

**Affecting Public Support for
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Evidence from a Survey
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Affecting Public Support for Economic Policies: Evidence from a Survey Experiment about Rent Control in Germany

Abstract

We conduct a survey experiment among 18,000 respondents in Germany to examine the determinants of support for rent control policies. Highlighting undesirable price and supply effects lowers respondents' agreement with rent control, while pointing out that it can prevent displacement of low-income tenants increases agreement. However, while our treatments shift support for the policy into the hypothesized direction, the effect size decreases in misperceptions. Our results suggest that responsiveness to new information depends largely on prior beliefs, which affect perceived credibility and political neutrality of the received information. Mere information provision is therefore not sufficient to effectively alter policy views.

JEL-Codes: D720, D830, D910, R210, R310, R380, P430.

Keywords: rent control, efficiency, redistribution, survey experiment.

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1 INTRODUCTION

Rent control is a widely adopted regulation policy aimed at limiting rent increases. While supporters of rent control argue that it acts as an insurance against unaffordable housing and displacement, economists have long pointed out its adverse effects. Rent control tends to reduce the quantity and quality of rental housing, as landlords respond by selling apartments to owner-occupiers or refrain from required maintenance, and may lead to the misallocation and overconsumption of housing (Autor et al., 2014; Diamond et al., 2019; Glaeser and Luttmer, 2003). Among economists, there exists an unusually large consensus that rent control policies are inefficient and harmful.¹ Despite the scientific evidence, the popularity of various forms of rent control around the world is still unbroken.² As such, rent control is a prime example of a policy opposed by experts but joyfully embraced by the public.³

In this paper, we describe a randomized survey experiment among a representative sample of 18,000 German respondents that we designed to answer two important questions frequently asked in the context of such policies: “Which aspects of rent control motivate its high support in the public?” and “Can information provision alter the support for the policy?” In the survey experiment conducted in May 2021, we confront groups of participants with consequences of rent control policies as well as different aspects of the housing market, and subsequently elicit their attitudes towards rent control. By comparing the effect of five information treatments, we can infer which aspects of rent control drive respondents’ support for the policy. In addition to misperceptions about the efficiency costs of rent control, which has been the predominant explanation for disagreement about economic policies between experts and the general public (Caplan, 2002), we test if (i) demand for redistribution, (ii) negative sentiments against commercial investors, (iii) concerns about the affordability of housing, or (iv) the desire to prevent gentrification and the displacement of low-income tenants play a role for the assessment of rent control policies.

In addition to exploring which aspects matter for people’s support for rent regulation, our paper also tackles a broader issue that goes beyond the specific realm of rent control policies. We use our survey experiment to examine which respondents react to our infor-

¹To examine the extent of consensus on key economic questions, Alston et al. (1992) surveyed 1,350 US economists about agreement to a list of 40 propositions. The proposition most economists could agree on was “A ceiling on rents reduces the quantity and quality of housing available”, beating for example the rejection of wage-price controls as a useful tool for controlling inflation. This consensus has persisted until today (see www.igmchicago.org/surveys/rent-control/).

²See for instance [here](#) for evidence from the UK and [here](#) for evidence from the US.

³Nordhaus and Rivers (2023) show that there is a strong disagreement between experts and the public also in other fields of economic policy, for example climate policy or trade policy.

mation treatments and why they do or do not react. Specifically, we analyze how their reaction is connected to certain characteristics such as economic interests, knowledge, political orientation, and in particular people’s prior beliefs (Blinder and Krueger, 2004). Deepening our understanding about the effectiveness of information treatments about rent regulation across different strata of the population can help to improve the design of information treatments—and, more generally, of political communication—on other economic policies as well. We believe that this is an important question, in particular in times of increasing political polarization (Boxell et al., 2022; Gidron et al., 2020).

To ensure that the survey experiment concerned views about real world policies, we conducted it one year after the introduction of the Berlin rent cap. The Berlin rent cap (*Berliner Mietendeckel*) was introduced by the state of Berlin in February 2020 and mandated a new and drastic form of rent control. For existing rental contracts, the policy froze rents at their June 2019 level. For newly signed rental contracts, the policy stipulated upper limits for rents. The policy pertained to all apartments in Berlin, except for newly built units, and was controversially discussed in the media for weeks.⁴ In April 2021, one month before we launched our survey, the Berlin rent cap was declared unconstitutional for technical reasons by the Federal Constitutional Court. Notwithstanding its brief existence, Arlia et al. (2022) and Hahn et al. (2022) show that the rent cap has led to a reduction in the number of rental offers of regulated apartments and a disproportional rise of rents of unregulated apartments, as predicted by economic theory. Despite these adverse effects, the rent cap has been hugely popular in the German population.⁵ Overall, the Berlin rent cap was a particularly severe and salient form of rent control. As such, it constitutes an ideal policy against which we can evaluate our research questions.

We present two main findings. First, we show that compared to the control group, respondents are most responsive to information about negative effects of the rent cap on the quantity and quality of housing (henceforth referred to as “*Efficiency Treatment*”), and to information that a rent cap can help prevent gentrification and the displacement of existing tenants (henceforth referred to as “*Displacement Treatment*”). The average treatment effects in these treatments are large. Informing respondents about the efficiency costs of the rent cap decreases support by a full point on an 11-point Likert scale. Conversely, informing respondents that rent control measures can help to avoid displacement of low-income tenants increases support by 0.5 points. These findings suggest that individuals support “inefficient” policies like rent control not only due to a lack of understanding

⁴More details on the rent cap are provided in Appendix A.

⁵According to a representative opinion poll from Infratest dimap conducted in the month the law became effective, there was large majority support for the Berlin rent cap among voters in Germany. 71% of respondents were in favor of the rent cap. Support among tenants was even higher and amounted to 81% (See <https://www.tagesschau.de/inland/deutschlandtrend-2085.html>). As we show in Section 3, we find majority support for the Berlin rent cap also in our sample.

about the associated efficiency costs, but also because they care about other aspects of the policy not typically emphasized by economists. In contrast, providing information about differences in disposable household income between tenants and landlords, the share of private investors in the Berlin rental market, and the evolution of the income share spent for housing does on average not induce large shifts in support for the policy. We therefore do not find support for our hypotheses that some people might favor rent control out of demand for redistribution, negative sentiments against institutional investors, or concerns about the affordability of housing.

In a second, complementary survey conducted in October 2022 on a new sample of 12,000 respondents, we replicate our findings for the Efficiency Treatment and the Displacement Treatment. Furthermore, we show that the effect of a composite treatment combining the two is almost exactly equal to the sum of the two separate treatments. The Displacement Treatment also has a significant positive effect on donations to an organization lobbying for extensive rent regulation in Germany.

Second, turning back to our main survey from May 2021, we find that our information treatments affect predominantly those respondents whose prior beliefs do not deviate too strongly from the provided information. Individuals with large misperceptions barely change their support for the policy. Contrary to both intuition and theoretical predictions, we therefore estimate a diminishing treatment effect as misperceptions increase. While our paper thus demonstrates that, on average, it is possible to both increase and decrease support for rent control by pointing out positive and negative aspects of such policies, our treatments are not successful in convincing those respondents who hold strongly biased beliefs about these aspects ex-ante. Moreover, since prior beliefs are correlated with ex-ante support for the rent cap, our treatments mainly increase support for the rent cap among those respondents already favoring it, and decrease support among respondents not supporting the policy.

In the final part of the paper, we investigate the mechanisms behind this pattern. Our findings suggest that it might be partly attributed to a mere psychological effect, i.e. a type of *confirmation bias*: If people receive information that goes against their prior beliefs, they are skeptical and hesitate to accept it (Nickerson, 1998). We show that respondents with very biased beliefs tend to rate the information provided in the respective treatment as less credible than those whose prior beliefs were mostly correct.

However, we also find that prior beliefs are strongly correlated not only with support for the rent cap itself but also with political orientation in general. In particular, respondents who are not aware of the efficiency costs of the rent cap ex-ante tend to be more in favor of the policy and more left-wing than those who are aware of these costs. Analogously, respondents who do not believe that the rent cap can help prevent displacement tend to be less in favor of the policy and more right-wing than those who are aware of this

aspect. These findings suggest that prior beliefs are—at least to some extent—politically motivated, although the direction of causation between prior beliefs and political orientation probably goes both ways. Presenting respondents with information that goes against their politically motivated beliefs makes them not only view this information as less credible due to the above-mentioned psychological reasons, but also because it is discounted as politically biased. As a result, they are less likely to change their assessment of rent control policies in response to our treatments. These mechanisms can help explain why we affect mainly respondents whose prior beliefs do not differ too much from the information we provide in our treatments. Overall, our results are consistent with *prior-biased* updating (Benjamin, 2019; Charness and Dave, 2017) and show that apparently powerful behavioral patterns like the desire for cognitive consistency and motivated reasoning are not easily overcome by the mere provision of information.

Our paper relates to a growing strand of literature that uses survey experiments to study how people reason about economic policies, including taxes (Stantcheva, 2021), trade (Alesina et al., 2021; Alfaro et al., 2023), climate policies (Dechezleprêtre et al., 2022), and anti-discrimination policies (Alesina et al., 2021; Haaland and Roth, 2021). In line with our results, Stantcheva (2021), Alesina et al. (2021), and Douenne and Fabre (2022) reject the hypothesis of rational agents who update their beliefs accordingly in response to new information, as respondents with large misperceptions are barely affected by information provision or even update in the opposite direction. In contrast to many of these studies, however, we do not find that this pattern is mainly driven by political orientation. While misperceptions tend to be correlated with political orientation, the negative interaction between misperceptions and the treatment is very robust to flexibly controlling for political views.⁶ Instead, our findings are better explained by general psychological motives like a desire for cognitive consistency, confirmation bias and motivated reasoning.

Concerning attitudes towards rent control policies, Brandts et al. (2022) use refutational and non-refutational messages to affect support for rent control. They find that both types of messages moderately reduce misperceptions, but do not eliminate them. In a recent survey experiment, Müller and Gsottbauer (2021) show that information about the price and supply effects of rent caps lowers agreement with such policies. The main difference compared to our paper is that they do not examine the effects of information about other aspects than efficiency, such as displacement prevention or distributional concerns. Moreover, our paper explores how information about these positive and negative

⁶Specifically, the negative interaction between misperceptions and treatment is very robust to the inclusion of additional interaction effects between the treatment dummy and controls like political orientation or to what extent participants perceive the survey as ideologically biased, as well as a large set of further control variables.

aspects affects respondents of different political orientation and prior convictions in different ways, which allows us to draw conclusions about the effectiveness of information provision and policy communication more generally. Finally, our paper also relates to the literature examining differences in policy views between experts and the general public (Caplan, 2002; Haferkamp et al., 2009; Nordhaus and Rivers, 2023).

The paper proceeds as follows. Section 2 describes our surveys, the experimental design, and our hypotheses. Results are presented in Section 3. Section 4 concludes.

