

When do the Wealthy Support Redistribution?

Inequality Aversion in Buenos Aires

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1 INTRODUCTION

When do the wealthy support redistribution? Political economy theories produce conflicting expectations about the relationship between income and support for redistribution. Arguments based on material self-interest predict that the wealthy will systematically oppose redistribution: progressive taxes and transfers leave the wealthy worse off.¹ Several studies of advanced democracies lend support to this expectation.²

But there are reasons to suspect that self-interest is not the whole story. Behavioral economists offer extensive evidence that interpersonal comparisons can trump pocketbook considerations. In lab experiments, subjects have shown a willingness to give up some monetary payoffs if doing so allows them not to fall behind others; this is *disadvantageous inequity aversion* or *resentment*.³ Interpersonal comparisons can also mean that people suffer utility losses when others earn less than they do; this is *advantageous inequity aversion* or *altruism*.⁴ To the extent that political economists have studied the impact of interpersonal comparisons on preferences for redistribution in real-world settings, the discussion has been dominated by explorations of altruism, which can either complement pocketbook considerations⁵ or displace them.⁶

This focus has led to an imbalance. Though in the setting of the lab a “keeping-up-with-the-Joneses” mentality is not difficult to elicit, in real-world settings

¹ Meltzer and Richard 1981.

² Margalit 2013; Page, Bartels, and Seawright 2013.

³ Fehr and Schmidt 1999.

⁴ See Charness and Rabin 2002. *Altruism* usually connotes utility loss among upper income groups for those below them having less. We distinguish this cross-class altruism from intra-class *empathy* which connotes utility losses from harm done to one’s peers.

⁵ Dimick, Rueda, and Stegmueller 2017; Rueda and Stegmueller 2015.

⁶ Alesina and Giuliano 2011.

we know little about the effects of intra-class resentment.⁷ Testing for interpersonal comparisons in the real world is important since government interventions often do end up treating similarly situated individuals differently, as when taxes have discontinuous rate structures⁸ or economic actors can exploit loopholes.⁹ What's more, the *perception* that fiscal and public pricing policies impose iniquitous burdens may be widespread.¹⁰

Our paper takes a step toward correcting this imbalance. We take advantage of a unique opportunity to compare the impact of pocketbook concerns, altruism, empathy, and resentment in response to a real-world income shock. As described below, in 2012, the Argentine government quintupled public utility rates for gas, electricity, and water in some wealthy areas of Buenos Aires, while leaving rates unchanged for similar wealthy households in the same neighborhoods. Because the price hike was equivalent to a five-percent increase in the income tax paid by the typical household in the affected area, it might have provoked pocketbook reactions. But if behavioral experiments have external validity, interpersonal comparisons might also have been provoked. Because only affluent households saw their rates rise and because the government justified the hike on redistributive grounds it might have elicited altruism. And because some wealthy people's rates rose sharply while those of others remained unchanged, the rate hike might have provoked empathy among those who escaped the price hike and resentment among those who did not. We evaluate each of these possibilities by embedding a survey experiment, which induced people to make intra- and cross-class comparisons, within the quasi-experiment created by the Argentine government.

In addition to studying inequity aversion outside the lab, our study contributes new evidence on the redistributive preferences of the wealthy. While investigators often make assumptions about the attitudes of this subset of the population, they rely on

⁷ But see Cramer and Kaufman 2010; Lü and Scheve 2016.

⁸ See e.g., Romer and Romer 2014.

⁹ See e.g., Slemrod, Blumenthal, and Christian 2001.

¹⁰ Alm, McClelland, and Schulze 1992; Slemrod 2007.

representative sample surveys in which the number of wealthy respondents is small.¹¹ By drawing a representative sample of wealthy individuals, our study opens a window into the preferences of this hard-to-reach yet theoretically crucial population.

To anticipate our findings, resentment was the most salient reaction. Respondents withdrew support for redistribution when they were reminded that they paid for it while others were spared. Though perhaps this reaction is unsurprising, its dominance over all others is surprising and cuts against expectations in the literature. For instance, pocketbook effects were basically absent. People whose disposable incomes dropped substantially because of the price shock did not, for this reason alone, evince more anti-redistributive preferences. And whereas altruism has been much discussed in studies of distributive preferences, we find little evidence of it in the wake of a real-world fiscal shock: the cross-class redistributive aims of the policy had only modest resonance among our samples. Just as striking as the absence of altruism is the absence of empathy in wealthy Argentines' responses. Those who escaped the price shock and were reminded that some among their peers had been singled out to pay more were mostly unmoved.

2 HYPOTHESES

We posit four possible reactions to the price shock. The first one operates through the pocketbook: individual reactions to redistributive policies are driven by the impact of the price hike on income. The remaining reactions operate through interpersonal comparisons, which we classify along two dimensions (Table 1). First, do the wealthy compare themselves with similarly affluent neighbors or with lower-income citizens?¹² Second, when the reference group is their peers, are they more bothered by dropping

¹¹ As an exception see Page, Bartels, and Seawright 2013.

¹² Class affinities have been shown to influence preferences for redistribution. See Lupu and Pontusson 2011.

behind others or by rising above them?¹³

Table 1: **Types of Inequality Aversion**

<i>Form of Inequality Aversion</i>	<i>Reference Group</i>	
	Wealthy Peers	Lower-Income Groups
Advantageous	Empathetic	Altruistic
Disadvantageous	Resentful	

We describe people who do not incur an income loss due to the policy but are reminded that some of their peers did incur income loss as *empathetic*, in the sense that they dislike seeing others fall behind. People who feel *resentment* are ones who turn against redistribution when they suffer a loss and are reminded that others among their peers were spared this loss. In turn, people are *altruistic*, in our usage, to the extent that they are other-regarding and focus on the relative welfare of people below them on the income ladder.¹⁴

With these ideas in mind, we randomly assigned respondents whose rates rose or remained unchanged to treatments that elicited intra-class comparisons (emphasizing that the burden of redistribution was allocated differently among similar wealthy people) and cross-class comparisons (emphasizing the egalitarian goals of the policy). The following hypotheses focus on the effect of the price hikes and framings on support for the price hike and for other forms of redistribution:

H1: *Pocketbook orientation.* Experiencing a policy-induced income loss leads to reduced support for redistributive measures.

H2: *Resentment.* Experiencing a policy-induced income loss when it is salient

¹³ Fehr and Schmidt 1999.

¹⁴ Disadvantageous cross-class inequality aversion is not theoretically plausible. By definition, a wealthy person has a higher income than a lower-income person, so she cannot feel disadvantaged vis-à-vis lower-income groups.

that others in one's peer group avoided it leads to reduced support for redistributive measures.

H3: *Empathy*. Not enduring a policy-induced income loss when it is salient that others in the peer group did endure it leads to reduced support for redistributive measures.

H4: *Altruism*. Experiencing a redistributive policy that benefits poorer people leads to increased support for redistributive policies.

Table 2 summarizes the testable implications that each hypothesis generates.

