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Why Do People Demand Rent Control?

Daniel Müller (✉) Elisabeth Gsottbauer*

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Abstract

We conduct a representative survey experiment in Germany to understand why people support inefficient policies. In particular, we measure beliefs about and preferences for rent control – a policy that is widely regarded as harmful by experts. To tease out causal mechanisms, we provide randomly selected subsets of participants with empirical estimates about the effects of rent control on rent prices and housing supply and with information about the consensus among economists against rent control. We find that people update their beliefs and that this leads to lower demand for rent control. Left-wingers update their beliefs more strongly, which reduces the ideological gap in support for rent control by about one quarter. Providing information about economists’ rejection of this policy leads to the largest reduction in support. However, the main drivers of support for rent control are fairness considerations and profit motives. Our study also highlights the importance of trust since treatment effects are consistently larger among those who indicate trust in the scientific information provided to them.

Keywords: beliefs about rent control, demand for bad policies, survey experiment, trust in experts.

JEL classification: H10, H30, H31.

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1 Introduction

Economists have long understood the economic costs of price controls theoretically (Friedman and Stigler, 1946, e.g.) and empirically (Glaeser and Luttmer, 2003; Davis and Kilian, 2011). Consequently, they overwhelmingly oppose price controls in general and rent control in particular.¹ Regardless, rent control policies (i.e. the practice of limiting the price a landlord may charge) experience great support among the general public, which is not only evident in survey data but also from the fact that rent control policies are frequently implemented in practice.² It is important to understand why this is so, not only because housing markets are economically significant but also because answers to this question might also help understand differences in opinions of the general public and economists in other areas (e.g. carbon taxation). One answer to this puzzle might be that economists and the general public hold different beliefs about the effects of economic policies. Indeed, surveys indicate that the public’s opinions about economics often differ greatly from those of economists (Caplan, 2002; Jacob, Christandl, and Fetchenhauer, 2011; Sapienza and Zingales, 2013). Other reasons might be that people do not trust expert advice or that they make different equity-efficiency trade-offs.

In this paper, we aim to understand the disconnect between the opinion held by economic experts and the public in the domain of economic policy. To do so, we conduct a survey experiment among a representative sample of the German population, in which we (i) elicit beliefs about effects of rent control on rental housing supply and rent prices, (ii) measure demand for rent control and (iii) investigate whether beliefs about the workings of such a policy causally affect its support, and (iv) ex-

¹“IGM Expert Panel” at the University of Chicago: www.igmchicago.org/surveys/rent-control/.

²For instance, survey results from the “German Internet Panel” (Wave 14, from 2015)³, a representative online panel from the German population based at the University of Mannheim, show that 81% of 3,500 participants find rent control in Germany “rather good”, “good” or “very good”. Only 8% oppose such a regulation. Similar results are found in the US and the UK. The results from our survey among Germans show that the median support for rent control is a 7 on a 1-10 scale.

amine whether people follow expert advice. In particular, we compare demand for rent control in a control group to demand in three different treatment groups. The first treatment group, the *quantity treatment*, is provided with empirical estimates from Diamond, McQuade, and Qian (2019), who show that a rent control regulation in San Francisco reduced the supply of rental housing by about 15%. In the second treatment, the *price treatment*, participants are informed that this policy “likely drove up market rents in the long-run”. Both treatments thus provide information about the economic effects of rent control on housing supply and rents. The third treatment, the *expert treatment*, informs participants about the consensus among economists against rent control, exploiting a poll among leading economists.

In our survey, we elicit beliefs about quantity and price effects of rent control before and after treatment. This approach not only allows us to measure individual belief updating, but also puts us in a position to estimate the causal effect of beliefs on preferences. In addition, we elicit trust in the information provided to them and we ask respondents about the perceived fairness of rent control and whether they would profit financially from them. Our data thus allow us to examine the importance of trust and to disentangle the effects of (potentially mis-calibrated) beliefs from fairness concerns and selfish profit motives as drivers of support.

The main message of our paper is that people hold systematically mis-calibrated beliefs about the consequences of rent control and that these beliefs causally affect support for this policy. The same holds true when providing participants with expert recommendations against rent control. In more detail, the experiment delivers the following insights. First, we find that people hold too optimistic views about the consequences of rent control. The *quantity* treatment leads participants to expect larger reductions in supply and the *price* treatment causes participants to expect higher rents as compared to the control group. Second, both treatments lower support for rent control policies. This conclusion is not only evident in stated support but also in a revealed preference measure (an actual donation decision between two lobby groups). Third, respondents in the *expert* treatment lower their support for

rent control, too. The effect size is about twice as large as that of the first two treatments. Fourth, important determinants of support for rent control are fairness concerns and financial motives. Fifth, left-wingers are initially more optimistic about the effects of this policy and consequently update beliefs about supply-side reactions more strongly. As a result, the treatment cuts the left-right gap in support for rent control by about one quarter. Sixth, our data highlight the crucial role that trust in expert advice plays. Throughout, we consistently find that treatment effects are stronger for those who put more trust in the information provided to them.

We conduct several robustness checks that confirm the results of the paper. One concern one might have about survey experiments in general, is that participants answer the questions in a way they think is desired by the experimenter. We believe that this concern is less relevant in our case for several reasons. First, we include a block of unrelated questions between the information treatment and the outcome variables in order to veil the connection between both. In addition, and as highlighted by Haaland, Roth, and Wohlfart (2021), we use a neutral framing of the survey and the information treatments and employ a between-subject design in which demand effects are less likely. Second, we not only use a standard survey measure of preferences for rent control, but also a revealed measure (an actual donation decision) and find virtually the same result for both outcomes. Third, De Quidt, Haushofer, and Roth (2018) and Mummolo and Peterson (2019) show convincing evidence that experimenter demand is less of a concern in a variety of economic games and in survey experiments. Finally, we conduct an additional survey in which different treatments, in the style of de Quidt et al. (2018), try to “push” participants’ beliefs in different directions. Results from this survey show no evidence for the existence of experimenter demand effects in our survey. Another concern might be that the *quantity* information treatment, which provides a specific number to participants, leads to potential numerical anchoring effects (Tversky and Kahneman, 1974), that is, the quantity treatment might not actually update participants’ beliefs, but instead be the result of anchoring. We rule out such concerns in an additional survey in which

we provide different participants with a number and compare their beliefs to those of a control group that was not provided any number.

Our paper relates to several strands of literature. First, several papers have documented that voters suffer from systematic biases in decision-making, leading to poor policy choices. For instance, Kallbekken, Kroll, and Cherry (2011) find evidence for an aversion to implementing Pigouvian taxes in a laboratory experiment, even though this tax increases individual and social welfare. Sausgruber and Tyran (2005) investigate fiscal illusion of voters. In their experiment voters prefer higher indirect-over lower direct taxes although this decreases their profit. Moreover, Sausgruber and Tyran (2011) experimentally show that people prefer taxing others over taxing themselves, resulting in an income loss of up to 20%. Finally, Dal Bó, Dal Bó, and Eyster (2018) show that people might not anticipate equilibrium effects of new policies, leading to a demand for inefficient policies. An under-appreciation of equilibrium effects could also lead to a demand for rent control since people might not fully factor in the supply-side reactions (i.e. lower supply of housing). Second, we also relate to work that studies how beliefs of economists and the general public differ. For instance, Caplan (2002) finds that economists and the general public systematically disagree and hold different beliefs about economics issues. Further empirical evidence comes from Haferkamp et al. (2009) and Sapienza and Zingales (2013). Finally, our work relates to research that aims to understand how people reason about economic policies and taxes. For instance, Stantcheva (2021) provides comprehensive evidence in the areas of income and estate taxation. Related to the case of rent control is Brandts et al. (2022), who use a “refutation text” to influence support for rent control. They find that the text successfully shifts beliefs of undergraduate students about the usefulness of rent control in the direction of the beliefs of economic experts.³

The rest of the paper proceeds as follows. Section 2 explains the experimental

³An important difference to our study is the fact that the refutation text does not provide factual information about rent control. Instead, the text is designed to encourage critical thinking in students. The authors also do not measure beliefs about the effects of rent control.

design of the main survey in more detail. Next, Section 3 presents our results concerning the beliefs about rent control. Section 4 then illustrates our main results about the support for rent control. Section 5 discusses potential mechanisms that drive the main result and also presents the robustness towards experimenter demand and anchoring effects. Finally, Section 6 concludes.