2 EXPERIMENTAL DESIGN AND DATA

2.1 *Setting and Sample*

Our survey experiment was part of a large-scale online survey in cooperation with immowelt, a German online housing portal. The main survey was conducted in Germany in May 2021 and our final sample includes 18,000 respondents between 18 and 70 years of age, representative with respect to gender, age and occupational status, and stratified by the place of residence being in an urban, suburban or rural environment. Urban areas were deliberately oversampled for this study.⁷ Respondents were randomly allocated into one control and five treatment groups. This implies that each treatment group and the control group consists of around 3,000 individuals.

The panel, the programming and distribution of the survey, and the payments were administered by the professional survey company INNOFACT AG. Participation was voluntary, and the median completion time was around 20 minutes. All respondents in our sample fully completed the survey and received a remuneration of about 3 EUR.

2.2 *Experimental Design*

The questionnaire was structured as follows. At the beginning of the survey, respondents were asked to provide socio-demographic characteristics such as age, gender, and occupation, followed by a block of questions designed by immowelt concerning the housing situation of respondents and the Covid-19 pandemic’s effect on their future housing preferences. We then elicited attitudinal variables concerning for example political preferences or the frequency of economic news consumption, and implemented an attention check.

For the survey experiment, respondents were randomly allocated into one control and five treatment groups. Respondents in both the control and the treatment groups were provided with basic information about the Berlin rent cap. Respondents in the

⁷This was required by immowelt, but also useful for the purpose of this study, since rent control policies matter mostly for urban areas, and thus getting a clear picture of the opinion towards such policies in cities is important.

treatment groups then received an information treatment in addition, relating to different aspects of the Berlin rent cap and the Berlin housing market (described in Section 2.3 below). Before each information treatment, we elicited respondents’ beliefs regarding the provided information and asked them how certain they are about their beliefs. We then asked respondents to report their general assessment of the Berlin rent cap on an 11 point Likert scale. This constitutes our main outcome question. As pre-registered, we also asked additional questions eliciting views on various aspects of rent control measures like the Berlin rent cap, for example to what extent they are perceived as fair, how respondents believe different groups in the housing market are affected, and how they would be *personally* affected if a measure like the Berlin rent cap were introduced at their own place of residence or in the nearest major city.

Finally, we elicited beliefs about all statements presented in the other information treatments, except the belief already elicited prior to the treatment. Since respondents who received these questions concerning the statements shown in the other information treatments did not receive any information on the respective issues, the answers can be regarded as “prior beliefs” for these respondents.⁸ For the respective information treatment that a given treatment group had already received in the beginning, we elicited posterior beliefs. The survey concluded by asking how credible respondents found the provided information and whether they regarded the survey as politically biased.

The following list provides an overview of the questionnaire structure:

1. Socio-demographic characteristics
2. Questions on housing situation and future housing preferences (immowelt)
3. Attention check
4. Pre-Treatment questions: attitudinal questions
5. Basic information about the Berlin rent cap
6. Random allocation of respondents into control and treatment groups
7. Elicitation of prior beliefs (only respondents who are in one of the treatment groups, only on the aspect that we inform about in the respective treatment)

⁸In contrast, as described above we elicited prior beliefs for the respective treatment group *before* the information treatment. The reason why we did not elicit prior beliefs at the beginning for everybody is that we expect our treatments to impact views about rent control not only via the *information provision* per se, but also by simply making respondents *aware* of a certain aspect. For instance, when we ask people about the effects of rent controls on displacement, the question alone could be enough to make respondents *think* about this aspect. If they did not have this aspect in mind before, the question eliciting prior beliefs about rent control’s impact on displacement could already change their views on rent controls, even without the information treatment that “rent control policies can help to avoid displacement”. By eliciting prior beliefs in advance for everybody, we would have missed that part of the impact of our treatments.

8. Information Treatment (only respondents who are in one of the treatment groups)
9. Post-Treatment questions: main outcome question and additional questions eliciting views on various aspects of the Berlin rent cap
10. Elicitation of remaining beliefs (on those issues for which beliefs were not elicited in 7., and in particular on all issues in the control group)
11. Elicitation of posterior beliefs (only respondents who are in one of the treatment groups, only on the aspect that we inform about in the respective treatment)
12. Credibility and political bias questions

2.3 Treatment Groups and Hypotheses

All respondents were first informed about the Berlin rent cap in a neutral way.⁹

Control Group. No prior beliefs were elicited and no additional information about Berlin’s housing market, the rent cap and its effects was provided.

Efficiency Treatment (T1). We first elicited respondents’ beliefs about the effect of the Berlin rent cap on the supply of rental apartments and on rents not subject to the Berlin rent cap. Then we informed them that studies show that rent controls tend to reduce the supply of rental apartments, while non-regulated rents increase due to the rent controls.

Distribution Treatment (T2). We first asked respondents which group, landlords or tenants, they believe has a higher income on average. Next, we elicited respondents’ beliefs on how much the average income of tenants (landlords) is higher (in percent) compared to landlords (tenants). Then we informed them that landlords on average have a 54% higher income than tenants.

Landlords Treatment (T3). We first asked respondents which type of landlord (private-sector companies, private individuals, public authorities, or housing cooperatives) constitutes the largest group in Berlin, and what percentage of rental flats they own. Then we informed them that private-sector companies are the largest group and that they own 29% of all rental flats in Berlin.

⁹The exact wording of this statement and our treatments (translated into English) can be found in Appendix C. The English translation of the whole survey can be found [here](#).

Affordable Housing Treatment (T4). We first elicited respondents’ beliefs about how the income share spent on housing developed for tenants in Berlin from 2006-2018. We then informed respondents that the income share spent on housing had been largely unchanged during this period.

Displacement Treatment (T5). We first elicited respondents’ beliefs about whether a rent control measure like the Berlin rent cap can help to avoid displacement of low-income tenants from the city. We then informed respondents that studies show that rent control measures like the rent cap can help to prevent displacement of low-income tenants, similar to an insurance against unaffordable rents.

2.4 Hypotheses

If the respective aspects mentioned in our treatments matter for respondents’ support for the rent cap, we expect our treatments to work via two channels: a *priming channel*, making people think about a certain aspect that they might already be aware of, and an *information provision channel* that works by correcting people’s prior beliefs.¹⁰ While the priming effect should apply to all respondents, we expect the information provision effect to be stronger the more a respondent’s prior beliefs differ from the information we provide.¹¹ The treatment effects will thus depend on (i) whether the presented aspect matters for respondents’ views on the rent cap, and (ii) in what direction respondents’ prior beliefs are corrected. In the following, we describe the expected direction of our treatment effects given that (i) holds, i.e. that the respective aspect matters for support.

Concerning the direction of our treatment effects, we have unambiguous predictions for T1 and T5, whereas the sign of the effect depends on prior beliefs for the other three treatments. For the Efficiency Treatment (T1), we expect a negative treatment effect on average, since it informs about undesirable effects of rent control policies. We expect this effect to be stronger for those respondents with larger misperceptions ex-ante. Analogously, we expect a positive treatment effect for the Displacement Treatment (T5), because it informs about an effect of rent control policies often viewed as positive. Again,

¹⁰Since we deliberately did not elicit prior beliefs for respondents in the control group before our outcome questions and thus did not “prime” these respondents with any of the aspects we mention in our treatments, both the priming and the information provision effect will be part of the estimated treatment effect.

¹¹If the respective aspect matters for respondents’ support for the rent cap and if they believe in the information we provide, those respondents whose prior beliefs differ more from the provided information should (under certain regularity assumptions concerning their priors) update their beliefs more and thus revise their assessment of the rent cap more than the rest.

we expect the effect to be stronger for respondents with misperceptions, i.e. those who were not aware of this aspect ex-ante.

The Distribution Treatment (T2) informs respondents that landlords earn higher incomes on average than tenants. Since the rent cap is often viewed as a policy that “re-distributes” from landlords to tenants, informing about the unequal income distribution between those groups should increase agreement with the rent cap for those respondents who were not aware of this inequality (or how high it is) ex-ante. The effect on respondents who overestimate the extent of this inequality is ambiguous. On the one hand, they are ‘primed’ about the existing inequality, which should positively affect their support for the rent cap. On the other hand, they are also informed that the inequality is not as high as they thought, which might lower their support for the policy.

That private-sector companies such as *Deutsche Wohnen* and *Vonovia* are big players in the Berlin rental housing market and might benefit “unfairly” from rapidly increasing rents at the expense of tenants was often mentioned as one of the reasons for introducing the Berlin rent cap. Our Landlords Treatment (T3), which informs respondents that private-sector companies are the largest group of landlords in the Berlin rental housing market, should thus increase agreement with the Berlin rent cap for respondents who were not aware of this fact ex-ante or who underestimated the share of rental housing these companies own. Analogous to T2, the expected sign of the treatment effect is unclear for respondents who were aware that private-sector companies are the largest group of landlords and who overestimated their market share. The priming effect should lead to higher support for the rent cap, while the downward correction of the owned share might lower support.

The effect of the Affordable Housing Treatment (T4) also depends on respondents’ prior beliefs. Since the rent cap is often viewed as an instrument to bring down (or slow down the increase of) housing costs for tenants, informing respondents that the share of income spent on housing has in fact remained constant over the period 2006–2018 should decrease agreement with the rent cap for those respondents who thought this share has increased. On the other hand, agreement with the rent cap should go up for those respondents who thought that this share has decreased in the past.

2.5 *Complementary Second Survey*

We conducted a short second survey on a sample of 12,000 new respondents in October 2022, where we tested the effect of a composite treatment combining the Efficiency Treatment (T1) and the Displacement Treatment (T5). Furthermore, we added an incentivized outcome question where respondents could donate their potential gains from a lottery to an organization lobbying for extensive rent regulation in Germany, as well as an open-

ended question eliciting respondents’ thoughts about the Berlin rent cap. The survey was again representative of the German population with respect to gender, age, and occupational status, and stratified by the place of residence being in an urban, suburban, or rural environment. It was implemented by the survey company Bilendi.¹²

3 UNDERSTANDING SUPPORT FOR THE RENT CAP

3.1 Descriptive Evidence

We present key descriptive statistics for our sample as well as for the general German population in Appendix Table B.1. Individuals in our sample are more educated than the German population, and less likely to own their homes. These differences mainly result from the oversampling of urban areas. In all other aspects, differences compared to the general German population are minor. Appendix Table B.2 shows that treatment and control groups are well balanced across all relevant characteristics.

Respondents in the control group state high support for the Berlin rent cap. On a 0-10 Likert scale, where higher values signify higher approval, the sample mean (median) is 6.23 (7). 22% of respondents in the control group rate the rent cap as negative, 62% as positive and 16% as neutral.¹³ 72% favor introducing similar legislation also in other cities with tight housing markets.

