2017; Lupu and Warner 2017). This has led to a call for more fine-grained analyses into when representation may not be so starkly unequal (Lupu and Warner 2017). Any effort to test the relevance of unions for unequal representation confronts major challenges of measurement and causal interpretation. The extensive dataset we have complied – drawing on original administrative data on local unions as well as large public opinion surveys and legislative votes – allows us to address these issues to an extent previously impossible.

III. Data and empirical strategy

We have created a panel of legislators' roll-call votes matched to income-specific district preferences, and a district-level measure of union membership. Our empirical strategy is built on two pillars: district fixed effects and interactive controls. The fact that we have several roll-calls within a given congressional district allows us to specify a model with district fixed effects, which capture unobservable characteristics of districts (and states) such as historical legacies or the strength of partisan organization.[13] However, like every fixed effects model, this assumes that district-level confounders are constant over roll-calls. It does not account for confounders that affect *changes* in representation, and, in the worst case, change simultaneously with union membership. To address this, we allow a rich set of district characteristics to moderate the link between income groups and legislators' voting behavior. This amounts to estimating models including interactions between observed district characteristics and group preferences. In our most flexible specification we allow these to be non-linear.

[13] We also account for unobservables that impact all votes for a given bill simultaneously by including roll-call fixed effects.

We construct the data required to implement these models in three steps. First, we match CCES roll-call items to actual roll-call votes cast in the House of Representatives from the 109th to the 112th Congress. Second, we group about 278,000 CCES respondents into three income groups (based on externally established income thresholds), and estimate each group's preferred policy position on each roll-call vote separately for each Congressional district. Third, we create a measure of district-level union membership from administrative records. The resulting data-set is in district–legislator–roll-call form and contains income-group-specific preferences for each roll-call vote and district-level union membership. Our main analysis focuses on the same apportionment period, which generally holds district boundaries fixed (we show that the results are robust to cases of mid-period redistricting). As one robustness test, we estimate a two-period model (before and after re-apportionment) by adding data for the 113th Congress.

III.A. Roll-calls and constituency preferences by income group

The CCES is an ideal starting point for our analysis, since it is a nationally representative sample, includes more than thirty roll-call questions, and provides us with a large enough sample size to decompose income-group preferences by district. It addresses several data concerns that plagued initial research on unequal responsiveness in Congress (Bhatti and Erikson 2011).

The roll-calls included in the CCES concern key votes as identified by Congressional Quarterly and the Washington Post. They cover a broad range of issues (Ansolabehere and Jones 2010). Respondents are presented with the key wording of the bill (as used on the floor and in media reports) and are then asked to cast their own vote: "What about you? If you were faced with this decision would you vote for, against or not sure?" We match 23 roll-call items in the CCES to roll-call votes cast in the House of the 109th to 112th Congress. These cover important legislative decisions, such as the Affordable Care Act and attempts to repeal it, minimum wage increase, the ratification of the Central America Free Trade Agreement, and the Lilly Ledbetter Fair Pay Act. Contrary to widely usual agree–disagree survey measures of issue preferences, matched roll-call votes provide us with unequivocal evidence of policy congruence between respondent and legislator (Jessee 2009, Ansolabehere and Jones 2010: 585). Table A.1 in the appendix lists all matched CCES items and House bills included in our estimation sample.

Income groups Using our matched CCES–House roll-call dataset, we now calculate the support of a district's constituents' for each roll-call. Following previous work in the representation literature (Bartels 2008, 2016), we place survey respondents into three mutually exclusive and exhaustive income groups (low, middle, high), based on their relative position in the income distribution.

Establishing the relative position of a respondent in the income distribution entails two key issues: which income cutoffs to use, and at at which geographical level to calculate them. To assess the district-level representation of different income groups, it is desirable to measure public preferences at the district level and to calculate income bands with similar support, to avoid confounding the effects of income and group size. With this in mind, we use the 33rd percentile of the income distribution as the upper cutoff for the low-income group and 67th percentile as the lower cutoff for the high-income group. This means that the middle-income group refers to persons between the 33rd and the 67th percentile. We specify these income cutoffs by state and year, in order to account for the well-documented substantial differences in both average income and income inequality between US states (e.g., Frank 2009).[14]Our calculation uses the American Community Survey 1-year files, since it provides the highest quality household survey data with a high degree of population coverage, a non-discretized income variable from which we can estimate our percentiles of interest, a sampling design which allows us to subset the sample by state, and a sample of sufficient size (about 10.8 million households). Table A.2 in the appendix shows the distribution of income-group cutoffs. On average, our chosen cutoffs are close to those used in the established literature. The mean of our state-specific low-income cutoffs in 2012 is around $35,000, while Larry Bartels uses $40,000 (Bartels 2016: 240); our mean high-income cutoff is around $75,000, where Bartels employs a threshold of $80,000. However, beyond these averages lies some substantive variation. In some states, the low income cutoff is substantially lower, such as in Louisiana or Alabama, where the 33rd percentile is around $26,000. Similarly, some high income cutoffs reach well over $90,000, such as in Massachusetts and Connecticut.

With these cutoffs in hand, we place individuals into their respective income group. The CCES asks respondents to place their family's total household income into 14 income bins.[15] We transform this discretized measure of income into a continuous one using a nonparametric midpoint Pareto estimator (Henson 1967). It replaces each bin with its midpoint (e.g.,

[14]We chose states as geographical unit, since it is difficult to obtain reliable information on the 33rd and 67th income percentile on the district level. To account for any resulting differences in group size at the district level, we control for the size of the middle income group.

[15]The exact question wording is: "Thinking back over the last year, what was your family's annual income?" The obvious issue here is that it is not clear which income concept this refers to (or, rather, which on the respondent employs). In line with the wording used in many other US surveys, we interpret it as referring to market income.

the third category \$20,000 to \$29,999 gets assigned \$25,000), while the value for the final, open-ended, bin is imputed from a Pareto distribution (e.g., Kopczuk et al. 2010). Using midpoints has been recognized for some time as an appropriate way to create scores for income categories (without making explicit distributional modeling assumptions). They have been used extensively, for example, in the American politics literature analyzing General Social Survey (GSS) data (Hout 2004).

Group policy preferences We now turn to calculating the policy preferences for each of the three income groups in each district. For each matched roll-call, we estimate a linear probability model with separate coefficients for each income group. All coefficients are allowed to be state-specific. From this model we calculate the expected value of each roll-call vote for each of 278,734 CCES respondents. By aggregating these expected values by district and income group, we arrive at our district-level measures of low-, mid-, and high-income citizens' policy preferences, which we denote by $\theta^l, \theta^m, \theta^h$, respectively.

