Affluence and Congruence:
Unequal Representation Around the World

Short title: Affluence and Congruence

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Abstract

Do elected representatives reflect the preferences of the citizens they represent? Recent studies from the U.S. and a number of other democracies have found that legislators tend to represent better the preferences of affluent citizens. But we still know little about how widespread this bias is. To answer this question, we gathered every publicly available survey of elected representatives in the world and matched it with mass survey data. Our dataset consists of 92,000 elite observations and 3.9 million citizen observations spread across 565 country-years, 52 individual countries, and 33 years. Using a variety of methods, we find that around the world, legislators' preferences are consistently more congruent with those of affluent citizens. However, we also find that this inequality varies substantially by issue domain: while the affluent are better represented on economic issues, the poor seem to be over-represented on cultural issues.

Keywords: Congruence; unequal representation; mass surveys; elite surveys

Supplementary material for this article is available in the appendix in the online edition. Replication files are available in the JOP Data Archive on Dataverse (http://thedata.harvard.edu/dvn/dv/jop).

Do elected representatives reflect the preferences of citizens? This question is central to understanding how representative democracy works—and under what circumstances it works better (Dahl 1971; Pitkin 1967; Urbinati and Warren 2008). It also informs fundamental questions in political science about who gains, and who loses, from politics – and why. If elected representatives sometimes fail to reflect the preferences of citizens – or reflect the preferences of some citizens better than those of others – then it would be important to understand what circumstances attenuate or exacerbate those political inequalities. Such unequal representation may also worryingly erode citizens' evaluations of democratic institutions and decisions (e.g., Arnesen and Peters 2018; Mayne and Hakhverdian 2017).

And yet, the answer to this question remains elusive. Whereas some studies of the U.S. find evidence of unequal representation (e.g., Bartels 2008; Gilens 2012; Jacobs and Page 2005), others contest these findings on conceptual and methodological grounds (e.g., Bhatti and Erikson 2001; Branham et al. 2017; Brunner et al. 2013; Enns 2015). Outside the U.S., a growing body of work uncovers inequalities in representation (e.g., Bernauer et al. 2015; Giger et al. 2012; Lupu and Warner 2017; Peters and Ensink 2015; Schakel et al. 2020).[1] But these studies analyze single countries or small samples of cases, typically in Europe. Moreover, they measure both preferences and representation in very different ways and along different issue dimensions, making it difficult to aggregate their findings. Some of these findings also rely on problematic correlational measures (see Achen 1977; Matsusaka 2001). As a result, we still lack a sense of the extent to which modern electoral democracies around the world achieve the ideal of equal representation.

There are reasons to be skeptical that representation around the world is as unequal as some U.S. scholars suggest. Most of them attribute it to the outsize influence of money in American politics (e.g., Bartels 2008; Flavin 2014; Gilens 2012). If this is true, then we should expect less inequality in other contexts, where the role of money in politics is typically more circumscribed. On the other hand, other possible reasons for unequal representation – such as biases in political participation or the fact that elected representatives themselves tend to be

[1] For a recent review, see Peters (2018).

affluent – would certainly apply to a broader range of cases (Carnes and Lupu 2015; Gallego 2015; Peters 2018; Schlozman et al. 2012).

Our aim in this paper is to offer a broadly comparative perspective, one that uses a coherent and comparable set of rigorous measurement and estimation strategies to arrive at some general conclusions about the breadth of unequal representation around the world. We do this by bringing to bear the broadest possible dataset of comparative, high-quality mass-elite data. We gathered every publicly available academic survey of elected representatives and matched each one to a nationally representative mass survey. Our sample consists of 92,000 elite observations and 3.9 million citizen observations spread across 565 country-years, 52 individual countries, and 33 years. Comprising over 800 survey studies, this represents more than a tenfold increase in country-years over prior comparative studies, and much wider geographic and temporal coverage. We also improve on previous studies by using multiple approaches to calculate congruence between mass respondents and elected representatives. Finally, we draw on a broad set of coordinated mass and elite surveys in Latin America, Sub-Saharan Africa, and Sweden to examine whether inequalities in representation are consistent across different types of issues.

We consistently find that mass-elite congruence on the left-right dimension is significantly and substantially higher for the affluent than it is for the poor. On this dimension, affluence bias appears to be widespread among modern electoral democracies. Yet we also find that unequal representation varies substantially by issue domain. While the affluent are better represented on economic issues, the poor seem to be overrepresented on cultural issues. Around the world, representation appears to be both more unequal than previously thought and unequal in different ways across issues.

Democracy or Plutocracy?

What kind of standard should we use to assess democratic representation? Canonical theories typically divide the representative process into two stages: first, congruence or opinion

representation—the process of generating a body of representatives that reflects the preferences of the electorate—and then, responsiveness—the process by which these representatives generate policies that reflect citizens' preferences (Achen 1978; Miller and Stokes 1963).[2] Others have also focused on descriptive representation (Pitkin 1967), recently noting that elected representative tend to be far more affluent than the citizens they represent (Carnes 2013; Carnes and Lupu 2015).

Recent U.S. research on biases in representation has focused on responsiveness, but both earlier and comparative studies often focus on congruence. A large body of research, focusing mostly on advanced democracies, has developed around the question of how congruent representatives are with overall mass preferences (e.g., Converse and Pierce 1986; Esaiasson and Wlezien 2017; Miller et al. 1996; Soroka and Wlezien 2010). Comparative scholars have also been concerned with how differences across countries—especially electoral institutions—condition that congruence (e.g., Blais and Bodet 2006; Golder and Stramski 2010; Huber and Powell 1994; Lupu et al. 2017; Powell 2013). And more recent studies examine whether certain groups enjoy disproportionately more congruence with their elected representatives (e.g., Bernauer et al. 2015; Giger et al. 2012; Schakel and Hakhverdian 2018).

Like this comparative literature, we focus in this paper on congruence. In the theoretical framework developed by Miller and Stokes (1963), congruence is a necessary step in the process of representation (see also Powell 2004). While we cannot infer every behavior from representatives' stated policy preferences, we know that they regularly act upon those preferences, particularly in the important agenda-setting phase of the legislative process (e.g., Carnes and

[2] The two terms are sometimes defined differently. For instance, a recent special issue on "Advances in the Study of Democratic Responsiveness" subsumed studies of both responsiveness and congruence (see Esaiasson and Wlezien 2017). Lax and Phillips (2012) define both responsiveness and congruence in terms of policy outcomes. We follow the convention in most comparative work to define responsiveness in terms of policy outcomes and congruence in terms of preferences or positions.

Lupu 2015; Schwindt-Bayer 2006). Moreover, mass-elite congruence means that elected representatives are "not found persistently at odds with the wishes of the represented" (Pitkin 1967: 210), an important element of representation. Theorists have also highlighted the normative value of congruence with regard to descriptive representation (e.g., Mansbridge 1999). Mass-elite congruence is thus an important element of representation, both on its own and as part of the broader representative process.

Empirically, congruence also seems to affect important democratic indicators like the public's satisfaction with and trust in democratic institutions and decisions (Arnesen and Peters 2018; Mayne and Hakhverdian 2017; Stecker and Tausendpfund 2016; Wlezien 2017). Finally, because data on congruence are more widely available cross-nationally, studying congruence allows us make broad comparisons over space and time, something that is critical to understanding how modern electoral democracy works for citizens around the world.

