**Supplementary Appendix for**

**When do the Wealthy Support Redistribution?**

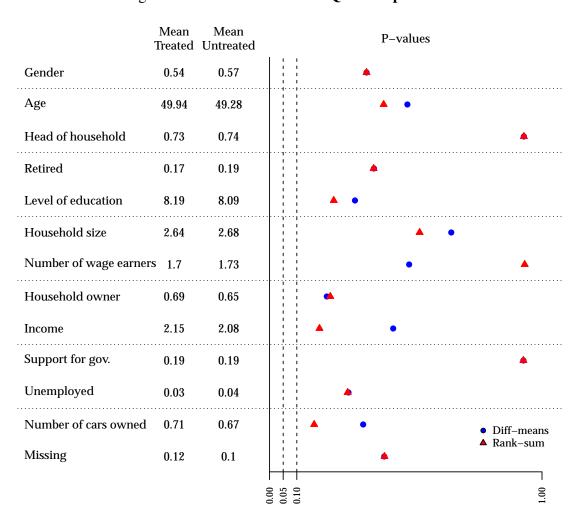**Inequality Aversion in Buenos Aires**

# Contents

# 1 Balance tests

**Natural experiment**

To evaluate whether as-if random assignment to the price hike is plausible, we conduct balance tests comparing treated and untreated groups on observable covariates. We use t-tests and rank-sum tests of each covariate on an indicator of treatment assignment. The covariates used in the balance plot presented are:

- *Gender:* Equals 1 if the respondent is female and zero otherwise.

- *Head of household*: Equals 1 if the respondent is the main income earner, and zero otherwise.

- *Retired*: Equals 1 if the respondent is retired, and zero otherwise.

- *Owner*: Equals 1 if the respondent is a home owner, and zero otherwise.

- *Unemployed*: Equals 1 if the respondent is unemployed, and zero otherwise.

- *Education*: Takes values from 1 (no education) to 10 (post-graduate degree).

- *Household size:* Number of people in the household.

- *Number wage earners:* Number of people who contribute to household's income.

- *Support for government:* Equals 1 if voted for government candidates in either national or local elections, zero otherwise.

- *Income:* An ordinal measure of income that takes values one (less than 5,000 pesos per month) through four (more than 15,000 pesos per month).

- *Cars:* Number of owned cars.

Most of our indicators of wealth have missing observations and are subject to potential measurement error as a result of misreporting – a typical problem when dealing with high income samples. To deal with these issues, we follow the rules suggested in Gerber and Green (2012, p. 214): If less than 10% of the covariate's values are missing, we recode the missing values to the overall mean. This is the case with: *Earners*, *Owner*, and *Cars*. If 10% or more of the covariate's values are missing, we include a missingness dummy as an additional covariate and recode the missing values to a constant value. This is the approach we take in the case of *Income*. The results of the balance tests are plotted in Figure A1. No covariate yields a statistically significant difference at conventional levels.

Figure A1: **Balance Statistics: Quasi-Experiment**

| | Mean Treated | Mean Untreated |
| --- | --- | --- |
| Gender | 0.54 | 0.57 |
| Age | 49.94 | 49.28 |
| Head of household | 0.73 | 0.74 |
| Retired | 0.17 | 0.19 |
| Level of education | 8.19 | 8.09 |
| Household size | 2.64 | 2.68 |
| Number of wage earners | 1.7 | 1.73 |
| Household owner | 0.69 | 0.65 |
| Income | 2.15 | 2.08 |
| Support for gov. | 0.19 | 0.19 |
| Unemployed | 0.03 | 0.04 |
| Number of cars owned | 0.71 | 0.67 |
| Missing | 0.12 | 0.1 |



Covariate data from authors' survey.