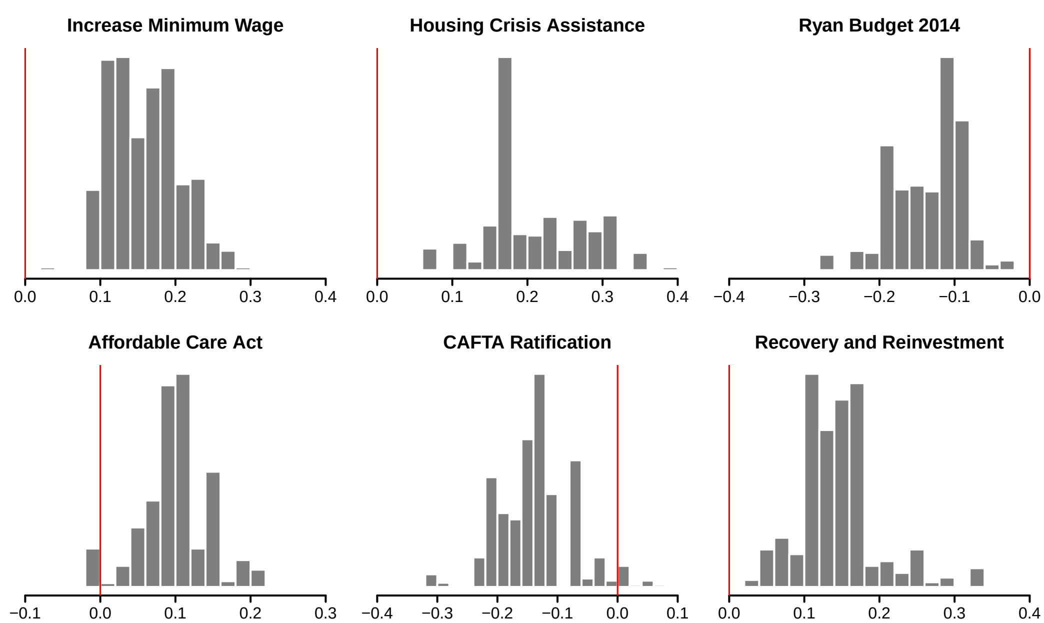

FIGURE I
Distribution of high-income – low-income preference gap for 6 matched roll call-votes.

Our data shows considerable variation in the distance of the policy preferences of those at the top and those at the bottom, which we illustrate in Figure I. It plots histograms of the difference between high and low income preferences ($\theta^h - \theta^l$) in Congressional districts for six selected roll-calls. For salient issues, such as increasing the minimum wage (the Fair Minimum Wage Act) or housing crisis assistance (the Housing and Economic Recovery

Act) low-income citizens are more supportive than their high-income counterparts in each and every district. On other issues, such as the ratification of CAFTA, the difference between low- and high-income citizens is smaller in some states, while still large in others. We will employ this variation over both roll-calls and districts to estimate legislators' differential responsiveness to changes in policy preferences of different income groups, and how it might be moderated by union strength.

III.B. Union density

To overcome limits of survey-based measures of union membership at the level of congressional districts, we calculate district-level union membership for the U.S. House of Representatives using administrative records. Based on the Labor-Management Reporting and Disclosure Act (LMRDA) of 1959, unions have to file mandatory yearly reports with Office of Labor-Management Standards (OLMS)[16]. A mandatory port of each report is the number of members a union has. Failure to report, or reporting falsified information, is made a criminal offense under the LMRDA, and reports filed by unions are audited by the OLMS. This makes LM forms a reliable source of information on unions and their members.

Using LM forms provides two important advantages over using measures derived from surveys. First, mandatory administrative filings are likely more reliable than population surveys, which often suffer from over-reporting (e.g., Southworth and Stepan-Norris 2009: 311; Card 1996) and unit-nonresponse.Second, and most importantly, they allow us to estimate union membership numbers for smaller geographical units, which are usually unavailable in population surveys (to protect respondents' confidentiality) or only covered with insufficient sample sizes.[17]

We created a database of almost 30,000 local unions based on 358,051 individual reports, which we cleaned, validated, geocoded, and matched to congressional districts. The number of union members in each congressional district can then be readily obtained as the sum of all reported union members. Figure II shows the distribution of union membership (as percentage of the total population) in congressional districts averaged for the 109th to 112th

[16]The Civil Service Reform Act (CSRA) of 1978 introduced a similarly comprehensive system of reporting for federal unions. For a summary of LMRDA's legislative history, see Aaron (1960)

[17]Despite fairly large overall number of respondents in CCES, union membership is a relatively rare event at the district level. The most prominent data set on union membership, compiled by Hirsch et al. (2001) based on the Current Population Survey (CPS), provides estimates for states and metropolitan statistical areas based on the CPS. Congressional district identifiers are not available.

Congress. It demonstrates that there is substantial variation in unionization between electoral districts even within states, which would be ignored by a state-level analysis.

A potential drawback of using LM forms is that some unions are exempt from filing requirements. Each and every private sector union is required to submit a report, but under some specific conditions public sector unions are exempt. Thus, while unions representing postal or federal employees are covered, unions that exclusively represent state, county, or municipal government employees are exempt. However, even they have to file if at least one of their members is a private sector employee. In practice, this leads to almost complete coverage, as during the latter part of the twentieth century unions are increasingly organizing workers across different sectors and occupations (Lichtenstein 2013: 249).[18]

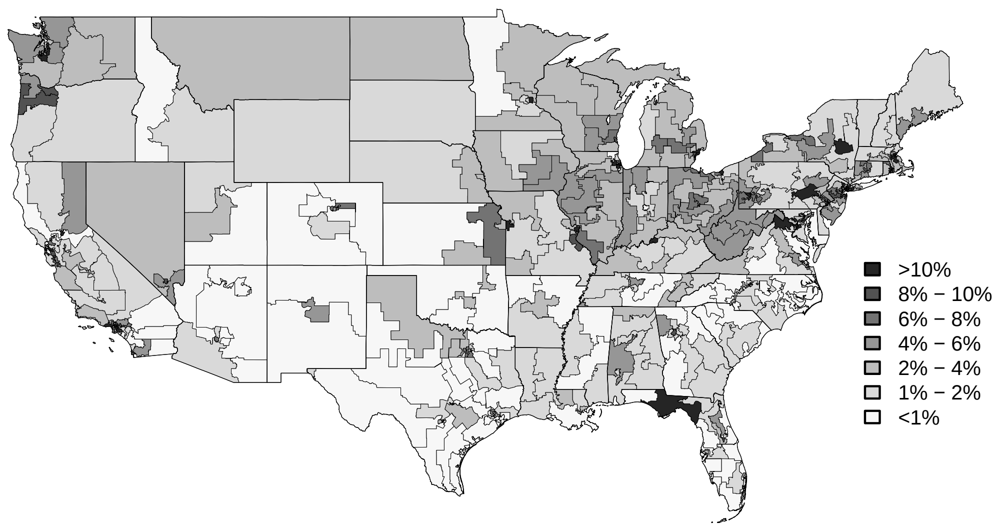

FIGURE II
Union membership in congressional districts

[18]While there is no "gold standard" of accurate union membership numbers, Becher et al. (2018) compare LM form data against the widely used CPS-based measure of state-level union density and find that the two measures agree to a large extent (their correlation, averaged over all years, is 0.86). On average unionization levels based on estimates from the CPS are 1.9 percentage points higher than counts of members from LM forms. This difference is consistent with some degree of over-reporting, induced by CPS's broad question wording (Southworth and Stepan-Norris 2009: 311). It can also be interpreted as an upper bound for the non-coverage of some public sector unions in our data, confirming that LM forms provide a rather comprehensive accounting of unions.