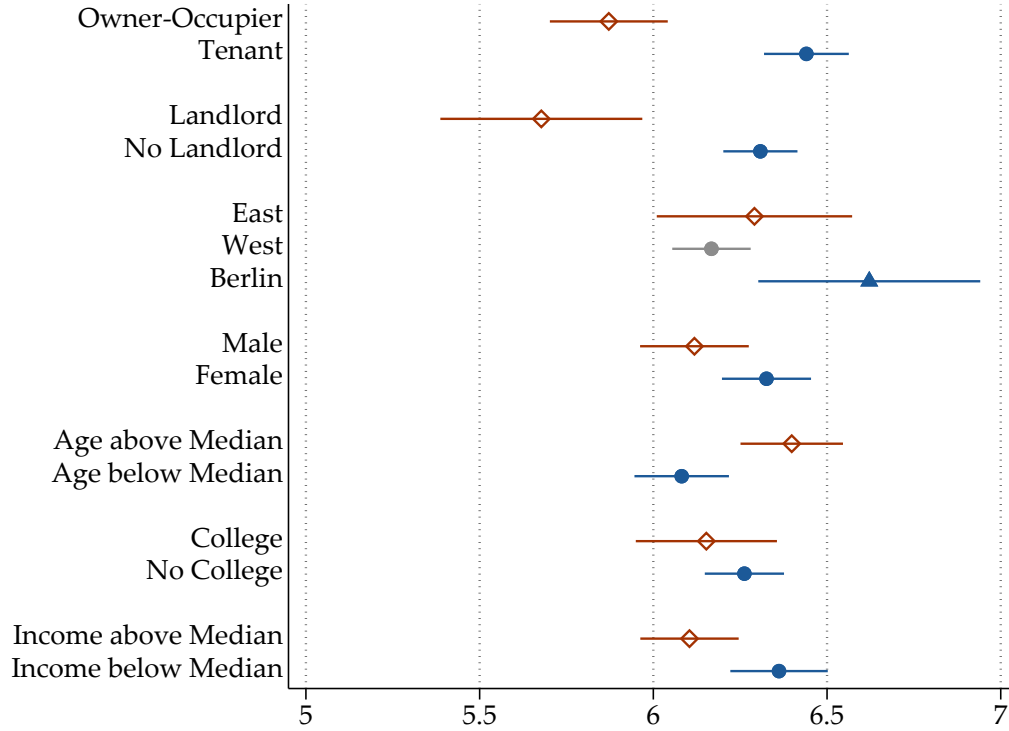
Figure 1 presents descriptive evidence on agreement with the Berlin rent cap by demographic characteristics in the control group. As expected, landlords and owner-occupiers, two groups for whom rent control runs counter to economic interests, are less in favor of the rent cap than tenants and people who do not own property. Respondents who live in Berlin are more in favor of the rent cap than those who live in other parts of Germany. We also find that agreement is increasing with age and decreasing with income. In addition, as shown in Appendix Figures B.1–B.2, agreement is higher among respondents who voted for left-leaning parties in the last federal election of 2017 (*Die Linke*, *Bündnis 90/Grüne*, *SPD*), and slightly higher for respondents living in the 14 largest German cities.

Notwithstanding these differences by subgroups, the rent cap finds majority support in all considered partitions of our data. Even landlords or voters of political parties typically opposing government interventions in markets are on average weakly in favor of the policy. This is despite the fact that most landlords correctly anticipate that they will be negatively affected by the rent cap. These patterns give a first indication that economic self-interest is not the only relevant factor when assessing the policy.

¹²The translated English questionnaire of the second survey can be found [here](#).

¹³“Negative” here means a rating of 0-4 on the 11 point Likert scale, “positive” implies an assessment of 6-10, and “neutral” is 5.

Figure 1 – Support for the Rent Cap by Subgroups



Notes: This figure shows how support for the Berlin rent cap varies by subgroup. Support is measured on a 0-10 Likert scale (0 very negative assessment, 10 very positive assessment). “East” indicates whether respondents live in former GDR states, “West” comprises respondents who live in (former) West German states. Both categories exclude respondents who reside in Berlin, which is the third (separate) category in this graph.

3.2 Average Treatment Effects

To examine average treatment effects, we estimate the following OLS regression

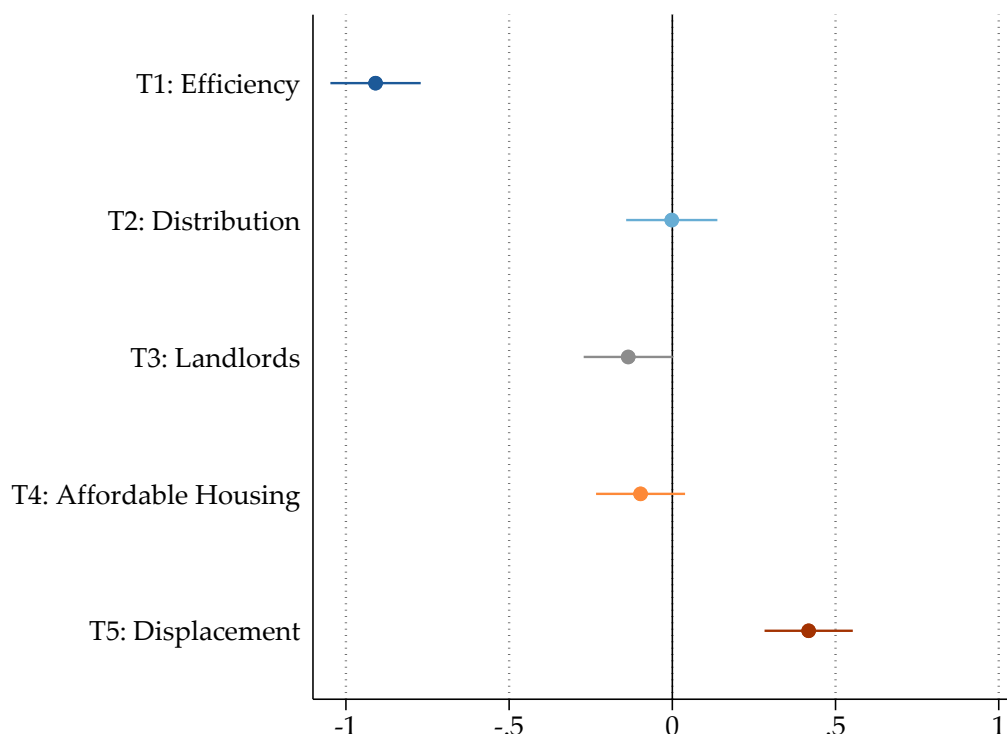
$$Y_i = \alpha + \beta T_i + \gamma X_i + \varepsilon_i, \quad (1)$$

where Y_i measures the respondents’ views about the Berlin rent cap, T_i is a treatment dummy, and X_i denotes an optional control vector. In this regression, β provides an estimate of the average treatment effect (ATE).

Figure 2 displays the results for our main outcome question, where we elicit general agreement with rent control policies like the Berlin rent cap. We find a significant negative ATE for the Efficiency Treatment (T1), and a significant positive ATE for the Displacement Treatment (T5). For respondents who receive the Efficiency Treatment, support for rent control policies like the Berlin rent cap is on average 1 point lower (on a 0-10 Likert scale) compared to the control group, corresponding to 34% of a standard deviation. In

the Displacement Treatment, support increases by 0.5 points compared to the control group (15% of a standard deviation). We do not find significant ATEs for the three other treatments.¹⁴

Figure 2 – Average Treatment Effects: General Agreement with the Rent Cap



Notes: This figure plots the estimated coefficients from OLS regression (1) (as well as 95% confidence intervals) per treatment for our main outcome variable: *In your opinion, how would you rate a rent control policy such as the Berlin rent cap?* (elicited on a 0-10 Likert scale).

We find somewhat smaller ATEs for our additional outcome questions, where we ask respondents whether they are in favor of introducing the rent cap also in other German cities with tight housing markets (Appendix Figure B.4), whether they think rent regulations like the Berlin rent cap are fair, and how they would assess the impact on them personally if a measure like the Berlin rent cap were introduced in their hometown or in the nearest major city (Appendix Figure B.5).¹⁵

¹⁴Figure B.3 in the Appendix shows that average treatment effects are very similar when we drop respondents who did not pass the attention check prior to the survey experiment. The only exception is the average treatment effect for the Landlords Treatment (T3), which becomes marginally significant.

¹⁵We also elicit how respondents assess the impact of a measure like the Berlin rent cap on current tenants, tenants looking for an apartment in the future, landlords and property owners if it were introduced

As outlined in Section 2.3, the insignificant ATEs for the Distribution (T2), Landlords (T3) and Affordable Housing (T4) treatments might be due to the fact that these aspects do not matter in people’s assessment of the rent cap, which is thus not affected by new information on these aspects. However, the direction of the treatment effect in these three treatments depends on respondents’ prior beliefs, and significant treatment effects of opposite sign might therefore cancel out on average. For instance, if half of respondents in the Affordable Housing Treatment think that the income share spent on housing has increased, and half think that this share has decreased, the former group’s agreement with the rent cap might decline and the latter group’s agreement might go up after informing them that the share has in fact remained constant. As a result, we might observe no average treatment effect.

For the Efficiency (T1) and the Displacement Treatment (T5), we might observe significant ATEs because the respective aspects matter for respondents’ opinion about the rent cap irrespective of prior beliefs, suggesting that our treatments work mainly via the priming channel. Alternatively, if the information provision channel also matters, we might find stronger effects for respondents whose prior beliefs differ more from the provided information. To find out which mechanisms are at play in all five treatments, we next investigate treatment effect heterogeneity with respect to misperceptions in prior beliefs.

3.3 *Treatment Effect Heterogeneity with Respect to Respondents’ Misperceptions*

To test for treatment effect heterogeneity with respect to respondents’ misperceptions, we estimate an extended model

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 M_i + \beta_3 T_i \times M_i + \beta_4 X_i + \varepsilon_i, \quad (2)$$

where Y_i measures the respondents’ views about the Berlin rent cap, T_i is a treatment dummy, M_i measures respondents’ misperceptions, and X_i is an optional control vector. In this regression, β_3 estimates how treatment effects change with the degree of misperceptions.