In conceptualizing representation, the unit of analysis is also crucial. Scholars of representation in the U.S. sometimes focus on *dyadic representation*, the extent to which politicians represent their districts (e.g., Hill and Hurley 1999). In Western Europe, studies often measure the congruence between voter preferences and the policy positions of their preferred party (e.g., Bernauer et al. 2015; Miller et al. 1996). Others studying these parliamentary settings simply compare the median voter with the median legislator or the median government legislator (Huber and Powell 1994).

Like other comparative empirical work on congruence (e.g., Golder and Stramski 2010; Powell 2009), we focus in this paper on what some scholars call *collective representation*. We ask whether representative bodies collectively reflect the preferences of the electorate. Why not simply study the preferences of the median voter? Scholars of elections often draw on canonical theories of electoral competition and focus their analyses on the preferences of the median.[3] But a long tradition in political theory emphasizes that democratic representation requires that

[3] In studies of inequality, researchers often assume that the median voter is middle-income, an assumption that may in fact be dubious in some contexts.

minority views, not just the preferences of the majority or the median, ought to be included in deliberative bodies (see Dahl 1998; Pitkin 1967; Urbinati and Warren 2008; Weissberg 1978). Imagine, for instance, a legislature made up entirely of representatives whose positions align with the median voter. Or a legislature made up entirely of men in a context where the median voter is a man. Surely neither is the ideal of democratic representation, even though the median voter's preferences would be well-represented.

Moreover, since we are interested in broad cross-country comparisons, it also makes practical sense to study collective representation. In some contexts, electoral rules (e.g., closed-list PR) produce little connection between representatives and their district constituents, making it difficult to compare dyadic representation across countries. In other systems (e.g., open-list PR and many presidential systems), political parties are all but irrelevant so it would be misleading to compare constituents to parties. Focusing on collective representation allows us to characterize congruence across time and space in a comparable way and allows such contextual differences to become possible explanatory variables rather than confounding how we interpret our measures.

We study these particular dimensions of representation – collective representation in terms of congruence – not because they are the only ones that matter. But, like every other empirical study of representation, we must choose which dimension of this broad concept to tackle. And these particular dimensions are both normatively important and tractable given the broad comparative perspective we wish to contribute to debates about unequal representation.

Many factors may lead collective representation to favor some citizens over others. Although scholars of U.S. politics tend to blame the outsize influence of campaign contributions (Bartels 2008; Flavin 2014; Gilens 2012), other explanations are also plausible. Poor citizens may be less likely to vote or participate in politics more generally than the rich, allowing reelection-motivated incumbents to discount their preferences (e.g., Gallego 2015; Peters 2018; Schlozman et al. 2012). Representatives may be catering to the preferences of the most informed citizens, which also happen to be the most affluent (Erikson 2015). Or elected officials may better

represent the affluent because they themselves are affluent (Carnes and Lupu 2015), and the affluent are increasingly detached from the rest of society (Thal 2017).

Another alternative – one that is less troubling from a normative perspective – is that elected representatives respond to issue publics (Converse 2006), reflecting the preferences of some citizens better on some issue dimensions and those of other citizens on other issue dimensions (see Gilens 2012). If the rich care more about economic issues and the poor more about cultural or religious issues (e.g., Frank 2007; Tavits and Letki 2014), then we might expect elected representatives to reflect better the economic preferences of the rich and the cultural preferences of the poor (Roemer 1998). But to study this, we need to measure representation by issue dimension, something only a handful of studies outside the U.S. consider (e.g., Lesschaeve 2017; Rosset and Stecker 2019; Schakel and Hakhverdian 2018), typically in a single country or around a single issue item given the scarcity of these kinds of data.

Data from Around the World

In order to compare mass and elite preferences, we first gathered information on the left-right self-placements of elected representatives. We focus on representatives collectively elected by a national electorate, thus setting aside subnational legislators. We collected all the publicly available surveys of these representatives or candidates for these offices from cross-national and national data repositories, as well as a general literature search.[4] We included an elite survey in our dataset if the respondents were elected legislators—or, in the case of candidate surveys, the survey allows us to establish whether the respondent was elected—and where the full population of legislators was sampled.[5] Finally, our dataset only includes surveys

[4] Further information about sources, variables, and coding decisions are available in Sections A.4-A.6 of the online appendix.

[5] Although our dataset includes Members of the European Parliament, our statistical analyses reported in Figure 1 exclude them because they may not be directly comparable to national

that asked representatives to place themselves on a scale with "left" and "right" anchors (or close variants thereof, such as "liberal" and "conservative").

In some country-years, we have access to more than one elite survey, and given the relatively small population of legislators, there is a nontrivial chance that these samples overlap. Including multiple overlapping samples has the potential to turn even otherwise negligible nonresponse bias into a real concern, since the legislators who respond to multiple surveys may be different from those who respond only to one. To avoid this problem, we chose only one elite sample per country-year, selecting the one for which fieldwork was most proximate to each calendar year in a legislative term. For instance, a survey from 2007 would be dropped in favor of a survey from 2004 for an observation in 2005. When multiple surveys were fielded at approximately the same time, we prioritized larger surveys with greater cross-national comparability (e.g., as part of the Comparative Candidates Survey).[6] Our final elite sample includes 92,000 unique legislator-year observations.

One common concern with elite survey data is the extent to which elite samples are representative of the population of legislators. If a legislator's decision to respond to the survey is correlated with her left-right position, then we are unlikely to recover a sample that accurately characterizes the distribution of representatives' preferences, and our measure of congruence will be biased. Despite scholars' "understandable suspicion" about biases in representativeness (Laver 2014: 214), various studies have failed to find any notable patterns suggesting strategic selection into legislator surveys (Byrne and Theakston 2016; Fisher and Herrick 2013; Saiegh 2009; Smith et al. 1990).

Even so, we address the potential for nonresponse bias in two ways. In our main analysis, we poststratify our elite samples by gender and party affiliation (Bailer 2014; Maestas et al.

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legislators. However, in Table A8 in the online appendix, we show that our results are robust to including MEPs in the analysis.

[6] Our results are robust to using all elite surveys simultaneously—that is, not dropping any potentially duplicate samples (see Table A8 in the online appendix).

2003), recovering a distribution of legislators that more closely resembles the population as a whole.[7] As an alternative to weighting, in analysis reported in Table A8 in the online appendix, we also examine congruence with a limited sample of elite surveys that achieved a response rate of at least 80 percent.[8] Because these surveys nearly capture the entire universe of legislators, higher response rates raise the lower bound on a sample's unrepresentativeness (a 100 percent response rate corresponds to a perfectly representative sample). Across both of these approaches, we find no evidence to suggest that our results are affected by nonresponse bias.

For each elite sample meeting our criteria, we also gathered data on contemporaneous mass preferences. We began by identifying the legislative term that each elite survey sampled, information that was either available in the data or could be coded from other sources.[9] We then matched these elite surveys with mass surveys that included both left-right self-placement and some measure of affluence from any of the years during the elite respondents' term. For instance, an MP surveyed in 2004 for a 2003-2005 term would be matched to mass survey respondents from 2003, 2004, or 2005.