III.C. Statistical specifications

For each roll-call vote j, we have measured preferences of low and high income citizens in a given district, $(\theta^l_{jd}, \theta^h_{jd})$. We focus on the contrast of low versus high income groups, but provide a robustness test that shows that including the preferences of the middle class (as well as their relative population size) does not change our core results. For each district, the level of (logged) union membership is denoted by U_d. We specify relevant confounders in X_d. Depending on the particular specification (discussed in the next section) these will include various district characteristics, measures of historical state union policies and state fixed effects, measures for the capability of districts' workers to organize collective action, as well as non-linear transformation of these. For ease of interpretation, we have scaled all inputs to have mean zero and unit standard deviation. Our model for Congressional representatives' voting behavior is the following linear probability specification:

$$\begin{aligned} y_{ijd} = & \alpha_j + \delta_d + \mu^l \theta^l_{jd} + \mu^h \theta^h_{jd} + \eta^l (U_d \times \theta^l_{jd}) + \eta^h (U_d \times \theta^h_{jd}) + \\ & \beta^l (X_d \times \theta^l_{jd}) + \beta^h (X_d \times \theta^h_{jd}) + \varepsilon_{ijd} \end{aligned}$$

The key terms here are the interactions between union membership and the respective preferences of the affluent and the poor, $U_d \theta^h_{jd}$ and $U_d \theta^l_{jd}$. Thus, when η^l and η^h are zero the group-specific preference coefficients μ^l and μ^h indicate the change in the probability of legislators casting a supportive vote induced by a standard deviation change in the respective preferences of the poor and the affluent. The coefficient η^l indicates the marginal effect of a standard deviation change in logged union density on the responsiveness of legislators' votes to the preferences of the poor. The corresponding marginal effect for the affluent is given by η^h. Our theoretical expectation is that $\mu^l > 0$ and $\mu^h < 0$.

In order to mitigate the influence of unobserved confounders affecting legislators' voting behavior, we account for time-constant unobservables on the district-level by including district fixed effects, δ_d, and we account for unobserved (or unmeasured) characteristics of each bill by including roll-call fixed effects, λ_j.[19] Despite this, one may be worried that changes in responsiveness attributed to unions are spurious. To provide a stricter test of the moderating effect of unions, we include the interactions between observed controls and group preferences $X_d \theta^l_{jd}$ and $X_d \theta^h_{jd}$. As discussed before, this includes state-level fixed effects and district-level observables. Finally, ε_{ijd} are white-noise residuals assumed independent

[19] Note that non-interacted ("main") effects of district-level union membership and covariates (which vary between districts, but are constant over roll calls) are absorbed in δ_d. This does not imply the assumption that they are zero. We do not need to identify their values to assess the moderating effect of unions.

of covariates. In all specifications presented below, we account for heteroscedasticity and arbitrary within-district correlations when calculating standard errors (Cameron and Miller 2015: 324).

IV. RESULTS

Before turning to discussing the impact of union organization, we want to give a sense of the overall picture of legislators' responsiveness emerging from our data. Estimating a model as described above with district fixed effects but no further preferences-confounder interactions and without accounting for local union organization (setting β^l, β^h and η^l, η^h to zero), we find a clear gap in the responsiveness of legislators to the preferences of low- versus high-income individuals. A standard deviation increase in the preferences of the affluent is linked to a 22 percentage point increase in the probability of legislators to cast a corresponding vote. In contrast, a standard deviation increase in the preferences of the poor induces a change in legislators' behavior three times smaller (at 7 percentage points). This gap is significant in both the substantive and statistical sense.

IV.A. The role of unions

Table I presents results from six specifications examining the responsiveness of legislators in the House of Representatives to the district-level preferences of low-income and high-income constituents, respectively, conditional on varying levels of local union organization.

In specification (1) we estimate our model with district fixed effects but no further preferences-confounder interactions (setting β^l and β^h to zero). We find that a standard deviation increase in district union membership increases legislators' responsiveness to the poor by almost 15 (± 2) percentage points, while at the same time decreasing the advantage in responsiveness enjoyed by the affluent by about 8 (± 1) percentage points. To give a more intuitive illustration of the magnitude of the impact of different levels of district union organization, we compute the marginal effects of low-income and high-income preferences on legislative votes across the whole range of union membership observed in the data. Figure III shows these calculations together with 90 and 95% confidence intervals (based on specification 1 in Table I). It shows that while legislators' responsiveness to low-income persons increases as unionization increases, legislators' responsiveness to high-income persons declines. A sizable part of the decrease in the poor-affluent responsiveness gap is due to the increasing responsiveness of legislators to the preferences of low income constituents.

Even after accounting for district fixed effects, however, these results are still vulnerable to factors that interact with group preferences to produce changes in legislators' voting behavior. Following accounts of winner-take-all politics (Hacker and Pierson 2010), for instance, one plausible alternative explanation is that the moderating effect we have been ascribing to unions mostly reflects that state governments choose policies that determine the ability of unions to organize, or some broader bundle of (unobserved) state level policies and institutions that conditions the responsiveness of legislators. In line with this concern, studies have shown that right-to-work and collective bargaining laws regulating the formation and management of unions in the private or public sector have clear political effects on turnout and partisan vote shares (Feigenbaum et al. 2018; Flavin and Hartney 2015). In specification (2) we therefore add two measures of historical state union policy, the share of years with right-to-work legislation and collective bargaining agreements. These enter X_d and are interacted with group preferences. In specification (3) we go one step further and allow for *any* state-level characteristic to moderate the marginal effect of income group preferences on legislators vote choice by including state-specific constants in X_d which are interacted with group preferences. Both results show that accounting for these possible confounders does not change our core picture of the role of local union organization: where local unions are stronger (in terms of membership numbers) the responsiveness gap between the affluent and the poor is reduced.

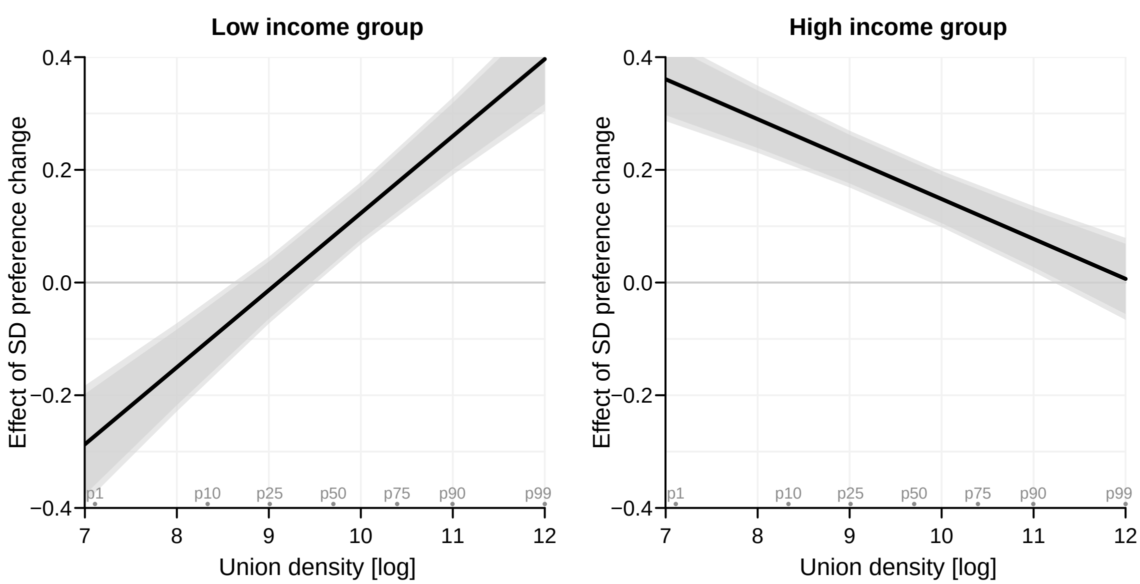

FIGURE III
Marginal effects of low- and high-income group preferences conditional on union density

TABLE I

Union density and representation. Marginal effect of standard deviation increase in union membership on marginal effect of preferences of low and high income groups on legislator vote.