As another test of covariate balance between individuals exposed to the price hike and untreated, Table A1 shows results from regressions of the treatment indicator on a variety of observables. An $F$-test of joint significance suggests that pre-treatment covariates fail to predict treatment assignment (p-value = 0.558).
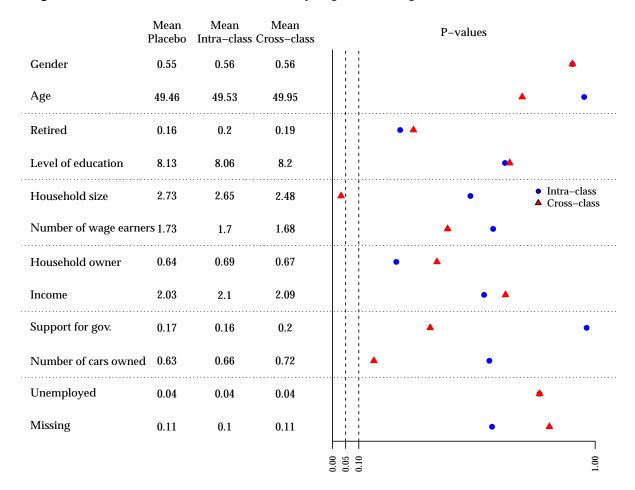
Table A1: **F-test of pre-treatment covariates on being assigned to the price hike.** **The dependent variable is treatment assignment. Robust standard errors in parentheses.**

|  | Model 1 |
| --- | --- |
| Constant | 1.423*** |
|  | (0.118) |
| Gender | −0.014 |
|  | (0.033) |
| Age | 0.001 |
|  | (0.001) |
| Retired | −0.076 |
|  | (0.057) |
| Education | 0.002 |
|  | (0.010) |
| Household Size | −0.010 |
|  | (0.015) |
| Wage Earners | −0.022 |
|  | (0.027) |
| Gov. Supporter | −0.010 |
|  | (0.041) |
| Unemployed | −0.085 |
|  | (0.087) |
| Household Owner | 0.030 |
|  | (0.039) |
| Income | 0.029 |
|  | (0.020) |
| N. of Cars | −0.004 |
|  | (0.027) |
| Missing | 0.148* |
|  | (0.089) |
| $R^2$ | 0.011 |
| Num. obs. | 1005 |
| F statistic | 0.889 |

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

**Survey experiments**

To assess the internal validity of the survey experiment, Figure A2 shows results from balance tests comparing the survey experimental groups using the placebo group as baseline. Only one covariate for the cross-class treatment, out of twelve, appears to differ across groups. This suggests that the individuals assigned to the experimental framings are statistically indistinguishable along observable covariates.

Figure A2: **Balance tests: *t*-tests for survey experiments (placebo used as baseline).**

| | Mean Placebo | Mean Intra–class | Mean Cross–class |
|---|---|---|---|
| Gender | 0.55 | 0.56 | 0.56 |
| Age | 49.46 | 49.53 | 49.95 |
| Retired | 0.16 | 0.2 | 0.19 |
| Level of education | 8.13 | 8.06 | 8.2 |
| Household size | 2.73 | 2.65 | 2.48 |
| Number of wage earners | 1.73 | 1.7 | 1.68 |
| Household owner | 0.64 | 0.69 | 0.67 |
| Income | 2.03 | 2.1 | 2.09 |
| Support for gov. | 0.17 | 0.16 | 0.2 |
| Number of cars owned | 0.63 | 0.66 | 0.72 |
| Unemployed | 0.04 | 0.04 | 0.04 |
| Missing | 0.11 | 0.1 | 0.11 |

## 2   Fieldwork details

To understand the motivations behind the policy design, we interviewed staff of the Ministry of Planning. Our interviews officials were conducted on May 5, 2012, May 15, 2012, March 25th, 2013. In addition, on June 5 and June 7, 2012, we interviewed officials in an energy distributors' trade group and an electrical utility. None of our interviewees wished to be identified by name.