Table 2: **Predictions**

Hypothesis	Treatment Group	Comparison Group	Effect on Preferences for Redistribution
<i>Pocketbook</i>	Rates Rose, No Framing	Rates Stable, No Framing	(−)
<i>Resentment</i>	Rates Rose, Intra-Class Inequity Framing	Rates Rose, Neutral Framing	(−)
<i>Empathy</i>	Rates Stable, Intra-Class Inequity Framing	Rates Stable, Neutral Framing	(+)
<i>Altruism</i>	Cross-Class Inequity Framing Rates Rose + Stable	Neutral Framing Rates Rose + Stable	(+)

3 THE PUBLIC PRICE SHOCK

The 2012 price hike in Buenos Aires was an attempt to shift away from highly subsidized public utility rates, a holdover from the deep 2001-2002 recession.¹⁵ Even some relatively wealthy Argentines faced difficulties paying for basic services, and Peronist governments kept gas, water, electricity, and public transportation prices uniformly low. A decade later, when the crisis had given way to a commodity-export boom and incomes had rebounded, this pricing structure was anachronistic and wasteful. It was anti-redistributive, in a country governed by a center-left political party;

¹⁵ Murillo 2009.

it underwrote heavy consumption; and it was a drain on the treasury. The government found itself in a “policy trap”.¹⁶

The price hike implemented by the government of Cristina Fernández produced starkly divergent rates among residents of wealthy neighborhoods in the City of Buenos Aires. Prices rose by 500% for some affluent households while remaining unchanged for other households in the same neighborhood. (Prices did not rise either in any middle- or working-class areas of Buenos Aires or in other cities.) Households located across the street from one another in some instances faced vastly divergent utility rates.

The price hikes were large in magnitude, even for wealthy rate-payers. 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 six percent. If one thinks about the increment to a person’s payments for public utilities as an additional tax, this would represent about a doubling of taxes paid on income by a typical person in our sample, from 6.6% to 10.7%.¹⁷

The policy design reflected multiple and sometimes conflicting imperatives.¹⁸ An international financial crisis and mounting domestic public spending placed fiscal pressures on the Argentine treasury, which therefore sought to boost revenues sharply. In turn, raising public-utility rates on the wealthy was consonant with the government’s redistributive goals.¹⁹ And the targeted areas that were, indeed, wealthy. The household incomes of people in our samples, whether their rates rose or remained unchanged, placed them in the top five percent of Argentine households.²⁰ But at the same time the

¹⁶ Bril-Mascarenhas and Post 2014.

¹⁷ See Section 3 in Appendix.

¹⁸ This interpretation is based on interviews with staff in the Ministry of Planning. See Section 2 in Appendix details.

¹⁹ See “El gobierno insiste en que no es un ajuste la reducción de beneficios,” *La Nación*, November 18, 2011.

²⁰ Based on calculations using Permanent Household Survey (EPH), fourth trimester, 2011, National Statistics and Census Institute (INDEC).

government was anxious to avoid an upsurge in inflation. Raising rates in some wealthy areas but not others was a way to dampen the inflationary effects of the price hikes.

Why did the government use geographic targeting criteria rather than, say, energy consumption or household income? The government's choices reflect considerations of efficiency and administrative capacity. In interviews, government officials reported having considered targeting households by income or consumption. But ultimately the staff did not trust the accuracy of its household-level data on income, and believed consumption to be an unreliable indicator of social class. Therefore the blunt instrument of geography emerged as a reasonable and quick option.

4 RESEARCH DESIGN

The government announced the price increases in November 2011, and raised rates for different utilities at distinct moments, beginning in March 2012. By September 2012, it had increased rates in all of the targeted areas. Our telephone survey was carried out in October and November.

4.1 *The Quasi Experiment*

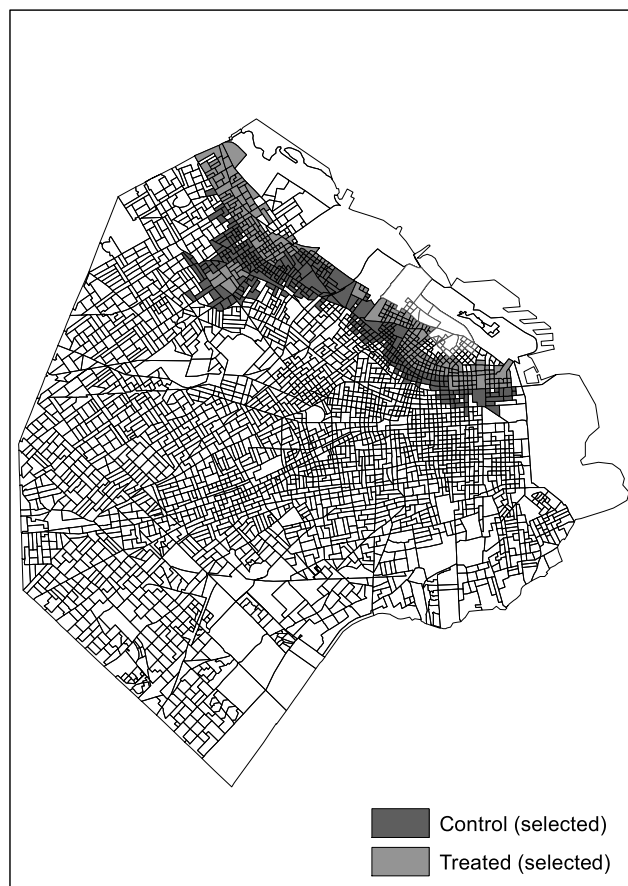
Crucial to our research strategy is that the price hikes were assigned in a manner that was as-if random, so that those who saw their rates spike were no different, on average, than those whose rates remained unchanged. Assignment to the price hikes would *not* have been as-if random if political operatives had been able to choose whose rates would rise and whose would not. But in fact the government lacked the motivation and capacity to select the targeted areas based on political loyalties. Clientelistic party machines, which trade favors for votes, are widespread in other parts of the country but absent in the wealthy areas exposed to the price hike.²¹ Even if the government

²¹ Stokes et al. 2013.

attempted to assign the price hike to households based on past vote choice, the way in which voters are assigned to voting stations prevented it from doing it precisely enough to match the individuals in the vicinity of the borders covered by our sampling frame.

We went to additional lengths to maximize the similarity of our subjects across conditions. Our sample included people who did and who did not experience the price hike living within a three-block radius. We used nearest-neighbor matching applied to census tract data to select similar treatment and control areas from which to sample (Figure 1 shows sampled census tracts). Balance tests suggest that as-if random assignment to the price hike is plausible. Individuals in the treatment and control groups are statistically indistinguishable along a host of observables.²²

Figure 1: **Control and treated census tracts for the final sample**



²² See Section 1 in Appendix.

4.2 *The Survey Experiment*

A key feature of our research design was a survey of people residing on both sides of the policy border. 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 interviews included survey experiments that probed the sensitivity of respondents' preferences for redistribution to interpersonal comparisons. We take advantage of individuals' limited knowledge about how widespread the increases were in their neighborhood to frame reactions to the policy.²³ The survey experiment entailed randomly assigning respondents on each side of the policy border to one of four treatment groups:²⁴

- The **intra-class inequality** treatment made salient interpersonal comparisons between wealthy peers. The wording depended on whether the individual's rates had risen or remained unchanged.
- The **cross-class inequality** treatment made salient its potential for redistribution from the wealthy to the poor, emphasizing the altruistic dimensions of the policy. This question, like the placebo, is the same for those who experienced and those who avoided the price hike.
- The **placebo** treatment group heard a neutral description of the policy.
- The **control** received no framing of the policy and were not asked for an opinion about it. This group allows a clean test of the pocketbook effect of the price hike on attitudes towards redistribution that is not contaminated by priming the policy.