2 Experimental Design and Data

Overview and Sample. The survey experiment is administered in a sample of the German population exploiting the infrastructure provided by a professional survey company. By construction, the sample is representative along age, income and gender.⁴ The main study was conducted in May 2021. In total, 4,034 respondents participated in the main survey (we pre-registered 4,000).⁵ The main survey includes two attention checks (Haaland, Roth, and Wohlfart, 2021).⁶ As pre-registered, we drop those participants who do not pass the attention checks.

Prior beliefs. After a short introduction, we elicit participants’ baseline priors about the effects of rent control policies on supplied housing quantities and rents. In two steps we ask all participants if they think rent control affects the supply of rental housing. In the first step, participants are asked whether they believe in an increase,

⁴Table 8 in the Appendix displays summary statistics separately for each treatment for those three variables plus education, trust in the study, as well as price- and quantity priors. A series of ranksum tests displays no significant differences in means between the treatments and the controls for each of those variables.

⁵We pre-registered the experimental survey, sample size and our empirical approach at AsPredicted.org (No. 64761). Figure 8 in the Appendix summarizes the survey design. Description of the variables and coding of responses is summarized in Section 7.3. Translated instructions and screenshots of all surveys are provided in a Supplementary File.

⁶The first one, immediately after the introduction, asks the participant in an open-ended question to indicate “brown” in response to the question about her favorite color. The second one is located after the information intervention and asks the participant about the topic of the information intervention in a multiple choice question.

decrease or no change in the supply of rental housing, as compared to a situation without rent control. Participants who indicate a decrease or an increase in the supplied quantity have the opportunity to further specify their answer with the help of a slider (in percentage changes). We choose this design to ensure that respondents do not confound positive and negative percentage changes. We elicit beliefs about the impact of rent control on the level of rents on a seven-point scale ranging from “much higher” to “much lower” rent prices. We opt for this qualitative scale because the study by Diamond et al. (2019) does not provide numerical estimates of price effects.

Information treatments. Participants are then randomized into three different information treatments and one control group. The first treatment group (*quantity*) receives information from the study by Diamond, McQuade, and Qian (2019), which shows that “the supply of rental housing decreased by 15 percent” in San Francisco due to the introduction of rent control. The second treatment group (*price*) receives information about the finding from the same study, namely that rent control “likely drove up market rents in the long run”. The third treatment group (*expert*) receives information about the consensus among economists against rent control. Specifically, we report poll results from the IGM Economic Expert Panel indicating that “more than 80% of participating economists oppose rent control due to predominantly negative effects on the housing market, which harms tenants and landlords in the long run”. Finally, participants in the *control* group receive information about an unrelated economic study concerning the economic consequences of Brexit. We opted for an “active” control group to keep the length of the survey constant for all respondents. The information presented to all four groups was intentionally kept as concise as possible.

Socio-demographics. Next, we include questions on participants’ socioeconomic background such as age, gender, income, education, household size, employment status and place of residence and we also assess participants’ housing situation. These questions also create a break between the information treatment and elicitation of

the posterior beliefs.

Posterior beliefs. In this block, we again elicit price and quantity beliefs using the same type of questions as before.

Support for rent control. Next, we ask all respondents for their attitudes towards rent control on a 10-point scale from (1) “very strongly oppose” to (10) “very strongly support”. Our secondary measure of support is the decision to donate 100 euros to either a pro- or an anti-rent control lobby group (note that this question was asked at the very end of the survey). Participants could donate only the full amount. Not donating was not an option. We then (truthfully) told participants that we will randomly select one respondent and donate the amount of 100 euros according to the decision made by that participant.

Other attitudes. We measure several other attitudes including support for price controls in general, left-right economic ideology and support for free markets. Moreover, we assess fairness perceptions (of rent control) and respondents’ profit motives (whether they would financially profit from rent control). We also elicit efficiency concerns (whether they think people stay in their apartments longer and whether they think there is inefficient matching in the rental housing market due to rent control) and concerns about rent prices (whether they think that rent prices are a problem). Finally, we assess trust in the economic study provided in the information treatment on a 5-point scale, ranging from low to high trust.

3 Beliefs About Rent Control

A first important question to ask about our data is what beliefs people hold about the quantity and price effects of rent control and whether participants update these beliefs in response to the treatments. To answer these questions, Figure 1 displays priors and posteriors about supply (left-hand panel) and price reactions (right-hand panel) for each treatment. The first thing to notice is that, on average, prior beliefs about the quantity reactions are negative and around -8.5 percentage points and

that priors about price reactions are close to the expectation that rent control does not change rents. Next, we find evidence that participants in the *quantity* treatment downward-adjust their posterior beliefs about supply reactions and participants in the *price* treatment upward-adjust their posteriors about price reactions, suggesting that in this experiment both treatments induced more pessimism about the workings of this policy.

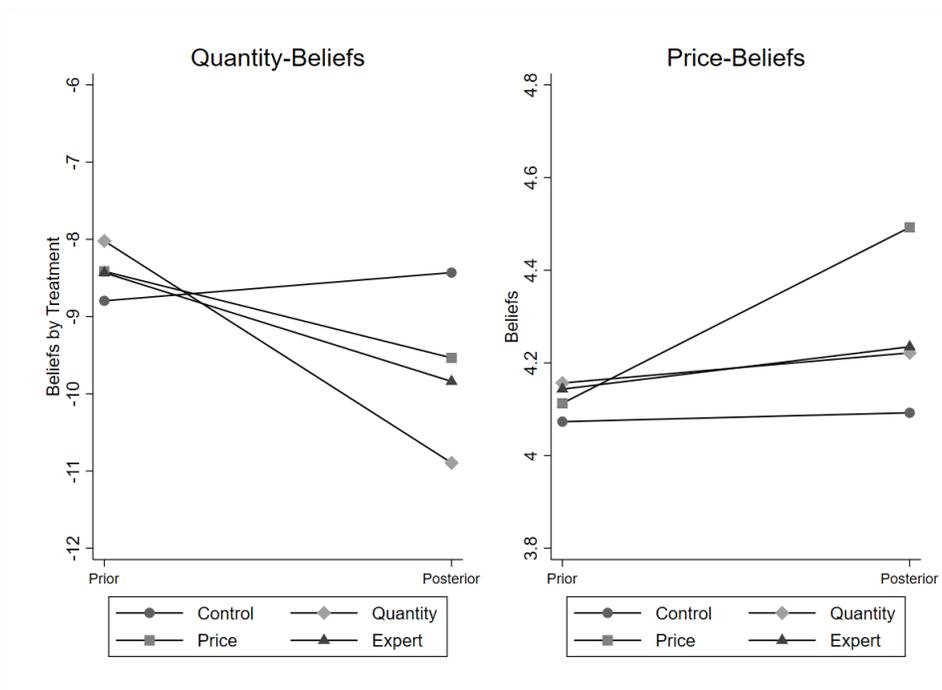


Figure 1: Belief updating about supply- (left panel) and price reactions (right panel) by treatment. Supply change is measured in percent (negative values imply a decrease in supply) and price change on a scale from 1-7 where 7 means “much higher rents”, 1 means “much lower rents” and 4 means “no change”.