## 3   The economic impact of the price hike

Based on official sources, interviews with public officials, and guidance from an accountant, we estimate that the price hike had a substantial pocketbook effect. According to media reports, the average utility bill, summing gas, electricity, and water, was 125 pesos; after the price hike, it rose to 620 pesos. Mean monthly incomes reported by our samples were 10,000 pesos. As mentioned in the text of the manuscript, before the rate increase, utility payments absorbed 1.25% of median monthly individual incomes in the areas where rates were later increased. After the hikes they absorbed more than 6%. If one thinks about the increment to a person's utility payments as an additional tax, it would represent about a doubling of taxes paid on income by a typical person in our sample, from 6.6% to 10.7%. These calculations are based on 2012 figures and accordingly suppose a person whose gross annual income is 144,000 pesos per year, is married, and has one child. Her annual income tax bill would be 8,000 pesos. With the utility rate hike, she pays 7,440 in utilities rather than 1,500. (We are grateful to Ana Patrizio for this analysis). This calculation leaves aside indirect taxes, which are substantial: most goods and services are subject to a 21% sales tax. Of course, the typical utility consumer in our sample is paying for a service rendered and is not paying taxes directly to the government. Instead, the government regulates the rate structure of semi-private utility companies and pays them negotiated subventions to reduce payments by households.

# 4   Did the price hike target opponents?

Did the government have the ability and incentives to use the subsidy withdrawals to reward supporters and punish opponents *within* affluent neighborhoods? (Dunning 2012). As we explained in the manuscript, we believe the basic answer to be no. It is not obvious that officials could have used electoral data to sort opponents into the price-hike group; and our interviews turned up no evidence that they did do so. In a polarized electorate in which pro- and anti-Peronist (and -Kirchnerist) stances map fairly predictably onto class divisions, the numbers of clear Kirchner supporters in these precincts was modest. And the government lacked neighborhood-by-neighborhood measures of variation in political support. Electoral data are not disaggregated at the census-tract level and census tracts do not exactly match electoral precincts. Furthermore, given the way in which voters are allocated to polling stations, our sampling frame covering three blocks around the policy border makes our respondents equally likely to vote on each side of the border. Neither could party machines provide the government with information to target the price hike in a fine-grained way. In contrast to low-income neighborhoods where Peronist organizations are well established, in high-income neighborhoods there is little in the way of a party organization and hence few alternative sources of information on the nature of political affiliations of residents.

# 5  Sampling

Figure A4 maps the census tracts targeted by the government for the price hike.

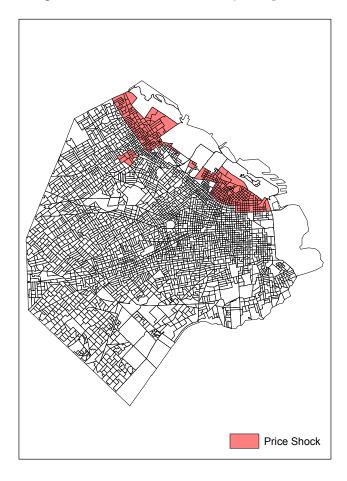Figure A3: **Districts selected by the government**

Figure A4 maps the census tracts that we selected after matching the ones targeted by the government with untreated census tracts. Matching was based on a nearest-neighbor algorithm using census data.
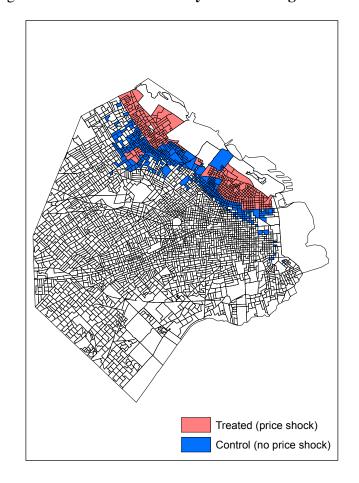
Figure A4: **Districts selected by Nearest-Neighbor matching.**



Treated (price shock)
Control (no price shock)

# 6  Survey experimental vignettes

A key feature of our research design was a survey of people residing on both sides of the policy border. Our telephone interviews were carried out by the survey research firm MORI during the months of October and November, 2012. We drew a sample of approximately 500 people from the population on each side of the policy border, for a total sample size of 1,005 heads of household. The sampling procedure incorporated quotas for age and gender. In total, 558 men and 447 women were interviewed; the average age of respondents across the full sample was 49.