Definition of Misperceptions. For our five treatments, our main variables measuring misperceptions are defined as follows: In the Efficiency Treatment (T1), we asked two prior belief questions: one question about the effect of the Berlin rent cap on the supply of rented

in a city with a tight housing market. Again, treatment effects are similar compared to the estimates for our main outcome question (see Figure B.6).

flats, and one question about the effect of the rent cap on rent prices of flats in the unregulated market segment not subject to the rent cap (cf. Appendix A). Respondents could choose between three options for each of the questions (increased/decreased/remained constant). Each of these questions could thus be answered correctly (if respondents chose the options “supply has decreased” or “unregulated rents have increased”, respectively), slightly wrong (if respondents said that supply or unregulated rents “have remained constant”) or completely wrong (if respondents said that “supply has increased” or “unregulated rents have decreased”). Based on these answers, we coded a misperception variable ranging from ‘0’ for individuals with no misperceptions to ‘5’ for individuals who believe that the supply of rental housing would increase and unregulated rents would fall.¹⁶

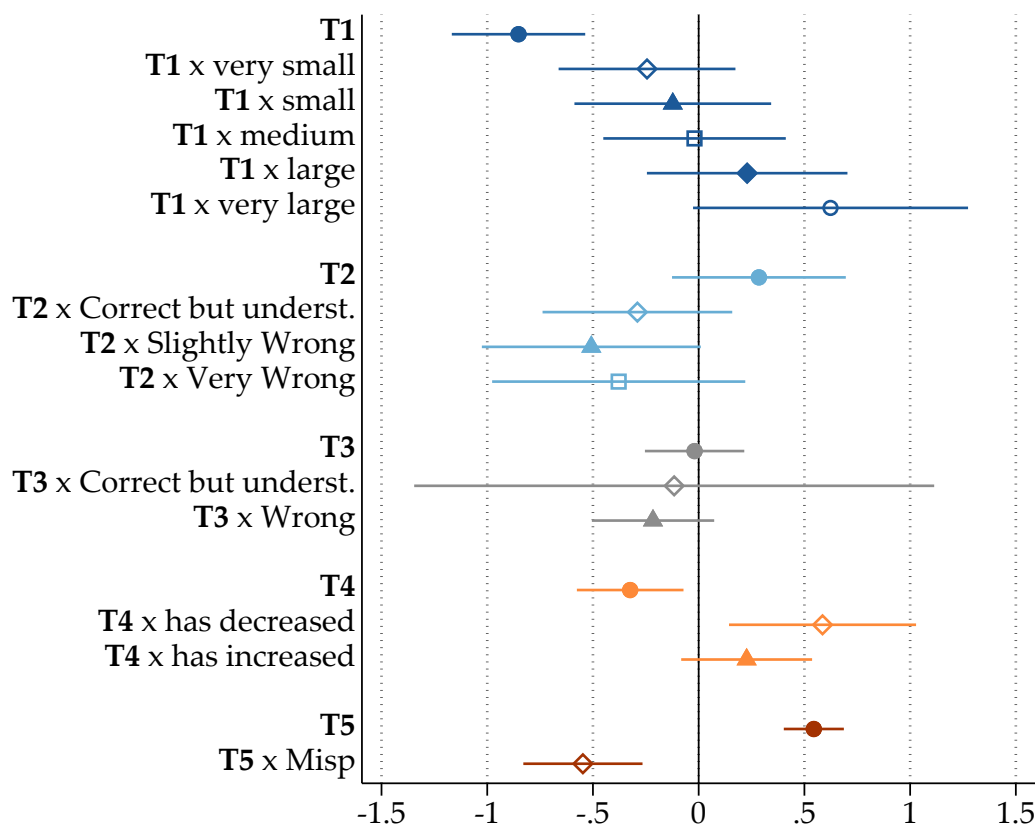
In the Distribution Treatment (T2), our measure of misperceptions is coded as ‘0’ if respondents correctly answered the first question (stating that on average landlords earn higher income than tenants) but overestimated the income gap between landlords and tenants, ‘1’ if they got the first question right but then underestimated the income gap, ‘2’ if they answered that tenants and landlords earn the same in the first question, and ‘3’ if they estimated that tenants earn more than landlords. In the Landlords Treatment (T3), we similarly define a categorical variable for misperceptions which takes the value ‘0’ if respondents correctly answered the first question (that private-sector companies own the highest share of rental flats in Berlin) but then overestimated the share, ‘1’ if they got the first question right but then underestimated the share and ‘2’ if they wrongly answered that private-sector companies do not own the largest share of rental flats in Berlin. In the Affordable Housing Treatment (T4), misperceptions are coded as ‘0’ if respondents correctly state that the income share spent on housing has remained constant for tenants in Berlin from 2006 to 2018, ‘1’ if they say the share has decreased and ‘2’ if they believe the share has increased. Finally, in the Displacement Treatment (T5), where we asked respondents whether they think the rent cap can help prevent displacement of low-income tenants, we code their misperceptions as ‘0’ if respondents correctly answered with “yes”, and ‘1’ otherwise.

Baseline Results Misperceptions. Figure 3 plots the interaction term between the respective treatments and the misperception variables. The first estimate (circle) in each treatment refers to the treatment effect among respondents with unbiased beliefs, followed

¹⁶For respondents who chose the correct answer for both questions, misperceptions are coded as ‘0’. If they got one of the questions slightly wrong and the other one correct, misperceptions are coded as ‘1’ (very small). If they got one question completely wrong and the other question correct, misperceptions are coded as ‘2’ (small). For respondents who got both questions slightly wrong, misperceptions are ‘3’ (medium). Respondents with one completely wrong and one slightly wrong answer have misperceptions coded at ‘4’ (large), and if both answers are completely wrong, this yields the highest value of misperceptions at ‘5’ (very large).

by the difference with respect to this group for respondents with varying types of misperceptions. The corresponding full regression Tables B.3-B.10 can be found in Appendix B.¹⁷

Figure 3 – Interaction Treatment Effect and Misperceptions



Notes: This figure plots estimates of the interaction between the respective treatments and the misperception variable (β_3 from Equation 2) with the corresponding 95% confidence intervals. T1: Efficiency Treatment, T2: Distribution Treatment, T3: Landlords Treatment, T4: Affordable Housing Treatment, T5: Displacement Treatment.

Starting with the Efficiency Treatment (T1), Figure 3 reveals significant treatment effect heterogeneity with respect to misperceptions. The treatment has a strong negative effect for people with no misperceptions. The effect becomes stronger for respondents with small misperceptions—i.e. those who answered the prior belief questions almost correctly—but diminishes for respondents with large misperceptions (see also Appendix Table B.4, showing split sample regressions for each category of misperceptions). Taken

¹⁷Figure B.7 shows that results do not change if we drop respondents who did not pass the attention check.

together, respondents with higher misperceptions are on average less likely to change their assessment of the rent cap in response to the Efficiency Treatment than those with small or no misperceptions. For respondents with the most severe misperceptions—believing that the rent cap increases housing supply and decreases rents in the unregulated sector—we find essentially no treatment effect. Note that in general, respondents’ support for the rent cap in the control group is increasing with misperceptions: the less respondents know about or believe in the rent cap’s efficiency costs, the more they support the policy (Appendix Table B.3).

For the Distribution (T2) and the Landlords Treatment (T3), where ATEs are zero, we do not find significant effect heterogeneity with respect to misperceptions either (see Tables B.5 and B.6). Taken together, this suggests that the respective aspects—income inequality between tenants and landlords, and private-sector companies dominating the rental housing market in Berlin—are not important for people’s assessment of the rent cap.

While the ATE is insignificant for the Affordable Housing Treatment (T4) as well, Figure 3 shows that the treatment lowers support for the rent cap for those respondents without misperceptions, while there is no effect on respondents who believe the share has either increased or decreased (see also split sample results presented in Appendix Table B.8). Respondents with misperceptions are thus again hesitant to update their views on the policy.

A similar picture emerges for the Displacement Treatment (T5). Figure 3 and Table B.9 demonstrate that the treatment increases agreement with the rent cap only for those respondents without misperceptions. For respondents who did not think the rent cap could help prevent displacement ex-ante, the effect is zero (compare also with split sample regressions presented in Table B.10). Analogous to our findings for the Efficiency Treatment (T1), we find that prior beliefs are correlated with ex-ante support for the rent cap also in this treatment: The second row of Table B.9 shows that respondents in the control group who do not think that the rent cap can help prevent displacement ex-ante are substantially less in favor of the rent cap than those who do.

Our information treatments therefore do not change respondents’ views about the rent cap mainly by correcting misperceptions. Instead, the treatments affect predominantly those respondents who are at least to some extent already aware of the presented information, while people with large misperceptions about the rent cap’s effects are not significantly affected by those treatments. Our treatments thus appear to work mostly via the priming channel, i.e., by putting the respective aspects front and center of people’s minds when thinking about the rent cap’s pros and cons.

To summarize, simply providing information does not change respondents’ views about the rent cap. Contrary to what we hypothesized in Section 2.3, our treatments affect

mainly respondents with correct or almost correct prior beliefs. Moreover, since prior beliefs are correlated with ex-ante support for the rent cap, our treatments mainly increase support for the rent cap among those respondents already favoring it, and decrease support among respondents not supporting the policy. The following subsection explores these at first glance paradox findings further.

3.4 *Credibility, Ideological Bias and Political Orientation*

Our post-experimental questions concerning the survey’s perceived ideological bias and the credibility attested to the provided information help to shed light on the mechanisms underlying the low responsiveness of respondents with strong misperceptions. Table 1 shows the existence of a negative correlation between the severity of respondents’ misperceptions and their assessment of how credible the provided information was. To some extent, this might be a simple psychological reflex: when presented with information contradicting their (prior) beliefs, people might be skeptical of that information and rate it as less credible than information confirming their beliefs. For the Efficiency (T1) and the Displacement (T5) Treatment, we additionally find a significant correlation between the severity of misperceptions and to what extent the survey was rated as being ideologically biased.¹⁸ Moreover, respondents’ prior beliefs are not only correlated with ex-ante support for the rent cap, but also connected to their political orientation in general. Indeed, the last row in Table 1 shows that being left-leaning is positively correlated with misperceptions about the undesirable price and quantity effects of rent caps, while right-leaning individuals are more likely to misperceive the rent cap’s positive effect on preventing displacement. Right-leaning individuals are also more likely to have misperceptions about the income differentials between landlords and tenants and about the market share of private-sector companies in the Berlin rental housing market.

However, the treatments do not have heterogeneous effects depending on political orientation per se (see Figure B.9), and the effect heterogeneity with respect to prior beliefs remains significant if we control additionally for an interaction of the treatment dummy with political orientation as well as other control variables and their interaction with the treatment dummy (Appendix Tables B.11 and B.12). The effect heterogeneity thus appears indeed with respect to prior beliefs.¹⁹

¹⁸The sign of the coefficient in the second row of Table 1 informs about the direction of the perceived ideological bias. The positive sign for T1 thus implies that respondents with misperceptions about the aspect informed in this treatment tend to view the survey as biased to the right, and vice versa for T5.

¹⁹As detailed in Section 2.2, we elicit posterior beliefs in all treatment groups at the end of the survey. Figure B.8 in the appendix shows that respondents do on average ‘update’ their beliefs (in the expected direction). However, we find that the negative association between the perceived credibility of our information treatments and misperceptions in prior beliefs documented in Table 1 persists and also holds for posterior beliefs. We suspect that some respondents have interpreted our posterior belief questions as an

Table 1 – Correlation between Misperceptions and Attitudes

| Misperceptions | T1 | T2 | T3 | T4 | T5 |
|----------------|-----------------------|------------------------|------------------------|------------------------|------------------------|
| Credible | -0.135*** (0.0473) | -0.0903*** (0.0129) | -0.0230 (0.0154) | -0.0367*** (0.0131) | -0.105*** (0.0138) |
| Ideological | 0.168*** (0.0554) | 0.0181 (0.0174) | -0.001 (0.0191) | 0.0118 (0.0157) | -0.0540*** (0.0175) |
| Left | 0.210*** (0.0677) | -0.0675*** (0.0188) | -0.0756*** (0.0218) | 0.0298 (0.0189) | -0.154*** (0.0199) |
| Observations | 2042 | 2103 | 2049 | 2024 | 2036 |
| R^2 | 0.013 | 0.030 | 0.007 | 0.006 | 0.069 |

Notes: This table reports multivariate OLS regression coefficients between having misperceptions about the respective facts mentioned in each treatment (T2, T3 and T4 misperceptions coded here for simplicity as 0 = no misperceptions and 1 = misperceptions [based purely on respondents’ answer to the first prior belief question]) and (i) rating the provided information as credible (on a scale from 1 = very implausible to 4 = very credible), (ii) rating the survey as ideologically biased (on a scale from 1 to 5, where 1 is “very left-leaning” and 5 is “very right-leaning”), and (iii) respondents’ political orientation (a dummy variable that is 1 if the respondent is “left-wing”, i.e. has voted for either *SPD*, *Bündnis90/Grüne* or *Die Linke*) in the last federal election. This variable is missing for one third of our respondents, which explains the reduced sample size. T1: Efficiency Treatment, T2: Distribution Treatment, T3: Landlords Treatment, T4: Affordable Housing Treatment, T5: Displacement Treatment.