	(1)	(2)	(3)	(4)	(5)	(6)
District union membership						
× Preferences Poor	0.145	0.138	0.159	0.082	0.101	0.087
	(0.018)	(0.021)	(0.024)	(0.024)	(0.017)	(0.018)
× Preferences Affluent	−0.075	−0.075	−0.085	−0.043	−0.056	−0.071
	(0.012)	(0.014)	(0.016)	(0.016)	(0.011)	(0.011)
District fixed effects	✓	✓	✓	✓	✓	✓
Group preferences						
× union policy		✓				✓
× state constants			✓			
× organization capacity				✓		✓
× district covariates					✓	✓

Note: N=13,101. $N_d = 534$. 23 roll-call votes from 109th to 112th Congress. Linear probability models with standard errors robust to arbitrary within-district correlation and heteroscedasticity. All models include roll-call fixed effects. Entries are marginal effects of union density, η^l and η^h. Specifications (2) to (5) include coefficients for interaction (β^l, β^h) of group preferences with state- or district-level confounders . Specification (2) includes two measures of historical state union policymaking, the share of years with right-to-work legislation and collective bargaining agreements. (3) interacts group preferences with state fixed effects as a proxy for all state-level confounders (constant over bills). (4) includes a measure of district-level capacity to organize collective action, the the number of NLRB union certification elections. (5) includes a large set of district-level characteristics (population size, share of female, black, Hispanic, BA degree, employed in manufacturing, and median household income). Specification (6) includes all of the previously described measured variables as well as their nonlinear transforms (up to a third order polynomial). We select among this high-dimensional vector of controls using the post-double-selection LASSO (Chernozhukov et al. 2015).

A more subtle problem not ruled out so far concerns a form simultaneity bias at the district level. There may be district-level factors shaping both the propensity to be a union member and to be politically active. If less-affluent individuals with a higher capacity to organize and to solve collective action problems cluster (or sort themselves into) specific districts, our estimates of the marginal impact of district union membership on responsiveness will be overly optimistic. Such a propensity may reflect social capital (Putnam 1993), critical historical junctures in labor organizations (Ahlquist and Levy 2013), social networks, or something else.

To tackle this problem, we gathered data that provide a proxy for the capacity of a districts' workers to organize collective action: the average number of union certification elections in a district. Certification elections (conducted by the National Labor Relations Board) are a useful proxy, since holding an election requires overcoming a costly organizational hurdle: at least 30 percent of employees have to sign an authorization cards stating that they want to be represented by a union. A union organizer also has a non-trivial probability of being (illegally) fired by her employer (Schmitt and Zipperer 2007). We use the NLRB's database to extract all attempts to certify (or de-certify) a local union.[20] We geocode each individual case report and locate it in a congressional district. We then use the (logged) average number of cases in a district over the last seven years to proxy organizational potential (again, interacted with group preferences) in specification (4).

Not surprisingly, we find that organizational capacity moderates of responsiveness. In districts where workers are better at organizing, electoral representatives are more responsive to changes in the preferences of the poor. However, as specification (4) in Table I shows, even after accounting for this confounder we find that local union membership impacts responsiveness: a standard deviation increase in the (logged) number of union members still increases legislators' responsiveness to the preferences of the poor by 8 (± 2) percentage points, while lowering their responsiveness to the preferences of the affluent. This rules out the interpretation that the moderating effect of unions is merely an artifact of a broader propensity to overcome collective action problems. Actual union organizations matter.

Furthermore, we find very similar results in specification (5) where we allow for a large number of district-level covariates to interact with group-level responsiveness. The vector of covariates includes population size, race (share of African American and Hispanics), education (share with BA or higher), the share of the working population employed in manufacturing, and median household income.[21]

In our final specification, we aim to allow for a complex combination of all previously mentioned covariates (including higher order interactions) while also relaxing the functional form assumed for all confounders. So far, we have assumed them to enter linearly. In specification (6) we allow covariates to enter as polynomials (up to third order) and interact with other covariates. Since this produces a high-dimensional vector of controls (relative to the size of our within-district data set), we employ a post double-selection LASSO estimator (Chernozhukov et al. 2015) to select an optimal set of controls, which is then interacted with group-level preferences and entered into our model. The result of specification (6) shows that even when allowing for much more flexible forms of confounding, a standard deviation increase in union membership increases responsiveness to low income constituents by almost 9 (± 2) percentage points.

[20] The vote concerns a bargaining unit; the average size is 25. There are about 2200 elections each year. Elections do not include voluntary card check recognition by employers, which is the exception (Budd 2018: 199).

[21] Descriptive statistics for all covariates included in our analysis are given in Appendix Table A.3.

IV.B. Additional specification

In this section, we describe a number of alternative specifications exploring the sensitivity of our results and further empirical implications.

Including middle class Our main models only include the policy preferences of low and high income constituents. In Specification (1) in panel (A) we include the policy preferences of the middle income group and in (2) we also account for the fact that the relative size of the middle class in a district might shape legislators' voting behavior and thus affect the representation gap between low- and high-income constituents. We account for the latter by including the number of constituents in each district classified as middle income.[22] Our results show that including the middle class has little effect on the estimated impact of union strength on the representation of income groups; our findings are now slightly more pronounced. An increase in union strength increases representation of low-income constituents, while it reduces the representational advantage held by high-income constituents. The results from this saturated specification rule out the possibility that the responsiveness of legislators to low-income persons merely reflects the responsiveness to the middle-income group, which is the favored in median voter models.

Social capital In our main model already account for one key unobservable: the capacity of a districts' workforce to organize collective action. But the question remains if our results are driven by broader societal characteristics. One might be concerned that some districts are characterized by citizens that are more likely to be involved in social and political life. Districts high in social capital will see individuals that are more likely to interact socially (in social clubs, sport organizations, or churches) where they learn how to solve collective action problems and find their political voice (Verba et al. 1995). Such districts might be high in union membership as well. To examine this possibility, we use a structural measure of social capital: the number of churches per inhabitant in a given district. We compute it by mapping county-level data from the Religious Congregations and Membership Study to congressional districts. For details, see appendix A.3. In specification (3), this measure enters our model in form of an interaction with district preferences, to account for the fact that legislator responsiveness is larger in districts with more social capital. Our results indicate that accounting for this confounder slightly dampens our estimates, but it does little to change our substantive finding.

[22] One issue when using all three income groups is large correlated measurement error. This is less of a concern here due to the large sample size of the CCES. On average, our estimates are almost 30 times larger than their corresponding standard errors.

TABLE II
Additional specifications.