The interviews included survey experiments that probed the sensitivity of respondents' preferences for redistribution to interpersonal comparisons. We exposed some respondents to information about the differential targeting of similarly wealthy households, thus emphasizing intra-class inequities. We exposed others to information about its potential for redistribution from the wealthy to the poor, thus emphasizing the altruistic dimensions of the policy. Thus, the survey experiment entailed randomly assigning respondents on each side of the policy border to one of four groups: intra-class comparisons, cross-class comparisons, placebo, and control.

The **intra-class inequity** treatment asked about the policy, making salient interpersonal comparisons with wealthy peers. This means that the exact question wording depended on whether the individual's rates had risen or remained unchanged. The version of the question asked of people whose rates did not change is in boldface:

> *In recent months, the national government modified residential rates for gas, electricity, and water in some areas of the city of Buenos Aires. This measure eliminated subsidies in some high-income areas of the city, but retained them in others that have the same income levels, as defined for example by square footage of residences, garbage collection taxes, and levels of expenditures. In your case, whereas the government decided to withdraw [**maintain**] the subsidies for gas, electricity, and water for households on your block, households less than three blocks away lost [**kept**] them.*[1]

The **cross-class inequality** treatment makes salient the egalitarian goals of the policy:

> *In recent months, the national government modified residential rates for gas, electricity, and water in the highest income areas of the city of Buenos Aires. This measure did not affect the poorest areas of the city, which kept their subsidies. According to an independent study prepared by the University of Buenos Aires, this*

---

[1] The three block comparison was in all cases true.

*decision had the effect of making the cost of living more equal between of those with*

*the higher and lower incomes in the city of Buenos Aires.*[2]

People in the **placebo** group heard a neutral description of the policy:

*In recent months, the national government modified residential subsidies for gas,*

*electricity, and water in some areas of the city of Buenos Aires.*

The placebo allows us to assess whether any observed differences across survey-experimental

treatments were the effect of mentioning the policy *per se* rather than of the treatment's framing.

In contrast with the other groups, people assigned to the **control** received no framing of

the policy and were not asked for an opinion about it.

## 7 Outcome Variables

The wording of the survey questions measuring our key outcomes are:

**Support for the price hike policy**

*How would you characterize the government's decision? Would you say it was very*

*good, good, neither good nor bad, bad, or very bad?*

**Redistribution**

*Some people think that the state should reduce differences between the rich and the*

*poor, whether by increasing taxes on the richest families or by giving economic*

*assistance to the poor. Others think that the state should not intervene and that the*

*free market is the best mechanism for reducing poverty. On a scale of 1 to 7, where 1*

*means that the state should not intervene to reduce income differences, and 7 means*

*that the state should intervene to reduce income differences, which statement is closer*

*to your opinion?*

**Unemployment insurance**

---

[2] The mentioned study does not exist. Respondents were debriefed at the end of the survey.

*How much do you agree or disagree with the following phrase: The state should*

*provide a basic income so that unemployed people can pay their expenses? On a*

*scale of 1 to 7, where 1 means that the state should not provide unemployment*

*insurance, and 7 means that the state should provide unemployment insurance, which*

*statement is closer to your opinion?*

Table A2 displays levels of support for the price-shock policy and for redistribution and unemployment insurance, all of them unconditioned by quasi-experimental or survey treatments. (The number of respondents to the policy question, in the northeast cell of Table A2, is smaller because this question was not asked of the control group.) The average level of support for the price hike is roughly in the middle of the range of possible scores – around three on a five-point scale for the price adjustment, and between three and 4.5 on seven-point scales for the remaining questions.

Table A2: **Summary of Dependent Variables**

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|----------|------|-----------|------|------|-----|
| Policy | 2.988 | 1.203 | 1 | 5 | 754 |
| Redistribution | 4.452 | 2.134 | 1 | 7 | 980 |
| Unemployed | 3.893 | 2.213 | 1 | 7 | 992 |

# 8   LATE assumptions

The consistent estimation of local average treatment effects (LATEs) with instrumental variables requires additional assumptions (Angrist and Pischke 2008). The exclusion restriction is plausible: we find no reason to expect that location (with only a few blocks separating individuals who lost and kept their subsidies) would directly affect one's worldviews, except through the policy. Second, our instrument is very strong as shown by the $F$-statistic of the first stage regression of the price hike treatment on geographic location (See Table A3.)