Our findings are consistent with individuals being *prior-biased* in updating their policy preferences upon receiving information (Benjamin, 2019; Charness and Dave, 2017): When the presented information confirms their views (which are to some extent shaped by their political identity), preferences are reinforced in the direction of their prior beliefs. However, when the presented information goes against their views, respondents are either not affected, or they might even double-down on their views and preferences (Alesina et al., 2021; Alfaro et al., 2023).^{20 21}

‘attention check’ (something they are trained to recognize and respond to as participants of online survey panels) and thus their answers are more likely to reflect how well they have remembered the provided information rather than their ‘true’ posterior beliefs about the respective aspects.

²⁰Evidence for prior-biasedness in how individuals update their views is also documented in Barrera et al. (2020), Chopra et al. (2022), Nyhan et al. (2020), Nyhan and Reifler (2010), and Soroka (2006).

²¹Following Benjamin (2019) and Charness and Dave (2017), Alfaro et al. (2023) provide a formal description of this prior-biased updating behavior, which we can adapt for our setting: Let A denote the event that “the rent cap is good”, to which an individual assigns prior probability $p(A)$, and let A^C refer to the event that “the rent cap is bad”, which holds with complementary prior probability $p(A^C) = 1 - p(A)$. Conditional on receiving information treatment T , the posterior odds of A relative to A^C can be written as:

$$\frac{\pi(A|T)}{\pi(A^C|T)} = \left(\frac{p(T|A)}{p(T|A^C)} \right)^\kappa \frac{p(A)}{p(A^C)}. \quad (3)$$

The case $\kappa = 1$ corresponds to standard Bayesian updating (according to Bayes’ rule). Assume that, if treatment T confirms A (e.g., if T is T5, i.e. “the rent cap helps prevent displacement”), we have

Taken together, these observations can help explain the lack of treatment effect for respondents with (large) misperceptions. We conclude that respondents who received information that differed (too much) from their (potentially politically motivated) prior beliefs discounted this information as having low credibility and being ideologically biased, and thus did not feel inclined to change their views about rent control policies upon receiving it.

Designing Successful Information Treatments and Political Communication

Our findings concerning treatment effect heterogeneity with respect to prior beliefs complement the results in Alesina et al. (2021), Alfaro et al. (2023), Douenne and Fabre (2022), and Stantcheva (2021): Mere information provision is typically not sufficient to affect support for economic policies. If individuals hold politically motivated prior beliefs,²² information that threatens those beliefs is perceived as ideologically biased and thus less credible, and people are less willing to revise their assessment of economic policies in response. Instead, the provided information convinces mostly respondents who are already more in favor (or against) the respective policy ex-ante. This presents potential challenges for the design of successful information treatments and political communication in general. How can we create them such that we do not only convince those who are already “on our side”?

A chance finding we made as a result of our research design can provide a first step towards answering this question. Remember that we asked two prior belief questions in our Efficiency Treatment (T1): One question on the expected effect of the rent cap on the supply of rental housing, and one question on the effect on rents in the unregulated sector. Respondents could thus either answer both questions correctly, both incorrectly, or they could be correct in one and wrong in the other aspect. We find the smallest treatment effects for respondents with large misperceptions (i.e. those who were completely wrong in at least one question and at least slightly wrong in the other). However, while we find

$\frac{p(T|A)}{p(T|A^C)} > 1$, so the individual would update in favor of A with standard Bayesian updating; conversely, if T disconfirms A (e.g., T is T1, i.e. “the rent cap has efficiency costs”), we have $\frac{p(T|A)}{p(T|A^C)} < 1$ (and thus an individual would update in favor of A^C under Bayes’ rule). Prior-biased updating corresponds to $\kappa = c_1(\mathbf{1}(T \text{ confirms } A)) + c_2(\mathbf{1}(T \text{ disconfirms } A))$, where $c_1 > 0$ and $c_2 < 0$. Irrespective of whether T confirms or disconfirms A , we thus end up with $\frac{\pi(A|T)}{\pi(A^C|T)} > \frac{p(A|T)}{p(A^C|T)}$ and the prior is reinforced. (A situation where individuals do not react to information that is dissonant with their priors would correspond to $c_2 = 0$.)

²²This seems to be in particular the case for “partisan” policies such as rent control, (income and wealth) redistribution and positive affirmative action towards blacks, where the partisan positions are relatively clear. Stantcheva (2022) finds evidence that this is somewhat less of an issue for trade, where information treatments seem to work irrespective of priors and political orientation. Trade used to be a clear partisan issue before the Trump presidency (with—broadly speaking—Republicans “in favor” and Democrats “against” free trade) but positions have become somewhat confused ever since Trump (a Republican) started campaigning against free trade.

large treatment effects for people with no misperceptions (i.e. who answered both prior belief questions correctly), we find even slightly larger treatment effects for respondents with small misperceptions (see also Table B.4). These are respondents who answered one question correctly and got the other question wrong. These results suggest that individuals were quite responsive to information that “goes against” their prior beliefs in one aspect, if their prior beliefs were confirmed on another aspect. Perhaps this affirmation concerning one aspect increases trust in the “new” and opposing information on the other aspect, and thus lowers concerns about ideological bias and credibility. However, since our experiment was not designed to specifically analyze this phenomenon, we can only speculate about the mechanisms at play here and acknowledge that this presents an interesting follow-up question that is left for future research.

3.5 *Further Heterogeneity*

Apart from analyzing heterogeneity with respect to respondents’ misperceptions, we study heterogeneity in treatment effects based on the background information on our respondents collected in the survey. Specifically, we test whether there are differences according to economic interests and place of residence: between tenants and owner-occupiers, landlords and no landlords, East and West Germany, respondents living in urban, suburban, or rural areas, and respondents living in Berlin. Furthermore, we investigate if treatment effects differ by gender, age, income, education, political orientation, the consumption of news on economic affairs, and preferences regarding the role of the government in the economy. While, as Figure 1 shows, baseline support for the rent cap varies with economic interests as expected, we do not find significant treatment effect heterogeneity with respect to any of these categories (results available upon request).²³

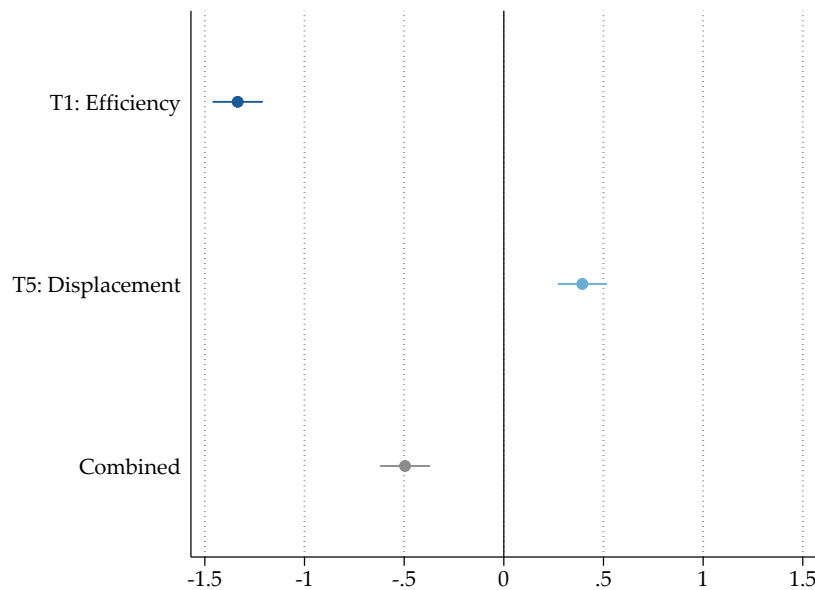
3.6 *Results from Second Survey*

In our main survey, we find that support for the rent cap can be both increased and decreased by providing information about positive and negative aspects of the policy. But what happens if people get information about positive and negative (side-)effects of the rent cap simultaneously? In order to explore this question we ran a second, complementary survey in October 2022 on a new sample of 12,000 respondents to test the effect of a composite treatment combining the two treatments where we found significant ATEs in

²³In addition to being correlated with political orientation, we also find that prior beliefs are correlated with economic interests, which suggests some degree of motivated reasoning not only according to political orientation but also according to personal (expected) gains or losses from the policy. However, as pointed out above, there is no treatment effect heterogeneity with respect to economic interests, and the effect heterogeneity with respect to misperceptions remains significant even if we additionally control for the interaction between economic interests and the treatment dummy.

the main survey, i.e., the Efficiency Treatment (T1) and the Displacement Treatment (T5). As Figure 4 demonstrates, we almost perfectly replicate the findings from the main survey concerning the direction and size of treatment effects for the separate treatments. Furthermore, the effect of the composite treatment is almost the exact sum of the two, thus resulting in a significant dampening of respondents’ assessment of the rent cap relative to the control group.

Figure 4 – ATEs complementary second survey: General Agreement with the Rent Cap



Notes: This figure plots the estimated coefficients from OLS regression (1) (as well as 95% confidence intervals) per treatment for our main outcome variable: *In your opinion, how would you rate a rent control policy such as the Berlin rent cap?* (elicited on a 0-10 Likert scale). Follow-up survey from October 2022.

In addition to our main outcome question concerning respondents’ assessment of the rent cap, we also added an incentivized outcome question. Respondents were informed that among all participants in our survey, 20 persons would be randomly picked to receive an extra remuneration of 50 Euros.²⁴ We then informed them that in case they would be among the winners, they could decide to donate a share of the extra remuneration to an organization lobbying for extensive rent regulation in Germany.²⁵ As shown in Figure 5, we find that, while there is no significant effect on donations in the Efficiency

²⁴Respondents were not informed about the number of participants and hence couldn’t infer their chances of winning.

²⁵The organization is called “Kampagne Mietenstopp”. Respondents were informed that this organization ‘is a civil society, non-partisan alliance that campaigns for a nationwide rent freeze.’