Table 1 reports results from linear regressions, in which belief updating – the individual difference between the posterior and the prior – is regressed on the treatment dummies and controls.⁷ Columns (1) to (6) use updating with regard to quantity

⁷The set of controls is the same throughout the paper and consists of a dummy that indicates

effects and columns (7) and (8) use updating with regard to price effects of rent control as dependent variable. The results confirm that the aforementioned findings are statistically significant. That is, participants in the *quantity* treatment downward-adjust their posteriors by about 3.5 percentage points and participants in the *price* treatment upward-adjust their posteriors by about 0.4 points on a 1-7 scale relative to the control group. Next, it is insightful to run the quantity-belief updating regressions separately only for those who hold priors above – columns (3) and (4) – and below – columns (5) and (6) – minus 15%. It turns out that only the former group engages in negative updating (–6.4 and –8.7, respectively), the latter group conducts positive updating (around 2-3 percentage points, but not significantly so in column 5). This finding indicates that the average treatment effects mask important individual-level heterogeneity in the sense that the treatment leads some people to upward-adjust their (previously very negative) priors, partly counteracting the effect of the treatment on the average updating.⁸ Table 1 also shows that updating is stronger for participants who put high trust in the study (compare column (3) with (4) and column (5) with (6)). Again, this finding highlights the importance of trust in the context of expert communication.

It is also noteworthy that there is some updating about quantity reactions in the *price*- and the *expert* treatment, relative to the control treatment. Although this effect is not always statistically significant and is weaker than the effect of the *quantity* treatment, it does suggest a form of cross-learning: participants seem to use the information provided in those two treatments to re-consider their beliefs about supply-side reactions. In particular, it seems reasonable that participants in the *expert* treatment wonder *why* experts are against rent control and then conclude that the effects on housing supply are more negative than they previously thought.

Finally, Table 5 in the Appendix displays results from regressions, in which the German nationality, a gender dummy, a dummy that is equal to 1 if the respondent works in a full time job, the income, the highest educational level attained, and state-fixed effects. “High trust” is defined here as 4 or 5 on a 1-5 scale when asked whether they would trust the study.

⁸Figure 5 in the Appendix highlights this point visually.

confidence in both the quantity and the price posterior are regressed on the treatment dummies (and controls). It turns out that participants in the *quantity* treatment indicate higher confidence in their posterior beliefs than do those in the control group. This finding does not hold for the *price* treatment.

Belief	Quantity						Price	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Updating								
Quantity	-3.478*** (0.93)	-3.047*** (1.12)	-6.362*** (1.05)	-8.699*** (1.43)	2.252 (1.65)	3.399** (1.65)	0.044 (0.05)	0.064 (0.05)
Price	-1.578* (0.84)	-1.091 (1.00)	-2.038** (0.96)	-2.353* (1.28)	-0.333 (1.52)	0.566 (1.64)	0.367*** (0.05)	0.465*** (0.06)
Expert	-2.670*** (0.87)	-2.737*** (1.00)	-2.427** (0.98)	-3.329*** (1.28)	-2.532 (1.63)	-2.449 (1.66)	0.071 (0.04)	0.100** (0.05)
Constant	-0.074 (2.14)	-0.629 (2.89)	-5.263** (2.43)	-6.108* (3.30)	4.444 (3.66)	5.599 (4.62)	0.028 (0.13)	-0.053 (0.15)
High trust only	No	Yes	No	Yes	No	Yes	No	Yes
Restrictions prior	No	No	> -15	> -15	< -15	< -15	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,031	1,768	1,938	1,043	1,033	686	3,031	1,768
R^2	0.008	0.014	0.030	0.052	0.022	0.040	0.025	0.041

Table 1: OLS regressions with posterior minus prior as dependent variable. Heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. *Quantity*, *Price* and *Expert* are treatment indicators. Includes only participants that passed the attention checks. Controls are dummies indicating German nationality, gender, full time workers as well as income, education and state-fixed effects. *High trust* indicates self-reported high trust in the information treatments.

4 Support for Rent Control

4.1 Main Results

We now turn to the main research question of this paper: what determines support for rent control? Our data puts us in a position to study the correlation between beliefs and support (via the priors) as well as the causal effect (via the information treatments). First, Figure 2 delivers three insights: (i) overall support for rent control is large (7 on a 1-10 scale in the control group); (ii) all three treatments significantly reduce support relative to the control group (both stated and revealed support); and (iii) the *expert* treatment displays the largest effect size.

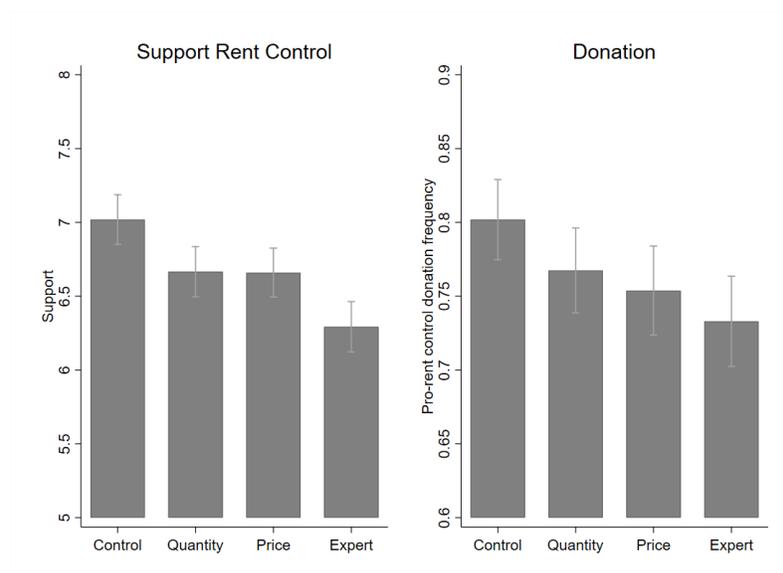


Figure 2: Support rent control (left-hand panel, 1-10 scale) and frequency of donation to pro-rent control initiative (right-hand panel) by treatment. Whiskers indicate 95% confidence intervals.

Table 2 displays results from regressions, in which stated – columns (1) and (2) – and revealed – columns (3) and (4) – support serve as dependent variables. The treatment dummies, fairness concerns, profit expectations, political ideology, a

	Support Rent Control		Donation	
	(1)	(2)	(3)	(4)
Quantity	-0.201** (0.09)	-0.341*** (0.11)	0.004 (0.02)	-0.041* (0.02)
Price	-0.256*** (0.09)	-0.373*** (0.12)	-0.015 (0.02)	-0.045* (0.03)
Expert	-0.556*** (0.09)	-0.804*** (0.11)	-0.035* (0.02)	-0.093*** (0.03)
Fairness	1.278*** (0.05)	1.378*** (0.07)	0.132*** (0.01)	0.140*** (0.01)
Profit	0.231*** (0.02)	0.229*** (0.03)	0.013*** (0.00)	0.014** (0.01)
Landlord	-0.662*** (0.12)	-0.597*** (0.14)	-0.096*** (0.03)	-0.095*** (0.04)
Ideology	-0.132*** (0.02)	-0.117*** (0.03)	-0.033*** (0.00)	-0.035*** (0.01)
Rents are problem	0.252*** (0.02)	0.237*** (0.03)	0.035*** (0.00)	0.041*** (0.01)
Constant	1.146*** (0.37)	0.971* (0.51)	0.382*** (0.07)	0.315*** (0.10)
Trust only	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3,030	1,767	3,030	1,767
R^2	0.536	0.582	0.268	0.307

Table 2: OLS regression with support for rent control as dependent variable. Heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. *Quantity*, *Price* and *Expert* are treatment indicators. Controls include nationality, gender, employment, income, education and a set of state-fixed effects. *High trust* indicates self-reported high trust in the information treatments.

dummy indicating whether a participant rents out, the perceptions of rents as well as our standard set of controls constitute the independent variables. The table again confirms the main conclusions from above: all three treatments decrease support for rent control. Thus, beliefs play a causal role in determining policy preferences. The effect size is thereby largest in the *expert* treatment, and roughly equal in the *price* and the *quantity* treatment. Again, the effects are larger for those individuals who report higher trust in the information provided to them, see columns (2) and (4). This finding highlights the key role that trust in expert knowledge plays in spreading knowledge.⁹

Other lessons from this empirical exercise are that (i) profit motives, (ii) fairness concerns and (iii) political ideology play an important role, independently of beliefs. That is, participants who expect to profit from rent control are more supportive of rent control (being a landlord likely captures a similar motive), people who believe that rent control is “fair” and people who lean more to the left are more supportive of rent control. Finally, participants who think the current level of rents in Germany is a problem are also more supportive of rent control.¹⁰

In order to better compare the relative predictive power of each regressor, we also conduct a (non-preregistered) dominance analysis (Azen and Budescu, 2003).¹¹ From the regression model in column (2) of Table 2, we find that *fairness* contributes around 55% of the total predictive power of the empirical model, *problem perception* around 15% and *profit* around 13%. The relative contribution of each treatment dummy, of political ideology and the landlord dummy lies below 10% in each case.