Estimation with instrumental variables also requires the absence of "defiers," that is, individuals who receive treatment when assigned to control, and move to control when assigned to treatment. The design of the price hike featured an exception that forced individuals in the non

Table A3: **F-test. First stage: regression of treatment receipt on treatment assignment.**

|  | Model 1 |
|---|---|
| Constant | $-0.361^{***}$ |
|  | $(0.041)$ |
| Geographic location (0,1) | $0.576^{***}$ |
|  | $(0.026)$ |
| $R^2$ | 0.331 |
| Adj. $R^2$ | 0.331 |
| Num. obs. | 1005 |
| F statistic | 497.291 |
| RMSE | 0.409 |

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

treatment areas into the treatment group. Residents of buildings that the government designated as *luxury* saw their rates rise, even when they were located in areas where the remaining household were untreated.[3] Five percent of respondents in the untreated areas were affected by this requirement. Similarly, in line with the social justice motivations endorsed by the government, some people with lower incomes and chronic health problems living in treated areas could apply to retain their lower rates.[4] In our survey, 19% of respondents in treated precincts reported having retained the lower rates. Finally, simply by filling out an online form, anyone in the country could voluntarily give up subsidies and shift to the higher rate structure. The names of people who did so were published on a government website. The government publicized this option with the motto "Argentina, a country of good people." Three percent of our sample reported having accepted higher rates voluntarily. Though the absence of defiers cannot be proved, the compliance problems described above reflect "always takers" rather than defiers: residents of luxury buildings and voluntary withdrawals represent individuals who lost subsidies regardless of geographic location.

---

[3] Luxury buildings were defined by the government as those that have a swimming pool and a gymnasium. 5.4% in the treated regions reported living in luxury buildings.

[4] These requirements were: having a chronic disease and being enrolled in a social program, among other.

# 9 Extended results and robustness tests

## 9.1 Intent-to-Treat analysis

Given non-compliance with treatment receipt, our main identification strategy relies on instrumental variables. But our results hold when using intent-to-treat (ITT) analysis where we regress the key outcomes on our instrument for exposure to the policy – a geographic-location variable indicating on which side of the policy border a respondent's household lies. Table A4 shows ITT estimates for the effect of the policy (Hypothesis 1). Figure A5 replicate figures 3 and 4 in the manuscript. No significant changes are discovered using an ITT estimator strategy. All coefficients are estimated using Ordinary Least Squares and robust standard errors.
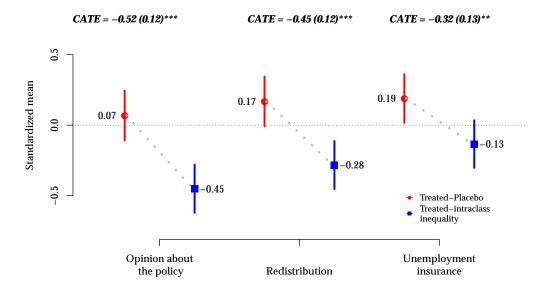
Table A4: **Intention to Treat (ITT) estimates: Attitudes towards redistribution and unemployment insurance by exposure to price hike.**

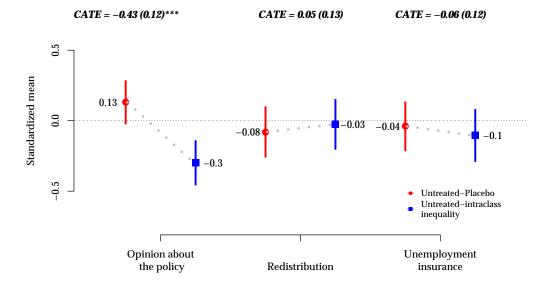|  | Redistribution | Unemployment |
|---|---|---|
| Constant | 0.276 | 0.212 |
|  | (0.195) | (0.201) |
| Geographic location (0,1) | $-0.089$ | $-0.123$ |
|  | (0.123) | (0.126) |
| $R^2$ | 0.002 | 0.004 |
| Adj. $R^2$ | -0.002 | -0.000 |
| Num. obs. | 245 | 248 |
| F statistic | 0.525 | 0.949 |
| RMSE | 0.961 | 0.993 |