²³ A leading newspaper found that a majority viewed subsidy withdrawals as the "most important piece of economic news" of the year. *La Nación*, "El fin de los subsidios, el tema económico de 2011 según los lectores," December 3, 2011. On uncertainty about targeting see *Clarín*, "Una resolución que se anunció antes de que fuera debatida," November 27, 2011, and *La Nación*, "Crecen las quejas de los usuarios," January 13, 2012.

²⁴ See Section 6 in Appendix for exact wording.

Table 3: **Number of Observations per Experimental Group**

<i>Quasi Experiment</i>	<i>Survey Experiment</i>			
	Placebo	Control	Intra-class	Cross-class
Control	113	123	125	123
Treated	137	128	129	127

The combination of the four groups in our survey experiments with the exposure or non exposure to the price hike produces eight experimental groups of roughly 125 individuals (Table 3).

5 ANALYSIS

5.1 *Empirical Strategy*

We examine several outcomes that gauge preferences for redistribution. One outcome of interest was support for the price adjustment policy. After the introductory framings just discussed, we posed the question, *How would you characterize the government's decision? Would you say it was very good, good, neither good nor bad, bad, or very bad?* Also of interest are spillover effects: any impact the price shock, and framings of it, might have on attitudes towards redistribution, and unemployment insurance.²⁵

Our core estimation strategy is instrumental-variables regression.²⁶ This approach allows us to consistently estimate the causal effect of the policy in the presence of some non-compliance with treatment assignment.²⁷ It means that our estimate of any pocketbook effect of the price hike on preferences is a local average treatment effect

²⁵ See Section 7 in Appendix for exact wording and descriptives.

²⁶ All results hold when using intent-to-treat analysis where we regress our dependent variables on household location. See Section 9.1. in Appendix

²⁷ The government allowed some exceptions to the subsidy withdrawals. For a description, see the appendix.

(LATE).²⁸ The instrument is measured only by location, defined by whether the respondent's home is on one side or the other of the policy border. Given the absence of public records, we measured treatment receipt with a survey question that asked whether the respondent had experienced the price hike.²⁹

In addition, our interest in the joint influence of rate increases and framings focuses our attention on conditional average treatment effects (CATEs). Thus, we estimated separate instrumental-variables regressions of each outcome on the following independent variables: (i) exposure to the price hike instrumented by a binary indicator of treatment assignment, (ii) a binary indicator of survey experimental condition, and (iii) the interaction of these two variables. We obtained the standard errors 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.

Testing Hypothesis 1: Pocketbook Effects

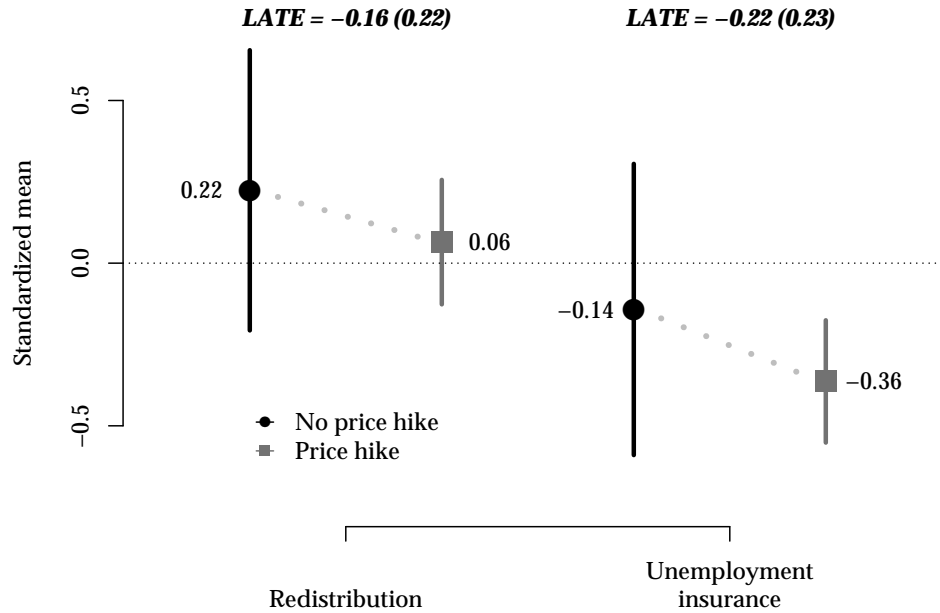
We first estimate the pocketbook effect: did wealthy people who have just endured a sharp utility rate increase display heightened opposition to redistribution? For this test we focus on those respondents who were assigned to the control group in the survey experiment, and compare their attitudes conditional on exposure to the price hike. Results from instrumental variables tests of the pocketbook hypothesis are reported in Figure 2. The evidence in its favor is scant. Support for redistribution and unemployment insurance were very similar between the rate-hike and rates-stable groups.³⁰ None of the differences in mean levels of support is significantly different than zero. Pocketbook considerations, then, were not much in evidence.

²⁸ Section 8 in the Appendix explains how the assumptions required for consistent LATE estimation apply to this context.

²⁹ Approximately 80% of respondents were "compliers". See Table A3 in the Appendix for details.

³⁰ Recall that we did not mention the policy to the survey experimental control group, so this outcome cannot be measured for these respondents.

Figure 2: Testing Hypothesis 1: Pocketbook Effects



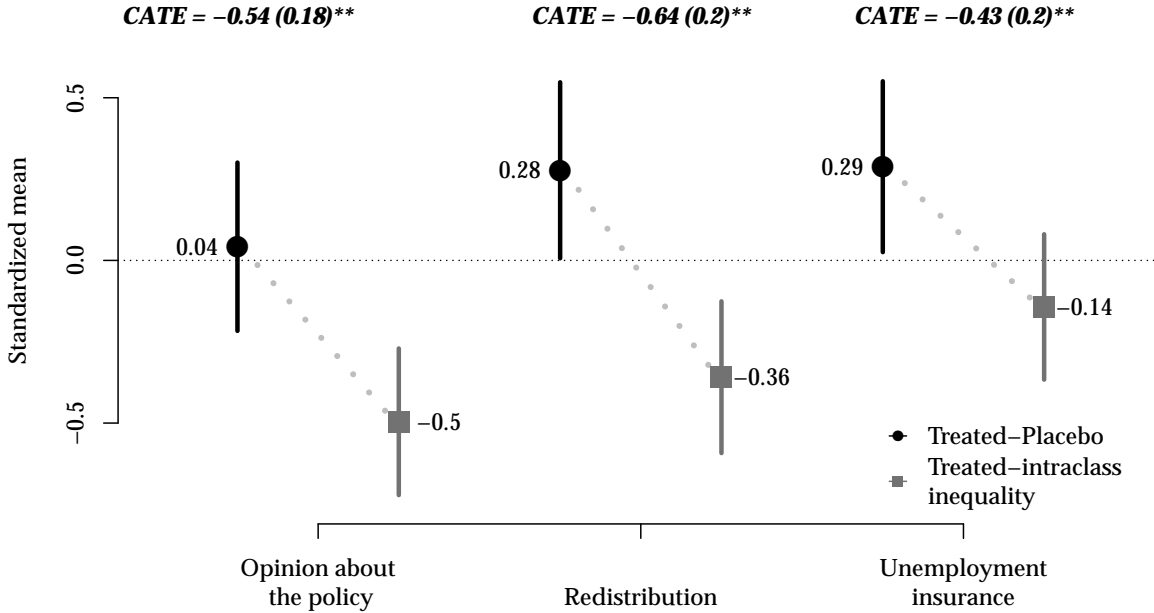
Support for redistribution and unemployment insurance by exposure to price hike among respondents in the control group. Numbers in bold are Local Average Treatment Effects (LATE) with robust standard errors in parentheses ($***p < 0.01$, $**p < 0.05$, $*p < 0.1$). Black dots (no price hike) and dark grey squares (price hike) represent mean standardized level of each outcome by treatment group; vertical segments are 95% confidence intervals.