⁹Table 7 in the Appendix displays the results of a correlational exploration in which the prior beliefs, and not the treatment dummies, are included in the regressions. We find that priors correlate with preferences for rent control in the expected way, except that the price prior is not statistically significant in the “donation regression”.

¹⁰Regarding the control variables, which are included in the regressions but not displayed in the table, we find that being male, holding a higher educational degree and earning a higher income are robustly associated with less support for rent control.

¹¹This analysis uses the average difference in the R^2 between all subsets of models with and all subsets of models without x_i to calculate the relative predictive power of x_i .

4.2 Heterogeneous Treatment Effects: Political Ideology and Education

We put forward two additional (pre-registered) hypotheses. The first hypothesis is that left-wingers hold less well-calibrated (too optimistic) beliefs about the effects of rent control and consequently update their priors more strongly than do right-wingers. Previous literature suggests that ideology can play a role in explaining public policy preferences (Stantcheva, 2021, e.g.). The second hypothesis is that more educated participants hold better calibrated beliefs and update more strongly than do less educated participants.¹² This is in line with other literature which indicates that education has a positive impact on people’s support for efficient economic policies (Baranzini and Carattini, 2017, e.g.). In a nutshell, the first hypothesis is supported by the data, the second is not.

The top panels of Figure 3 show that left-wingers hold higher (less negative) quantity-priors, but also that they update more strongly (downwards). This asymmetric updating significantly closes the ideological gap in beliefs (left-hand panel). There is no such closing of the gap when it comes to price beliefs (right-hand panel). Instead, both right- and left-wingers equally engage in updating, which leads to parallel lines in Figure 3. As expected, both groups conduct virtually no updating in the control group. The main difference is that right-wingers start from a higher level of price beliefs, such that posteriors of right-wingers in the control group are virtually on the same level as posteriors of left-wingers in the treated group.

Interestingly, this asymmetric belief updating leads to a reduction in the left-right gap in support for rent control: while left-wingers reduce their support, the support of right-wingers is almost unaffected by the treatments (see Figure 4). Consequently, the gap is reduced by about one quarter relative to the control.¹³

¹²We define “high education” as having at least a high school diploma (at least a 4 on the 5 point education scale) and “right-wing” as indicating at least a 6 on the 1-11 ideology scale. Both definitions are basically median splits of the answers to the two underlying variables.

¹³The unconditional gap in support between left- and right-wingers is 1.4 in the control group.

Regarding the second hypothesis, we find no evidence for differences in belief updating according to high or low education, see the two panels at the bottom of Figure 3. In each case the lines are parallel for treatment and control, respectively, indicating no differences in belief updating. These conclusions hold for both quantity and price beliefs.

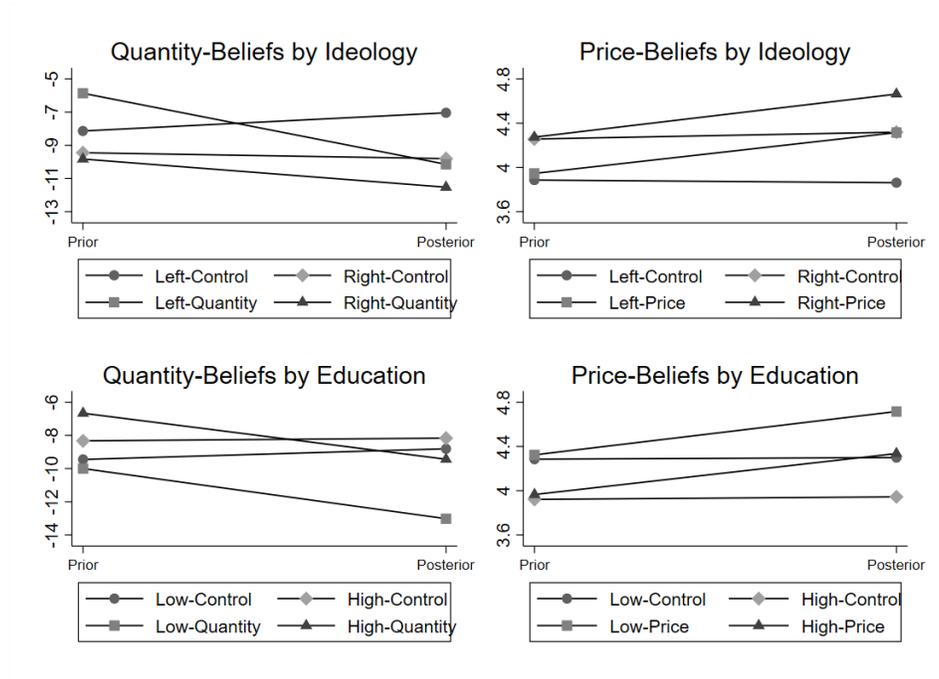


Figure 3: Belief updating by left- and right-wing political ideology (top panels) and by education (bottom panels). In the legend below the panels, *left* means left-winger, *right* means right-winger, *low* means low education and *high* means high education.

We accompany the previous analysis by regressing belief updating on the treatment dummies and their interactions with political ideology – in columns (1)-(4) – and education, respectively – in columns (5)-(8). Table 6 in the Appendix presents

The same gap is 1.22 for *quantity*, 1.06 for *price* and 1.03 for *expert*. Thus, the ideology gap is cut down by about $\frac{1.4-1.03}{1.4} = 26\%$ in the *expert* treatment. The same numbers are 13% (*quantity*) and 24% (*price*), respectively.

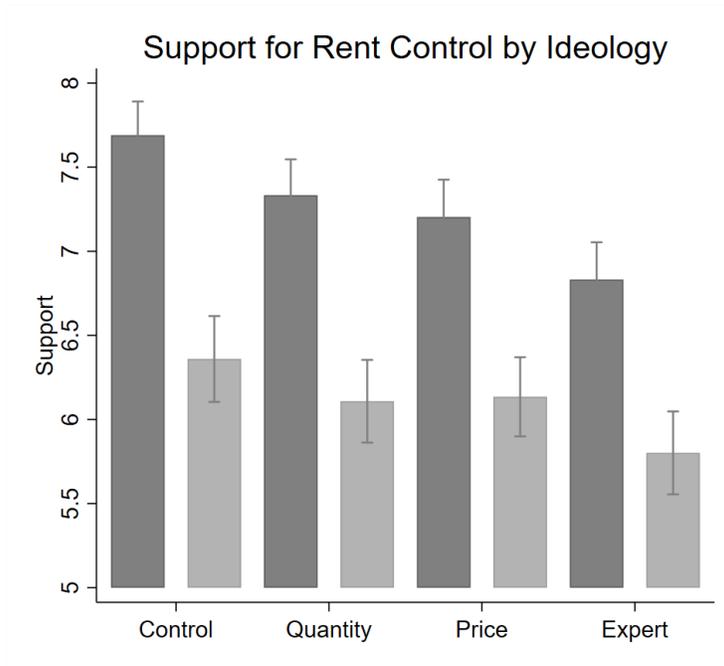


Figure 4: Support for rent control by left- and right-wing political ideology, separately for each treatment. Darker color indicates left-wingers, brighter color right-wingers. Whiskers indicate 95% confidence intervals.

the results (again, odd-numbered columns use all available data whereas even-numbered columns only use data from individuals who find the information presented to them trustworthy).