	Low income		High income	
A: Robustness tests				
(1) Middle class preferences	0.101	(0.020)	−0.055	(0.014)
(2) Size of middle class	0.101	(0.017)	−0.056	(0.011)
(3) Social capital	0.095	(0.018)	−0.053	(0.012)
B: Bill ideology				
(5) Bill-type × district FE	0.052	(0.011)	−0.024	(0.007)
C: Union heterogeneity				
(7a) Public union	0.113	(0.020)	−0.063	(0.013)
(7b) Non-public union	0.083	(0.019)	−0.046	(0.012)
D: Party heterogeneity				
(8a) Democrat	0.034	(0.010)	−0.015	(0.008)
(8b) Republican	0.024	(0.010)	−0.028	(0.009)

Note: Based on specification (5) in Table I. Entries are marginal effects of standard deviation increase in union membership on marginal effect of preferences of low and high income groups on legislator vote. Cluster-robust standard errors in parentheses. Specification (1) includes preferences of the middle income group. Specifications (2) and (3) include size of the middle class and a measure of district-level social capital (the number of churches per inhabitant, N=12,742) interacted with θ_{jd}. (4) excludes cases with within-apportionment period redistricting (N=11,805). (5) Includes data from an additional apportionment period (the 113th Congress), the estimated model includes district×apportionment-period fixed effects (N=15,648). (6) is a specification test of the LPM using the trimmed estimator (Horrace and Oaxaca 2006) (N=5,928). Specification (7) includes party-specific baseline responsiveness and party-specific union shift in responsiveness.

District-bill ideology fixed effects Panel (B) allows the district fixed effects to vary by the ideological direction of the bill. Based on the partisan vote margin of the roll call vote, we define a dummy variable for conservative roll calls. This indicator is interacted with the district effects, effectively creating 2×435 fixed effects. One may argue that this is a more conservative approach. Its advantage is that the fixed effects are more exhaustive and may do a better job at dealing with unobservable political factors. On the other hand, it requires the use of information based on the voting outcome. In any case, the results from this more demanding specification confirm our results. An related approach (note shown) simply splits the sample into liberal and conservative bills. This also supports our findings.

Effect heterogeneity Panel (C) deals with an issue that has received short shrift so far: the role of public unions. As we have discussed in our data section, the LM forms on which we base our measure of union membership do not distinguish between private and public unions. However, recent research has stressed the particular characteristics of public unions and their political influence (e.g., Anzia and Moe 2015; Flavin and Hartney 2015). Thus one may ask whether our results depend on the type of union. Perhaps public sector unions are too narrow to mitigate unequal responsiveness? In order to calculate an approximate measure of a district's number of public union members, we identify likely public unions based on their name and create separate union membership counts for "public" and "non-public" unions.[23] Specification (7) shows that the marginal effect of districts' public union membership on the responsiveness of legislators to the preferences of the poor is sizable (and statistically different from zero). Notably, the coefficient on "non-public" unions remains consistent in sign and magnitude.

Panel (D) explores the heterogeneity of our estimates with respect to party. The specification we choose allows for differences in the impact of local union organization between Democratic and Republican legislators. More importantly, we also allow for party-specific baseline responsiveness, capturing the fact that legislators affiliated with the Democratic party are more responsive to low-income group preferences to begin with. It is important to keep in mind the shortcomings of such an analysis. Party membership of a district's legislator is itself likely to be endogenous to union strength: in districts where local unions are stronger Democratic candidates are more likely to be elected (Becher et al. 2018). Specification (8) in Table II shows the marginal impact of local union membership after accounting for party-specific baseline responsiveness. We find that a standard deviation increase in union membership adds a 3 percentage points increase to Democratic legislators' (already high) responsiveness. It also increases the responsiveness of Republican lawmakers (who start with a much lower level of responsiveness to the poor) while it decreases their responsiveness to the affluent.

Further robustness tests We also conducted a number of additional specification test. A two period analysis adds roll calls from the 113th Congress (and corresponding district-level union membership and covariates) and includes district$\times$period fixed effects. We also estimate our model excluding roll-calls casts in districts where some form of redistricting occurred at some point during the apportionment period (this amounts to excluding Georgia and Texas). A further technical test concerns our use of a linear probability model for binary left-hand-side variables. We employ the trimmed estimator suggested by Horrace and Oaxaca (2006). In all specification we find our results to be substantively unaltered.

[23] We select unions that are likely to contain public employees by using regular expressions containing terms such as "firefighters", "police", "county" or "public" employees. While this does not, of course, yield a precise classification of public unions, it captures the degree to which (likely) public employees are present in a given district.

IV.C. External shocks to district economy

A fundamental issue when trying to estimate the impact of district-level union organization on political representation in Congress is that unobserved district characteristics shape both a district's economic structure (and thus the distribution of incomes within a district) as well as the size of union membership. In this subsection we exploit a large economic shock that is likely exogenous to unionization but that substantially impacts the economic conditions of some districts. Since the late 1990s, the US and other developed economies have seen a dramatic and unexpected increase in important competition from developing countries, most notably China. Economists have demonstrated that this "China shock" has lead to adverse effects on labor markets concentrated geographically in trade-exposed areas: declining manufacturing employment, higher unemployment, lower labor force participation, and lower wages (for a summary, see Autor et al. 2016).[24] Autor and coauthors estimate that areas exposed to a unit increase in Chinese imports per worker see a reduction in average household income per adult of about $500 (Autor et al. 2013: 2150).

We use this unexpected economic shock to study the link between union strength and representation in districts particularly exposed to them. To do so we map 10-year equivalent changes in Chinese imports per worker during the 2000s from commuting zones as calculated by Autor et al. (2013) to congressional districts.[25] Figure IV plots the change in district-level exposure to Chinese imports (in percent) and illustrates the heterogeneity of the impact across districts. We also map an alternative measure of district-specific exposure, the comparative advantage (in terms of production and transportation costs) of China relative to the US.

Table III shows results from a series of models that examine how local union organization shapes representation in districts exposed to the China shock. All specifications include an interaction between income group preferences with beginning-of-period union density (set to the same year – 2000 – as the base year for the Chinese import exposure calculation). In specification (1) the degree of import exposure interacts with both the baseline responsiveness and union-preference terms. We then evaluate the marginal effect of a standard deviation change in union membership for districts exposed to import shocks at the 75th per-

[24] Crucially, short-term labor mobility was limited and wage losses are concentrated in the lower part of the income distribution.

[25] Commuting zones (CZ) are Census based "travel-to-work" areas constructed by aggregating counties. There are 741 commuting zones to which we map each congressional district. Some districts are completely covered by a single CZ, for others we assign spatial weights using dasymetric weighting (using the population distribution from Census Block data). Figure A.2 shows polygons for CZ and districts.

