Supplementary Information for: "Like 'em Rich? Public Perceptions and Opinions of Politicians' Wealth"

Contents

A1   Wealth in Candidate Pools in Brazil, Chile, and India

Figure A1 shows the distribution of wealth in recent national elections for the lower legislative chamber in Brazil, Chile, and India. The winners in all three countries are on average considerably wealthier than the losers (note that the x-axis is on a log scale).

Figure A1: Distribution of Candidate Wealth

A2   Survey Details

Our first U.S. survey was fielded through The American Social Survey (TASS) to a sample of 1,224 respondents drawn from the AmeriSpeak panel. The panel is sampled from the 2010 NORC National Frame, a multistage probability sample representative of the U.S. household population based on the comprehensive U.S. Postal Service address data supplemented by fieldwork from NORC. The AmeriSpeak panel is selected from sampling strata of the NORC National Frame based on the composition of race/ethnicity and age. While TASS is administered online, the recruitment and vetting procedures ensure a high-quality respondent pool.[1] Fielding took place in June 2023. Participants were compensated with the cash equivalent of $5.

Our second survey in the U.S. was fielded through YouGov, to a sample of 3,182 respondents drawn from YouGov's online panel. While YouGov's panel is not a probabilistic sample of the U.S. population, its sampling and weighting procedures have been shown to perform well in approximating the target population (Brick, 2011; Rivers, 2007). YouGov's sampling frame contains information on gender, age, race, education, party identification, political ideology, and political interest. The demographic portion is constructed by stratified sampling from the American Community Survey. Data on voter registration status and turnout are from the Current Population

---

[1]For more details, see: https://amerispeak.norc.org/content/dam/amerispeak/research/pdf/AmeriSpeak%20Technical%20Overview%202019%2002%2018.pdf.

Survey. Data on political interest and party identification are drawn from voter files and the Pew Religious Life Survey. YouGov quota-samples against this sampling frame, with additional selection through propensity score matching of the sociodemographic distribution of completed interviews to that of the sampling frame. Fielding took place in May 2024; participation in the survey was compensated with redeemable points worth about $3.

The surveys in Chile and Brazil were fielded through Netquest, an online survey company, to 1,200 respondents in each country. Respondents were drawn from Netquest's proprietary online panel.[2] Opt-in online respondent pools in Latin America are not representative of the target population (the voting-age public), with over-representation of individuals of higher socioeconomic status and in urban areas (Castorena et al., 2022). To improve representativeness, we employed quota sampling based on targets in terms of gender, education, age, and rural/urban area of residence based on census data and the marginal distributions from the 2021 round of AmericasBarometer, one of the highest-quality surveys in the region.[3] Fielding took place in November/December 2023 (Chile) and January 2024 (Brazil). Netquest compensates participants with Korus points (its digital coin), with the value varying depending on the individual's prior rate of participation in the panel.

The survey in India was fielded through the survey firm IBRureau[4] to 1,301 respondents. Because of lower internet penetration and greater socioeconomic inequalities than in the other three countries, we fielded interviews both online through IRBureau's online panel (70 percent of the sample), and face to face (30 percent). Similar to surveys in Chile and Brazil, the opt-in online pools in India are not representative of the voting-age national and state populations. Therefore, both online and face-to-face respondents were quota-sampled based on targets in terms of age, gender, caste, income, and urban/rural residence based on the marginal distributions from a combination of two sources: (1) the 2019 National Election Study Post Poll run by the Lokniti Programme at the Centre for the Study of Developing Societies (CSDS), an autonomous social science research institute in Dehli;[5] (2) The Indian Government's 2012 Employment and Unemployment National Sample Survey, the primary source of labor force data in India.[6] The offline respondents were sampled in the twelve largest Indian States (excluding Rajasthan due to logistical difficulties and safety concerns). As needed, the survey was fielded in English, Hindi, Tamil, Malyalam, Kannada, Telugu, and Bengali. The offline interviews were done on portable devices using the same interface as the online surveys. Fielding took place in November/December 2023. Online participants received 250 rupees (about $3); face-to-face participants received 750-800 rupees (about $9).

To further enhance the representativeness of our samples, in all our analyses, we use post-stratification weights. The weights were provided to us by TASS and YouGov. We developed the weights in the remaining surveys through raking against the marginal distributions of the variables used for sampling quotas.

To guard against concerns about the quality of responses in opt-in online surveys in the non-U.S. surveys, we included three open-ended textual questions as well as an item that mixed in real and fictitious response options (Kennedy et al., 2020). We used these items to continuously screen out and replace low-quality respondents during fielding. On average, approximately 7 percent of responses were screened out and replaced this way. In the offline portion in India, we required the

---

[2]https://www.netquest.com/en/online-surveys-investigation.
[3]See: https://www.vanderbilt.edu/lapop/chile/ABCHL2021-Technical-Report-v1.0-FINAL-eng-110921.pdf for Chile, and https://www.vanderbilt.edu/lapop/brazil/ABBRA2021-Technical-Report-v1.0-FINAL-eng-110921.pdf for Brazil.
[4]https://www.irbureau.com/.
[5]See: https://www.lokniti.org/media/PDF-upload/1579771857_30685900_download_report.pdf
[6]See: https://www.data.gov.in/catalog/employment-and-unemployment-national-sample-survey

interviewers to demonstrate a (de-identified) proof of identification for a random sample of 50% of face-to-face respondents.

A3 Conjoint Experiment Details

Table A1 shows the attribute values in each country, as well as the randomization probabilities for each attribute value. All attribute values were randomly assigned independently. In line with the recommendations by de la Cuesta, Egami and Imai (2022), we calibrated the attribute value assignment probabilities to approximate the distribution of attribute values in actual elections. To maximize the efficiency gains, the attribute values of candidate wealth were assigned with equal probability. The wealth values represent the deciles of the actual distribution of wealth among members of the national legislature in each country, based on the data from Klašnja and Motolinia (2025). For the remaining attributes, we used either the joint probabilities (as indicated in Table A1 below) or marginal probabilities, based on the relevant information on candidate pools in the most recent national legislative election. In the U.S. TASS survey, due to the complexity of the survey design, all attribute values were assigned with equal probability. To align the attribute value distributions with the other surveys, we developed weights through raking against the joint and marginal distributions of attribute values used in the YouGov survey. These weights are applied in all of our conjoint analyses.

To decrease artificiality, respondents were shown either a name (U.S., Chile, and India) or an image (Brazil) that conveyed their gender and ethnicity/race (or religion in India). Prior research has demonstrated the validity and effectiveness of this approach (Butler and Homola, 2017; Chauchard, Klašnja and Harish, 2019). In the U.S., we used the names from the list generated by Butler and Homola (2017), who validated the names' utility in unambiguously distinguishing gender (male/female) and race (White/Black/Hispanic). In Chile, based on the full list of candidates for the last three national legislative elections, as well as the most common names among the Mapuche (the largest indigenous group in Chile) from Corvalan (2024), we compiled a list of 200 common Chilean names. In India, we compiled a list of 70 names conveying gender and religion (Hindu/Muslim) based on the full list of candidates for the last two federal and state legislative elections. Indian names do not unequivocally distinguish caste, and so we added the caste attribute directly. Finally, because names in Brazil straddle racial and gender lines, we developed a set of 26 images using two AI facial generation applications.[7] The images showed fictitious candidates of similar age and varied their skin color and gender. The images are available at this link.

Within each pair of profiles, the name or image was drawn randomly without replacement, so that the two candidates could not have the same name or image within or across tasks. Except in the U.S., where party primaries make evaluating candidates affiliated with the same party natural, the party attribute was also drawn without replacement for each task, so that the two candidates could not belong to the same party. Except for the name or photograph, which was always shown first, the order of the attributes was randomized across respondents, but kept fixed across the tasks for each respondent.[8]

Figure 1 in the text showed the sample conjoint profiles in the TASS survey. Figure A2 shows the sample profiles in the remaining surveys.

---

[7]https://generated.photos/face-generator and https://www.playform.io/facemix.
[8]Due to miscommunication with the survey firm, the attributes in the conjoint in India were shown in fixed order.

Table A1: Conjoint Experiment Profile Attribute Values and Randomization Probabilities

Attribute	Value (Randomization probability)
US	
Name	From a list of names in Butler and Homola (2017) (White/male 60%, White/female 13%, Black/male 13%, Black/female 5%, Hispanic/male 5%, Hispanic/female 4%)
Party	Democrat (50%), Republican (50%)
Occupation	Lawyer (45%), Accountant (8%), Teacher (8%), State Legislator (5%), CEO (16%), Surgeon (8%), Farmer (5%), Small business owner (5%)
Wealth	$120,000 (11%); $350,000 (11%); $625,000 (11%); $1 million (11%); $1.5 million (11%); $3 million (11%); $4.75 million (11%); $10 million (11%); $35 million (11%)
Education	Ivy-League University (15%), State University (50%), Small College (30%), Community College (5%)
Religion	Protestant (30%), Catholic (30%), Evangelical Protestant (30%), None (10%)
India	
Name	From a list of 70 names (male names 80%, female 20%; Hindu 95%, Muslim 5%)
Party	BJP (45%), INC (40), BSP (10%), Aam Aadmi Party (5%)
Occupation	Advocate (15%), Chartered accountant (10%), Teacher (10%), MLA (20%), CEO (15%), Surgeon (10%), Farmer (10%), Small business owner (10%)
Wealth	90 lakh rupees (11%), 2 crore (11%), 3 crore (11%), 4 crore (11%), 5 crore (11%), 8 crore (11%), 15 crore (11%), 30 crore (11%), 50 crore (11%)
Education	10th Pass (10%), 12th Pass (15%), Graduate (50%), Postgraduate (25%)
Caste	General (45%), OBC (30%), ST (10%), SC (15%)
Brazil	
Image	From a set of 26 images (Branco 30%, Branca 15%, Pardo 25%, Parda 10%, Preto 5%, Preta 5%, Amarelo 2.5%, Amarela 2.5%, Indigeno 2.5%, Indigena 2.5%)
Party	Partido dos Trabalhadores (20%), Partido Liberal (25%), União Brasil (15%), Progressistas (10%), Movimento Democrático Brasileiro (10%), Republicanos (10%), Partido da Social Democracia Brasileira (10%)
Occupation	Agvogado (15%), Contador (15%), Professor de educação básica (7.5%), Legislador estadual (20%), CEO (15%), Cirurgião (10%), Agricultor (7.5%), Pequent empresário (15%)
Wealth	R$150.000 (11%); R$350.000 (11%); R$550.000 (11%); R$700.000 (11%); R$1 milhões (11%); R$1,3 milhões (11%); R$1,75 milhões (11%); R$2,5 milhões (11%); R$4,5 milhões (11%)
Education	Ensino Médio (30%), Ensino Técnico Profissionalizante (15%), Universidade (45%), Pós-graduação (10%)
Religion	Protestante (20%), Católico (65%), Evangélico (10%), Ninguna (5%)
Chile	
Image	From a list of 200 names (Male/Non-Mapuche 50%, Female/Non-Mapuche 40%, Male/Mapuche 5%, Male/Mapuche 5%)
Party	Renovación Nacional (15%), Unión Demócrata Independiente (15%), Partido Socialista de Chile (15%), Partido Republicano (10%), Partido Demócrata Cristiano (15%), Partido Comunista de Chile (5%), Independiente (25%)
Occupation	Abogado (15%), Contador (15%), Maestro de escuela (10%), Consajero regional (10%), CEO (15%), Cirujano (10%), Agricultor (10%), Propietario de una pequeña empresa (15%)
Wealth	$5 millones (11%); $10 millones (11%); $30 millones (11%); $40 millones (11%); $55 millones (11%); $80 millones (11%); $150 millones (11%); $200 millones (11%); $350 millones (11%)
Education	Universidad tradicional (25%), Universidad Privada No tradicional (25%), Instituto Profesional (25%), Centro de Formación Técnica (25%)
Religion	Protestante (15%), Católico (70%), Evangélico (10%), Ninguna (5%)