The coefficient on the interaction of *quantity* and the right-wing dummy ($Quantity \times One$) in columns (1)-(2) is smaller in absolute terms than the interaction of *quantity* and the left-wing dummy ($Quantity \times Zero$), confirming that left-wingers update their beliefs more strongly. A Wald test shows that the coefficients are statistically significantly different at the 10%-level in column (1), but not in column (2). Regarding the interaction of political ideology and price beliefs, a Wald test shows, as expected, no statistically significant differences between belief updating of left- and right wingers in the price treatment. There is, however, evidence for

statistically significant differences in updating in the *expert* treatment: right-wingers react more strongly to expert advice than left-wingers do.

Finally, the results presented in columns (5)-(8) of Table 6 confirm again that quantity-belief updating is significant in the *quantity* treatment, but also taking place to a smaller degree in the *expert* treatment. Only participants in the *price* treatment engage in price-belief updating. Additional Wald tests for differences between the estimated coefficients reveal that in no case, there is evidence that highly-educated participants update their beliefs more strongly or hold more well-calibrated beliefs than lower educated subjects.

5 Mechanisms and Robustness

5.1 Mechanisms

In this section, we shed more light on the potential (not pre-registered) mechanisms that drive support for rent control. First, while the analysis in the previous section has carved out the important role that fairness concerns play, so far, it is unclear what notion of fairness people have in fact in mind when they consider rent control. If these fairness views depend on beliefs about the effects of the policy, we would expect that the treatments also shift the perceived fairness of rent control.

To examine this hypothesis, columns (1) and (2) of Table 3 present results from regressions in which fairness perceptions are regressed on the treatment dummies and the control variables. Again, column (2) uses data only from individuals who find the information trustworthy. All in all, we find little support for the idea that the treatments shift fairness views, except in case of the *expert* treatment, in which case the coefficients are statistically significantly negative, indicating that this treatment renders fairness perceptions of rent control less favorable.¹⁴ Reasons for this result

¹⁴We note that if the treatments shift one of the variables that we use as control in the main section, this would potentially render the coefficient estimates in Table 2 inconsistent. However, first of all, we find little evidence that the treatments shift these variables. Second, when we regress

might be that the *expert* treatment induces people to think about *why* economists reject rent control and conclude that this rejection might partly be due to fairness reasons.

The effect on support for rent control might also operate via their on influence personal profit expectations. For example, pessimistic news about the effect of rent control on the quantity supplied might lead people to adjust their profit expectations. To test this hypothesis, we regress profit expectations regarding rent control policies on the treatment dummies (and, as usual, controls). Columns (3) and (4) of Table 3 show no evidence for such a hypothesis since all coefficients are not statistically significant at conventional levels and are small in magnitude. Thus, our data do not support the idea that the treatments shift perceptions of the pecuniary effects of rent control.

Next, we examine whether the treatments lead people to lower their expectations about the economic efficiency of rent control. We do so in two ways. First, we examine a particular sort of inefficiency: we ask whether survey respondents expect that tenants rent their apartments longer than they would without rent control (see columns (5) and (6) in Table 3). Second, we ask participants whether they consider it a problem that due to rent control it might not always be the person who values the apartment the most, might get it (see columns (7) and (8) in Table 3). Both questions point at concerns that economists typically worry about in their opposition toward rent control, see Diamond, McQuade, and Qian (2019). As the table shows, there is no evidence that the treatment affect the awareness of the problem that rents might stay longer in their apartments than in the case without rent control. In case of the full sample in column (7), there is also no evidence that the treatments change the expectation of inefficient allocations in the rental housing market. However, when we focus the attention on high-trusting individuals, see column (8), we find evidence that the treatments render beliefs about efficiency more pessimistic. To sum up, we support for rent control on these variables using only data from the control group, we find that the conclusions remain virtually identical.

find mixed evidence that efficiency concerns are shifted through the treatments, if anything, this is only holds true for those who trust the information provided.

	Mechanisms							
	Fairness		Profit		Rent longer		Efficiency	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quantity	-0.090*	-0.099	0.021	0.000	-0.041	0.051	-0.099**	-0.172***
	(0.05)	(0.06)	(0.08)	(0.11)	(0.05)	(0.06)	(0.05)	(0.06)
Price	-0.056	-0.114*	0.029	-0.025	-0.074	-0.034	-0.058	-0.211***
	(0.05)	(0.07)	(0.08)	(0.11)	(0.05)	(0.07)	(0.05)	(0.07)
Expert	-0.094**	-0.219***	-0.026	-0.159	-0.073	-0.042	-0.094*	-0.246***
	(0.05)	(0.06)	(0.08)	(0.10)	(0.05)	(0.06)	(0.05)	(0.07)
Constant	3.650***	4.000***	5.715***	5.878***	3.738***	3.862***	2.233***	2.279***
	(0.13)	(0.18)	(0.21)	(0.29)	(0.12)	(0.17)	(0.13)	(0.17)
Trust only	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,030	1,767	3,030	1,767	3,031	1,768	3,031	1,768
R^2	0.040	0.060	0.032	0.031	0.009	0.017	0.022	0.026

Table 3: OLS regression with different outcome variables as dependent variable. Heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. *Quantity*, *Price* and *Expert* are treatment indicators. Includes only participants that passed the attention checks. Controls include nationality, gender, employment, income, education and a set of state-fixed effects. *High trust* indicates self-reported high trust in the information treatments. *Fairness* is coded on a 5-point Likert scale where 1 means “unfair” and 5 means “fair”. *Profit* is coded on a 1-10 scale where 1 means “financially harmed” and 10 means “financially profit”. *Rent-longer* is coded on a 5-point Likert scale where 1 means “no, not longer” and 5 means “yes, longer”. *Efficiency* is coded on a 5-point Likert scale where 1 means “no problem” and 5 means “severe problem”.

Finally, we study whether the information provided to the subjects in the three different treatments leads to spillover effects to related policy areas and to changes in political attitudes. One hypothesis, for example, is that such spillovers are present because people generalize their newly acquired knowledge about rent control to price controls in general and consequently also adjust their political attitudes, since they realize that more right-leaning attitudes are more in line with such a policy preference. Consequently, we examine whether the treatments groups exhibit different attitudes towards (i) support for “price controls in general”; (ii) political ideology; and (iii) support for “free markets” compared to the control group.

Table 4 displays the results. Columns (1)-(2) thereby use “support for price controls” as a dependent variable (coded on a 1-10 scale where higher values mean more support). The statistically significant negative coefficient on the *expert* dummy indicates that people in this treatment (but not in the other treatments) are less in favor of price controls than participants in the control group. Since our data has also provided evidence that participants in the *expert* treatment update their *quantity* beliefs and their fairness perceptions, this finding again supports the hypothesis that this treatment induces broader learning than the other two treatments. Next, columns (3)-(4) provide weak evidence that the *quantity* treatment renders participants more economically right-wing (*ideology* is defined on a 1-11 scale whereas 1 means “left-wing” and 11 means “right-wing”), but only significantly so for high-trusting participants. In general, however, none of the treatments shifts political ideology. Finally, columns (5)-(6) show no evidence of informational spill-overs to support for free markets (“support for free markets” is defined on a 1-10 scale where 10 means “full support”): all three treatments dummies are not statistically significant different from zero.

	Support price controls		Ideology		Support free markets	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantity	-0.033 (0.13)	-0.240 (0.17)	0.148 (0.10)	0.243** (0.12)	-0.010 (0.10)	0.119 (0.13)
Price	0.049 (0.13)	-0.077 (0.18)	0.073 (0.10)	0.134 (0.13)	-0.127 (0.10)	-0.041 (0.13)
Expert	-0.324** (0.13)	-0.529*** (0.17)	0.016 (0.09)	0.150 (0.13)	-0.008 (0.10)	0.183 (0.13)
Constant	7.657*** (0.35)	8.577*** (0.46)	5.573*** (0.24)	5.082*** (0.33)	5.315*** (0.27)	5.282*** (0.34)
Trust only	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,031	1,768	3,031	1,768	3,030	1,767
R^2	0.068	0.096	0.070	0.079	0.054	0.063

Table 4: OLS regression with different outcome variables as dependent variable. Heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. *Quantity*, *Price* and *Expert* are treatment indicators. Includes only participants that passed the attention checks. Controls include nationality, gender, employment, income, education and a set of state-fixed effects. *High trust* indicates self-reported high trust in the information treatments.