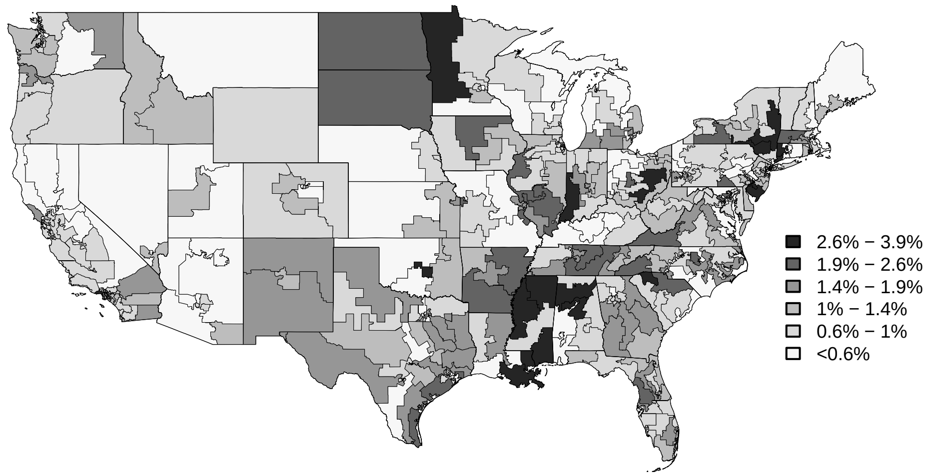

FIGURE IV
District exposure to Chinese imports (10-year equivalent changes, 2000s)

centile. To give a sense of the economic magnitude, these are districts where (in our sample) the median household income per adult dropped by about $813 due to increases in Chinese competition. In specification (2) we estimate the same model but instrument changes in Chinese imports using the instrument popularized by (Autor et al. 2013). As an alternative to the triple interaction with trade exposure, specification (3) instead splits the sample and focuses on those districts exposed to Chinese import growth above the 75th percentile. We repeat the same specifications for our measure of Chinese comparative advantage.

We find that among the districts exposed to large economic shocks those that had high levels of union membership saw an increase in responsiveness of legislators to preferences of their low income constituents. A standard deviation increase in union density is associated with a 13 ($\pm$2) percentage point increase in responsiveness. This results is slightly more marked when instrumenting trade exposure with trade exposure in other developed nations, and it even obtains when focusing on the much smaller split district sample, or when substituting a measure of change in Chinese comparative advantage. It is noteworthy that the decrease in the poor-affluent responsiveness gap is mostly due to the increase responsiveness of legislators to the preferences of low income constituents. The corresponding marginal decrease in responsiveness to preferences of the affluent is much smaller (in the comparative advantage specifications it is indistinguishable from zero).

TABLE III

Marginal effect of standard deviation increase in union membership on marginal effect of low and high income group preferences on legislator vote in districts exposed to large Chinese import shocks.

	Δ *Chinese imports*			Δ *comparative advantage*	
	OLS	2SLS	Split	OLS	Split
	(1)	(2)	(3)	(4)	(5)
Union density in 2000					
× Low income preferences	0.134	0.153	0.095	0.067	0.159
	(0.021)	(0.021)	(0.036)	(0.020)	(0.055)
× High income preferences	−0.055	−0.066	−0.023	−0.020	−0.054
	(0.014)	(0.014)	(0.025)	(0.014)	(0.039)

Note: Trade shock in (1)-(2) and (4)-(5) calculated at 75th percentile of import exposure distribution. Models (3) and (5) split the sample and use only districts above the 75th percentile of distribution of import shocks. All models include state and roll-call fixed effects and full set of district-level covariates. Cluster-robust standard errors. Δ *Chinese imports* refers to 10-year equivalent Chinese import growth per worker of CZ in 2000s matched to congressional districts (using spatial weighting by population). IV model instruments change in import growth in the US using changes in growth in 8 developed countries following Autor et al. (2013). Test statistics for weak identification (eigenvalue rank test) are all above critical levels. Δ *comparative advantage* refers to the comparative advantage in production or transportation costs of China relative to the US [per worker]. Calculated from residual of a Gravity model of trade (cf. Autor et al. 2013: Appendix B).

V. Conclusion

Dahl (1961) famously asked who governs in a polity where political rights are equally distributed among adult citizens but there are large inequalities in income and wealth that may be used as political resources. In the wake of rising income inequality in the United States and other advanced economies, scholars have identified the question of political inequality as one of the central challenges facing democracy in the twenty-first century. While the scientific debate is ongoing and some results are open to different interpretations (Erikson 2015), a growing number of studies has documented striking patterns of unequal responsiveness by income. When policy preferences diverge across income groups, legislators and public policy are biased toward the affluent at the expense of the middle-class and especially the poor (Bartels 2016; Ellis 2012; Gilens and Page 2014). Many recent works conclude by asking what factors may improve political representation of the economically disadvantaged.

We contribute to this body of research by analyzing whether labor unions serve as a collective voice institution that limit unequal representation in the House of Representatives. Against the wide-spread view that unions are either too weak or too narrow to mitigate political inequality in the national arena, we find that the district-level strength of unions is clearly linked to the responsiveness of legislators to different income groups. While legislators are more responsive to the preferences to the affluent than those of the poor on average, the representation gap is highly variable. It is much less pronounced in districts where union density is relatively higher (though not high by international comparison), which includes more than one-third of all districts. This result is in line with evidence on state-level policy responsiveness (Flavin 2016). We also find that unions matter for how legislators respond to competing opinions in the context of another fundamental economic shift: the dramatic increase in import competition from China experienced in many local labor markets across the country.

Our findings cast a somewhat less pessimistic light on democratic representation in Congress. Despite high income inequality, polarization, expensive campaigns, and a legislature dominated by affluent politicians (Carnes 2013; Gilens 2012; Hacker and Pierson 2010; McCarty et al. 2006), our evidence indicates that unequal representation is not hard-wired into the fabric of American democracy. Analyzing heterogeneity in the impact of unions, we find evidence that it is not restricted to a particular party. We also find suggestive evidence that public sector unions, to whom union membership has been shifting over the last decades (Ahlquist 2017; Rosenfeld 2014), do not appear to be less of a collective voice for the less well-off than private sector unions.

Admittedly, the observational nature of our data makes it difficult to draw causal conclusions. However, our within-district research design combined with rich data on possible confounds and flexible statistical specifications allows us to rule out a host of alternative explanations. We demonstrate that the moderating effect of unions on legislative responsiveness is not simply a result of state-level policies or institutions, district-level socio-economic structure, workers' propensity to organize, or broader patterns of associational life, and it persists in the face of exogenous economic shocks. Our empirical strategy was made possible by combining fine-grained data on unions, calculated from administrative sources, with extensive public opinion data capturing within-district variation in opinion polarization across 23 issues. As a result, we believe that it is unlikely that the robust effects of unions revealed in our analysis are spurious. More broadly, a focus on real-world variation in mass organizations is a necessary complement to field-experimental studies of unequal responsiveness and their ability to isolate biases in response to personal contacts as well as the effectiveness of particular strategies of influence (Butler 2014; Kalla and Broockman 2016).

Additional research could analyze the mechanisms driving our findings in greater detail. We find evidence consistent with the argument that the political power of unions rests in part on its ability to mobilize campaign contributions (see Appendix **??**). This result helps explain the puzzle documented by previous studies that inequalities in turnout or contacting officials or alone to not appear to explain most of the observed income gap in political responsiveness (Bartels 2008; Ellis 2012; Erikson 2015). But additional work might examine complementary channels. For example, how responsive are politicians to the threat of voter mobilization by unions? And how relevant is the ability of unions to shape the preferences of their members (Ahlquist et al. 2014; Kim and Margalit 2016)? Furthermore, in line with canonical theories of representation recent research has documented that state-legislators or congressional staffers systematically mis-perceive the preferences of constituents, with a bias toward conservative and corporate views (Broockman and Skovron 2018; Hertel-Fernandez et al. 2017). This calls for a better understanding of whether and how organized labor shapes the crucial link between actual and perceived constituent preferences.