A4 Balance and Diagnostic Tests

Table A2 shows the balance tests for the demographic variables in our surveys with respect to the randomized order of the two experimental arms. The fourth column shows the p-value from the two-sample Wilcoxon rank-sum test, with the null hypothesis that the samples are from populations with the same distribution. Large p-values mean the inability to reject this null (at standard

Figure A2: Sample Conjoint Profiles

(a) U.S. YouGov

Candidate A	Name	Candidate B
Caitlin Schneider	**Name**	Terrance Booker
Surgeon	**Occupation**	Teacher
State University	**Education**	Small College
Catholic	**Religion**	Catholic
Republican	**Party**	Republican
$35 million	**Non-real estate family wealth**	$4.75 million

(b) India

	Candidate A	Candidate B
Name	Mousumi Das	Meena Bharti
Party	BJP (Bharatiya Janata Party)	BSP (Bahujan Samaj Party)
Occupation	CEO (Chief Executive Officer)	MLA (Member of the Legislative Assembly)
Wealth	4 crore rupees	90 lakh rupees
Education	Graduate	12th Standard Pass
Caste	OBC	General/Upper

(c) Chile

Candidato A	Características	Candidato B
Marisol Jara	**Nombre**	Teresa Castillo
Abogado	**Ocupación**	CEO
$40 millones	**Riqueza Familiar**	$55 millones
Partido Demócrata Cristiano	**Partido**	Unión Demócrata Independiente
Católico	**Religión**	Católico
Centro de Formación Técnica	**Educación**	Universidad tradicional

(d) Brazil

Candidato A	Características	Candidato B
	Foto	
Advogado	**Ocupação**	professor de educação básica
Católico	**Religião**	Protestante
R$350.000	**Riqueza Familiar**	R$150.000
Ensino Médio	**Educação**	Ensino Técnico Profissionalizante
Republicanos	**Partido**	Partido dos Trabalhadores

levels of significance). The final two columns show the lower- and upper-bound p-values from the two-sample equivalence rank sum test, with the null hypothesis that samples were drawn from populations different by at least plus (upper bound) or minus (lower bound) the level of tolerance defined in units of the test statistic's distribution $\epsilon = 3$. Low p-values indicate the rejection of this null in favor of equivalence within the tolerance. The table shows that in the large majority of cases, a combined inference based on the standard and equivalence tests suggests balance, implying successful randomization.

Table A3 shows the balance tests with respect to the randomization of the information treatment in the information experiment. The table shows the p-values for the same test statistics as in the previous table.

Table A4 shows the balance tests with respect to the randomization of each attribute in the conjoint experiment. The cells represent the p-values from a joint Wald test of a demographic variable indicated in a row on the set of all the values of a conjoint attribute indicated in a column. Large p-values indicate a failure to reject the null hypothesis of the attribute values as a group not being jointly predictive of the variation in the demographic variable. This is by and large the case, implying a successful randomization of attributes in the conjoint design.

Table A2: Balance Tests: Experiment Order

	N	Mean, Info. exp. 1st	Mean, Info. exp. 2nd	Difference	Ranksum p-value	Equivalence lower p-value	Equivalence upper p-value
Female	4,928	0.51	0.49	-0.01	0.23	0.04	0.00
Age: 18-24	4,928	0.13	0.13	-0.00	0.42	0.00	0.01
Age: 25-34	4,928	0.22	0.22	0.00	0.35	0.00	0.02
Age: 35-44	4,928	0.21	0.19	-0.02	0.23	0.04	0.00
Age: 45-54	4,928	0.18	0.20	0.02	0.13	0.00	0.07
Age: 55+	4,928	0.27	0.26	-0.00	0.09	0.09	0.00
Education: Less than HS	4,928	0.19	0.20	0.01	0.38	0.00	0.02
Education: HS	4,928	0.23	0.26	0.03	0.04	0.00	0.18
Education: Some college	4,928	0.21	0.20	-0.02	0.19	0.05	0.00
Education: College	4,928	0.18	0.15	-0.03	0.09	0.10	0.00
Education: Post-graduate	4,928	0.18	0.18	0.01	0.75	0.00	0.00
Work status: Working	4,928	0.59	0.57	-0.02	0.61	0.01	0.00
Work status: Not working but w/ job	4,928	0.03	0.03	-0.00	0.14	0.00	0.06
Work status: Not working but looking	4,928	0.07	0.07	0.00	0.78	0.00	0.00
Work status: Retired	4,928	0.11	0.12	0.01	0.56	0.01	0.00
Work status: Other	4,928	0.20	0.21	0.01	0.51	0.00	0.01
Marital status: Married	4,928	0.58	0.60	0.01	0.38	0.00	0.02
Marital status: Single	4,928	0.29	0.29	-0.00	0.87	0.00	0.00
Marital status: Divorced	4,928	0.05	0.05	-0.00	0.26	0.03	0.00
Marital status: Separated	4,928	0.04	0.04	-0.00	0.38	0.02	0.00
Marital status: Widowed	4,928	0.03	0.03	-0.00	0.63	0.01	0.00
Religion: Majority	4,912	0.48	0.48	0.01	0.49	0.00	0.01
Religion: 2nd largest	4,912	0.12	0.15	0.02	0.20	0.00	0.04
Religion: Other	4,912	0.15	0.13	-0.02	0.13	0.07	0.00
Religion: Unaffiliated	4,912	0.25	0.24	-0.01	0.53	0.01	0.00
Attend religious services: Never	4,928	0.36	0.37	0.01	0.84	0.00	0.00
Attend religious services: Rarely	4,928	0.18	0.17	-0.01	0.62	0.00	0.01
Attend religious services: Once per month	4,928	0.08	0.08	-0.01	0.92	0.00	0.00
Attend religious services: Once per week	4,928	0.17	0.17	0.00	0.24	0.03	0.00
Attend religious services: More than once per week	4,928	0.21	0.22	0.00	0.35	0.00	0.02
Race/ethnicity: Majority	4,928	0.53	0.52	-0.02	0.08	0.10	0.00
Race/ethnicity: Largest minority	4,928	0.25	0.26	0.01	0.62	0.00	0.01
Race/ethnicity: 2nd largest minority	4,928	0.11	0.14	0.03	0.00	0.00	0.57
Race/ethnicity: Other minority	4,928	0.11	0.09	-0.02	0.20	0.04	0.00
Urban	4,928	0.70	0.70	0.01	0.96	0.00	0.00
Income: Below median	4,928	0.51	0.47	-0.04	0.01	0.35	0.00
Income: Above median	4,928	0.22	0.22	0.01	0.66	0.00	0.01
Assets: Bottom quartile	4,928	0.21	0.19	-0.02	0.19	0.05	0.00
Assets: 2nd quartile	4,928	0.18	0.20	0.02	0.09	0.00	0.10
Assets: 3rd quartile	4,928	0.19	0.19	0.00	0.91	0.00	0.00
Assets: Top quartile	4,928	0.21	0.20	-0.01	0.35	0.02	0.00
Ideology: Very conservative	4,926	0.11	0.11	-0.01	0.68	0.00	0.00
Ideology: Somewhat conservative	4,926	0.19	0.19	0.00	0.39	0.00	0.02
Ideology: Moderate	4,926	0.28	0.29	0.01	0.66	0.00	0.01
Ideology: Somewhat liberal	4,926	0.24	0.23	-0.01	0.77	0.00	0.00
Ideology: Very liberal	4,926	0.19	0.18	-0.00	0.46	0.01	0.00
Supports incumbent party	4,928	0.37	0.39	0.02	0.22	0.00	0.04