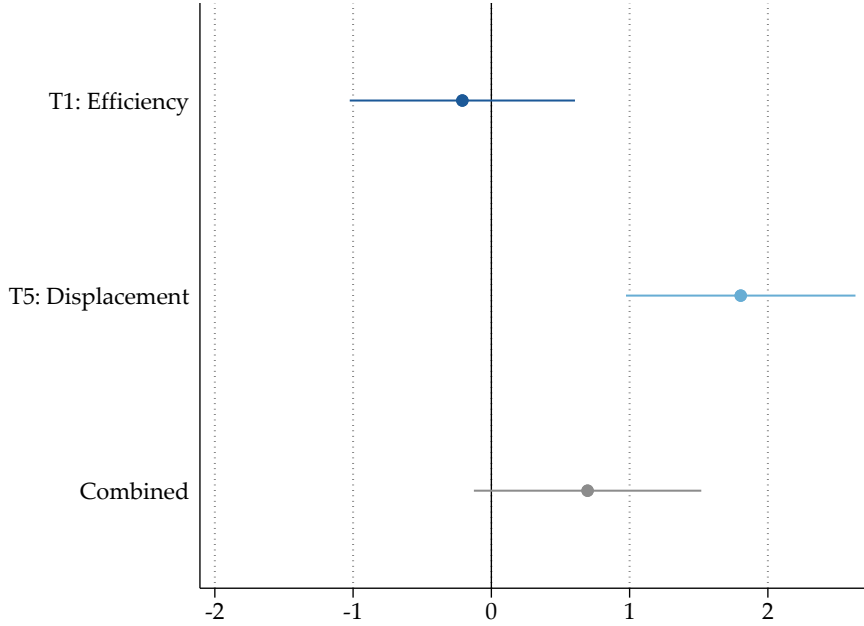
Treatment and the combined treatment, donations are on average almost 2 Euros higher in the Displacement Treatment compared to the control group where the average donation amounts to 16 Euros. We interpret this finding as a validation of the significant treatment effects we observe on our main outcome question.²⁶

The second survey also included an open-ended survey question eliciting respondents' considerations in favor or against the Berlin rent cap. Analyzing and categorizing the responses to the open-ended survey question from respondents in the control group, we find that our information treatments capture the most pressing issues about rent control and the housing market voiced by respondents. Protection of low- and middle-income households against displacement, concerns about the affordability of living in a city like Berlin, negative sentiment towards institutional investors as well as positive distributive effects appear to be the most important pro-arguments expressed by supporters of the Berlin rent cap. The most frequently mentioned argument against a policy like the Berlin rent cap is its perceived negative effect on the quantity and quality of rental apartments, going hand in hand with an increasing rental demand (more detailed results available upon request).

Finally, we recontacted respondents 1–4 weeks after the October 2022 survey in an obfuscated follow-up (the response rate was 75%, i.e. approximately 9,000 respondents replied to this follow-up request) to re-elicite the main outcome question and test the persistence of our treatment effects. As shown in Appendix Figure B.10, treatment effects persist and are still significant, but substantially smaller.

²⁶That we do not find a significant effect of the Efficiency Treatment (or, for that matter, of the combined treatment) is likely explained by the fact that we offer only one organization—a pro-rent-control lobby—for donation purposes. In order to find comparable effects for the Efficiency Treatment, we would have had to offer respondents the option to alternatively donate to a single-purpose organization lobbying for the abolishment of all types of rent control in Germany. However, to the best of our knowledge, such an organization does not exist in Germany.

Figure 5 – ATEs Follow-up Survey: Donation to Rent-freeze Lobby



Notes: This figure plots the estimated coefficients from OLS regression (1) (as well as 95% confidence intervals) per treatment for the incentivized donation question (elicited on a 0-50 Euro scale). Follow-up survey from October 2022.

4 CONCLUSION

This paper employs a large-scale randomized survey experiment among 18,000 respondents in Germany to examine the determinants of support for rent control. By comparing the effects of five information treatments that make respondents aware of the effects of rent control policies and the structure of the Berlin rental housing market, we analyze which aspects affect their support for the Berlin rent cap. We find that respondents who are informed about the negative efficiency effects agree less with the rent cap on average compared to the control group, while approval rises for those who are informed that rent control policies can help prevent displacement of low-income tenants. However, both treatments affect predominantly those who already know about these effects. Respondents with large misperceptions do not react to the treatments. We also find that the latter group tends to rate our survey as ideologically biased and the information provided in the respective treatment as less credible than those whose prior beliefs were mostly correct. Information about the income distribution between landlords and tenants and the share of private-sector companies in the Berlin rental market does not affect respondents' opinion about the rent cap. Informing respondents that the income share spent on housing has remained constant over the last decade lowers agreement for the rent cap, but again only for those respondents who already knew about this fact.

Our paper demonstrates that it is possible to both increase and decrease approval with rent controls by pointing out positive and negative effects of such policies. In particular, undesirable price and supply effects as well as displacement of low-income tenants matter in people’s considerations. These findings suggest that high public support for economic policies which are deemed “inefficient” by economists and other experts cannot exclusively be explained by a lack of understanding of the efficiency costs associated with these policies. In addition, people care about other aspects of such policies that are not usually emphasized by economists.

However, our treatments are not very successful in convincing those respondents who hold misspecified beliefs about these effects *ex-ante*. Instead, they predominantly affect respondents who already know about the aspects mentioned in the treatments. Our treatments thus seem to work mostly via the priming channel—i.e. by putting certain aspects front and center in people’s minds while they evaluate rent cap policies—rather than via the information provision channel. Moreover, since prior beliefs are correlated with *ex-ante* support for the rent cap, our treatments mainly increase support for the rent cap among those respondents already favoring it, and decrease support among respondents not supporting the policy. Similar to Stantcheva (2021) and Alesina et al. (2021), we find that the effectiveness of information treatments aimed at affecting support for economic policies depends on respondents’ own political orientation. If respondents receive information threatening their prior beliefs, they tend to discount it as ideologically biased and not credible, and are unlikely to revise their policy preferences.

Our results reveal potential challenges for the design of successful information treatments and—more generally—political communication. Simply providing information about positive or negative effects of economic policies does not necessarily change attitudes in the population. These are often fundamentally shaped by values and deep-seated ideological convictions. In view of these aspects, approaches that undermine the established partisan entrenchments and avoid one-sidedness by including different and even conflicting perspectives on a topic are perhaps more promising.

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A THE BERLIN RENT CAP

The Berlin rent cap (*Berliner Mietendeckel*) was introduced by the state of Berlin in February 2020 and mandated a new and drastic form of rent control. For existing rental contracts, the policy froze rents at their June 2019 level. For newly signed rental contracts, the policy stipulated upper limits for rents, which were not allowed to exceed by more than 20%. The upper limits were based on the rent index from 2013, which reflected the evolution of rents from 2008–2012. It further depended on the residential area (poor, middle, high), the year of construction and the equipment of the apartment. There were limits to rent increases after modernization (1 EUR per square meter). The Berlin rent cap pertained to all apartments in Berlin, with only few exceptions. Most importantly, newly constructed apartments which became ready for occupancy after January 2014 were excluded from the Berlin rent cap. The rent cap was initially introduced for a period of 5 years. On April 15, 2021, the Federal Constitutional Court declared the Berlin rent cap unconstitutional, since the federal government had already made a law regulating rents and a state government could not impose its own law that infringed upon that, and thus rendered the Berlin rent cap null and void.

The rent cap constituted a particularly restrictive form of rent control, leading to a two-tier rental market in Berlin (Arlia et al., 2022; Hahn et al., 2022). Based on rental offers advertised by immowelt, a large German online property portal, Arlia et al. (2022) show that one year after the introduction of the Berlin rent cap, the number of rental offers of regulated apartments had dropped, while no such effect was observed for unregulated apartments. As intended by the law, rents of regulated apartments had plummeted significantly. At the same time, rents of unregulated apartments had continued their upward trend and risen even faster than in other major German cities. As shown by Arlia et al. (2022), this strong increase in rents in the unregulated market segment can be attributed to an excess demand stemming from a reduction in the number of rental offers in the regulated market segment.

B ADDITIONAL TABLES AND FIGURES

B.1 Balance Table and Descriptive Statistics

Table B.1 – Descriptive Statistics

| | Germany | Survey Sample | | |
|----------|---------|---------------|--------|------|
| | Mean | Mean | Median | SD |
| Age | 45 | 44 | 42 | 16 |
| Male | .49 | .47 | 0 | .50 |
| East | .15 | .14 | 0 | .34 |
| Berlin | .04 | .10 | 0 | .30 |
| Owner | .47 | .36 | 0 | .48 |
| Landlord | .13 | .13 | 0 | .33 |
| Income | 3490 | 3088 | 2831 | 1798 |
| College | .19 | .27 | 0 | .45 |

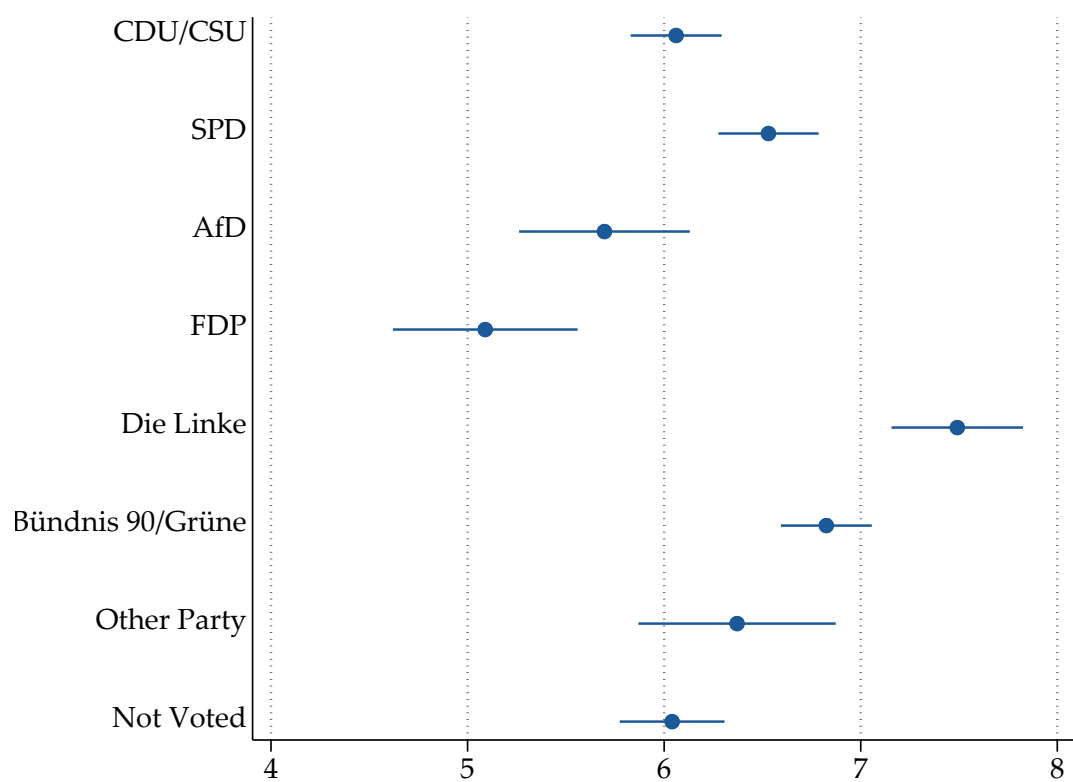
Notes: This table shows key descriptive statistics for our full sample of respondents (Survey Sample) as well as population averages (Germany). Population averages are obtained from the German Statistical Office for age, sex, the share of people living in Eastern Germany (East) and in Berlin, and the share of owner-occupiers (Owner); and from the 2018 wave of the German Socio-economic Panel (SOEP) for the share of landlords, monthly household income and for education.