Testing Hypothesis 2: Resentment

What happens when subjects whose rates rose are reminded that other affluent households escaped it? The question points to the most salient and robust finding of our study: the fragility of support for redistribution in the face of invidious comparisons among the wealthy. Figure 3 compares levels of support for redistribution among people whose utility rates rose, depending on whether they were exposed to the intra-class inequity or the placebo treatment. Support for the price-adjustment policy, for redistribution and for unemployment insurance – all slipped sharply when people whose rates rose were reminded that they had been singled out whereas their neighbors were left alone.

The evidence, then, suggests a strong predisposition among the wealthy toward

Figure 3: Testing Hypothesis 2: Resentment



Support for the policy, redistribution and unemployment insurance among subjects *exposed to the price hike*, comparing intra-class inequality with placebo. Numbers in bold are Conditional Average Treatment Effects (CATE) with robust standard errors in parentheses ($***p < 0.01$, $**p < 0.05$, $*p < 0.1$). Dark grey squares (intra-class inequality) and black dots (placebo) represent means; vertical segments are 95% confidence intervals.

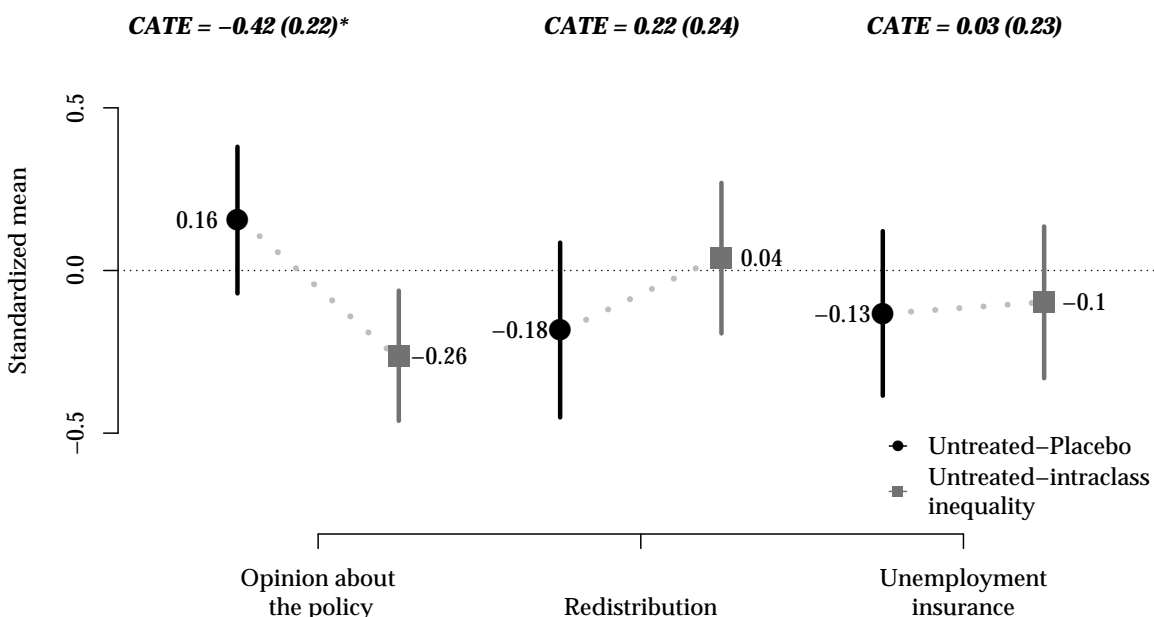
disadvantageous inequality aversion – i.e. resentment. The price increase did not in itself turn large numbers against redistribution. But when those who endured the price shock were reminded that their losses were not shared by others of their economic stratum, their views of redistribution soured.

Testing Hypothesis 3: Empathy

How did people whose rates remained unchanged respond to the intra-class inequity framing? If they also evinced low levels of support, the inference would be that they experienced empathy (advantageous inequity aversion) vis-à-vis their peers. But this group evinces nothing like the across-the-board collapse of support for redistribution on display among people in the intra-class inequity framing whose rates rose. We do see a drop in support for the price-hike policy itself when those with stable rates were reminded of the price hike (Figure 4). But this drop in support does not spill

over onto broader attitudes towards redistribution. The asymmetrical reaction among people who did and did not experience the price hike suggests that, while there may be a norm of fairness in play, a norm that says, “similarly situated individuals should get the same fiscal treatment,” such a norm is less potent than the invidious comparisons on display in Figure 3.

Figure 4: Testing Hypothesis 3: Empathy



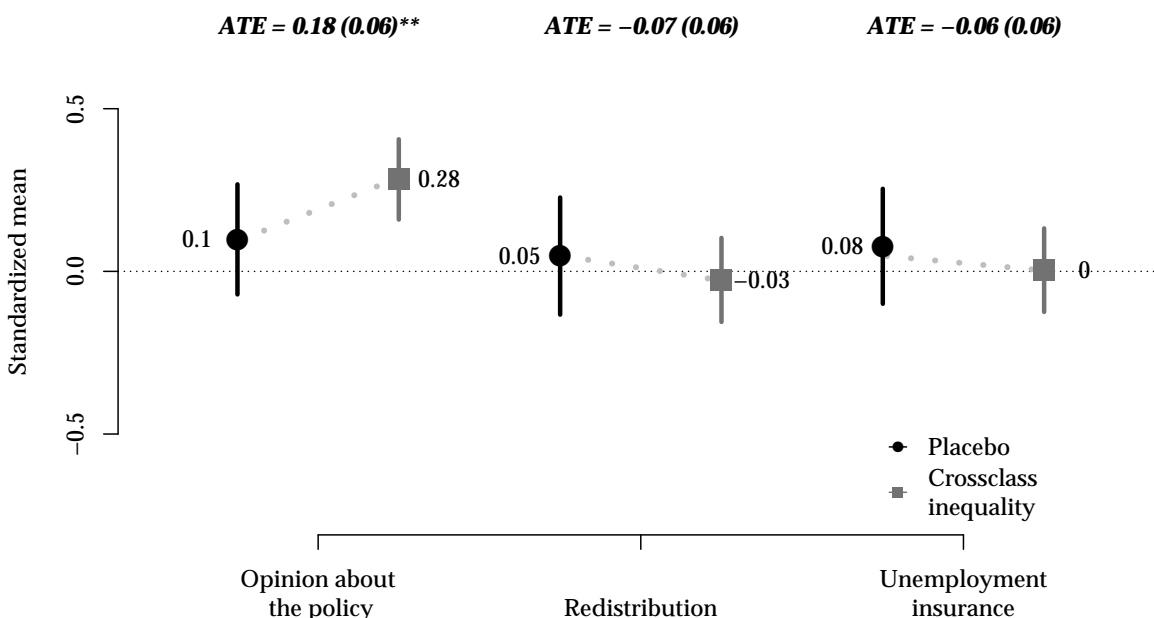
Support for the policy, redistribution and unemployment insurance among subjects *not exposed to the price hike*, comparing intra-class inequality with placebo. Numbers in bold are CATEs with robust standard errors in parentheses (** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Dark grey squares (intra-class inequality) and black dots (placebo) represent means; vertical segments are 95% confidence intervals.