Table A3: Balance Tests: Information Experiment

	N	Mean, Info. exp.: Control	Mean, Info. exp.: Treatment	Difference	Ranksum p-value	Equivalence lower p-value	Equivalence upper p-value
Female	4,928	0.50	0.51	0.01	0.70	0.00	0.00
Age: 18-24	4,928	0.13	0.13	0.01	0.61	0.00	0.01
Age: 25-34	4,928	0.23	0.21	-0.01	0.29	0.03	0.00
Age: 35-44	4,928	0.21	0.19	-0.02	0.04	0.16	0.00
Age: 45-54	4,928	0.18	0.19	0.01	0.31	0.00	0.02
Age: 55+	4,928	0.26	0.27	0.01	0.12	0.00	0.07
Education: Less than HS	4,928	0.19	0.20	0.01	0.85	0.00	0.00
Education: HS	4,928	0.24	0.25	0.01	0.85	0.00	0.00
Education: Some college	4,928	0.20	0.21	0.01	0.44	0.00	0.01
Education: College	4,928	0.17	0.16	-0.01	0.97	0.00	0.00
Education: Post-graduate	4,928	0.19	0.17	-0.02	0.40	0.02	0.00
Work status: Working	4,928	0.59	0.57	-0.01	0.51	0.01	0.00
Work status: Not working but w/ job	4,928	0.03	0.03	0.00	0.55	0.00	0.01
Work status: Not working but looking	4,928	0.07	0.07	0.00	0.88	0.00	0.00
Work status: Retired	4,928	0.11	0.11	0.00	0.56	0.00	0.01
Work status: Other	4,928	0.20	0.21	0.00	0.99	0.00	0.00
Marital status: Married	4,928	0.59	0.59	0.01	0.45	0.00	0.01
Marital status: Single	4,928	0.29	0.28	-0.01	0.26	0.03	0.00
Marital status: Divorced	4,928	0.04	0.06	0.02	0.01	0.00	0.39
Marital status: Separated	4,928	0.04	0.04	-0.01	0.12	0.07	0.00
Marital status: Widowed	4,928	0.03	0.03	-0.01	0.18	0.05	0.00
Religion: Majority	4,912	0.48	0.48	0.00	0.44	0.00	0.01
Religion: 2nd largest	4,912	0.14	0.13	-0.00	0.15	0.06	0.00
Religion: Other	4,912	0.14	0.14	0.00	0.64	0.01	0.00
Religion: Unaffiliated	4,912	0.25	0.25	-0.00	0.56	0.00	0.01
Attend religious services: Never	4,928	0.37	0.35	-0.01	0.80	0.00	0.00
Attend religious services: Rarely	4,928	0.17	0.17	-0.01	0.57	0.01	0.00
Attend religious services: Once per month	4,928	0.08	0.08	0.01	0.85	0.00	0.00
Attend religious services: Once per week	4,928	0.16	0.18	0.02	0.10	0.00	0.09
Attend religious services: More than once per week	4,928	0.22	0.21	-0.01	0.23	0.04	0.00
Race/ethnicity: Majority	4,928	0.53	0.53	0.00	0.38	0.00	0.02
Race/ethnicity: Largest minority	4,928	0.25	0.26	0.02	0.89	0.00	0.00
Race/ethnicity: 2nd largest minority	4,928	0.12	0.12	-0.00	0.76	0.00	0.00
Race/ethnicity: Other minority	4,928	0.10	0.09	-0.02	0.16	0.06	0.00
Urban	4,928	0.70	0.69	-0.01	0.95	0.00	0.00
Income: Below median	4,928	0.50	0.48	-0.02	0.66	0.01	0.00
Income: Above median	4,928	0.21	0.23	0.02	0.48	0.00	0.01
Assets: Bottom quartile	4,928	0.19	0.20	0.01	0.29	0.00	0.03
Assets: 2nd quartile	4,928	0.20	0.19	-0.01	0.56	0.01	0.00
Assets: 3rd quartile	4,928	0.20	0.18	-0.01	0.24	0.03	0.00
Assets: Top quartile	4,928	0.21	0.20	-0.01	0.99	0.00	0.00
Ideology: Very conservative	4,926	0.11	0.11	0.01	0.50	0.00	0.01
Ideology: Somewhat conservative	4,926	0.18	0.20	0.02	0.29	0.00	0.03
Ideology: Moderate	4,926	0.29	0.27	-0.02	0.42	0.01	0.00
Ideology: Somewhat liberal	4,926	0.24	0.23	-0.01	0.57	0.01	0.00
Ideology: Very liberal	4,926	0.18	0.19	0.01	0.93	0.00	0.00
Supports incumbent party	4,928	0.38	0.38	0.01	0.84	0.00	0.00

Table A4: Balance Tests: Conjoint Experiment

	Wealth	Gender	Race/Ethnicity	Occupation	Education	Religion
Female	0.42	0.51	0.67	0.24	0.77	0.32
Age: 18-24	0.87	0.83	0.69	0.32	0.01	0.09
Age: 25-34	0.32	0.50	0.69	0.86	0.18	0.14
Age: 35-44	0.66	0.54	0.25	0.55	0.64	0.81
Age: 45-54	0.68	0.20	0.03	0.82	0.11	0.51
Age: 55+	0.76	0.92	0.46	0.97	0.00	0.53
Education: Less than HS	0.63	0.14	0.62	0.08	0.02	0.24
Education: HS	0.99	0.34	0.88	0.09	0.45	0.44
Education: Some college	0.34	0.14	0.38	0.20	0.51	0.46
Education: College	0.44	0.72	0.12	0.63	0.24	0.42
Education: Post-graduate	0.94	0.62	0.06	0.25	0.51	0.70
Work status: Working	0.54	0.97	0.14	0.01	0.76	0.52
Work status: Not working but w/ job	0.32	0.61	0.26	0.42	0.95	0.23
Work status: Not working but looking	0.11	0.50	0.05	0.02	0.56	0.31
Work status: Retired	0.94	0.13	0.42	0.09	0.00	0.46
Work status: Other	0.90	0.45	0.85	0.03	0.55	0.05
Marital status: Married	0.45	0.23	0.12	0.19	0.03	0.09
Marital status: Single	1.00	0.17	0.04	0.45	0.57	0.14
Marital status: Divorced	0.34	0.76	0.88	0.19	0.01	0.65
Marital status: Separated	0.16	0.70	0.01	0.33	0.02	0.77
Marital status: Widowed	0.70	0.45	0.07	0.37	0.13	0.34
Religion: Majority	0.51	0.78	0.60	0.31	0.55	0.60
Religion: 2nd largest	0.47	0.48	0.31	1.00	0.51	0.39
Religion: Other	0.33	0.85	0.04	0.02	0.16	0.65
Religion: Unaffiliated	0.57	0.43	0.06	0.94	0.41	0.72
Attend religious services: Never	0.69	0.71	0.09	0.01	0.41	0.90
Attend religious services: Rarely	0.20	0.53	0.01	0.00	0.97	0.60
Attend religious services: Once per month	0.13	0.62	0.97	0.42	0.18	0.50
Attend religious services: Once per week	0.38	0.24	0.15	0.40	0.09	0.95
Attend religious services: More than once per week	0.03	0.92	0.25	0.27	0.72	0.48
Race/ethnicity: Majority	0.06	0.09	0.64	0.61	0.00	0.41
Race/ethnicity: Largest minority	0.07	0.30	0.90	0.27	0.00	0.42
Race/ethnicity: 2nd largest minority	0.89	0.99	0.72	0.16	0.09	0.93
Race/ethnicity: Other minority	0.83	0.23	0.85	0.89	0.76	0.76
Urban	0.76	0.78	0.03	0.00	0.34	0.57
Income: Below median	0.82	0.55	0.00	0.00	0.32	0.41
Income: Above median	0.60	0.94	0.86	0.45	0.83	0.42
Assets: Bottom quartile	0.36	0.74	0.89	0.10	0.55	0.75
Assets: 2nd quartile	0.51	0.57	0.28	0.49	0.24	0.69
Assets: 3rd quartile	0.27	0.90	0.60	0.01	0.76	0.65
Assets: Top quartile	0.48	0.89	0.83	0.07	0.25	0.89
Ideology: Very conservative	0.62	0.78	0.72	0.88	0.20	0.14
Ideology: Somewhat conservative	0.58	0.61	0.08	0.00	0.77	0.77
Ideology: Moderate	0.32	0.29	0.59	0.90	0.25	0.08
Ideology: Somewhat liberal	0.20	0.80	0.38	0.68	0.96	0.02
Ideology: Very liberal	0.76	0.48	0.68	0.76	0.84	0.54
Supports incumbent party	0.19	0.24	0.01	0.11	0.13	0.23

Table A5 shows the diagnostic tests with respect to the order of the wealth attributes in the conjoint, addressing the question as to whether the wealth AMCEs vary with respect to the order in which the wealth attribute appeared in a conjoint profile. The cells are the p-values from a joint Wald test of the difference in a wealth AMCE for a candidate in a particular wealth quintile (relative to the candidate in the lowest wealth quintile) when wealth is ordered in the first position after the profile name or image, and any other position. In other words, the Wald test evaluates the significance of all the interaction terms between a wealth AMCE and the variable indicating the wealth attribute's order in a conjoint profile. Large p-values indicate a failure to reject the null hypothesis of no wealth attribute order effects. The different columns test the order effects for each outcome variable we examined in the paper. The results indicate no presence of attribute order effects.

Table A5: Balance Tests: Conjoint Experiment Wealth Attribute Order Effects

	Candidate choice	Clientelism	Effectiveness	Lobby capture
Candidate wealth: 2nd quintile	0.62	0.53	0.24	0.61
Candidate wealth: 3rd quintile	0.34	0.68	0.35	0.17
Candidate wealth: 4th quintile	0.77	0.62	0.60	0.75
Candidate wealth: Top quintile	0.73	0.33	0.15	0.45

Table A6 shows the diagnostic tests with respect to the order of the tasks in the conjoint, addressing the question as to whether the wealth AMCEs vary with respect to the task the respondent was performing. In other words, we examine whether our respondents changed their response to the wealth attributes as they repeated the conjoint tasks (five times in the non-YouGov surveys, three times in the YouGov survey). The cells represent the p-values from the same type of test as in the previous table, evaluating the significance of all the interaction terms between a wealth AMCE and the variable indicating the task number. The different columns again test the task order effects for each outcome variable. The results indicate no presence of task order effects.

Table A6: Balance Tests: Conjoint Experiment Task Order Effects

	Candidate choice	Clientelism	Effectiveness	Lobby capture
Candidate wealth: 2nd quintile	0.92	0.60	0.64	0.13
Candidate wealth: 3rd quintile	0.30	0.96	0.89	0.09
Candidate wealth: 4th quintile	0.42	0.75	0.91	0.60
Candidate wealth: Top quintile	0.46	0.26	0.38	0.11

Finally, Table A7 evaluates the presence of profile order effects – whether respondents reacted differently to a particular wealth attribute when it featured in the first profile compared to the second profile. The cells show the p-values of the interaction term between a wealth AMCE and an indicator of the profile order in which the attribute value appeared. The different columns again test the order effects for each outcome variable. The results indicate no presence of profile order effects.