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

## Figure A5: **ITT analysis of Resentment (H2) and Empathy (H3)**

Panel 1: ITT, Disadvantageous Intra−Class Inequality Aversion



Panel 2: ITT, Advantageous Intra−Class Inequality Aversion



Numbers across the top represent the Conditional Average Treatment Effect (CATE) using the placebo group as baseline. Red dots and blue squares represent means and vertical segments 95% confidence intervals (See legends for details). Robust standard errors of the CATE in parenthesis (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

## 9.2 Full Results

Table A5 displays the raw coefficients from the instrumental variables regressions used to produce the figures plotting conditional average treatment effects. Each column corresponds to an outcome. We estimated CATEs using instrumental variable regression of each item on natural-experiment condition, survey- experiment condition, and their interaction. We used a binary variable to indicate geographic location and its interaction with the survey experiment. The variable $Treatment$ thus stands for a binary indicator of exposure to the price hike "treatment" instrumented by geographic location. We obtained the standard errors for the LATEs, CATEs and ATEs using the Huber-White estimator. To ease interpretation, we have rescaled all ordinal dependent variables to have a mean of zero and a standard deviation of one.

Table A5: **Full results of instrumental variables estimation**

|  | Policy | Redistribution | Unemployment |
|---|---|---|---|
| Constant | 0.155 | −0.183 | −0.132 |
|  | (0.126) | (0.134) | (0.132) |
| $Intraclass$ | −0.417** | 0.221 | 0.034 |
|  | (0.170) | (0.181) | (0.178) |
| $Treatment$ | −0.114 | 0.459* | 0.420* |
|  | (0.222) | (0.236) | (0.232) |
| $Crossclass$ | 0.111 | 0.148 | 0.084 |
|  | (0.181) | (0.191) | (0.188) |
| $Intraclass \times Treatment$ | −0.120 | −0.857*** | −0.465 |
|  | (0.291) | (0.310) | (0.304) |
| $Crossclass \times Treatment$ | 0.146 | −0.444 | −0.295 |
|  | (0.313) | (0.330) | (0.325) |
| $Control$ |  | 0.407** | 0.274 |
|  |  | (0.190) | (0.190) |
| $Control \times Treatment$ |  | −0.619* | −0.641* |
|  |  | (0.329) | (0.327) |
| Num. obs. | 754 | 980 | 992 |
| RMSE | 0.962 | 1.005 | 1.005 |

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

### 9.3 Testing pocketbook hypothesis using all treatment groups

Recall that in the manuscript we tested the pocketbook hypothesis by only considering individuals who were not asked about the policy before being asked about their preferences for redistribution. Because this "control" group was not primed to think about the policy we believe that it provides a cleaner test of the pocketbook hypothesis. As an additional test, column 1 in Table A5 shows results of instrumental variables regression where we regress opinions about the policy on $Treatment$ (exposure to the price hike instrumented by geographic location), assignment to the intraclass and crossclass framings, and the interaction between $Treatment$ and each of the framings. While potentially subject to priming, this approach allows us to include opinions about the policy as an outcome variable. Contrary to the pocketbook hypothesis, the results show that exposure to the price hike has no systematic effect on opinions about the policy. The insignificant coefficient for the component $Treatment$ term shows that the price hike did not elicit different opinions about the policy between treated individuals and untreated individuals who were not exposed to any framing. In turn, the insignificant coefficients for both interactive terms terms suggest that the price hike did not elicit different opinions among people exposed to either framing.