5.2 Robustness Check: Experimenter Demand

Here, we present results from a separate online survey experiment among a sample of the German population in which we evaluate the importance of experimenter demand effects following the design by De Quidt, Haushofer, and Roth (2018). The sample was recruited via a commercial survey provider.¹⁵ All participants answer

¹⁵The survey was conducted in December of 2021. The sample size is $N = 100$ in each treatment. The number of observations needed was gauged via a power calculation relying on the point

exactly the same belief questions (both, quantity and price beliefs) as in the main survey. The difference to the main survey is that respondents are randomly assigned to two different treatments before each of the two belief elicitations. Specifically, in the *up* treatment, we explicitly tell respondents that “we expect participants who receive these instructions to indicate that rent control increases supply [*prices*] more than they normally would”. In the *down* treatment, we tell respondents that “we expect participants who receive these instructions to indicate that rent control decreases supply [*prices*] more than they normally would”. Both treatments thus experimentally induce experimenter demand in different directions which allows us to gauge the strength of this effect in our survey.¹⁶ Respondents were first randomized to the *up* or *down* regarding the quantity-belief elicitation. Then they were randomized (independently) to the *up* or *down* price-belief elicitation.¹⁷

All in all, we find very little evidence for the presence of experimenter demand effects. It turns out that the economic differences between the treatment groups are small and not statistically significantly different: a Wilcoxon rank-sum test of the hypothesis of no difference between the *up* and the *down* treatment displays a p-value of 0.45 in the *quantity* treatment and a p-value of 0.15 in the *price* treatment. Figure 6 in the Appendix displays quantity- and price-beliefs for the quantity-demand treatment and price-demand treatment. There, it can also be seen that the average beliefs are in each case more or less in line with the average priors from the main survey.

estimates and standard deviations from the main survey. Roughly, our sample size allows us to identify differences of 5 percentage points in the quantity treatment and 0.4 in price beliefs with a power of 0.8. We consider both differences to be ‘small’.

¹⁶These are the weak demand treatments from De Quidt, Haushofer, and Roth (2018).

¹⁷Similar to the main survey, we also included a short block of socio-demographic questions as well as the support for rent control question in the survey. The exact instructions can be found in the Supplementary File.

5.3 Robustness Check: Anchoring

In this section, we present results that highlight the robustness of our study towards potential numerical anchoring effects. Specifically, one might be worried that the *quantity* treatment, which mentions the number -15% , might lead participants to adjust their posterior beliefs towards this number, without in fact affecting their truly held beliefs. To test for the importance of this concern, we run another online survey using a sample of the German population (again, recruited via a commercial survey provider). We provide a subset of the respondents with an irrelevant anchor of -15% before eliciting their beliefs. Participants in the control condition are not provided with any such numerical information (but they read the same text as the treatment group does).¹⁸

The data provide no evidence for anchoring effects. A Wilcoxon ranksum test is unable to detect differences between the anchoring treatment and the control group (with a p-value of 0.82). In addition, Figure 7 in the Appendix displays quantity beliefs in the anchoring treatment and the control group (as well as the price-beliefs for the sake of completeness).

6 Concluding Remarks

Few endeavors seem more timely than understanding the disconnect between the opinions held by the general public and by experts. We aim to contribute to this question in the realm of economic policy. Indeed, the answer to this question is important to understand why people select policies that are overwhelmingly rejected by experts. The evidence presented in the current paper shows that there is a role for mis-calibrated beliefs. In particular, we find that people update their beliefs about the consequences of rent control in response to our treatments. However, it turns out that fairness considerations and selfish profit motives are more important

¹⁸This survey was conducted in January 2022. Instructions for the anchoring survey can be found in the Supplementary File.

than beliefs about the efficacy of the policy. Policy advice should thus not stop at providing information about scientific evidence and expert advice, but should also aim to understand more clearly people’s notions of fairness and their expectations of profitability in the domain of economic policies. Moreover, our survey highlights the importance of trust in expert opinions since the treatment effects are consistently stronger for those who indicate trust in the information they were given. Our survey experiment thus suggests that it is important to understand what factors determine trust in expert advice and how to increase it.

An important question that the current paper cannot answer is why exactly people believe that rent control is fair (or not) – although we do find some evidence that they adjust their fairness evaluations in response to the treatments, which suggests that beliefs about the workings of the policy also might impact fairness perceptions. Moreover, we cannot really answer the question whether such people hold well-calibrated beliefs with respect to the financial gains and losses they themselves experience from this policy. Again, the fact that participants do not adjust their profit expectations in response to the treatments suggests, on average, that they might misjudge the profitability of rent control and that correcting these beliefs could alter support for rent control.

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7 Appendix

The Appendix proceeds as follows. Section 7.1 displays additional results from the main survey. Section 7.2 shows results from the two additional surveys, that were used as robustness checks. Finally, Section 7.3 provides more information on design and the variables of the main survey.

7.1 Additional Results

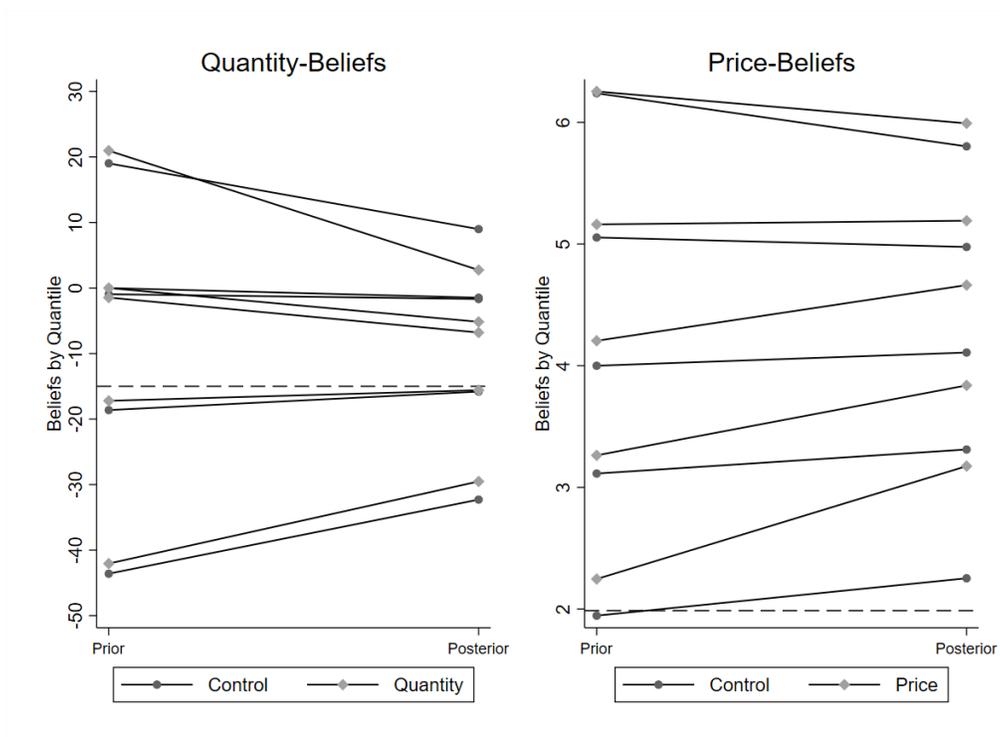


Figure 5: Belief updating: (i) supply reactions by (quantity) prior quantile separately for the quantity treatment and the control group (left-hand panel); (ii) about price reactions by (price) prior quantile separately for the price treatment and control group (right-hand panel).