Testing Hypothesis 4: Altruism

Did our samples evince support for the price-hike policy and for redistribution when reminded that the state’s intervention reduced the gap between the wealthy and the poor? The altruism (cross-class inequality) framing had a modest effect on people’s support for the price-adjustment *policy*. In comparison with the placebo group, average levels of support were higher in the cross-class treatment group, by a modest one-fifth of a standard deviation. Beyond views of the policy, however, the cross-class treatment

caused scarcely a ripple in our samples' opinions of state-led redistribution. Thus, Hypothesis 4 receives little support. A general lesson from our study, then, is that appeals to altruism and cross-class empathy left our affluent samples relatively unmoved. The power of these appeals paled compared to feelings of resent.

Figure 5: Testing Hypothesis 4: Altruism



Support for the policy, redistribution and unemployment insurance by exposure to cross-class inequality treatment or a placebo, pooling across price-hike and price-stable groups. Numbers across the top are the ATE of the cross-class inequality treatment compared to the placebo; robust standard errors of the ATE are in parenthesis (** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Black dots (placebo) and dark grey squares (cross-class treatment) represent means; vertical segments are 95% confidence intervals.

6 Rival Interpretations

Here we take up several possible objections and alternative interpretations of the findings.

Frame strength. One possible concern is that the intra-class inequity aversion framing was inherently stronger than the other ones³¹ If so, our main finding of

³¹ See Chong and Druckman 2007.

widespread resentment driving down support for redistribution would merely be an artifact of a particularly powerful frame. Yet, were this the case, we would expect this frame to have uniformly powerful impacts across our subjects. But the intra-class treatment had little impact on respondents who escaped the price hike. Thus it seems unlikely that the intra-class resentment findings was an artifact of a strong frame.

Might frame weakness explain the very modest impact of the altruism treatment? Perhaps wealthy residents of Buenos Aires, many of them staunch opponents of the Peronist government, had altruistic predispositions but were disinclined to believe that the government was capable of achieving redistributive results. With this problem in mind, we worded the cross-class treatment in a way that attributed the claim that the policy achieved redistributive goals to neutral experts, rather than to the government. In addition, an observable implication of the weak-frame interpretation suggests itself. In the framing literature, the strength of a frame is associated with its persuasiveness. Thus, a weak frame will influence individuals whose ideological priors are aligned with its message but leave the ideologically distant unmoved.³² We therefore explored whether the altruism frame elicited polarized reactions among respondents, depending on their prior ideological leanings measured by vote choice. We find no such polarization.³³

Compound treatment. Another question has to do with the exact nature of the price-hike “treatment.” While we used the government’s unusual rollout to study the impact of fiscal shocks on attitudes, perhaps the uneven rollout was itself the most salient treatment, rather than lost income. This possibility would have been of greater concern had we found that the price shock *per se* had generated opposition to the policy. But this is precisely not what we found. Neither income losses nor the rollout of the program turned people against it; their reactions relied on framings that made interpersonal comparisons salient.

Elite framing. The politicization of the price hike could contaminate our results if

³² Chong and Druckman 2007.

³³ See Section 10.3 in Appendix.

our treatments made respondents more sensitive to elite messages. We believe that this alternative explanation is implausible for two reasons. The first reason is that the nature of elite discourse would have led to quite different results. Rather than questioning the government's expressed progressive goals, the opposition criticized the government for masking an effort raise revenues appear as progressive income policy. Government elites, in turn, stressed the social benefits of redistributing income and generating revenue for social programs. The second reason is empirical. If elite discourse drove the results, left-leaning individuals, who might have taken their cues from the government, should not exhibit the patterns of resentments that we observe. But they did.³⁴ Further analyses show that the force of resentment was so strong that it eroded support even among a minority of respondents who were leftist government sympathizers.³⁵ Thus, the influence of elite discourse is unlikely to have lain behind the response of our samples to the intra-class treatment.

7 ELITE RESENTMENT AND THE FRAGILITY OF SUPPORT FOR REDISTRIBUTION

The Argentine government's unevenly imposed fiscal shock opened a window into the political-economic views of affluent citizens of a middle-income developing country. We find that wealthy people's reactions to redistributive measures had little to do with the measure's pocketbook effects, even when it amounted to a five-fold increase in their public utility bill. People who endured the price shock were not more opposed

³⁴ We evaluated this possibility replicating the analysis within two ideological subsets of the sample: leftists and conservatives. We asked respondents about their past voting behavior after the survey experiment, so it is possible that their recollections of past voting were influenced by the experiment. Yet the self-reported voting patterns, which we use to assign them to ideological types, in aggregate match the voting patterns in these districts of Buenos Aires. See Section 10 of the Appendix.

³⁵ See Figure A6 in Appendix for the results.

to it, nor to other forms of state intervention, than were their similarly situated peers who escaped the shock. Instead of pocketbook reactions, the clearest driver of opinion was resentment: an allergy to the idea that one had suffered where one's peers had not. The reaction was self-, not other-oriented. We found few signs of intra-class empathy among those whose rates remained unchanged.

Our study sheds light on the origins of preferences for redistribution, a debated topic among social scientists. Our strong research design allows us to overcome some of the pitfalls of conventional regression analyses and offer causal evidence on the preferences of individuals when they are exposed to a redistributive policy. This allows us to show that the resentful “keeping up with the Joneses” mentality identified in laboratory experiments might be a powerful force shaping attitudes towards redistribution in the real world.

Our study has important implications for research on the political economy of redistribution. We contribute to comparative research concerning the political conditions under which redistributive policies are viable. If one were to set about building support for redistribution, a lesson from our study is not to write the wealthy off from the outset. The loss of post-fisc income did not in itself mobilize opposition among the wealthy. And responses to questions in our survey about subjects' past voting behavior indicate that about a quarter of the sample came into the study probably supporting some redistribution.³⁶ For the majority of the sample that needed more persuading, the key strategic lesson is to steer clear of policies that might leave them feeling relatively ill-treated compared to their affluent peers. Any inkling of differential treatment aroused powerful antipathy, even among the minority of left-leaners.

Are these strategic lessons useful beyond Argentina, and beyond the kind of policies we study? One distinctive feature of the Argentine experience is the government's decision to impose a price shock in a starkly inequitable manner. But

³⁶ See Section 10 of the Appendix.

concerns about inequities in the incidence of burdens are hardly unique to the Argentine case. Tax codes, for instance, feature discontinuous rate structures and invite the exploitation of loopholes, not to mention partial compliance or evasion.³⁷ And there is evidence from both advanced and developing economies suggesting that perceived inequities in tax systems affect citizens' incentives to pay taxes.³⁸ For these reasons, the implications of our study are likely to be of relevance to a broader range of countries.