Table B.2 – Significant Differences in Means

| | T1 | T2 | T3 | T4 | T5 | Control |
|------------------|------|------|------|------|------|---------|
| Age | 44 | 43 | 43 | 44 | 44 | 44 |
| Male | .47 | .46 | .48* | .47 | .47 | .45 |
| East | .13 | .13 | .14 | .15 | .14 | .13 |
| Berlin | .09* | .10 | .10 | .10 | .11 | .11 |
| Owner | .37 | .36 | .36 | .37 | .35 | .37 |
| Landlord | .13 | .13 | .13 | .13 | .12 | .13 |
| Income | 3157 | 3102 | 3061 | 3005 | 3111 | 3089 |
| College | .27 | .28 | .27 | .28 | .28 | .27 |
| Interview Length | 1509 | 1672 | 1652 | 1659 | 1558 | 1545 |
| Observations | 2973 | 3042 | 3073 | 2982 | 3044 | 3028 |

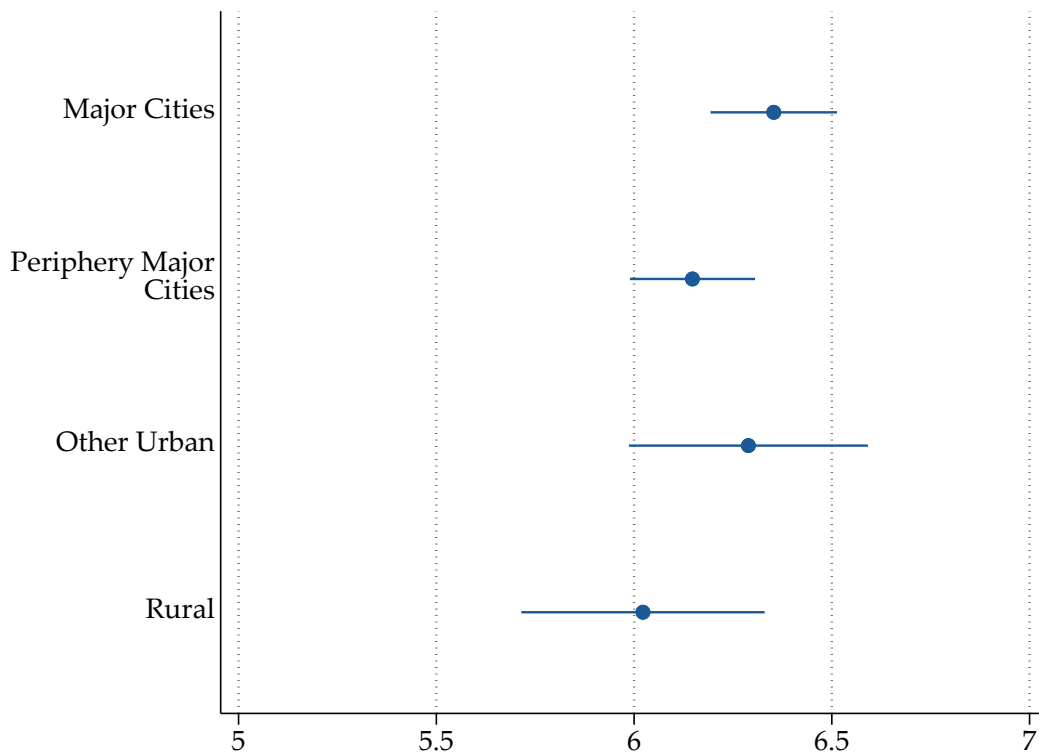
Notes: This tables shows averages in socio-economic characteristics, place of residence and interview length for the control and treatment groups. Interview length is expressed in seconds. Significant differences in means between each treatment and the control group at the 5% level are highlighted with an asterisk.

**Figure B.1 – Rent Cap Support in the Control Group
Heterogeneity by Voting**



Notes: This figure plots point estimates and 95% confidence intervals of the average support for the rent cap in the control group by voting behavior in the last German federal election in 2017.

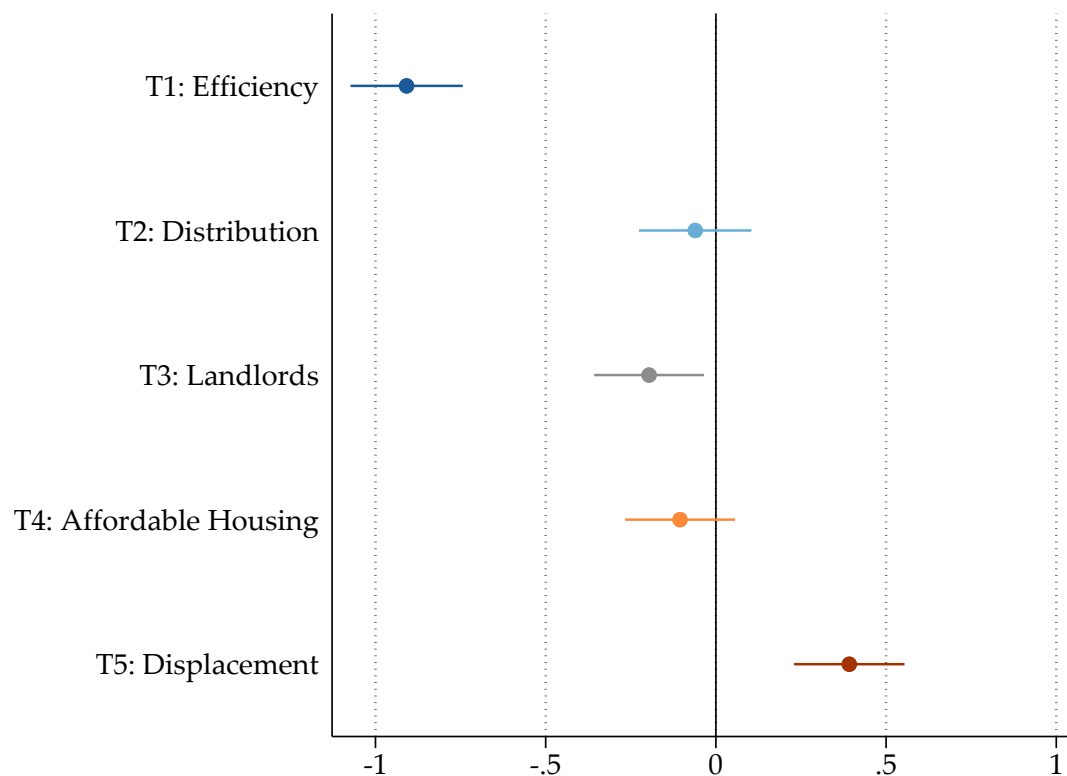
**Figure B.2 – Rent Cap Support in the Control Group
Heterogeneity by Region**



Notes: This figure plots point estimates and 95% confidence intervals of the average support for the rent cap in the control group by the region type of residence. "Major cities" refers to the 14 largest German cities with more than 500,000 inhabitants (Berlin, Bremen, Dortmund, Dresden, Düsseldorf, Essen, Frankfurt, Hamburg, Hannover, Köln, Leipzig, München, Nürnberg and Stuttgart). "Periphery major cities" refers to their suburbs, defined as all municipalities with less than 100,000 inhabitants within a driving distance of less than 45 minutes. "Other Urban" includes all German cities with 100,000-500,000 inhabitants. "Rural" is defined as municipalities with less than 20,000 inhabitants not included in the "Periphery major cities" category.

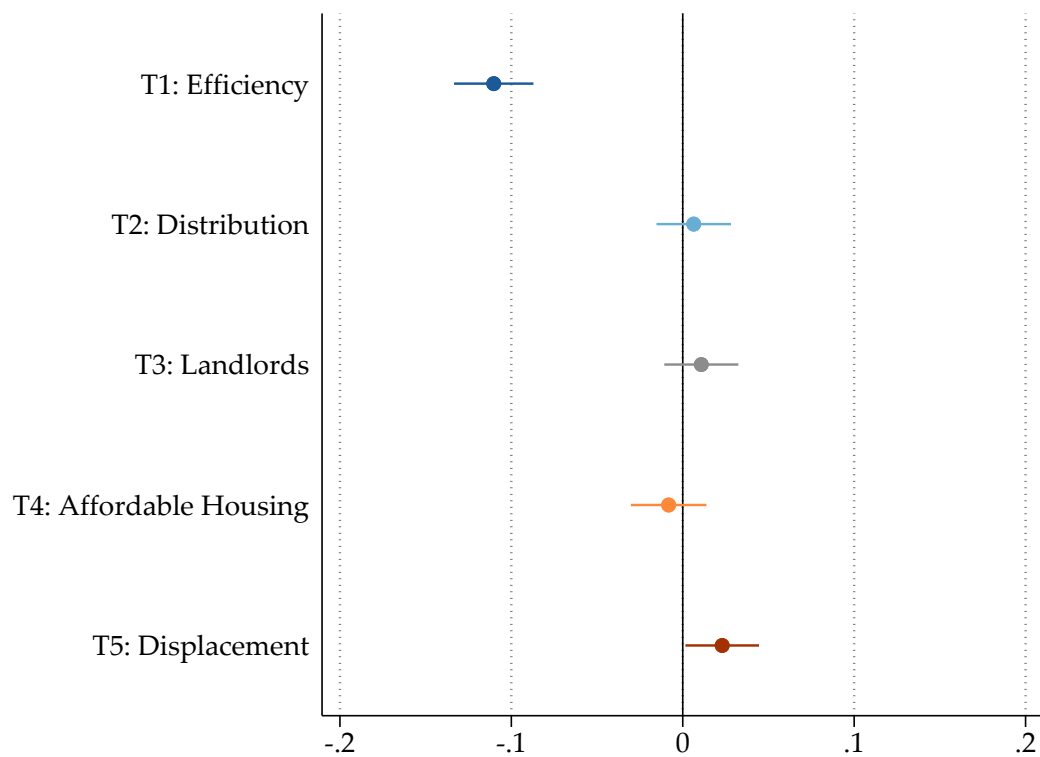
B.2 Robustness Test and Average Treatment Effects for Additional Outcome Variables