# 10    What role for ideology?

## 10.1    Measuring Ideology

Table A6: **Declared vote choice in past presidential elections (%)**

| Presidential candidate | N | % |
|---:|:---:|:---:|
| BINNER | 194 | 25% |
| CFK | 176 | 23% |
| CARRIO | 78 | 10% |
| DUHALDE | 52 | 7% |
| ALFONSIN | 44 | 6% |
| R. SAA | 28 | 4% |
| OTHER | 39 | 5% |
| BLANK/NULL | 164 | 21% |
| Total | 775 | 100% |

In the manuscript we report that the main findings regarding disadvantageous inequality aversion hold after disaggregating by ideology. The most reliable to measure ideology in our survey is through questions tapping vote choice in the 2011 Presidential elections. Table A6 presents the frequencies of each choice. As is often the case with questions tapping past vote choice, there is non-trivial missingness. 225 respondents did not answer the question and an additional 203 respondents supported declared supporting "OTHER" or casting a Blank or Null vote.

We used the 775 effective responses to classify respondents as left-leaning or conservative. Left-leaners, coded '1', are respondents who declared voting for either the Peronist Cristina Fernández de Kirchner (CFK) or the Socialist candidate Hermes Binner in the presidential race. Conservatives, coded '0', are those who voted for any of the remaining candidates. We preferred this conceptualization over a more traditional left-center-right dimension because these candidates made heterogeneous ideological appeals. This coding yields 65% conservatives (N=202) and 35% left-leaners (N=370).

There is a strong correlation between our measure of ideology and our samples' responses to questions about redistribution and statism. Table A7 shows that 53% of left-leaners considered

the increase in public utility prices either 'good' or 'very good', whereas only 24% of conservatives had these opinions. In turn, 49% of conservatives considered the policy either 'very bad' or 'bad' while only 26% of left-leaners held such negative views.

Table A7: **Opinion about the policy. Percentage responses by ideology.**

|  | Conservatives | Left-leaners |
|---|---|---|
| 1- Very bad | 19 | 8 |
| 2 | 30 | 18 |
| 3 | 26 | 22 |
| 4 | 21 | 35 |
| 5- Very good | 3 | 18 |

Attitudes towards redistribution showed a similar pattern (see Table A8). Forty-one percent of left-leaners chose the most-supportive stance on redistribution, compared with 15% of conservatives.

Table A8: **Support for redistribution. Percentage responses by ideology.**

|  | Conservatives | Left-leaners |
|---|---|---|
| 1- Completely against | 26 | 9 |
| 2 | 10 | 3 |
| 3 | 16 | 7 |
| 4 | 12 | 11 |
| 5 | 17 | 19 |
| 6 | 4 | 10 |
| 7- Completely in favor | 15.00 | 41.00 |

Attitudes towards unemployment insurance similarly also map quite well onto our ideology measure (Table A9).

Table A9: **Support for unemployment insurance. Percentage responses by ideology**

|  | Conservatives | Left-leaners |
|---|---|---|
| 1- Totally against | 27.00 | 16.00 |
| 2 | 11.00 | 4.00 |
| 3 | 15.00 | 12.00 |
| 4 | 13.00 | 12.00 |
| 5 | 11.00 | 18.00 |
| 6 | 5.00 | 9.00 |
| 7- Totally in favor | 17.00 | 27.00 |

As more rigorous evidence of the correlation, Table A10 presents results of regression analyses of the attitudes just described on our measure of ideology. Responses were standardized. Compared with conservatives, left-leaners are 0.66 standard deviations more supportive of the price-adjustment policy, 0.76 standard deviations more supportive of redistribution, and .43 standard deviations more supportive of unemployment insurance.

Table A10: **Regression of outcome measures on ideology**

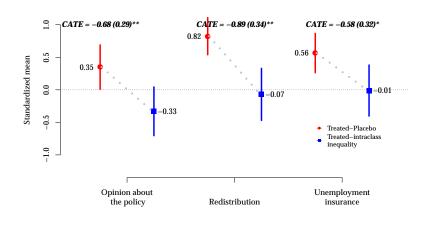|  | Opinion about the policy | Redistribution | Unemployment insurance |
|---|---|---|---|
| (Intercept) | $-0.33^{***}$ | $-0.41^{***}$ | $-0.17^{*}$ |
|  | (0.08) | (0.07) | (0.07) |
| Ideology | $0.66^{***}$ | $0.76^{***}$ | $0.43^{***}$ |
|  | (0.10) | (0.08) | (0.09) |
| Observations | 427 | 559 | 566 |