Since mass data are more widely available, we chose mass surveys more selectively. We privileged mass surveys that were either conducted as part of the same study as matching elite surveys or coordinated questions with a parallel elite survey. When neither of these types of mass

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[7] Weights are constructed using raking while ensuring each country-year is weighted equally. Where one of these variables (party affiliation and gender) is unavailable, we use only the available variable. Where neither is available, we weight each respondent equally. The weights for partisanship and gender range from 0.01 to 0.73, with a design effect is 1.70. Our main results are robust both to including only elite respondents for whom we have information about both partisanship and gender, and to not poststratifying the samples at all (see Table A7 in the online appendix).

[8] We also tested other thresholds of response rates, with no effect on our results.

[9] In two cases, information about the legislative term was not available so we coded the year the legislator was surveyed and matched the response to mass samples only in that year.

data were available, we used mass surveys in which the response scale was most similar to that of elites' responses. Finally, when arbitrating between the remaining options, we deferred to those mass surveys embedded in large, cross-national projects to increase comparability across country-years. Despite this minimal approach to adding mass samples, many country-years contain multiple citizen surveys. Yet unlike with elite data, the probability of overlapping samples is minimal, and so we use all available citizen responses. The resulting dataset includes nearly 3.9 million unique citizen-year observations.

To measure affluence, we develop a rank-ordering of indicators, which privileges measuring wealth over household income and occupational status.[10] Where we have data on ownership of durable goods (e.g., a car or refrigerator), we use multiple correspondence analysis to generate a factored index of affluence (see Filmer and Pritchett 2001). Where these data are not available, we use household income or occupation, in that order. We then generate quintiles from the material wealth and income variables, and we recode occupational data into general categories (e.g., "white-collar professional").[11] Because these affluence quintiles are computed separately for each country-year, our measures of class-based disparities in congruence are inherently relative to the national distribution of wealth. Thus, although "rich" and "poor" are likely to reflect very different levels of wealth across countries, we use these terms only in refererence to the most- and least-affluent quintiles within each country-year.

Our final sample includes 565 country-years, covering 52 countries and 33 years.[12]

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[10] We prefer measures of wealth because (1) nonresponse to questions about household income is typically high (these data are missing for 50% of respondents in our mass sample), and (2) occupational structures are difficult to compare across countries.

[11] Of the 565 observations in our data, 379 use asset wealth as a measure of affluence, 172 use household income, and 14 use occupation. Our main results are consistent if we focus only on the cases where we can measure affluence using wealth (see Table A8 in the online appendix).

[12] The countries are listed in Figure 2. The years are 1967-2015, although most of the data begin in the 1990s.

Although our dataset represents all of the publicly available data on elite preferences, most of the data come from Europe and Latin America. As a result, we cannot claim to have a representative sample of the world's democracies. But only additional data gathering will allow us to extend the analysis beyond these regions.[13]

Across such a large number of surveys, of course, the question about left-right self-placement varies. Most importantly, different studies offer respondents different response scales, typically ranging from 5 to 11 points. To make these responses comparable, we rescale them to range from -1 to 1. Since the scales themselves may affect responses, our analyses control for the scale used in each mass and elite survey and for the differences between the scales provided to elite and mass respondents in each country-year.[14]

Measuring Congruence

We analyze congruence in two ways. Our preferred method is to generate dyads between each mass respondent and each elite respondent in a particular country-year (see Boas and Smith 2019). We measure congruence as the left-right distance between each citizen-legislator pair and

[13] The U.S. is not in our dataset because no publicly available survey of Members of Congress has been conducted since Miller and Stokes (1963)—and their study did not ask a left-right item. (None of the studies of representation in the U.S. that we cite here rely on elite surveys.) We draw on a recent survey of parliamentarians in several African countries below, but neither these data nor the mass surveys conducted by Afrobarometer include a left-right item.

[14] Our data do not contain the anchoring questions required for joint rescaling methods, so we cannot rule out measurement problems from variation in how individuals interpret left-right scales. However, below we show that our results are consistent among respondents with high levels of political knowledge. Our similar findings using finer-grained issue positions in Latin America also give us further confidence that our left-right results are not artifacts of measurement problems.

then regress that distance on the citizen's level of affluence.[15] Our models also include citizen and legislator random effects to account for dyadic dependence (Aronow et al. 2015). Since our dependent variable is a measure of distance, larger values indicate less congruence.

This method is attractive for several reasons. Most importantly, it allows us to characterize the complete set of relationships between citizen preferences and legislator positions, thus measuring collective representation, our concept of interest. This measure captures both differences in the mean positions of voters and legislators and differences in the variances of the distributions.[16] Unlike other measures of congruence that collapse distributions into aggregate summary statistics, dyads allow us to model an individual's affluence directly while still capturing the full distribution of citizen-legislator relationships.

Using this dyadic approach increases our sample to 99 million observations. The size of this dataset and the effort to estimate all of the legislator and citizen random effects run up against computational constraints.[17] Instead, we compute two simplified models. First, we drop the legislator and citizen random effects and estimate the model using iterative weighted least squares (IWLS), which reads in "chunks" of data and updates a running coefficient estimate until all the

[15] Put formally, our ideal model is $y_{d(c,\ell)} \sim \mathcal{N}(\alpha + \mathbf{x}_{d(c,\ell)}^{\intercal}\boldsymbol{\beta} + \gamma_c + \delta_\ell, \sigma^2)$, where $\gamma_c \sim \mathcal{N}(0, \sigma_c^2)$ and $\delta_\ell \sim \mathcal{N}(0, \sigma_\ell^2)$. Here y is distance on the left-right dimension; $\mathbf{x}$ is a vector of indicator variables for each affluence quintile; $d(c, \ell)$ refers to the citizen-c, legislator-ℓ dyad; and the γ_c and δ_ℓ are random effects for citizens $c \in \mathcal{C}$ and legislators $\ell \in \mathcal{L}$. The coefficients of interest are $\boldsymbol{\beta}$.

[16] The alternative widely used in studies of congruence measures only differences in mean positions. Comparing the distances between poor/rich citizen mean positions and mean legislator positions, our results are very similar (see Section A.1 in the online appendix). Still, we prefer our measurement approaches because they also account for differences in the variances of the mass and elite distributions.

[17] We attempted to estimate these models on our university's high-performance computers, but they failed to converge within the maximum runtime of two weeks.

data are used. Although dropping random effects underestimates uncertainty, our point estimates are unaffected. As an alternative, we bootstrap estimates by taking 250 random samples of 50,000 observations, fitting our preferred model with random effects and computing quantiles from the 250 sets of coefficient estimates. Bootstrapping allows us to recover more accurate measures of uncertainty, but could introduce bias since our observations are dyads and, therefore, not independent across resamples. Both methods have disadvantages, but to the extent that they yield similar estimates, we should be confident that we have closely approximated what computing the full model would have returned.

Our second method for measuring congruence characterizes the distance between citizens' and legislators' preference distributions in each country-year. We compute the Earth Mover's Distance (EMD), a flexible measure that calculates the amount we would have to move probability mass from one distribution to transform it into the other distribution. The EMD has recently been shown to better capture similarity between distributions than alternative measures of congruence (Lupu et al. 2017). Higher values of the EMD indicate more distance between the two distributions, and so less similarity and lower congruence. The aggregate analysis using EMD collapses some of the information in our data, but has the advantage of being much more tractable computationally.