Table A7: Balance Tests: Conjoint Experiment Profile Order Effects

	Candidate choice	Clientelism	Effectiveness	Lobby capture
Candidate wealth: 2nd quintile	0.58	0.57	0.86	0.10
Candidate wealth: 3rd quintile	0.17	0.31	0.36	0.12
Candidate wealth: 4th quintile	0.70	0.41	0.67	0.64
Candidate wealth: Top quintile	0.15	0.45	0.07	0.65
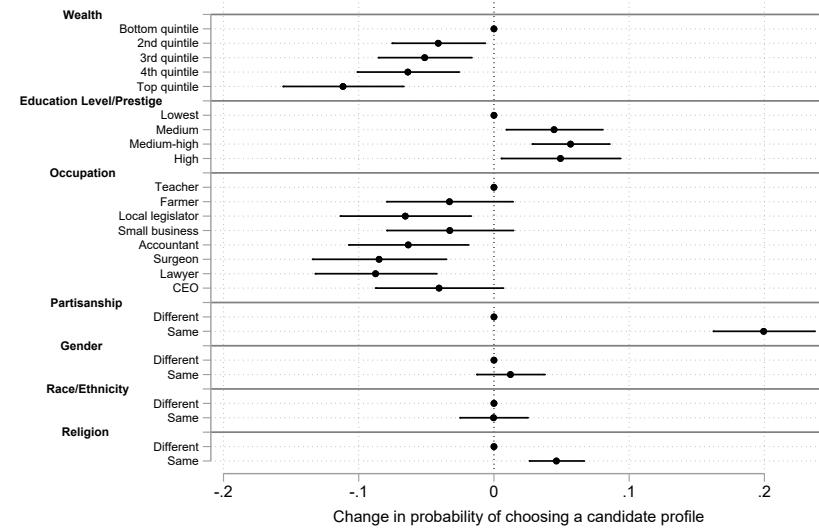
A5 Additional Results

A5.1 Conjoint Results by Country
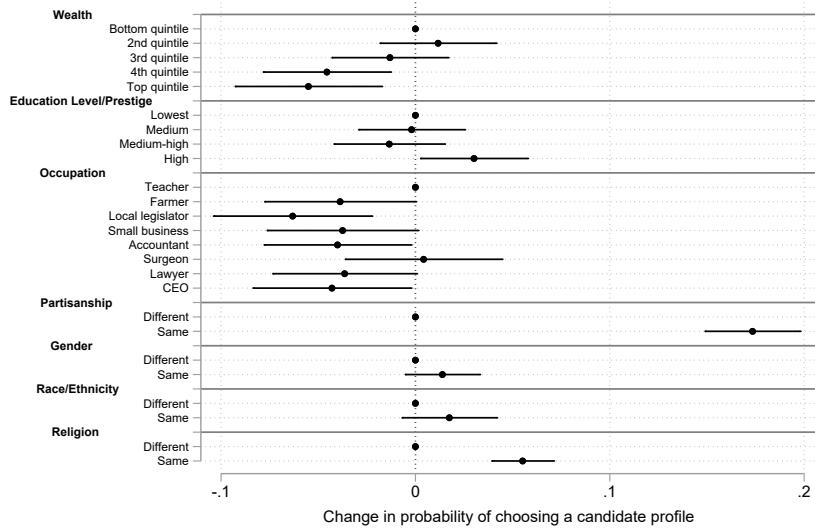
Figure A3 shows the candidate choice results from the conjoint experiment for each country. While the wealth AMCEs vary in magnitude, in each country, the wealthier candidates are less preferred to the less wealthy candidates. For example, compared to candidates in the bottom quintile of wealth, the vote share of the hypothetical candidates in the top quintile is lower by 11 percentage points in Brazil, 5.5 points in Chile, 10 points in India, and 12.5 points in the U.S. (all statistically significant at $p < .01$).

Figure A3: Conjoint Results by Country
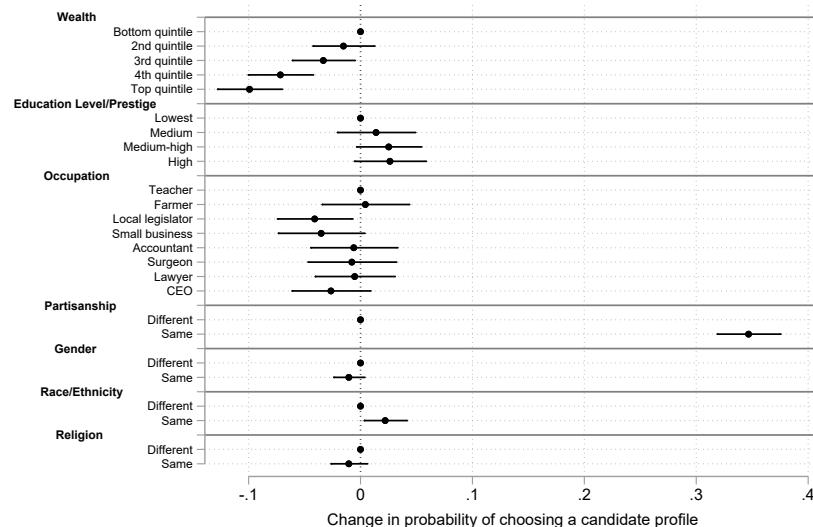
(a) Brazil
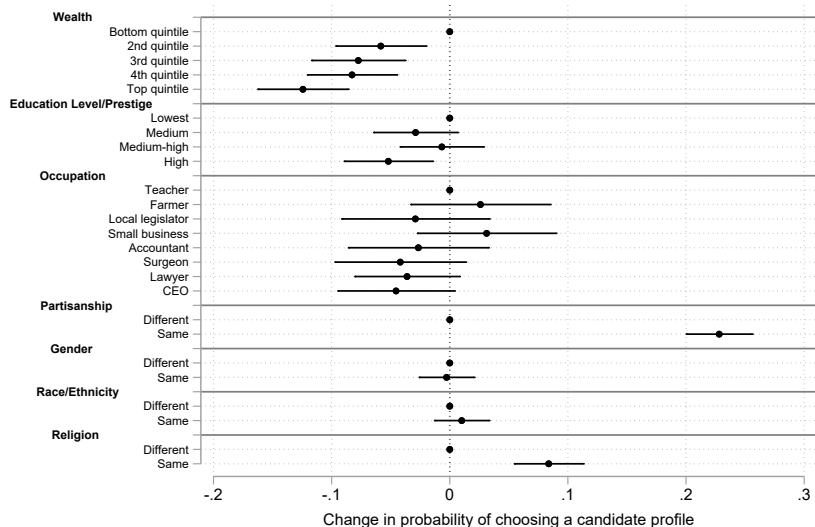
(b) Chile

(c) India

(d) US
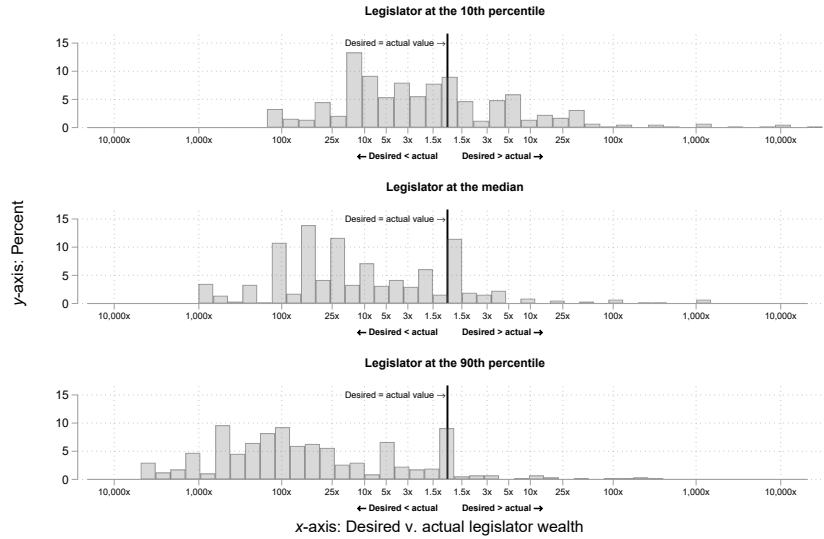
A5.2   Guesses and Desired Legislator Wealth Distributions by Country
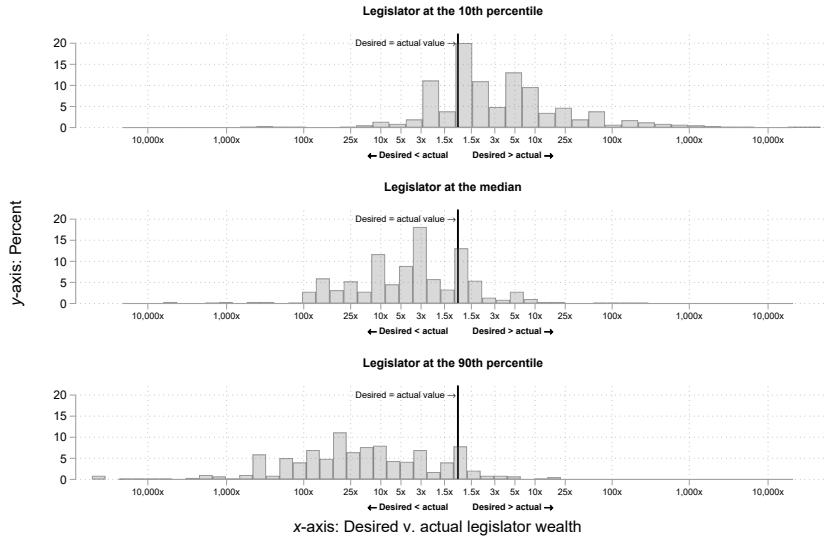
Figure A4 disaggregates Figure 3 from the paper by country, showing the desired wealth of legislators at the 10th, 50th, and 90th percentiles of the legislature's wealth distribution for respondents who were treated with the information about the legislators' actual wealth. The vertical line in each panel represents such legislators' actual wealth. The conclusions drawn in the paper, that respondents generally prefer lower wealth for legislators at the 50th and 90th percentiles apply to all four countries.