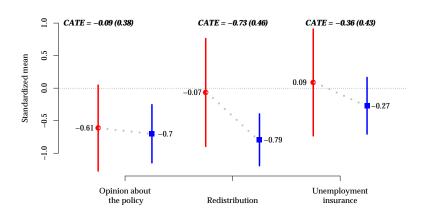$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Standard errors in parentheses

## 10.2 Testing resentment by ideological subgroups

Figure A6: **Testing resentment by ideology**



Support for the policy, redistribution, and unemployment insurance among subjects exposed to the price hike, by exposure to the intra-class disadvantageous inequality or placebo treatments, and by ideological orientation. The numbers across the top are the Conditional Average Treatment Effect of the intra-class inequality treatment for individuals exposed to the price hike, compared to the placebo; robust standard errors of the CATE are in parenthesis ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$). Blue squares (intra-class inequality) and red dots (placebo) represent means; vertical segments are 95% confidence intervals.

The main manuscript reports that resentment was such a compelling force that it eroded support among respondents with pro-redistributive attitudes. Though the level of support for redistribution was considerably higher among left-leaners than conservatives, Figure A6 SI shows that the intraclass treatment depressed support for redistribution by the same degree among

21

left-leaners as among conservatives. The relevant test was conducted by disaggregating the analysis of the intra-class framing CATE among ideological subgroups.

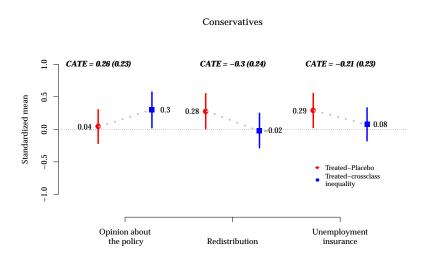## 10.3   Cross-class effects by ideology

Figure A7: **Testing altruism by ideology**



Support for the policy, redistribution, and unemployment insurance, by ideology and exposure to the cross-class inequality treatment or a placebo. Numbers across the top represent the Conditional Average Treatment Effect (CATE) of the cross-class inequality treatment, compared to the placebo. Robust standard errors of the CATE are in parenthesis ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$). Red dots (not treated) and blue squares (treated) represent means; vertical segments are 95% confidence intervals.

As mentioned in the manuscript, the framing literature offers two complementary alternative explanations for the null effect of the cross-class treatment on preferences for redistribution. One possibility is backfiring among individuals whose ideological priors are opposed to redistribution. According to Chong and Druckman (2007), "the weak frame may backfire especially among motivated individuals by causing their opinions to move in a direction opposite to the position advocated by the weak frame" (p. 111). This mechanism suggests that we should observe conservative individuals to become more opposed to redistribution when they receive the cross-class framing. A second and complementary mechanism is that individuals with pro-redistribution preferences will react positively to the cross-class framing. Thus, the prediction is that leftist will move towards more statism when exposed to the frame. We find no evidence

consistent with either conjecture. Figure A7 replicates the analysis in the main manuscript disaggregating the average effect of the cross-class treatment by ideology. Rather than polarization by ideology, the results show the predominance of null effects among groups. The only statistically significant effect of the cross-class framing is found among leftists when redistribution is the outcome of interest. But the effect operates in the opposite direction to what the framing literature would lead us to expect: leftists become more anti-redistribution when they are exposed to a frame suggesting that the price hike levelled the cost of living across classes.

# References

Angrist, Joshua and Jörn-Steffen Pischke. 2008. *Mostly Harmless econometrics: An Empiricist's Companion*. Princeton University Press.

Chong, Dennis and James Druckman. 2007. "Framing Public Opinion in Competitive Democracies." *American Political Science Review* 101(04):637–655.

Dunning, Thad. 2012. *Natural Experiments in the Social Sciences: A Design-Based Approach*. Cambridge University Press.

Gerber, Alan and Donald Green. 2012. *Field Experiments: Design, Analysis and Interpretation*. WW Norton.