To estimate the effect of affluence on congruence, we separately compute the EMD between legislators and each affluence quintile. We then simply regress these congruence measures on indicators for each affluence group, using the rich as the baseline.[18] We include fixed effects for country, year, and the original scale of the left-right item. We drop country-years

[18] Put formally, we estimate $y_{i,t} \sim \mathcal{N}(\alpha + \mathbf{x}_{i,t}^{\intercal}\boldsymbol{\beta} + \mathbf{u}_{i,t}^{\intercal}\boldsymbol{\theta}, \sigma^2)$, where y is the EMD, $\mathbf{x}$ is a vector of indicator variables for each affluence quintile, $\mathbf{u}$ are indicators for the fixed effects $\boldsymbol{\theta}$, countries are indexed by $i \in \mathcal{I}$, years are indexed by $t \in \mathcal{T}$, α is the intercept and $\boldsymbol{\beta}$ are the estimates of interest.

for which the elite sample included fewer than 30 legislator to ensure that our results are not driven by small samples.[19]

Is There An Affluence Bias?

Do these data reveal an affluence bias in representation around the world? Figure 1 shows the results of all three of our estimation methods. For each quintile of mass respondents, the figure shows how that group's predicted distance from its elected representatives compares to the predicted distance for the most affluent quintile. The leftmost estimates come from the dyadic model estimated using IWLS (hence the very tight confidence intervals), the middle estimates are 250 bootstrap replicates from the dyadic data, and the rightmost estimates come from models using the EMD.

These results imply that the distribution of less affluent citizens' left-right preferences are consistently further away from elected representatives' than those of the most affluent. Regardless of how we estimate these relationships, the evidence of an affluence bias is consistent. Moreover, at about 0.03, this difference is substantively meaningful. Since the mean EMD among the rich is 0.18, this effect size suggests that on average, less affluent voters can expect elected representatives' positions to be about 16 percent further from theirs than can more affluent voters.[20] Unequal representation appears to be quite common across democracies.

Among scholars of U.S. politics, there is debate about whether representation should be evaluated using the full set of available issues or the subset on which rich and poor citizens

[19] Our results are consistent if we set this threshold either lower or higher, or if we interact the affluence indicators with the indicator for question scale (see Table A8 in the online appendix).

[20] For reference, we also computed the average differences in means across our 565 country-years. The average difference between the least affluent and legislators is 0.17, compared to 0.15 for the most affluent. This difference is statistically significant and represents an effect size of 14 percent, in line with the results presented in Figure 1.

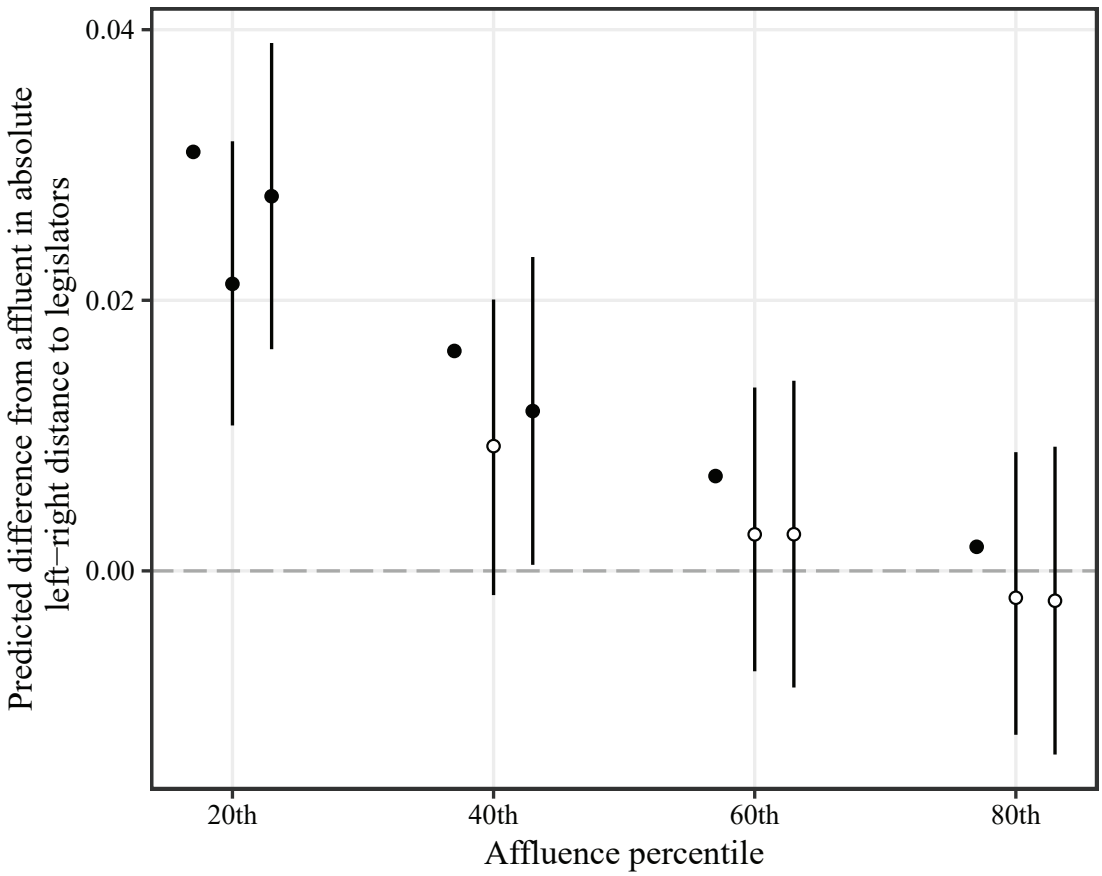

Figure 1: The affluence effect. Values represent the difference in predicted absolute left-right distance to legislators, relative to the most affluent quintile. Dots indicate point estimates with lines for 95% confidence intervals. For each quintile, dots on the left are from the dyadic model without random effects, estimated using IWLS, dots in the middle are mean estimates from 250 bootstrap replicates from the dyadic data, and dots on the right are from models using the EMD. See Table A1 in the online appendix for complete regression results.

disagree (see Gilens 2009; Soroka and Wlezien 2008). As in the U.S., our data similarly reveal a more pronounced affluence bias when the preferences of the rich and poor diverge. We reestimated our models on the 25 percent of country-years in which the absolute difference in mean left-right preferences between the least and most affluent citizens was greatest. The overall patterns of affluence bias are the same (see Table A2 in the online appendix), but twice as large. When the poor and rich disagree, the poor can expect to be 31 percent further away from their representatives than are the rich.

In contrast, we do not find statistically significant differences between the representation of the rich and that of the middle quintile. Figure 1 indicates that our point estimates comparing the middle to the top are positive, but we only measure them with precision with the IWLS approach, which likely understates uncertainty. Subsetting to the issues on which the rich and the middle quintile disagree most does not change these results (see Table A3 in the online appendix). This differs from some findings in the U.S. that middle-income voters are well-represented only when their preferences align with those of the rich.