Our findings should also encourage comparative research on unequal representation to examine the role of unions. Existing research in this emergent literature has focused on political institutions (Bartels 2017; Lupu and Warner 2017).

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A. APPENDIX

A.1. Data description

TABLE A.1
Matched CCES–House roll-calls included in our analysis.

Match	Bill	Date	Name	House Vote (Yea-Nay)	CCES Support (For-Against %)
(1)	HR 810	07/19/2006	Stem Cell Research Enhancement Act	235-193	57.0-43.0
(1)	HR 3	01/11/2007	Stem Cell Research Enhancement Act of 2007 (House)	253-147	57.0-43.0
(2)	HR 2956	07/12/2007	Responsible Redeployment from Iraq Act	223-201	58.2-41.8
(3)	HR 2	01/10/2007	Fair Minimum Wage Act	315-116	76.6-23.4
(4)	HR 4297	12/08/2005	Tax Relief Extension Reconciliation Act (Passage)	234-197	50.0-50.0
(4)	HR 4297	05/10/2006	Tax Relief Extension Reconciliation Act (Agreeing to Conference Report)	244-185	50.0-50.0
(5)	HR 3045	07/28/2005	CAFTA Implementation Act	217-215	47.5-52.5
(6)	HR 3162	08/01/2007	Children's Health and Medicare Protection Act of 2007	225-204	70.4-29.6
(6)	HR 976	10/18/2007	Children's Health Insurance Program Reauthorization Act (Presidential Veto Override)	273-156	70.4-29.6
(6)	HR 3963	01/23/2008	Children's Health Insurance Program Reauthorization Act (Presidential Veto Override)	260-152	70.4-29.6
(6)	HR 2	02/04/2009	Children's Health Insurance Program Reauthorization Act of 2009	290-135	70.4-29.6
(7)	HR 3221	07/23/2008	Foreclosure Prevention Act of 2008	272-152	49.7-50.3
(8)	HR 1424	10/03/2008	Emergency Economic Stabilization Act of 2008	263-171	26.8-73.2
(9)	HR 3078	10/12/2011	To implement the United States-Colombia Trade Promotion Agreement	262-167	51.1-48.9
(10)	HR 2346	06/16/2009	Supplemental Appropriations, Fiscal Year 2009	226-202	22.7-77.3
(11)	HR 2831	07/31/2007	Lilly Ledbetter Fair Pay Act	225-199	63.3-36.7
(11)	HR 11	01/09/2009	Lilly Ledbetter Fair Pay Act of 2009 (House Version)	247-171	63.3-36.7
(12)	HR 1913	04/29/2009	Local Law Enforcement Hate Crimes Prevention Act	249-175	61.5-38.5
(13)	HR 1	02/13/2009	Making supplemental appropriations for fiscal year ending 2009	246-183	50.3-49.7
(14)	HR 2454	06/26/2009	American Clean Energy and Security Act	219-212	54.9-45.1
(15)	HR 3962	11/07/2009	Affordable Health Care for America Act	220-215	49.9-50.1
(16)	HR 3590	03/21/2010	Patient Protection and Affordable Care Act	219-212	55.9-44.1
(17)	HR 4173	06/30/2010	Wall Street Reform and Consumer Protection Act of 2009	237-192	67.5-23.5
(18)	HR 2965	12/15/2010	Don't Ask, Don't Tell Repeal Act of 2010	250-175	63.2-36.8
(19)	HR 2775	10/16/2013	Continuing Appropriations Act of 2014	285-144	47.2-52.8
(20)	H Con Res 34	04/15/2011	House Budget Plan of 2011	235-193	18.7-81.3
(21)	H Con Res 112	03/28/2012	Simpson-Bowler/Copper Amendment to House Budget Plan	38-382	40.0-60.0
(22)	HR 2	01/19/2011	Repealing the Job-Killing Health Care Law Act	245-189	55.2-44.8
(22)	HR 8	08/01/2012	American Taxpayer Relief Act of 2012 (Levin Amendment)	170-257	55.2-44.8
(23)	HR 6079	07/11/2012	To repeal the Patient Protection and Affordable Care Act and health care-related provisions in the Health Care and Education Reconciliation Act	244-185	48.0-52.0
(23)	HR 45	05/16/2013	To repeal the Patient Protection and Affordable Care Act and health care-related provisions [...]	229-195	48.0-52.0
(23)	HR 596	02/03/2015	To repeal the Patient Protection and Affordable Care Act and health care-related provisions [...]	239-186	48.0-52.0
(24)†	H CON RES 25	03/21/2013	Establishing the budget for the United States Government for fiscal year 2014 and setting forth appropriate budgetary levels for fiscal years 2015 through 2023	221-207	22.0-78.0
(25)†	HR 2642	01/29/2014	Federal Agriculture Reform and Risk Management Act	216-208	61.5-38.5
(26)†	HR 3361	05/22/2014	USA FREEDOM Act	303-121	70.8-29.2

Note: The matching of roll calls to CCES items can be many-to-one. CCES Support refers to support levels in our final CCES sample (aggregated over districts, and income groups, with "don't know" answers excluded as they are not used consistently over CCES waves).

† Only included in two period analysis.

TABLE A.2
Distribution of income-group reference points by state.
Average threshold over all states and smallest and largest value

	33th percentile			67th percentile		
Year	Mean	Min	Max	Mean	Min	Max
2006	31230	20900	43600	67294	50450	90780
2007	32862	22400	45000	70521	53600	95030
2008	33861	24000	46400	72990	55000	99800
2009	33304	22850	46500	72603	55000	100000
2010	32499	23000	45760	71825	55000	98100
2011	32553	23000	47000	72616	55000	100000
2012	33595	23000	46600	74725	55100	105500
2013	34287	23600	49000	76586	57000	105000

Note: Calculated from American Community Survey 1-year files. Household sample excluding group quarters. All quantities are weighted. Average sample size for state-year cells is 2,243,556 households (min N= 206,136, max N = 12,758,656).

TABLE A.3
Descriptive statistics of analysis sample

	Mean	SD	Min	Max	N
Roll-call vote: yea	0.554	0.497	0.000	1.000	13171
Constituent preferences					
Low income	0.575	0.189	0.000	0.961	13812
Mid income	0.546	0.156	0.000	1.000	13812
High income	0.535	0.133	0.000	0.889	13812
Union membership [log]	9.692	1.063	6.094	13.619	13812
Population	7.029	0.730	4.697	9.980	13812
Share African American	0.125	0.147	0.004	0.680	13812
Share Hispanic	0.155	0.174	0.005	0.814	13812
Share BA or higher	0.276	0.097	0.073	0.645	13812
Median income	5.173	1.350	2.282	10.439	13812
Share female	0.508	0.010	0.462	0.543	13812
Manufacturing share	0.110	0.047	0.025	0.281	13812

Note: All controls calculated from American Community Survey, 2006-2013. Note that when entered in models variables are scaled to mean zero and unit SD.

a Calculated for each district as $([\Sigma_{i=1}^{K} s_i]^{-1} - 1)/(K-1)$, where $s \in [0,1]$ are employment shares in $K = 13$ occupational sectors.