Another particularity of the Argentine experience is that the redistributive policy involved a hike in public prices and not a change in the tax structure. But the conceptualization in political-economic models of taxation and redistribution, *a la* Meltzer and Richard, envision a flat tax. We cannot know whether a tax hike with arbitrary discontinuities would have yielded similar results. But it is important to underscore that consumer subsidies play a central role in redistribution in the developing world.³⁹ They represent a sizable share of the public budget and have substantial pocketbook implications, which are often to transfer income from the poor to the middle and upper classes. Given the salience of subsidies in the politics of redistribution, it is reasonable to use them as a window into the formation of preferences among the wealthy.

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³⁷ See e.g. Romer and Romer 2014; Slemrod, Blumenthal, and Christian 2001.

³⁸ Alm, McClelland, and Schulze 1992; Slemrod 2007; Torgler 2005.

³⁹ Bates 1981; Bril-Mascarenhas and Post 2014; Rickard 2012.

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Supplementary Appendix for
When do the Wealthy Support Redistribution?
Inequality Aversion in Buenos Aires

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1 Balance tests

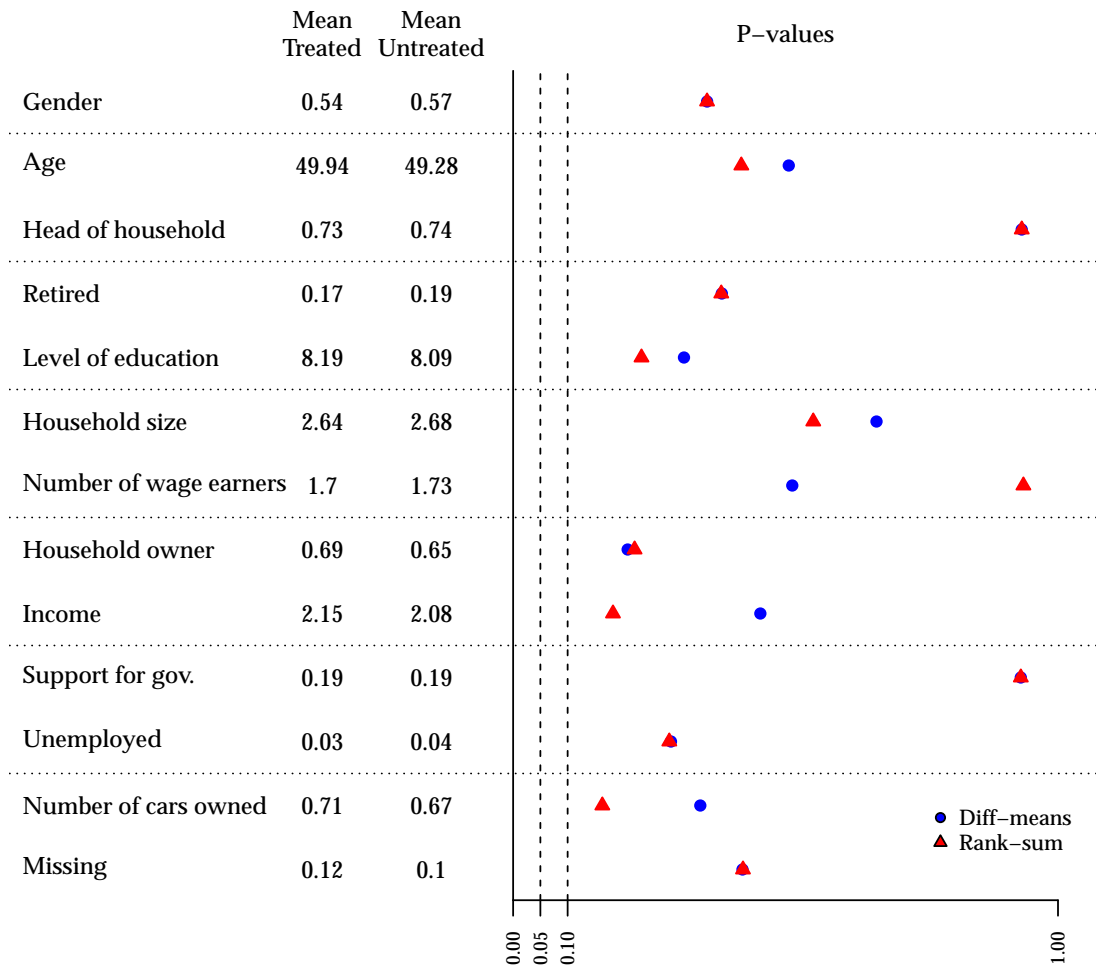
Natural experiment

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



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.

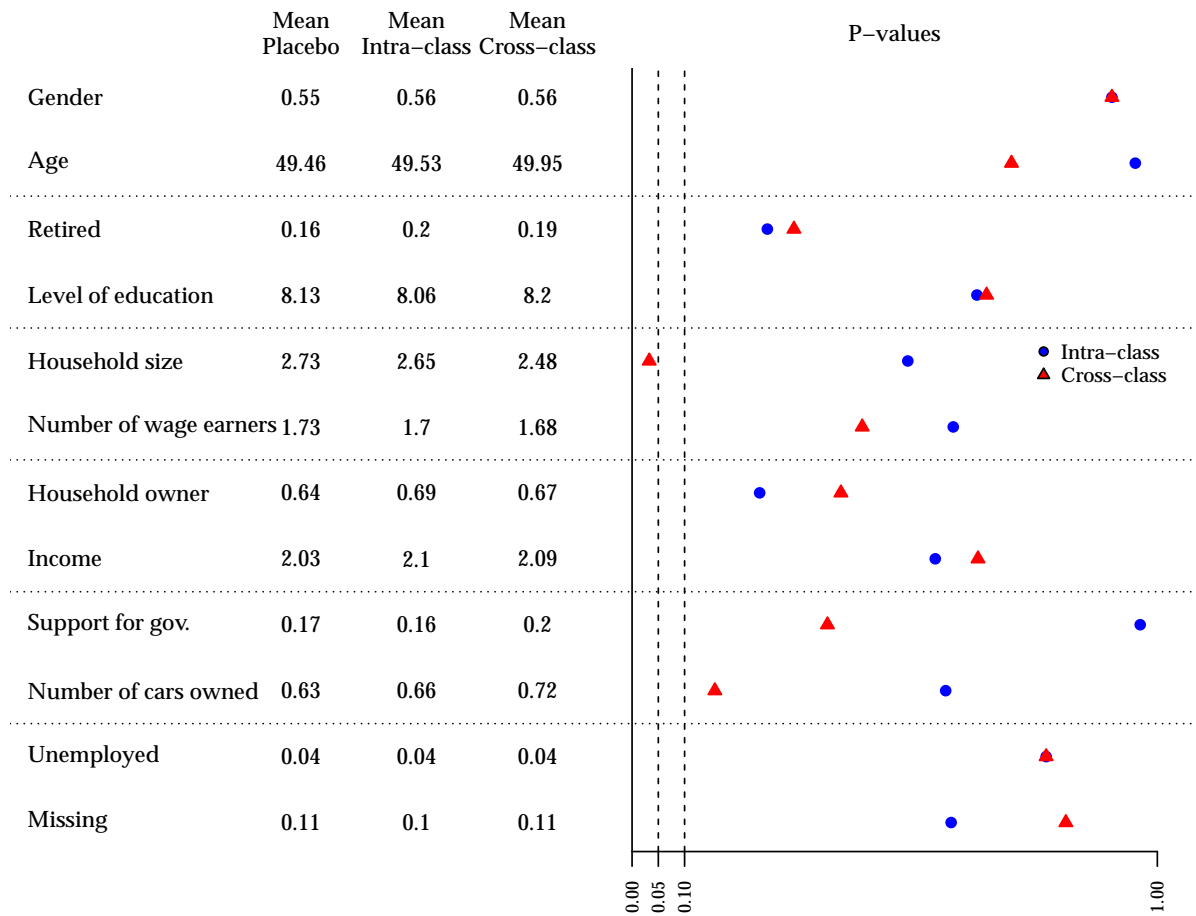
	Model 1
Constant	1.423*** (0.118)
Gender	−0.014 (0.033)
Age	0.001 (0.001)
Retired	−0.076 (0.057)
Education	0.002 (0.010)
Household Size	−0.010 (0.015)
Wage Earners	−0.022 (0.027)
Gov. Supporter	−0.010 (0.041)
Unemployed	−0.085 (0.087)
Household Owner	0.030 (0.039)
Income	0.029 (0.020)
N. of Cars	−0.004 (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).**