Figure A4: Desired Legislator Wealth Distributions by Country

(a) Brazil

x-axis: Desired v. actual legislator wealth

(b) Chile

x-axis: Desired v. actual legislator wealth

(c) India

x-axis: Desired v. actual legislator wealth

(d) US

x-axis: Desired v. actual legislator wealth
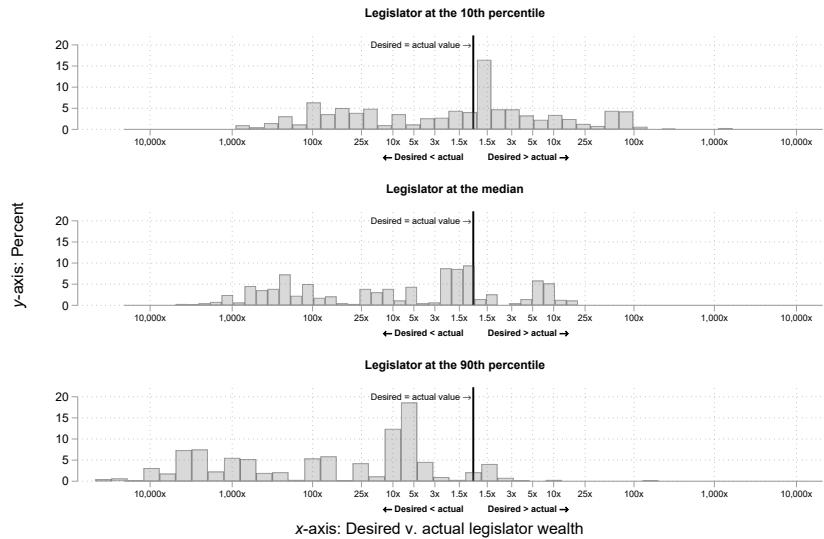
Figure A5 disaggregates Figure 9 from the paper by country, showing the respondents' guesses of legislators' wealth at the 10th, 50th, and 90th percentiles of the legislature's wealth distribution. The vertical line in each panel represents such legislators' actual wealth. The conclusions drawn in the paper, that respondents generally underestimate their legislators' wealth, apply to all four countries (except in the case of the national legislator at the 10th percentile of the distribution of wealth in Chile and India).
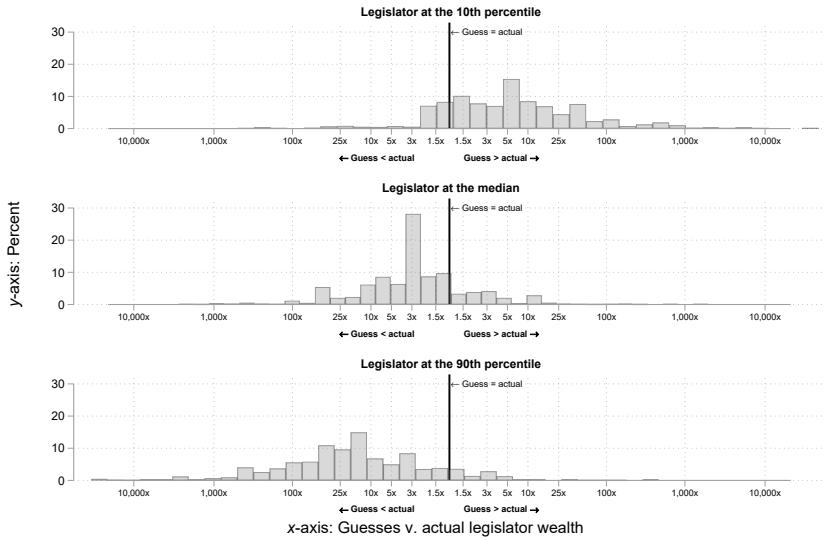
Figure A5: Respondents' Guesses vs. Actual Wealth of National Legislators
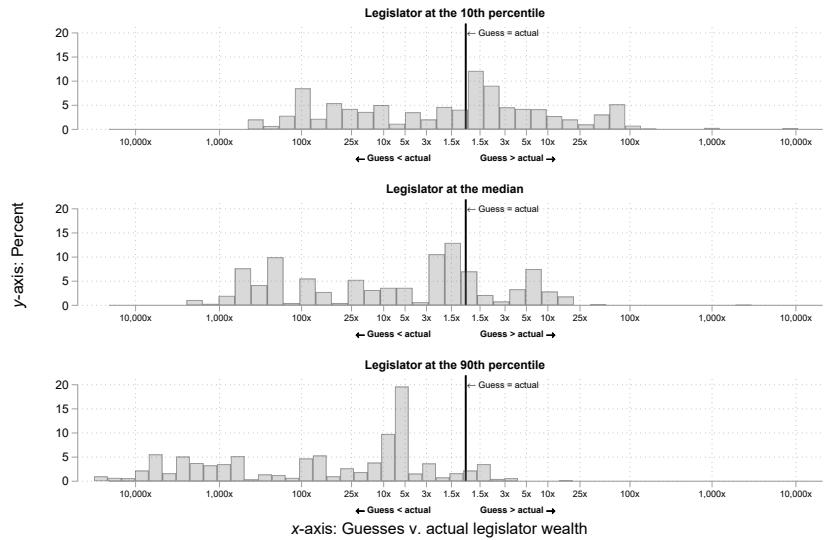
(a) Brazil
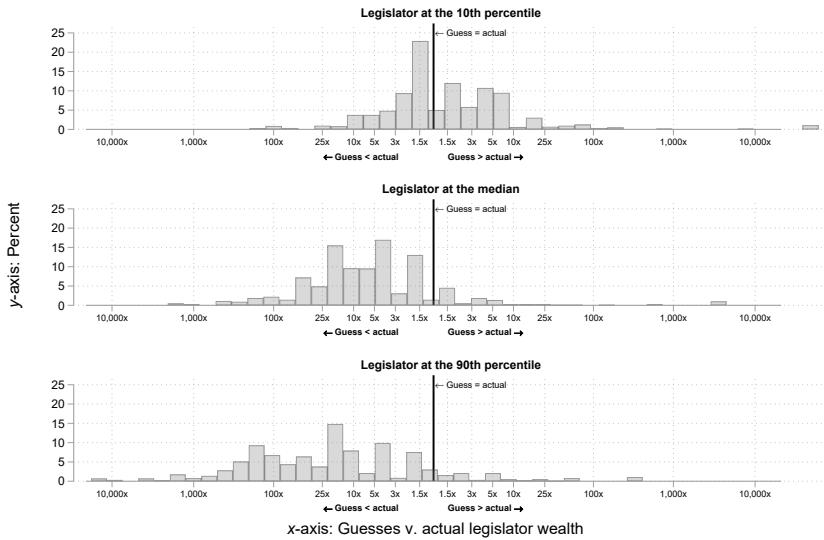
(b) Chile

(c) India

(d) US

A5.3 Formal Tests for Figures 3 and 9

Table A8 shows the formal, one-sample difference in means t-test results for the patterns in Figures 3 (column 1) and 9 (column 2), respectively. The cells show the one-tailed p-values. The results support the inferences made in the paper.

Table A8: t-Test Results for Figures 3 and 9

	Desired v. actual legislators' wealth	Actual v. guessed legislators' wealth
Legislator at the 10th percentile	0.01 (0.05)	0.32*** (0.04)
Legislator at the median	-2.02*** (0.05)	-1.83*** (0.04)
Legislator at the 90th percentile	-3.66*** (0.05)	-3.46*** (0.04)
Average across scenarios	-1.85*** (0.05)	-1.59*** (0.04)

Note: *p<0.1; **p<0.05; ***p<0.01.
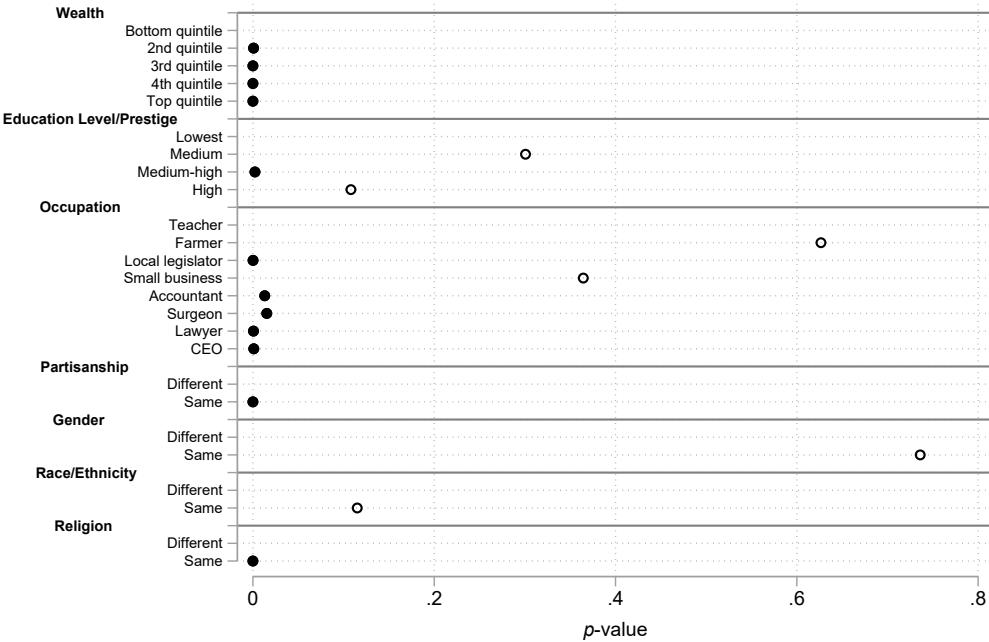
A5.4 Adjusting Conjoint Results for Multiple Testing and Measurement Error

Because we have many treatments in the conjoint experiment, some of the AMCEs we report as statistically significant may be false positives due to the large number of hypothesis tests (Liu and Shiraito, 2023). Figure A6 shows the results of the application of the Benjamini-Hochberg multiple-comparison correction (Benjamini and Hochberg, 1995). The correction controls the false discovery rate, by ordering the p-values of all the AMCEs from lowest to highest and designating as statistically significant at a selected α level only those p-values that satisfy the condition $p_k \leq \frac{k}{m}\alpha$, where k is the rank of each p-value, and m is the number of AMCEs. The procedure thus makes it increasingly harder to pass the α significance threshold as the number of tests grows. We adopt $\alpha = 0.05$. The dots shown in Figure A6 are the p-values of all the AMCEs in our main conjoint analysis. Solid dots indicate the AMCEs that remain significant at $\alpha \leq .05$ after the correction; hollow dots indicate the AMCEs that are not statistically significant according to this correction. As seen in the figure, all of our key treatment effects remain statistically significant based on this correction.

Since conjoint experiments are high-intensity response tasks, our data may be subject to measurement error from inattentive respondents. In line with recommendations by Clayton et al. (2023), in addition to the five ratings tasks, we included in our surveys in Chile, Brazil and India a sixth task in which the two candidate profiles were identical to the profiles each respondent received in the first task, only with the order of the profiles reversed.[9] Using this design, we estimate that the measurement error rate is 12.6% (i.e. that share of respondents gave different answers to outcome questions following the otherwise identical paired profiles). Using this rate, Figure A7 adjusts the

---

[9]We did not include this option in the U.S. TASS survey because we became aware of Clayton et al. (2023) after the survey was already finalized and set for fielding and could not be modified. We omitted this option from the U.S. YouGov survey due to space constraints. We apply the estimate of the measurement error from the other country surveys to the conjoint responses in the U.S. as well.