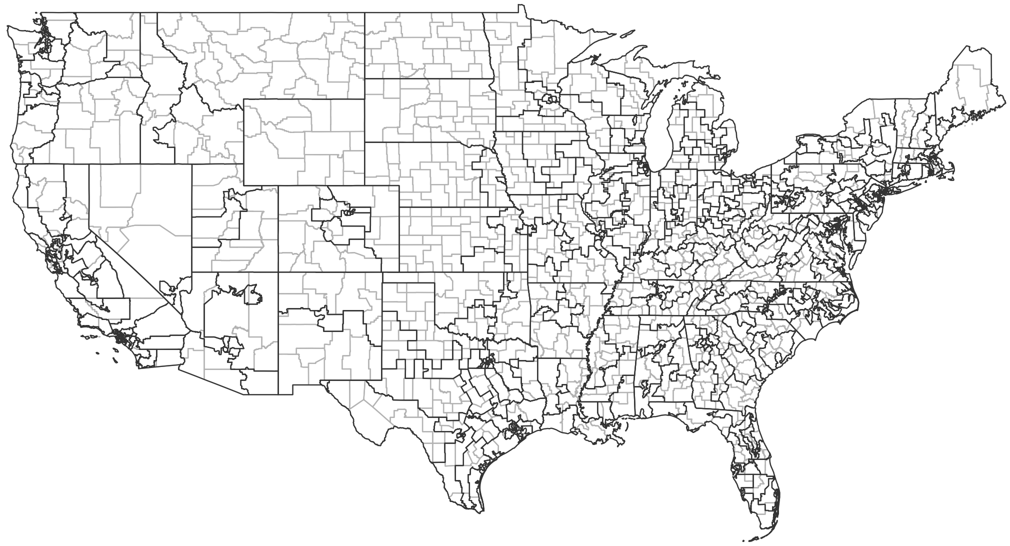

FIGURE A.1
Commuting Zones and Congressional Districts (based on 2010 Census).

A.2. Details on certification elections

In the empirical analysis reported in the main text, we use union certification elections as a proxy for the propensity of workers in a district to solve collective action problems. Here we provide some background on union certification elections and explain in more detail how we calculated the variable.

The formation of unions is regulated by the National Labor Relations Act (NLRB) enacted in 1935 (see Budd 2018: ch. 6). A successful union organization process usually requires an absolute majority of employees voting for the proposed union in a certification election held under the guidelines of the NLRB. Getting the NLRB to conduct an election requires that there is sufficient interest among employees in an appropriate bargaining unit to be represented by a union. For proof of sufficient interest, the NLRB requires that at least 30% of employees sign an authorization card stating they authorize a particular union to represent them for the purpose of collective bargaining. Building support and collecting the required signatures takes organizational effort. For workers, unionization has features of a public good. Everybody may gain through better conditions from collective bargaining, but contributing to the organizational drive is costly for each individual. Beyond mere opportunity costs, there also is a non-zero risk of being (illegally) fired by the employer for those especially active. If more than 50% of employees sign authorization cards, then the union can request voluntary recognition without a certification election. However, the employer has the right to deny this, in which case a certification election is held. In his labor relations textbook, Budd (2018: 199) notes that voluntary card check recognition is "the exception rather than the norm because employers typically refuse to recognize unions voluntarily."

Monthly election reports in PDF format were retrieved from the NLRB website, `https://www.nlrb.gov/` (November 2, 2017).

[Describe web scraping and geocoding.]

A.3. Churches data details

Measures of the number of churches in a given district, are not readily available for the years covered in our study. Therefore, we spatially aggregate county-level measures from the 2010 Religious Congregations and Membership Study to the congressional district level using asymmetric weighting (Mennis and Hultgren 2006).[26] To do so, we require three spa-

[26]See Goplerud (2015) for a recent introduction to political science and an illustration of its superiority to simply weighting by area.

tial layers. The first two layers are the mapping source and target geographies, i.e., counties and congressional districts. The third layer provides auxiliary information used to predict the spatial distribution of the quantity of interest in the first geography (counties) based on a third variable.

We use voting districts and their population in the 2010 census as the auxiliary layer. They are ideal for our application for three reasons. First, while voting district boundaries can change over time, they are generally nested within both counties and congressional districts. Second, they are geographically fine-grained (typically covering a low 4-digit number of voters). Third, the population distribution across them is likely strongly related to the population distribution of churches. We use geographic shapefiles provided by the Topologically Integrated Geographic Encoding and Referencing program of the US Census Bureau for counties in 2013, and voting districts in 2010 to map county-level data to each congressional districts in a given year.[27]

A.4. Nonparametric evidence for union-preferences interaction

[KRLS]

[27]For states with at-large districts, the aggregation procedure reduces to a simple summation, as counties by definition are perfectly nested within districts/states. For three states (Kentucky, Oregon, Rhode Island), no or incomplete voting district data is available from the Census Bureau, and we use 2010 county-subdivision as the auxiliary layer instead. For the remaining states, around 3.5% of the precinct-level population estimates were missing and ignored in the procedure.

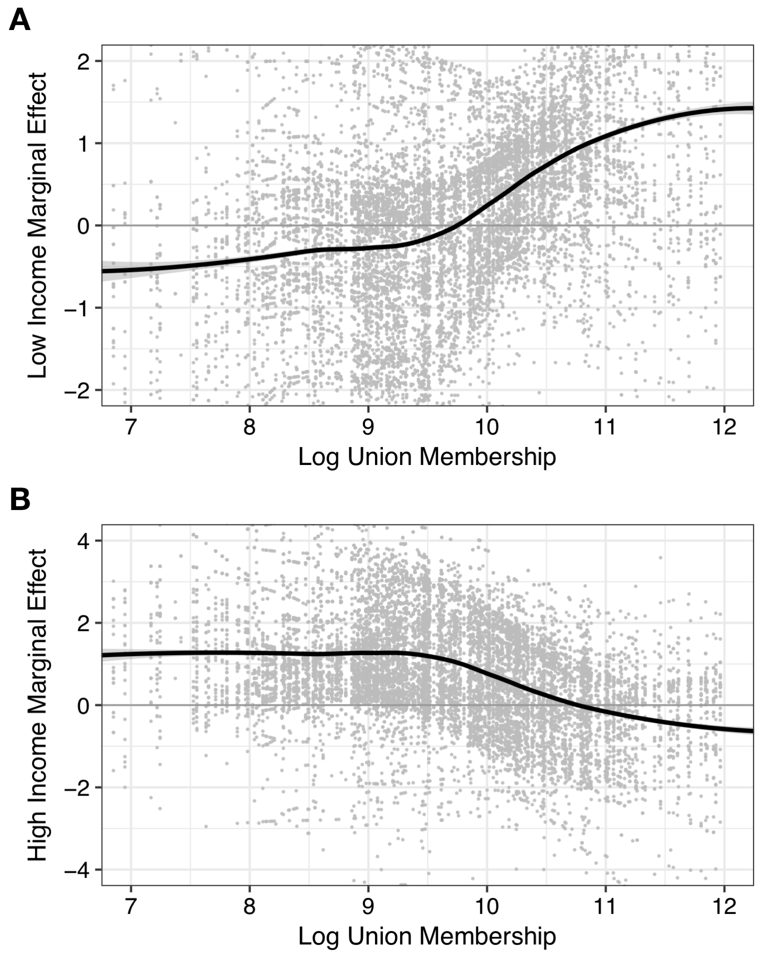

FIGURE A.2
Nonparametric estimate of marginal effect of low and high income preferences conditional on district union membership.