Confidence	Posterior Quantity		Posterior Price	
	(1)	(2)	(3)	(4)
Quantity	0.244*** (0.05)	0.300*** (0.06)	0.007 (0.05)	0.003 (0.06)
Price	-0.059 (0.05)	0.011 (0.07)	-0.086 (0.05)	-0.019 (0.07)
Expert	-0.001 (0.05)	0.030 (0.07)	-0.056 (0.05)	-0.092 (0.07)
Constant	2.652*** (0.13)	2.676*** (0.18)	2.711*** (0.13)	2.860*** (0.18)
High trust only	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3,031	1,768	3,031	1,768
R^2	0.081	0.091	0.047	0.046

Table 5: OLS regressions with heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. The dependent variable is the response to the question “How sure are you about your answer to the previous question?” following the posterior elicitation. *Quantity*, *Price* and *Expert* are treatment indicators. Columns (1) and (2) are quantity posteriors, columns (3) and (4) posteriors about the price reaction. Controls include nationality, gender, employment, income, education and a set of state-fixed effects. *High trust* indicates self-reported high trust in the information treatments.

Belief	Ideology				Education			
	Quantity		Price		Quantity		Price	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Control × One	0.349 (1.19)	-0.816 (1.28)	0.111* (0.06)	0.135* (0.07)	-1.708 (1.83)	-1.557 (1.98)	-0.100 (0.09)	-0.007 (0.12)
Quantity × Zero	-4.658*** (1.20)	-3.903*** (1.50)	0.088 (0.06)	0.130* (0.08)	-3.892** (1.67)	-3.537* (1.95)	0.084 (0.09)	0.124 (0.11)
Quantity × One	-2.165* (1.30)	-3.057* (1.59)	0.107* (0.06)	0.129* (0.07)	-4.935** (1.96)	-4.380** (2.17)	-0.085 (0.09)	0.022 (0.12)
Price × Zero	-1.369 (1.06)	-0.346 (1.38)	0.410*** (0.07)	0.544*** (0.09)	-2.631* (1.55)	-1.756 (1.72)	0.372*** (0.09)	0.447*** (0.11)
Price × One	-1.424 (1.20)	-2.485* (1.44)	0.434*** (0.07)	0.518*** (0.08)	-2.594 (1.88)	-2.313 (2.08)	0.261** (0.10)	0.470*** (0.14)
Expert × Zero	-3.173*** (1.20)	-3.235** (1.48)	0.032 (0.06)	0.045 (0.07)	-3.757** (1.51)	-3.587** (1.68)	0.096 (0.08)	0.058 (0.10)
Experts × One	-1.838 (1.14)	-3.052** (1.32)	0.215*** (0.05)	0.271*** (0.07)	-3.657* (1.95)	-3.856* (2.16)	-0.050 (0.09)	0.120 (0.12)
Constant	-0.335 (2.18)	-0.058 (2.96)	-0.036 (0.13)	-0.113 (0.15)	-0.089 (2.67)	-0.957 (3.61)	-0.131 (0.16)	-0.065 (0.19)
High trust only	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,031	1,768	3,031	1,768	3,031	1,768	3,031	1,768
R^2	0.010	0.015	0.028	0.046	0.009	0.015	0.026	0.042

Table 6: OLS regressions with posterior minus prior as dependent variable. Heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. *Quantity*, *Price* and *Expert* are treatment indicators. The reference group are left-wingers (low-educated) in the control treatment. *Right-wingers* are defined as 6 or higher on a 1 to 11 ideological scale. *Highly-educated* are defined as holding at least a high school degree (at least 4 out of 5 on the education variable). “One” means that either *right* dummy is equal to 1 (columns (1)-(4)) or that *high education* dummy is equal to 1 (columns (5)-(8)). “Zero” means dummies are zero. Controls include nationality, gender, employment, income, education and a set of state-fixed effects.

	Support Rent Control		Donation	
	(1)	(2)	(3)	(4)
Prior Quantity	0.011*** (0.00)	0.013*** (0.00)	0.001*** (0.00)	0.002*** (0.00)
Prior Price	-0.080*** (0.02)	-0.062** (0.03)	-0.003 (0.00)	0.003 (0.01)
Fairness	1.222*** (0.05)	1.324*** (0.07)	0.126*** (0.01)	0.135*** (0.01)
Profit	0.210*** (0.02)	0.206*** (0.03)	0.011** (0.00)	0.012** (0.01)
Landlord	-0.649*** (0.12)	-0.562*** (0.14)	-0.095*** (0.03)	-0.089** (0.04)
Ideology	-0.118*** (0.02)	-0.115*** (0.03)	-0.032*** (0.00)	-0.035*** (0.01)
Rents are problem	0.266*** (0.02)	0.251*** (0.03)	0.036*** (0.00)	0.042*** (0.01)
Constant	1.481*** (0.39)	1.062** (0.53)	0.407*** (0.08)	0.341*** (0.10)
Trust only	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3,030	1,767	3,030	1,767
R^2	0.541	0.582	0.272	0.309

Table 7: OLS regression with support for rent control on a 1-10 scale, where 10 means “full support” as dependent variable. Heteroscedasticity-robust standard errors in brackets below. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively. *Quantity*, *Price* and *Expert* are treatment indicators₃₂Includes only participants that passed the attention checks. Controls include nationality, gender, employment, income, education and a set of state-fixed effects. *High trust* indicates self-reported high trust in the information treatments.

7.2 Results: Robustness Checks

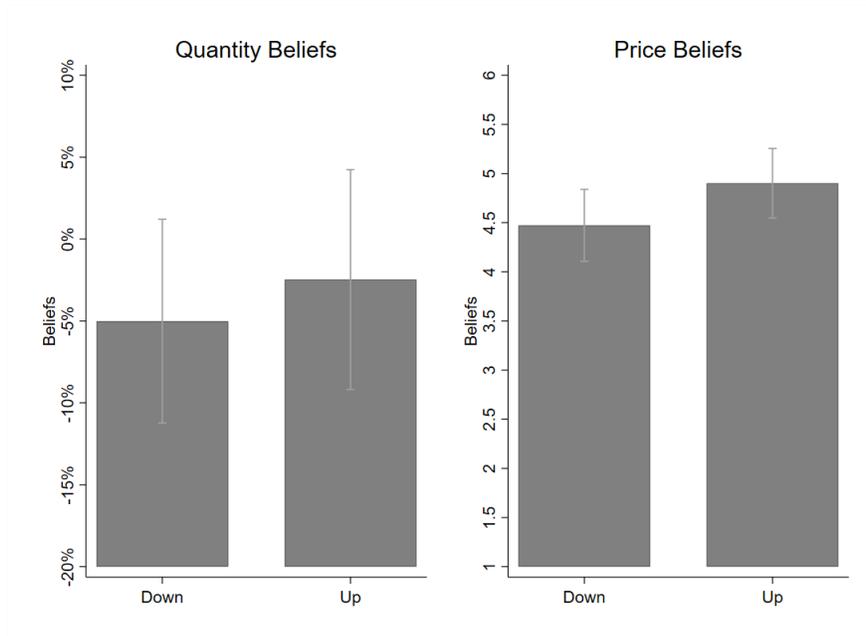


Figure 6: Experimenter Demand Survey. Quantity-beliefs by quantity-demand treatment (left-hand panel) and price beliefs by price-demand treatment (right-hand panel).