Figure A6: Multiple Hypothesis Testing Adjustment for Main Conjoint Results

point estimates and confidence intervals using the approach recommended by Clayton et al. (2023) (the confidence intervals are derived from 500 bootstrapped samples clustered by respondent). The results are substantively unchanged, with the wealth AMCEs being somewhat larger in (absolute) magnitude.
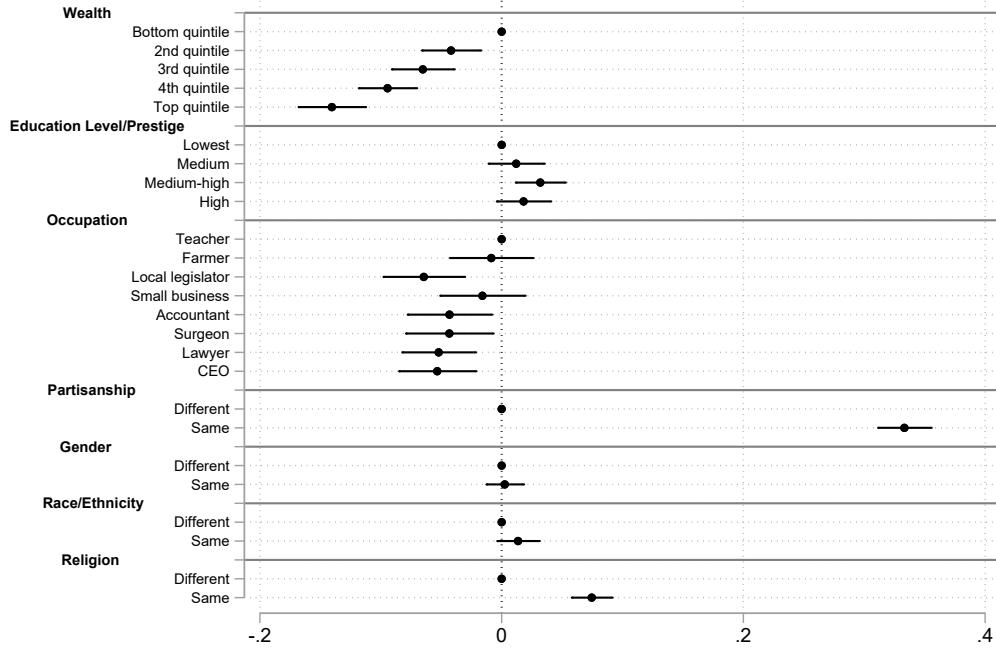
A5.5 Effects of Surprising Wealth

As outlined in hypothesis H1c in the paper, if wealth acts as a cue for other desirable qualities, it may be particularly effective when it is less expected. That is, a wealthy candidate may be perceived as especially worthy if their wealth signals an ability to overcome otherwise greater challenges in attaining economic success. We evaluate this possibility by comparing support (Figure A8) and perceptions of effectiveness in enacting legislation (Figure A9), between profiles for whom wealth may plausibly be more or less surprising. In particular, the left panel of each figure compares candidates from higher- and lower-earning professions (we code as higher-earning the occupations of lawyer, surgeon and CEO). We apply the same logic to profiles with higher v. lower education or educational prestige[10] (middle panel in each figure), and to male v. female profiles (right panel in each figure).

Figure A8 shows some, but inconsistent evidence of the effect of surprising wealth on candidate choice. The wealthiest women candidates do receive a higher vote share than the wealthiest men, but we see similar gender patterns for other wealth levels, too. Moreover, less wealthy men are still clearly preferred to wealthy women, not just wealthy men. The patterns for occupation and

---

[10]In Brazil and India, we code as lower education the profiles with high school or less, and as high education the profiles with a post-graduate degree. In US and Chile, we code as lower educational prestige the profiles from community college (U.S.) or Centro de Feormacíon Téchnica (Chile), and with high prestige the profiles with degrees from an Ivy League institution (US) or Universidad tradicional (Chile).

Figure A7: Measurement Error Adjustment for Main Conjoint Results

education are similar, and even less distinct in terms of differences among potentially surprising profiles.

Figure A9 shows even fewer differences in terms of perceptions of a candidate's effectiveness in enacting legislation. If anything, wealthier men are seen as more effective than wealthier women, but by and large, the differences are minor and not statistically distinguishable from zero. In sum, there is no evidence that potentially surprising wealth serves as a strong signal for effectiveness, and thus an inferential cue that drives electoral support.

In addition to examining its substantive effects, we may worry that our respondents' negative reactions to candidate wealth may in part be an artifact of seeing profiles with surprising wealth. For example, respondents may be outraged to see a profile of a candidate who is a teacher and has millions of dollars in assets. Seeing such a profile may in turn magnify their negative response to any other wealthy candidate. While we are skeptical that such carryover effects may exist given that we see no profile order effects (Table A7 above), we examine in Figure A10 our key results when excluding the surprising profiles, as defined in Figures A8 and A9. The results are substantively unchanged.

A5.6 Wealth as Signal of Clientelistic Benefits

Around the world, candidates and parties frequently distribute particularistic benefits to individuals in the hopes of securing their electoral support and signaling electoral viability and commitment to particular voter groups or constituencies (Aspinall et al., 2022; Kramon, 2017). Such clientelism is costly, and the public may reasonably expect wealthier candidates to be better positioned to afford it (Justesen and Markus, 2024; Sircar, 2018). To the extent that the public is receptive to the

**Figure A8:** Effects of Surprising Wealth on Candidate Choice

x-axis: Probability of supporting a candidate profile
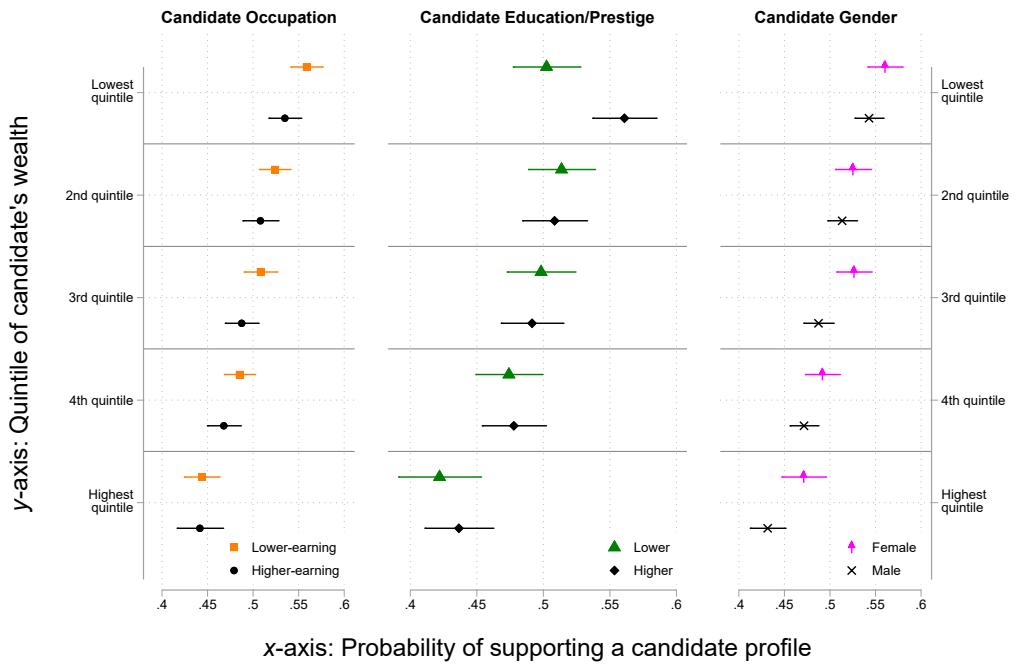
Figure A9: Effects of Surprising Wealth on Perceived Candidate Effectiveness

*x*-axis: Perceived probability of being an effective legislator
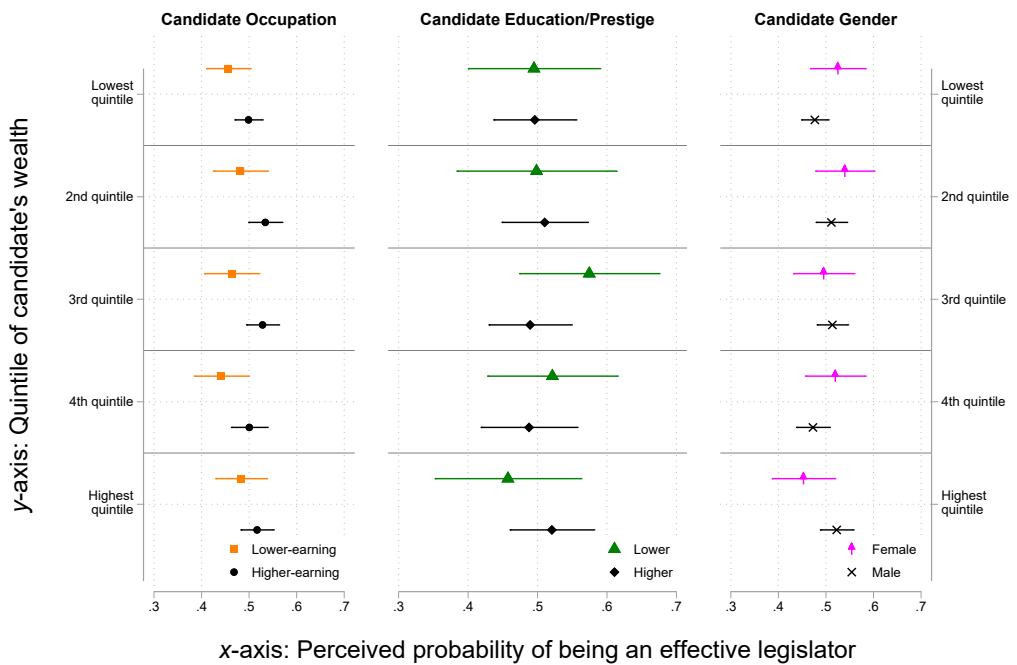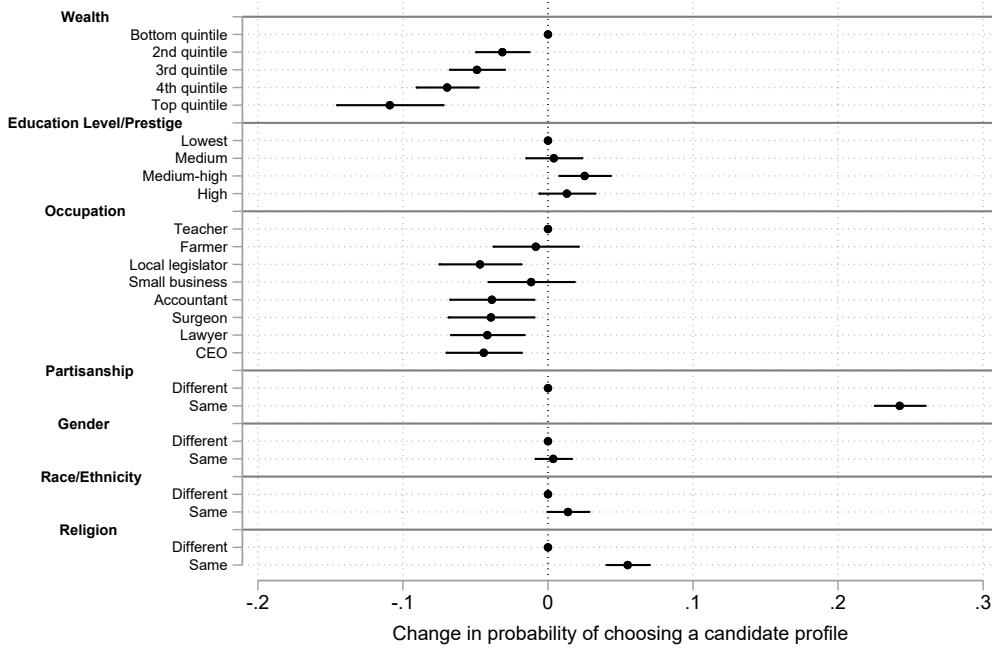
Figure A10: Main Results without Profiles with Surprising Wealth

provision of clientelistic goods as an electoral strategy, as is the case in various contexts (Hicken, 2011), wealthier candidates may be more desirable electorally.[11]