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

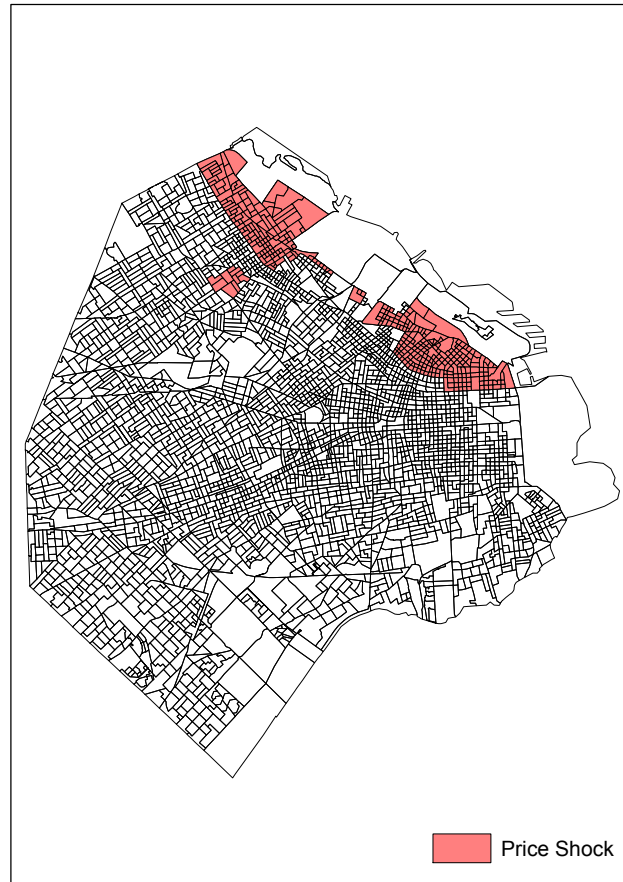
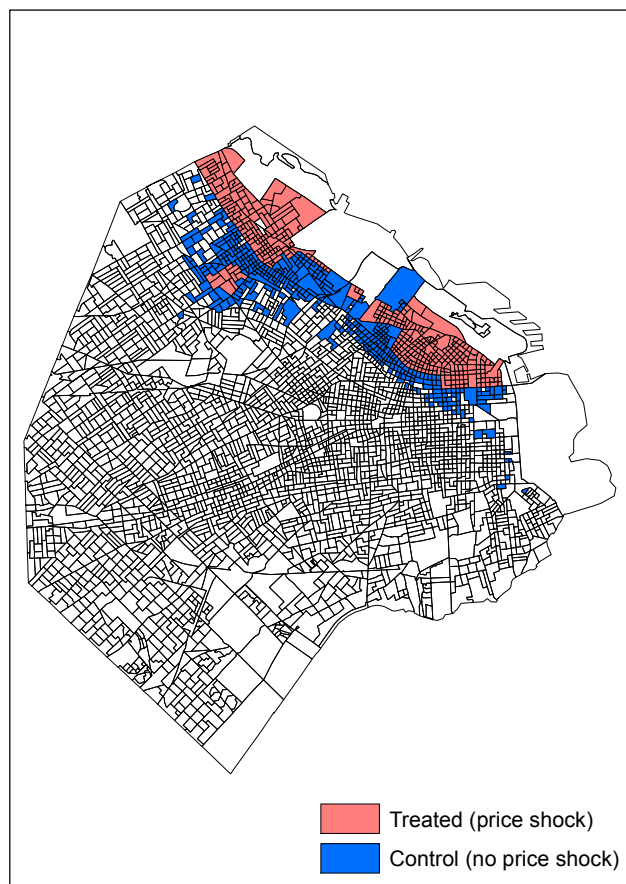


Figure A4 maps the census tracts that we selected after matching the ones targeted by the government with untreated census tracts. Matching was based on a nearest-neighbor algorithm using census data.

Figure A4: **Districts selected by Nearest-Neighbor matching.**



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.

¹ The three block comparison was in all cases true.

According to an independent study prepared by the University of Buenos Aires, this 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?

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

Unemployment insurance

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.)

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

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

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 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

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.

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.