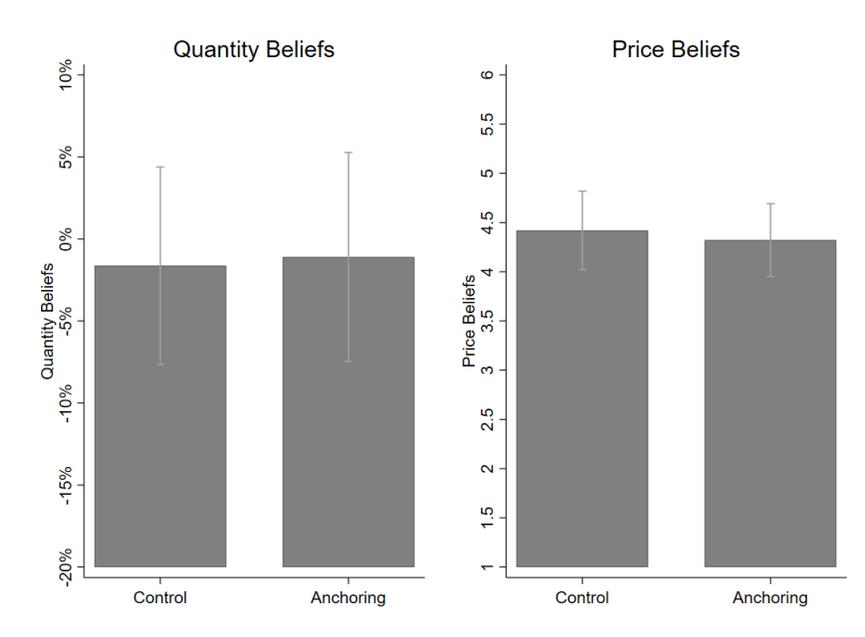


Figure 7: Anchoring treatment. Quantity- (left-hand panel) and price beliefs (right-hand panel). Anchoring treatment versus control.

7.3 Overview and Variables of the Main Survey

Summary Statistics	Control	Quantity	Price	Expert
Age	44.46 (15.71)	44.99 (15.57)	44.93 (15.45)	45.00 (15.49)
Income	1.99 (1.36)	2.03 (1.38)	2.11 (1.47)	2.10 (1.44)
Education	3.78 (0.99)	3.80 (0.99)	3.82 (0.99)	3.80 (0.98)
Male	0.45 (0.49)	0.49 (0.50)	0.48 (0.50)	0.49 (0.50)
Trust	3.27 (0.89)	3.30 (0.89)	3.30 (0.81)	3.29 (0.85)
Prior Quantity	-8.79 (24.72)	-8.02 (24.34)	-8.41 (22.95)	-8.43 (22.41)
Prior Price	4.07 (1.52)	4.15 (1.48)	4.11 (1.43)	4.14 (1.49)

Table 8: Summary statistics by treatment for all participants who passed the attention checks. Mean of each variable (standard deviation in brackets below). Income is in 1000 euros. Ranksum tests between the means of each variable in each treatment and the control never shows statistically significant differences.

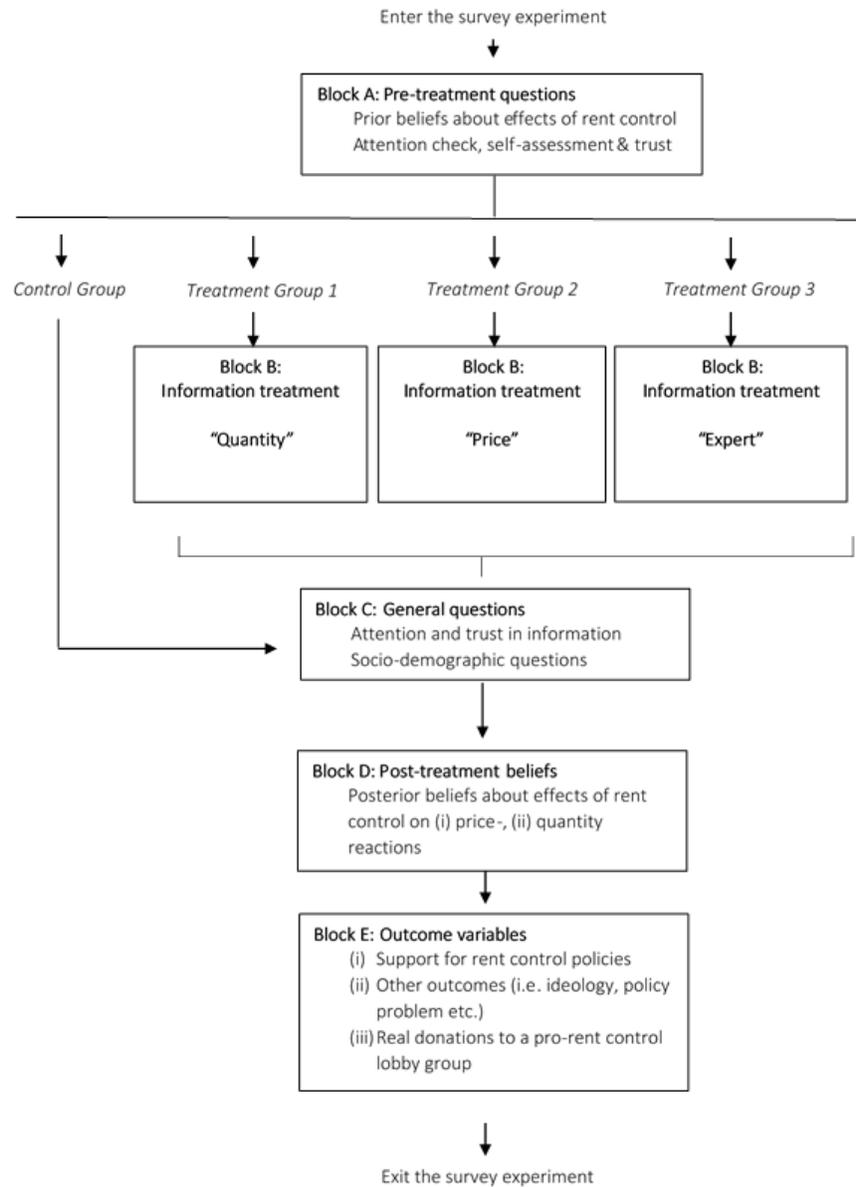


Figure 8: Overview of the main survey.

Main Variables	Question No.	Coding
<i>Beliefs</i>		
Belief supply	1	slider: 0%-100%
Belief price	3	1 (much lower) - 7 (much higher)
<i>Support rent control</i>		
Stated support	29	1 (strongly oppose) - 10 (strongly support)
Donation	41	1=pro-rent control; 0=otherwise
<i>Trust in Information</i>		
Trust Study	10	1 (low trust) - 5 (high trust), high trust=1 if trust >3, 0=otherwise
<i>Additional Variables</i>		
Support price control	30	1 (strongly oppose) - 10 (strongly support)
Ideology	31	1 (far left) - 11 (far right)
Fairness rent control	32	1 (unfair) - 5 (fair)
Support free markets	38	1 (strongly disagree) - 10 (strongly agree)
Profit	34	1 (hurt very much) - 10 (benefit very much)
Rent longer	27	1 (no effect) - 10 (very strong effect)
Problem efficiency	28	1 (no problem) - 5 (very severe)
Rents are problem	32	1 (no problem) - 10 (very severe)
<i>Socio-Demographics</i>		
German nationality	11	1=yes, 0=otherwise
Gender	12	1=male, 0=otherwise
Age	13	in years
Education	15	1 (no formal education) - 5 (university degree)
Income	14	12 income brackets for net income, variable divided by 1000
Employment	18	1=full time, 0=otherwise
Renting status	20	1=renting, 0=otherwise
Landlord	22	1=landlord, 0=otherwise

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Why Do People Demand Rent Control?

Abstract

We conduct a representative survey experiment in Germany to understand why people support inefficient policies. In particular, we measure beliefs about and preferences for rent control – a policy that is widely regarded as harmful by experts. To tease out causal mechanisms, we provide randomly selected sub-sets of participants with empirical estimates about the effects of rent control on rent prices and housing supply and with information about the consensus among economists against rent control. We find that people update their beliefs and that this leads to lower demand for rent control. Left-wingers update their beliefs more strongly, which reduces the ideological gap in support for rent control by about one quarter. Providing information about economists' rejection of this policy leads to the largest reduction in support. However, the main drivers of support for rent control are fairness considerations and profit motives. Our study also highlights the importance of trust since treatment effects are consistently larger among those who indicate trust in the scientific information provided to them.

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