Given the wide geographic and temporal coverage of our dataset, an obvious question is whether our finding of an on-average affluence bias is actually more circumscribed. Figure 2 shows the degree of affluence bias that we see in each country-year in our dataset (since 1995). Although there is variation over time and space, we see no obvious regional or temporal patterns. Cases of affluence bias (in shades of red) do not seem especially prevalent in more recent years nor limited to specific parts of the world, though certain countries do seem particularly biased. There is some evidence that affluence bias may be more pronounced in newer democracies than in older ones.

Figure 2 also shows that cases of affluence bias are far more common than the reverse (in shades of blue). Indeed, although we see multiple instances of the darkest share of red, the darkest blue never appears. The fluctuations from year to year within countries also demonstrates the substantial noise in our data, which is unsurprising for survey data. This reinforces the benefit of our large dataset over the much smaller datasets used in recent comparative work.

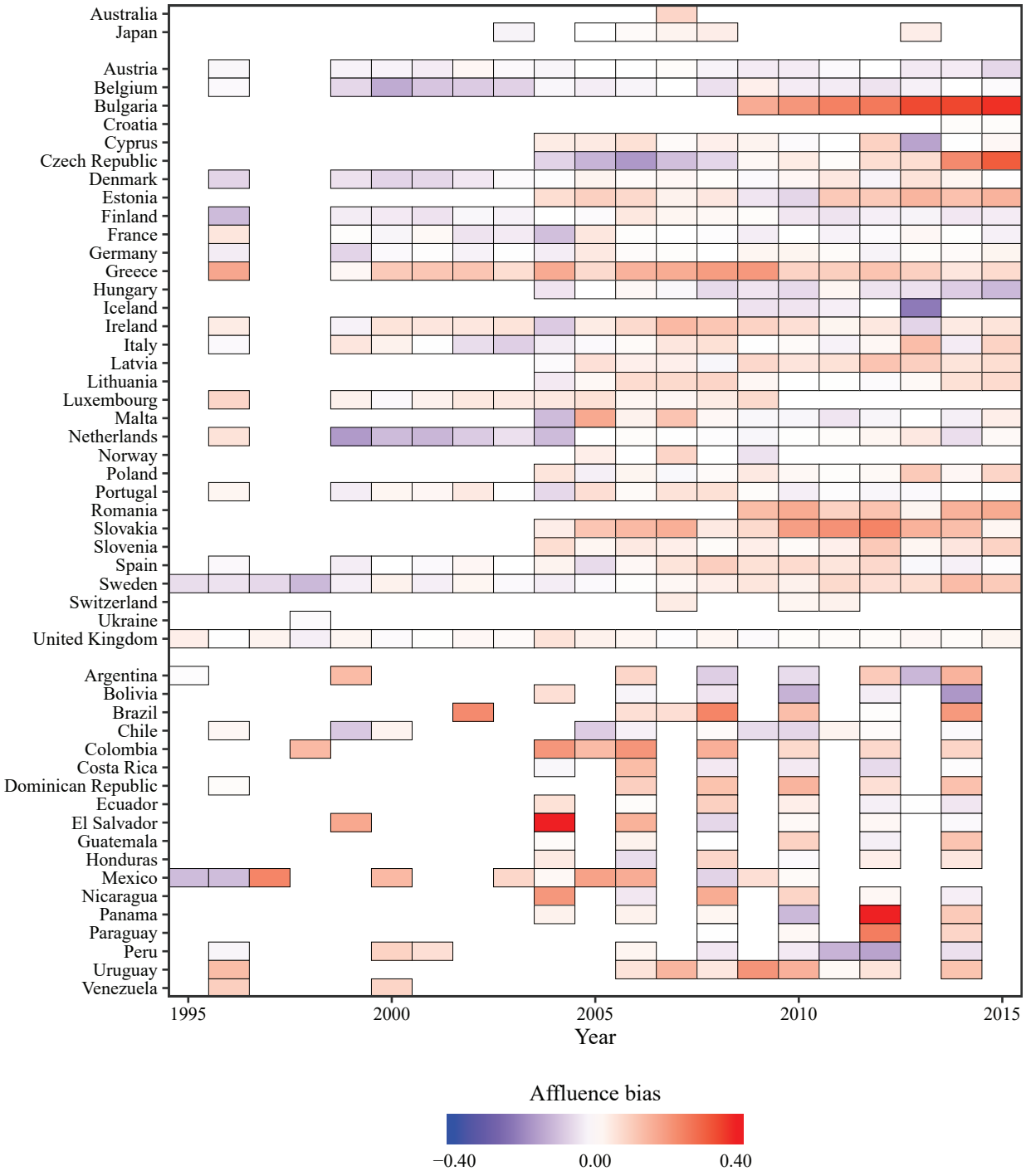

Figure 2: Affluence bias around the world. Each cell is shaded according to the size of the affluence effect. Darker red indicates greater bias in favor of the affluent, while darker blue indicates bias in favor of the less affluent. For clarity, observations before 1995 are not plotted.

These results imply that there is something systematic about many contemporary electoral democracies that leads elected representatives to reflect more closely the preferences of affluent citizens, a far cry from the ideal of democratic representation. At least in terms of left-right positions, the affluence bias documented in the U.S. seems to be the rule, not an exception.

Beyond Left and Right

Relying on left-right positions alone comes with certain limitations. These survey-based measures rely on conceptions of left and right that can vary across contexts and individuals (Bauer et al. 2017; Harbers et al. 2012; Zechmeister 2006), and respondents with less formal education often find it more difficult to use the scale (Zechmeister and Corral 2013). Moreover, overall congruence seems to be lower on specific issues than it is on the left-right dimension (Dalton 2017; Thomassen 2012; Thomassen and Schmitt 1997). In order to construct the largest possible comparative dataset, our main analysis relies on left-right placements. This is informative, but it also has limitations.

Fortunately, in a subset of cases, we have finer-grained measures of preferences. The AmericasBarometer and Parliamentary Elites in Latin America (PELA) surveys have harmonized the wordings and scales of a series of issue questions since 2010,[21] yielding high-quality data on mass-elite congruence in greater detail than is afforded elsewhere. Although this means focusing on just one region and a more limited period of time, these additional data allow us both to verify whether we see similar patterns in finer-grained data and to dig deeper into policy domains than is

[21] Our dataset includes the 2010, 2012, and 2014 AmericasBarometer mass surveys and the PELA survey from the matching legislative term. We do not have information on economic preferences in Panama because the economic questions were not asked in the AmericasBarometer surveys there. We also do not have data on Venezuela because PELA did not conduct legislator surveys there during this period.

possible with the single left-right item. The fact that we see similar results using different kinds of data — each with different limitations — makes us more confident about our results.

We focus on three issue-areas. First, to fix a baseline for comparison, we use the same 11-point left-right question we used in our main analysis. Second, we generate a factored index of economic preferences using four questions that asked respondents to rate their agreement (on a 7-point scale) with statements about the role of the state in ownership of natural resources, ensuring citizens' wellbeing, creating jobs, and providing healthcare.[22] Since the question wordings are nearly identical, we factor citizens and elites within the same country-year together. Finally, we examine preferences on cultural issues using a question that asked respondents how strongly they approve or disapprove (on an 11-point scale) of same-sex couples' right to marry. As above, we rescale the issue-areas to the range [-1,1], where lower values indicate the left, support for state intervention in the economy, and support for same-sex marriage. Since this dataset is orders of magnitude smaller than our complete cross-national dataset, we now simply use our preferred modeling strategy (mass-legislator dyads with citizen and legislator random effects).