To assess this possibility, in the surveys in Chile, Brazil, and India, following each conjoint rating task, we asked our respondents the following question: "Which candidate would be more likely to give gifts or money to voters during the election campaign?" We did not ask this question in the U.S. surveys as this type of exchange is exceedingly rare in the U.S. The results, shown in Figure A11, indicate that respondents had a strong expectation that wealthier politicians would be more likely to engage in clientelism.
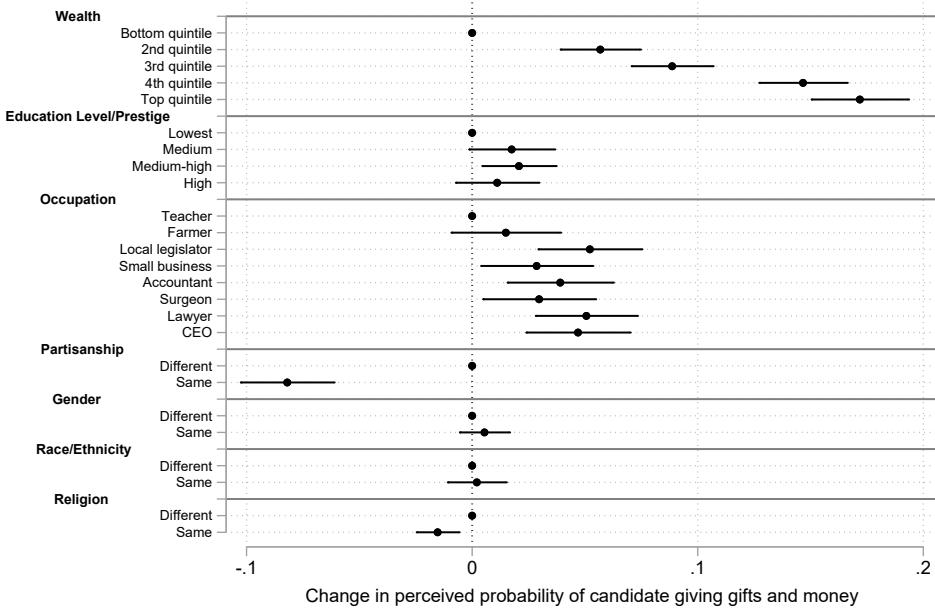
These perceptions may translate into greater expectations of receiving personal benefits from such politicians, and those expectations may underpin a direct preference for wealthier politicians and drive up their electoral support. While we cannot directly test this causal logic with experimental data on hypothetical candidates, we can still examine whether respondents with characteristics that the previous literature has identified as particularly likely to be targeted with clientelistic goods differ in their perceptions and candidate choices from other respondents. Prior work has shown that clientelism tends to be targeted toward individuals of lower socio-economic status – lower income, wealth, or education (e.g. Weitz-Shapiro, 2014). Also, scholars have shown that clientelism often operates through co-ethnic networks (e.g. Chandra, 2007; Chauchard, 2016). Figures A12-A15 therefore examine whether the lower-income, lower-wealth,[12] lower-education,[13] and co-ethnic respondents, respectively, exhibit: (a) greater expectations of clientelistic spending from wealthier candidates; and (b) greater support for wealthier candidates.

---

[11]This hypothesis was pre-registered in our initial Pre-Analysis Plan.

[12]We use the same measures of income and wealth as for the analysis of aspirations in Figure 4 in the paper. The difference is that we are excluding the US from the analysis here.

[13]Our survey item had five categories: less than high-school, high school, some college, gradauted college, postgraduate. We code respondents with high school or less as lower-education.

Figure A11: Candidate Attribute Effects on the Perceived Probability of Providing Gifts and Money During Campaigns

Change in perceived probability of candidate giving gifts and money

In each figure, the estimates in orange squares are for respondents who may be more likely to receive clientelistic goods, and estimates in black circles are for those respondents who may be less likely to receive them. Taken together, we find some, but inconsistent evidence that the groups of respondents plausibly more likely to receive clientelistic transfers are more likely to support wealthier candidates. Lower-income and especially lower-wealth respondents are more supportive of wealthier candidates (and less supportive of the less wealthy candidates) than their higher-income and wealthier counterparts. However, they are not significantly more likely to perceive wealthier candidates as being more prone to clientelism, making it hard to connect the expectations about clientelism with electoral support. By contrast, respondents expect wealthier co-ethnic candidates to engage in clientelism, but do not support such candidates at higher rates. Finally, we see no marked differences by respondent education.

A5.7 Support for the Wealthy as Status Aspiration vs Clientelism

With respect to heterogeneities in candidate preference by respondent income and wealth, we have no way of directly distinguishing in the data whether the public may support wealthy candidates as an expression of status aspirations or because of expectations of clientelistic benefits. However, we note here a piece of evidence that is potentially consistent with aspirations but not clientelistic expectations. Namely, we also asked in our survey whether our respondents thought the taxes on top incomes should be increased or not. Presumably, those who think the top rates should not be increased are more likely to harbor aspirations for higher economic status themselves. However, it is not clear why such attitudes should vary by expectations of clientelistic exchanges. These tax attitudes are highly predictive of support for wealthier politicians: those who oppose increasing the taxes on top incomes are no less supportive of wealthier candidate profiles than the less wealthy ones. Those who support increasing the taxes on the wealthy, on the other hand, are considerably

Figure A12: Respondent Income, Expectations of Clientelism, and Candidate Support
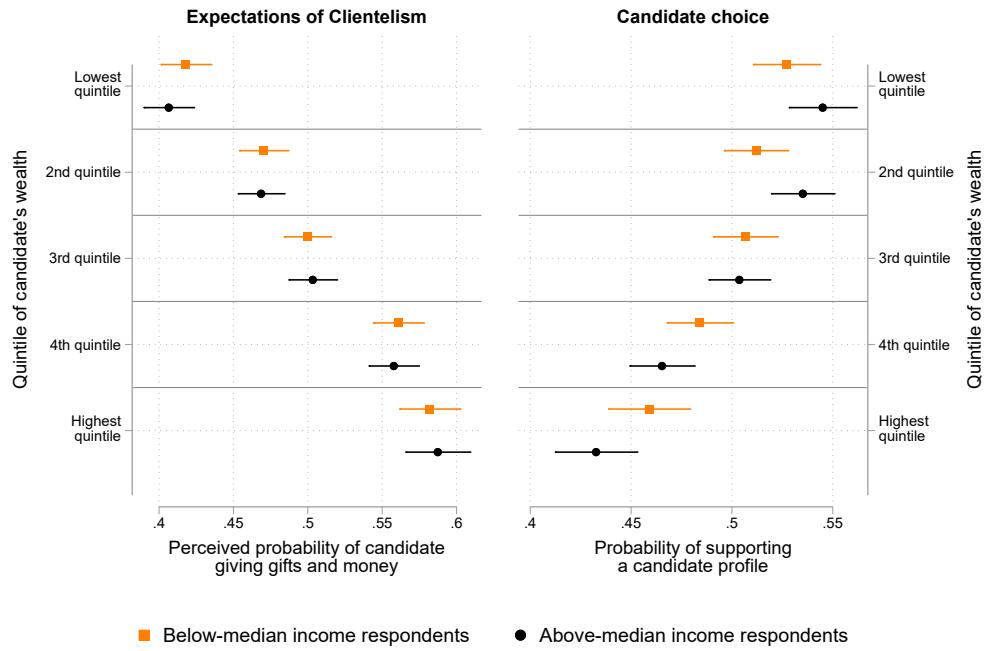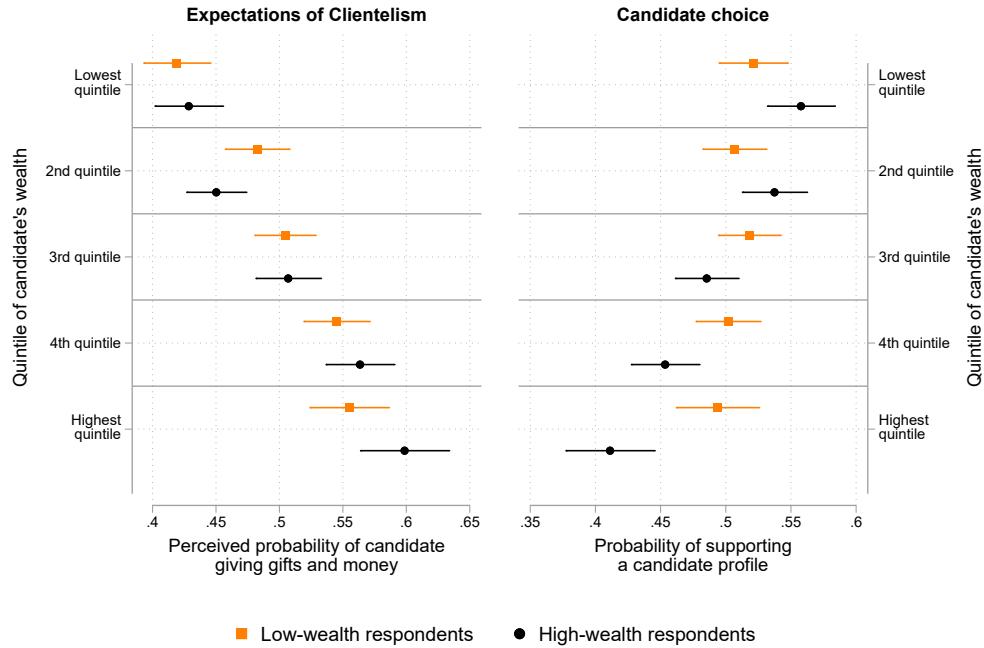

Figure A13: Respondent Wealth, Expectations of Clientelism, and Candidate Support

less supportive of wealthier candidates (these results are available upon request). Of course, these patterns are by no means dispositive, merely suggestive.