9 Extended results and robustness tests

9.1 Intent-to-Treat analysis

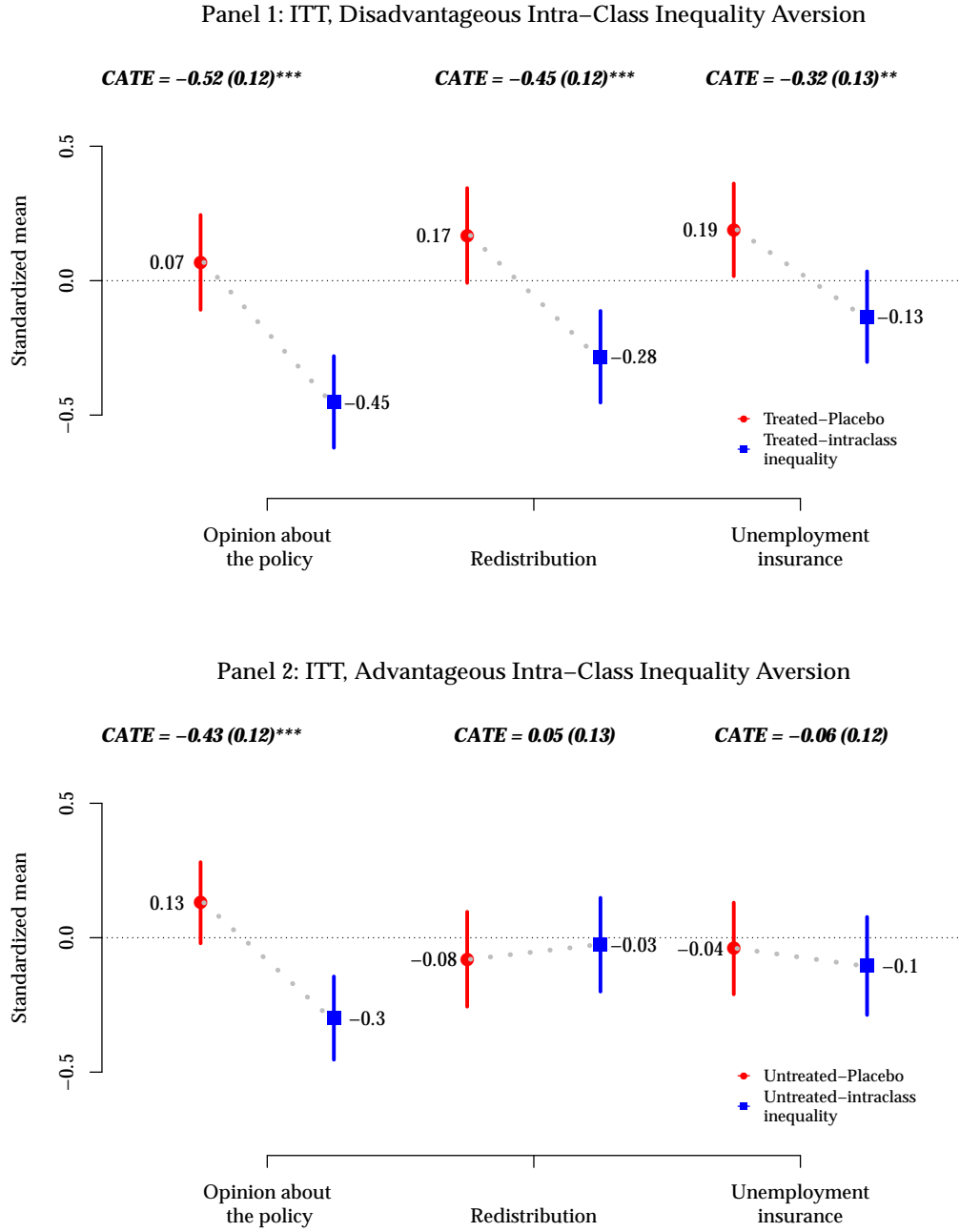
Given non-compliance with treatment receipt, our main identification strategy relies on instrumental variables. But our results hold when using intent-to-treat (ITT) analysis where we regress the key outcomes on our instrument for exposure to the policy – a geographic-location variable indicating on 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 (0.195)	0.212 (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)



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 (0.126)	−0.183 (0.134)	−0.132 (0.132)
<i>Intraclass</i>	−0.417** (0.170)	0.221 (0.181)	0.034 (0.178)
<i>Treatment</i>	−0.114 (0.222)	0.459* (0.236)	0.420* (0.232)
<i>Crossclass</i>	0.111 (0.181)	0.148 (0.191)	0.084 (0.188)
<i>Intraclass</i> × <i>Treatment</i>	−0.120 (0.291)	−0.857*** (0.310)	−0.465 (0.304)
<i>Crossclass</i> × <i>Treatment</i>	0.146 (0.313)	−0.444 (0.330)	−0.295 (0.325)
<i>Control</i>		0.407** (0.190)	0.274 (0.190)
<i>Control</i> × <i>Treatment</i>		−0.619* (0.329)	−0.641* (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 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 hold after disaggregating by ideology. The most reliable way 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 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**

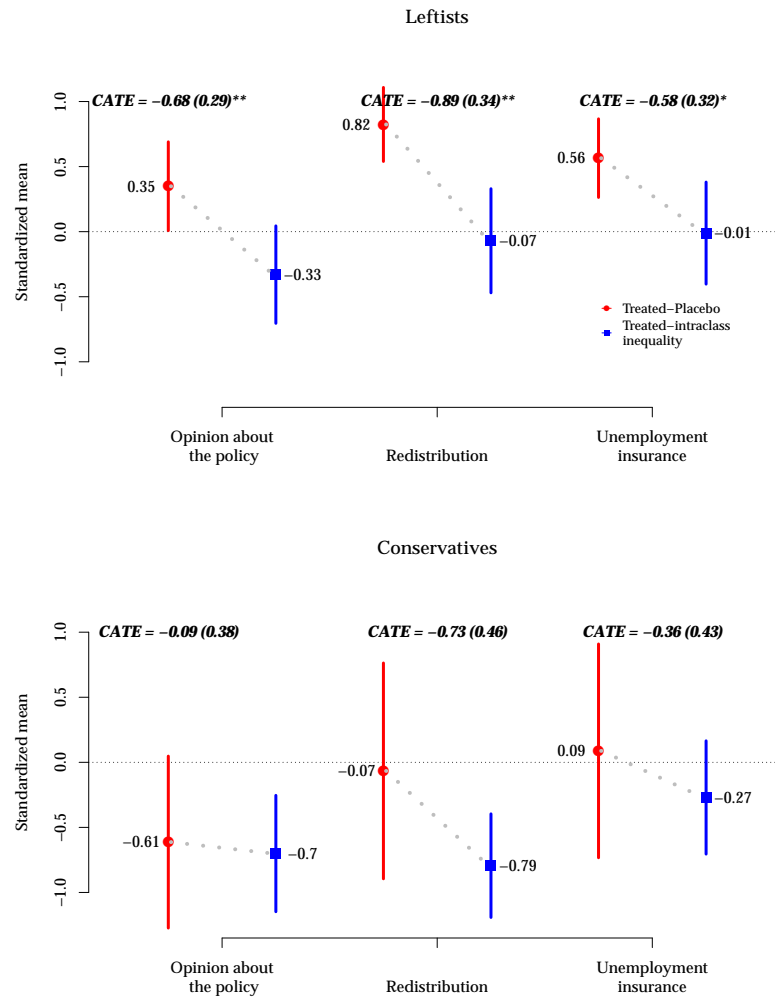
	Opinion about the policy	Redistribution	Unemployment insurance
(Intercept)	−0.33*** (0.08)	−0.41*** (0.07)	−0.17* (0.07)
Ideology	0.66*** (0.10)	0.76*** (0.08)	0.43*** (0.09)
Observations	427	559	566

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Standard errors in parentheses

10.2 Testing resentment by ideological subgroups

Figure A6: Testing resentment by ideology



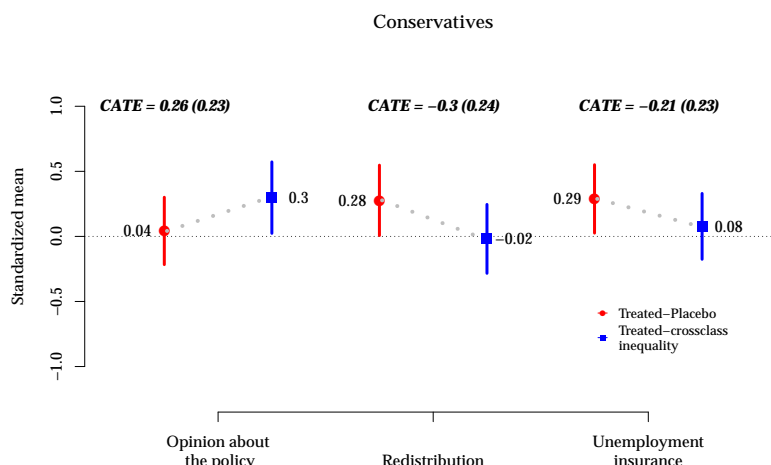
Support for the policy, redistribution, and unemployment insurance among subjects exposed to the price hike, by exposure to the intra-class disadvantageous inequality or placebo treatments, and by ideological orientation. The numbers across the top are the Conditional Average Treatment Effect of the 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 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.

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