Figure 3 reports the results from these models. As in the broader dataset, we find a similar affluence bias when we use left-right positions just in this Latin America sample. When we focus specifically on the index of economic preferences, we again find a very similar affluence bias. As with the left-right, there appears to be a graduated relationship between affluence and congruence: the wealthier quintiles seem closer to the legislature. These estimates suggest that congruence increases somewhat smoothly with affluence, though the data may be too noisy to estimate this relationship precisely. The substantive effects are somewhat smaller than in our global analysis: in left-right terms, the wealthiest voters can expect to be about 9 percent closer to legislators than can the poorest, and on economic issues about 7 percent closer.

However, we find the precise opposite with respect to cultural issues: the poor appear to be substantially *over*represented relative to the affluent on the issue of same-sex marriage—37

[22] This index has the added benefit of reducing measurement error, as compared to relying on a single survey item like left-right placement (Ansolabehere et al. 2008).

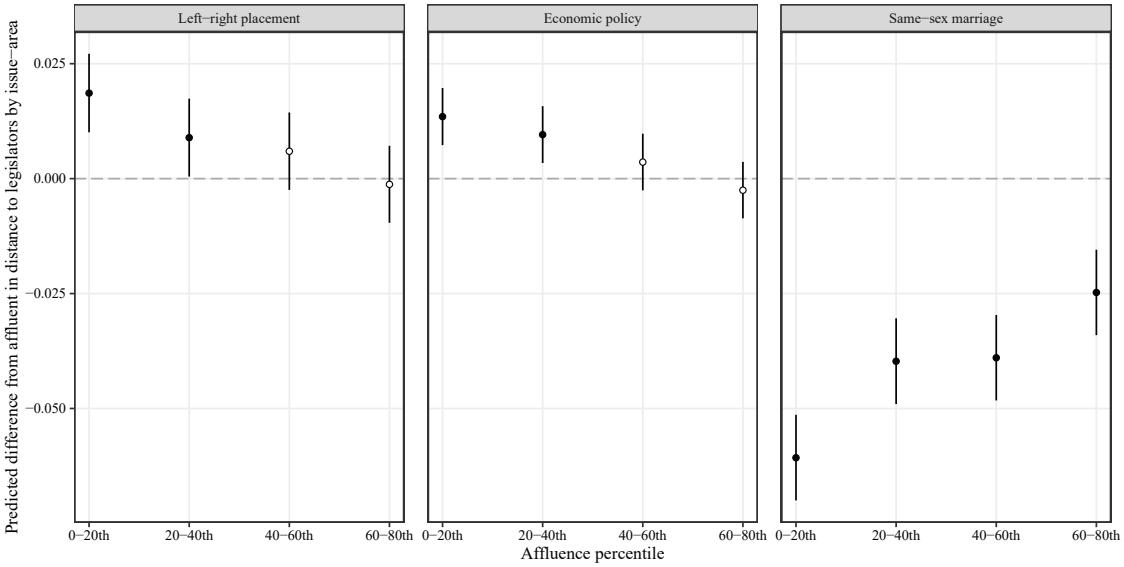

Figure 3: Affluence bias by issue-area in Latin America. Dots represent estimates of the relationship between mass affluence quintile and congruence on left-right placement, economic policy, and same-sex marriage. The baseline is the most affluent quintile. Lines indicate 95% confidence intervals. See Table A4 of the online appendix for complete regression results.

percent closer to legislators' preferences than the richest. Unfortunately, the LAPOP-PELA data only provide us with this one item capturing the cultural dimension, so we cannot generalize too far. However, as we note below, we find similar results with two other datasets. On both economic and cultural issues, we find evidence of political inequality, but on cultural issues it appears to favor the preferences of the poor.

No other datasets allow us to measure congruence on issues beyond the left-right to the extent that our Latin American data do, but we can offer some suggestive evidence that these results hold more broadly. These data come from Sweden and a number of Sub-Saharan African democracies. First, we examine harmonized citizen and legislator surveys conducted as part of the Swedish National Election Study, with waves covering seven election cycles between 1985 and 2010. Across all waves, respondents were given a series of policy suggestions and asked to respond on a scale from 1 to 5, where 1 indicates they think it is a "very good proposal" and 5 indicates it is a "very bad proposal." Prompts vary slightly across waves, but an example of the

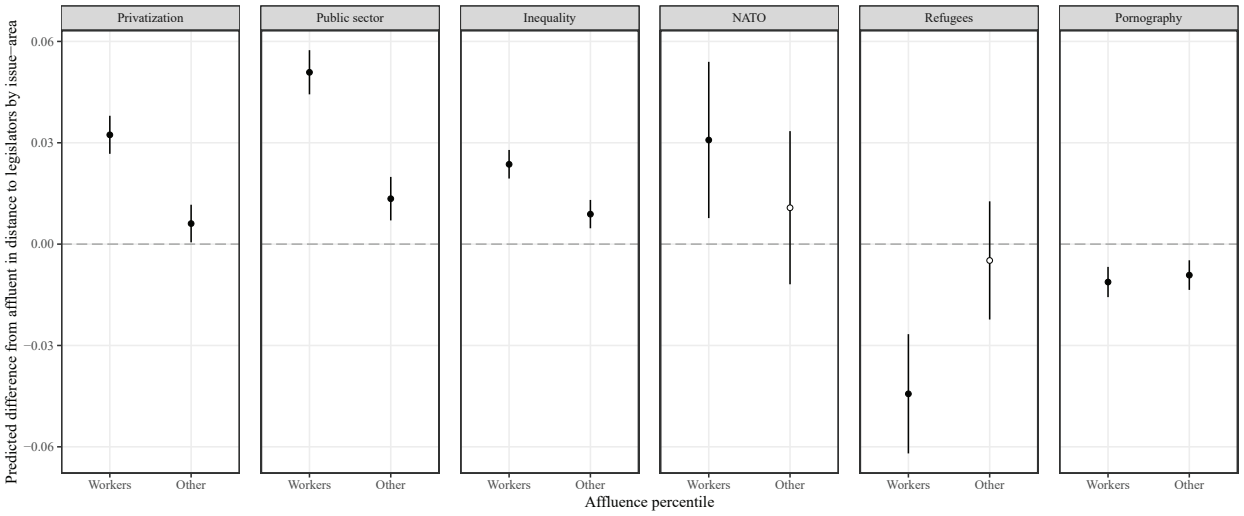

Figure 4: Affluence bias by issue-area in Sweden. Dots represent estimates of the relationship between mass occupation and congruence on privatization, the size of the public sector, inequality, NATO, refugees, and banning pornography. The baseline is white-collar professionals. Lines indicate 95% confidence intervals. See Table A5 of the online appendix for complete regression results.

specific prompt, from 1998, is, "What is your opinion about the proposal to: (1) Increase the proportion of health care run by private interests? (2) Reduce the public sector? (3) Reduce income differences in society? (4) Sweden should apply for membership of NATO? (5) Accept fewer refugees into Sweden? (6) Prohibit all forms of pornography?"