Figure A14: Respondent Education, Expectations of Clientelism, and Candidate Support
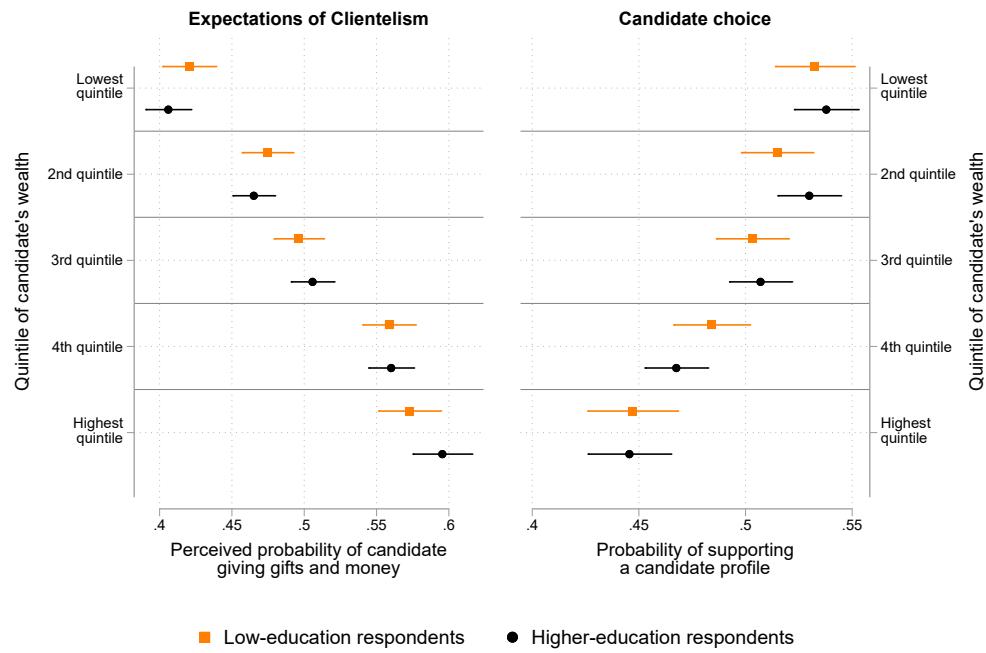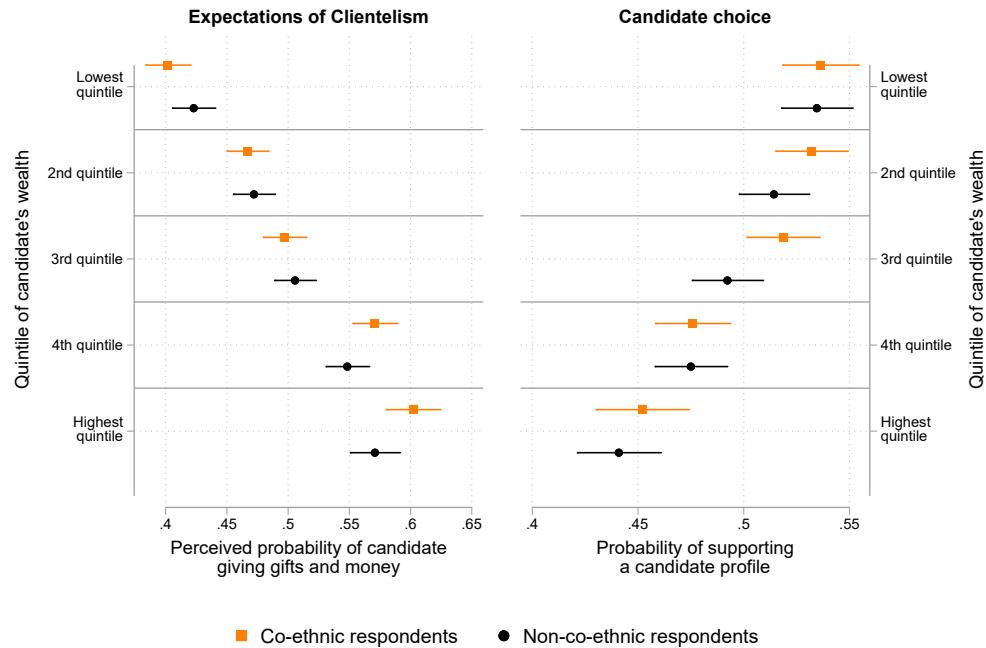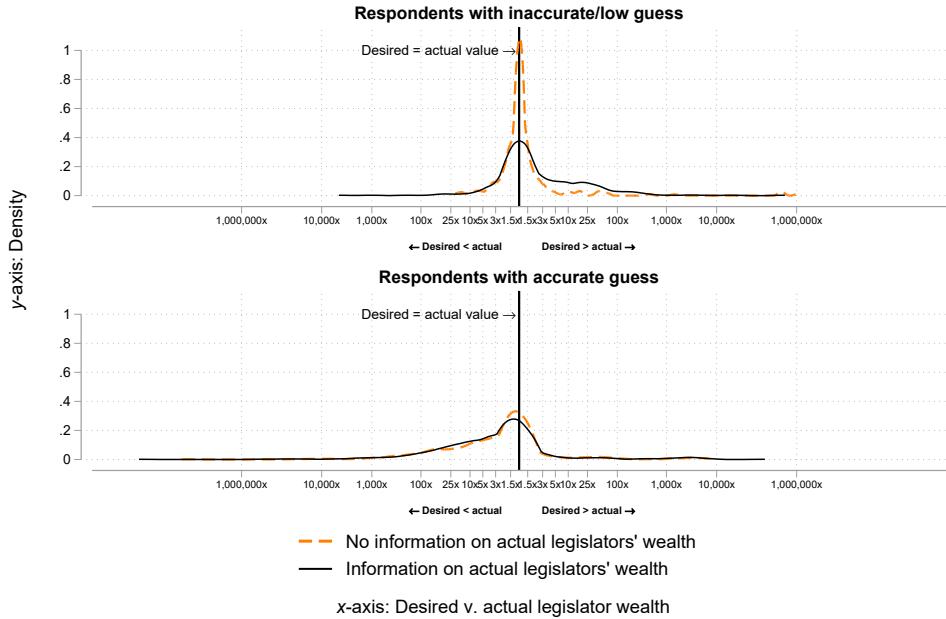

Figure A15: Respondent Co-Ethnicity, Expectations of Clientelism, and Candidate Support

A5.8 Desired Legislator Wealth by Accuracy of Respondent Wealth Priors

We saw in the paper that respondents with the less accurate guesses of legislators' actual wealth reacted to the information treatment in a surprising way: they increased (at the margin) rather than decreased the support for wealthier candidates. Figure A16 shows that a plausible reason for this is that the information treatment seems to have led a subset of these respondents to update their perceptions of how wealthy a candidate can, and perhaps should, be. The 'bump' in the density of the post-treatment desired wealth levels (averaged across the three legislator scenarios) to the right of zero among respondents with inaccurate priors in the upper panel indicates that they updated their views about politicians' wealth.[14] We see no such reaction in the lower panel among respondents with accurate priors. Formally, in response to the treatment, the respondents with inaccurate priors on average increase their desired wealth level by 70 percent ($p < .01$); those with accurate priors reduce their desired levels by 9 percent ($p = .6$); the difference between these two treatment effects is significant at $p < .01$.

Figure A16: Information Treatment and Desired Wealth by Accuracy of Respondent Wealth Priors

---

[14]We limit this analysis to the subsample where the respondents were given the information experiment first, to avoid contamination from the wealth levels the respondents would be exposed to through the conjoint experiment.

A6   References

Aspinall, Edward, Meredith L. Weiss, Allen Hicken and Hutchcroft Paul D. 2022. *Mobilizing for Elections Patronage and Political Machines in Southeast Asia*. Cambridge University Press.

Benjamini, Yoav and Yosef Hochberg. 1995. "Controlling the false discovery rate: a practical and powerful approach to multiple testing." *Journal of the Royal Statistical Society: Series B (Methodological)* 57(1):289–300.

Brick, J. Michael. 2011. "The Future of Survey Sampling." *Public Opinion Quarterly* 75(5):872–888.

Butler, Daniel M. and Jonathan Homola. 2017. "An Empirical Justification for the Use of Racially Distinctive Names to Signal Race in Experiments." *Political Analysis* 25(1):122–130.

Castorena, Oscar, Noam Lupu, Maita Schade and Elizabeth J. Zechmeister. 2022. "Online Surveys in Latin America." *PS: Political Science & Politics* pp. 1–8.

Chandra, Kanchan. 2007. *Why Ethnic Parties Succeed: Patronage and Ethnic Head Counts in India*. Cambridge University Press.

Chauchard, Simon. 2016. "Unpacking ethnic preferences: Theory and micro-level evidence from North India." *Comparative Political Studies* 49(2):253–284.

Chauchard, Simon, Marko Klašnja and S.P. Harish. 2019. "Getting Rich Too Fast? Voters' Reactions to Politicians' Wealth Accumulation." *The Journal of Politics* 81(4):1197–1209.

Clayton, Katherine, Yusaku Horiuchi, Aaron R. Kaufman, Gary King and Mayya Komisarchik. 2023. "Correcting Measurement Error Bias in Conjoint Survey Experiments." *American Journal of Political Science* 12(B2):1–11.

Corvalan, Alejandro. 2024. "MACEDA 3.0 1990-2021 Geocoded.".
  URL: *https://doi.org/10.7910/DVN/KTOUDQ*

de la Cuesta, Brandon, Naoki Egami and Kosuke Imai. 2022. "Improving the External Validity of Conjoint Analysis: The Essential Role of Profile Distribution." *Political Analysis* 30(1):19–45.

Hicken, Allen. 2011. "Clientelism." *Annual Review of Political Science* 14:289–310.

Justesen, Mogens K. and Stanislav Markus. 2024. "Tycoon candidates, electoral strategies, and voter support: a survey experiment in South Africa." *Business and Politics* pp. 1–19.

Kennedy, Courtney, Nick Hatley, Arnold Lau, Andrew Mercer, Scott Keeter, Joshua Ferno and Dorene Asare-Marfo. 2020. "Assessing the Risks to Online Polls from Bogus Respondents." Pew Research Center, February 18, 2020.

Klašnja, Marko and Lucia Motolinia. 2025. "Public Servants, Private Fortunes: The Causes and Consequences of the Wealth of Politicians." Working Manuscript.

Kramon, Eric. 2017. *Money for Votes: The Causes and Consequences of Electoral Clientelism in Africa*. Cambridge University Press.

Liu, Guoer and Yuki Shiraito. 2023. "Multiple Hypothesis Testing in Conjoint Analysis." *Political Analysis* pp. 1–16.

Rivers, Douglas. 2007. "Sampling for Web Surveys." Paper prepared for the 2007 Joint Statistical Meetings, Salt Lake City, UT.

Sircar, Neelanjan. 2018. Money in Elections: The Role of Personal Wealth in Election Outcomes. In *Costs of Democracy: Political Finance in India*, ed. Devesh Kapur and Milan Vaishnav. New York, NY: Oxford University Press pp. 36–73.

Weitz-Shapiro, Rebecca. 2014. *Curbing Clientelism in Argentina: Politics, Poverty, and Social Policy.* Cambridge University Press.