As in our main analysis, we construct mass-elite dyads from all citizen-legislator pairs within a wave, and then compute the absolute distance between their stated preferences on each issue area. The Swedish studies do not provide us with measures of mass wealth, but do provide occupations. So here we simply compare working-class respondents with white-collar professionals. We post-stratify the elite samples using data on partisanship and gender. The results in Figure 4 closely mirror those from Latin America: the poor are substantially underrepresented on economic issues but overrepresented on cultural ones.

We see similar patterns in Sub-Saharan Africa. Following Clayton et al. (2019), we match Afrobarometer mass data collected in 2008 and 2009 with surveys of MPs fielded by the African

Legislatures Project between 2008 and 2012 (Mattes and Mozaffar 2016).[23] Although no comparable policy questions were asked, both sets of surveys asked respondents to name the most important problems facing their countries. We follow Clayton et al. (2019) in coding these responses into categories. We examine four such issue categories: poverty, agriculture, social rights (e.g., "discrimination"), and violence (e.g., "crime and security" and "civil war"). We code each issue as 1 if the citizen or legislator mentioned it and -1 otherwise (matching the scale used in the main analysis).

Again we construct mass-elite dyads from all citizen-legislator pairs within a country-year, and then compute the absolute distance between their stated (binary) preferences on each issue area. We post-stratify the elite samples using data on partisanship and gender and then estimate our preferred specification described in the main text.

As Figure 5 shows, these results resemble those in Latin America and Sweden. Legislators are more likely to prioritize the economic issues that affluent citizens prioritize and the cultural issues the least affluent prioritize. Together, these three datasets indicate a consistent trend.

Direction of the Bias

Our discussion so far has focused on absolute biases, but we may also want to know the direction of the bias. To get an idea, we return to our finer-grained data for Latin America. The lefthand panel in Figure 6 plots the mean preferences of the poorest and richest mass quintiles

[23] As noted earlier, the African Legislatures Project data are not publicly available; here we simply analyze the replication data from Clayton et al. (2019). The resulting sample includes 24,000 citizens and 800 legislators across seventeen countries: Benin, Botswana, Burkina Faso, Ghana, Kenya, Lesotho, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, Zambia, and Zimbabwe.

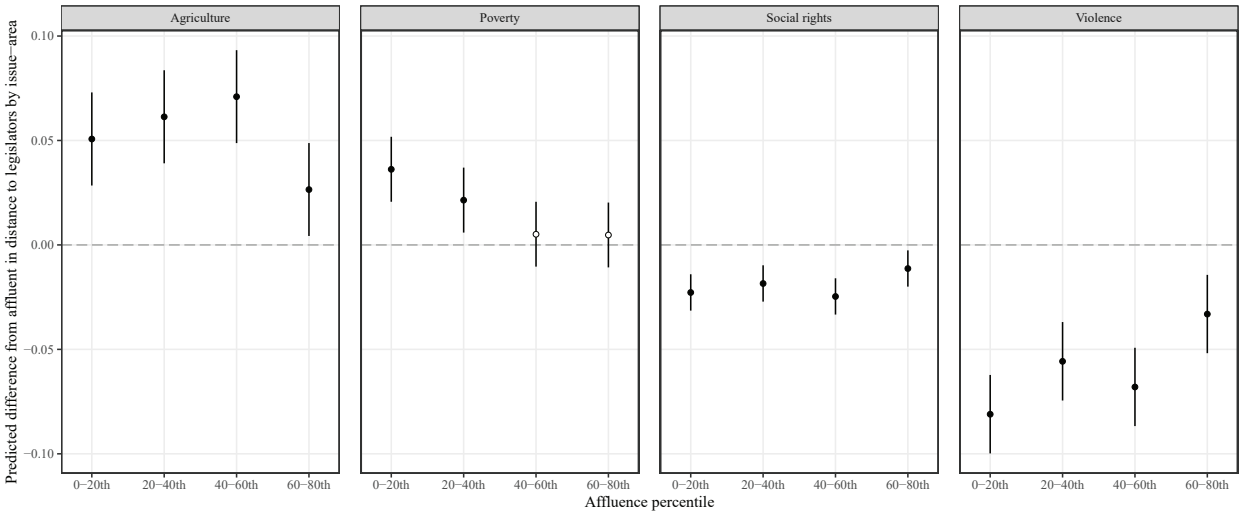

Figure 5: Affluence bias by issue-area in Africa. Dots represent estimates of the relationship between mass socioeconomic status and congruence on poverty, agriculture, social rights, and violence. The baseline is the most affluent quintile. Lines indicate 95% confidence intervals. See Table A6 of the online appendix for complete regression results.

along with the mean preference of legislators on economic issues by country.[24] For the sake of comparability, we normalize the average poor preference to zero for each country.

Quite intuitively, in nearly every country in the region, the rich on average prefer less state intervention in the economy than do the poor. The exceptions are Argentina and Honduras, where the difference between rich and poor is negligible. In most countries, legislators prefer even less state intervention in the economy than does the richest quintile, suggesting that their preferences are probably closer to the very affluent. Their rightward bias is particularly extreme in cases like Chile and Paraguay. In one case, Ecuador, legislators are in between the rich and the poor, but substantially closer to the rich. In only one case, Bolivia, do legislators on average prefer more state intervention in the economy than even the poor. But during the time-period for which we have data, Bolivia was governed by a populist leftist president who had written a new constitution that built in extraordinary electoral advantages for his leftist ruling party (Levitsky and Loxton

[24] These means pool across the matched samples we have for each country.

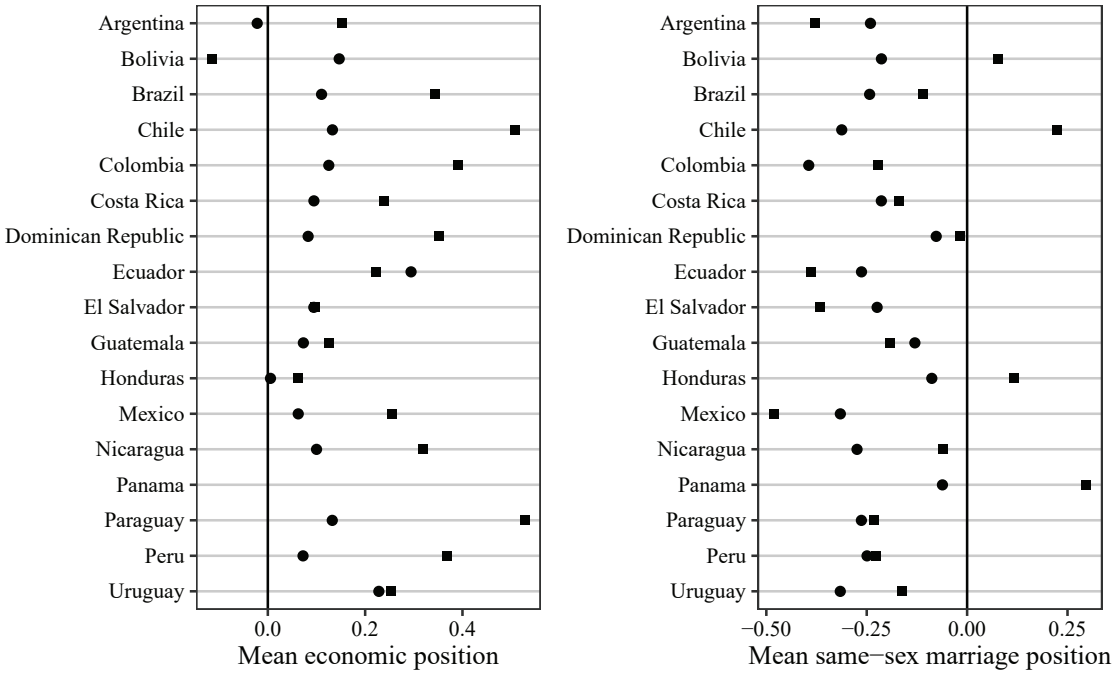

Figure 6: Mean economic and cultural preferences in Latin America. The left panel plots mean preferences on economic issues, while the right panel plots mean preferences on same-sex marriage. For each country, the mean preference of the poorest quintile of citizens is normalized to zero, with legislators' mean preference represented by squares and the richest quintile of citizens' mean preference in circles.

2013). So the unusual leftward bias in that case is unsurprising; indeed, had we had data on Venezuela for the same time period, we would have expected a similar pattern.

The righthand panel in Figure 6 plots mean preferences on same-sex marriage. Unlike on economic issues, more affluent citizens in every country in the region are more liberal when it comes to this cultural issue. Legislators, on the other hand, are either less supportive of same-sex marriage than the poor or somewhere between the poor and the rich on this issue.[25] Nearly a mirror image of the economic issues, in all but two countries, legislators' preferences are closer to those of the poor than to those of the rich. This result reinforces the limitations of focusing solely on left-right positions: the direction of the bias may depend on whether respondents have in mind

[25] This result parallels findings in the U.S. (Lax and Phillips 2009).

the economic or cultural dimension. Around the world, the poor appear to be most underrepresented on economic issues and overrepresented on cultural issues.

Unequal Representation and Democracy

A basic tenet of democracy is that citizens' preferences are equally reflected by their representatives. But recent research has raised doubts about whether modern electoral democracy fulfills this promise. Studies of the U.S. have arrived at a wide range of conclusions. Some find such consistent unequal representation that they conclude that, "America's claims to being a democratic society are seriously threatened" (Gilens and Page 2014: 577). Others find little or no evidence of inequality. A growing body of comparative research has been more consistent in finding unequal representation. And yet, these studies typically focus on one or a small subset of countries, and rely on different conceptions and measures of representation.

This paper takes a more global and inclusive approach than previous comparative work. Our aim is to employ a single conception of representation and a consistent type of data across the broadest sample of countries and years that we can. We want to be able to assess whether democracies around the world are delivering on the promise of equal representation for all.

Studying every available survey of national legislators matched with a mass opinion survey, we found that affluence bias is much more the norm than the exception. To be sure, some U.S. studies find that the rich—and only the rich—influence policymaking. Our comparative results are less damning. On average, our best guess is that middle-class citizens are somewhat less well represented than the rich, but our estimates are imprecise. The poor, on the other hand, seem to be significantly underrepresented in the average democracy. Representation may be *more* unequal in the U.S., but it is still unequal elsewhere. Around the world, less affluent citizens can expect their preferences to be less well reflected among their elected representatives than are the views of their more affluent neighbors.

We also find some evidence that the direction of inequality varies by issue domain. The preferences of the rich seem to be overrepresented in the area of economic policy, while the preferences of the poor appear to be overrepresented on cultural issues. In one sense, this is good news because it means that the poor are not *always* underrepresented. There is some comparative evidence that the poor and the rich may base their voting behavior on different issue domains (e.g., Calvo and Murillo 2019; De la O and Rodden 2008; Shayo 2009), so this may explain the divergence in unequal representation. If the rich care more about economic issues and the poor care more about cultural ones, then both social groups are getting what they want. On the other hand, there are also reasons to be skeptical about the extent to which this distribution of priorities is empirically accurate (e.g., Bartels 2006; Singer 2011). Whether differential priorities explain these inequalities in representation remains an open question.

There are other possible explanations for the differences we see across issue domains. One possibility is that conservative interest groups are stronger or more effective in both domains. On the economic side, labor union strength has declined around the world, often leaving corporate interest groups with a comparative advantage. On the cultural side, religious organizations like churches may have an advantage over those advocating more progressive positions. If elected representatives are in fact reflecting the preferences of strong interest groups rather than voters, we might expect them to take more conservative positions along both dimensions – consistent with what we find in Latin America. The fact that these positions reflect the preferences of rich voters along one dimension and those of poor voters along the other dimension would, in both cases, be coincidental.

Finally, it is possible that these different inequalities do not have a single, encompassing explanation. On the economic side, there are some obvious candidates for explaining why the rich are overrepresented, like campaign finance regulation, lower turnout among the poor, or poor people having less coherent or crystallized preferences – explanations some scholars have already examined in the U.S. context. Other possibilities, especially across countries, might include electoral institutions, economic conditions (most obviously economic inequality), government

ideology, and the role of civil society and organized interest groups (e.g., Bernauer et al. 2015; Klüver and Pickup 2019; Luna and Zechmeister 2005; Rasmussen and Reher 2019; Rosset et al. 2013).

Two additional explanations seem particularly plausible to us. One is that elected representatives misperceive the preferences of their constituents. Representatives' perceptions are an important link in the representational chain developed by Miller and Stokes (1963). There are reasons to think that with the spread of opinion polls, representatives' information about public preferences could be more accurate (Geer 1996), but there is also growing evidence of biases in how legislators and their staffs derive impressions of public opinion (Broockman and Skovron 2018; Butler 2014; Hertel-Fernandez et al. 2019). Another possibility is that elected representatives reflect better the preferences of the affluent because they themselves tend to be affluent, something that has recently received renewed attention (Carnes 2013; Carnes and Lupu 2015). We hope to study some of these explanations in future work.

Our analysis focuses on congruence and on collective representation, two among multiple other dimensions of the broad concept of democratic representation. As we note above, these choices are driven both by theoretical interest—theories of representation ascribe substantial normative significance to both congruence and collective representation—and empirical tractability, given our interest in taking a broad comparative perspective. This of course leaves open the possibility that the biases we uncover along these dimensions would not obtain if we were to focus instead on other dimensions, like responsiveness or median positions. Recent comparative studies and the documented relationships between legislator preferences and behavior make us skeptical of this possibility, but it is something future studies should examine. Even so, the fact that some groups of voters are consistently better represented in terms of congruence and collective representation is itself normatively troubling.

Comparative scholars ought to also take up a broader consideration of when and why representation becomes unequal. Our dataset includes all the available data, and more can be added as new elite surveys become available. We have used this large dataset to study inequalities

in representation across socioeconomic groups, but the data may well reveal other inequalities. Are men better represented than women? Are the preferences or urban residents better represented than those of rural residents? Are citizens living in some regions (e.g., capitals) or those from certain ethnic groups better represented? Our dataset can be used to evaluate a host of empirical questions on democratic representation beyond the ones we explore. Comparative studies of representation and congruence often focus on describing whole polities or on how institutions explain variation across countries. It is time we ask deeper questions about how and why modern democracies throughout the world represent citizens' preferences unequally.

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