Figure B.3 – Average Treatment Effects: Attention Check Passed



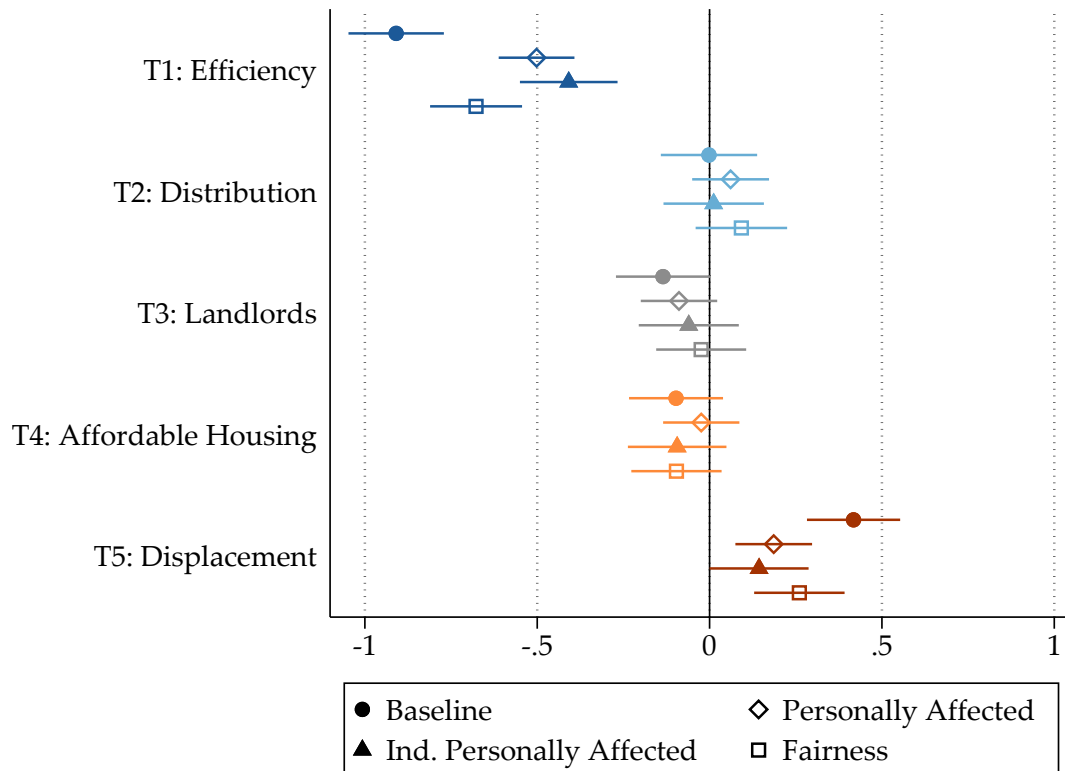
Notes: This figure plots the estimated coefficients from Equation (1) (as well as 95% confidence intervals) per treatment for our main outcome variable (general assessment of the Berlin rent cap, elicited on a 0-10 Likert scale) when respondents are dropped who did not pass the attention check at the beginning of the survey experiment. The attention check reads as follows: *In how many German cities with 500.000 or more inhabitants have you lived up to now? Please note that this is an attention check question. Please enter the number 33 in the following field (regardless of your correct answer to the above question).*

Figure B.4 – Average Treatment Effects: Introduce Rent Cap in Other Cities



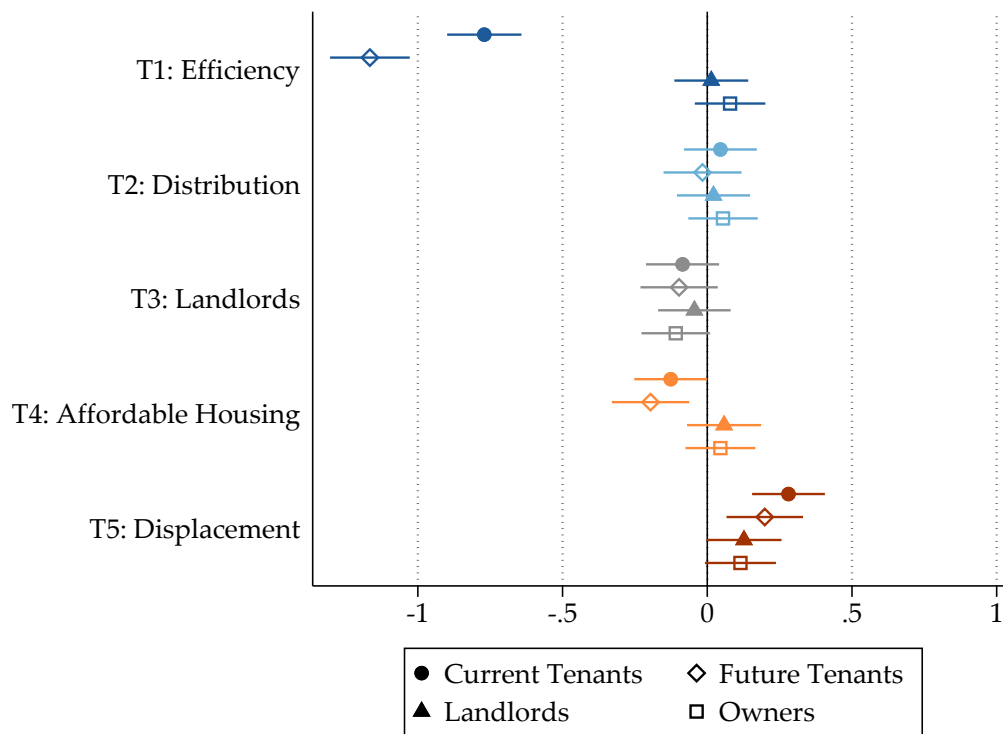
Notes: This figure plots the estimated coefficients from Equation (1) (as well as 95% confidence intervals) per treatment for our additional outcome variable: *Are you in favour of the federal government enacting rent regulations like the Berlin rent cap in cities with tight housing markets?* (yes or no).

Figure B.5 – Average Treatment Effects: Additional Outcome Variables



Notes: This figure plots the estimated coefficients from Equation (1) (as well as 95% confidence intervals) per treatment for our main outcome question (Baseline) and three additional outcome variables. (Diamond) Sample all respondents except Berlin or suburban Berlin: *If a measure like the Berlin rent cap were introduced where you live, how would you rate the impact on you personally or your household?* (0 very negatively -10 very positively); (Triangle) Sample = Suburban (except suburban Berlin) or rural: *If a measure like the Berlin rent cap were introduced in the nearest major city, how would you assess the impact on you personally or your household?* (0 very negatively -10 very positively); (Square) Full sample: *Do you think rent regulations like the Berlin rent cap are fair?* (0 very unfair -10 very fair). The estimates for the three additional outcome variables are compared to our baseline estimates of the overall assessment of the rent cap.

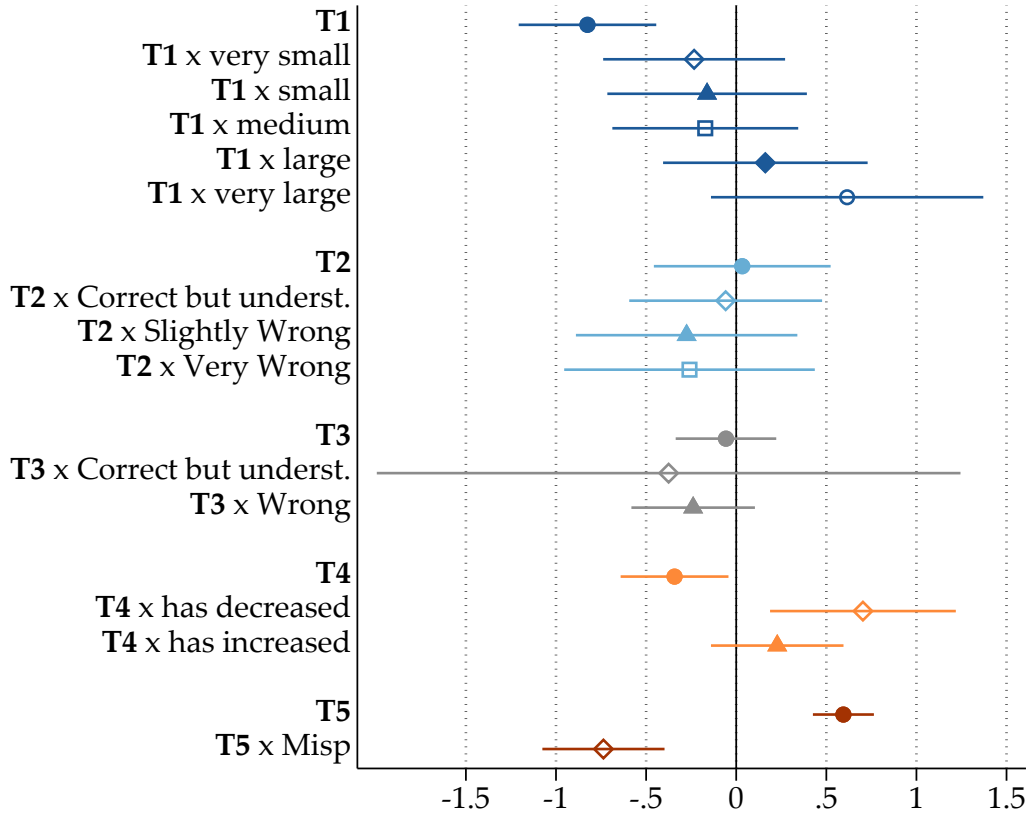
Figure B.6 – Average Treatment Effects: Is the rent cap beneficial for ...



Notes: This figure plots the estimated coefficients from Equation (1) (as well as 95% confidence intervals) per treatment for the question: *If a measure such as the Berlin rent cap were introduced in a city with a tight housing market, how would you rate the measure 1 for current tenants in the city? 2 for tenants looking for an apartment in the city in the future? 3 for landlords in the city? 4 for property owners in the city?* (0 very negatively -10 very positively).

B.3 Additional Results: Treatment Effect Heterogeneity by Misperceptions

Figure B.7 – Treatment Effect Heterogeneity by Misperceptions
Attention Check Passed



Notes: This figure plots the estimated coefficients from Equation (2) (as well as 95% confidence intervals) for the interaction terms (misperception category x treatment dummy, i.e. coefficient β_3 from equation 2) for each treatment for respondents who passed the attention check at the beginning of the survey experiment. T1: Efficiency Treatment, T2: Distribution Treatment, T3: Landlords Treatment, T4: Affordable Housing Treatment, T5: Displacement Treatment.

Table B.3 – Effect Heterogeneity of the Efficiency Treatment with Respect to Misperceptions

| | Support for Rent Cap |
|--------------------|----------------------|
| T1 (Efficiency) | -1.071*** (0.118) |
| Misperception | 0.177*** (0.034) |
| T1 x Misperception | 0.100** (0.046) |
| Observations | 6001 |
| R^2 | 0.043 |

Notes: This table reports results from Equation (2), for the sample of respondents in the control group and the Efficiency Treatment group. The severity of misperceptions is coded on a scale from 0-5 and treated as a continuous variable in this regression.

Table B.4 – Effect Heterogeneity of the Efficiency Treatment with Respect to Misperceptions—Sample Split Regressions

| | Support for Rent Cap | | | | | |
|----------------|----------------------|----------------------|----------------------|----------------------|----------------------|-------------------|
| Misperception: | 0 | 1 | 2 | 3 | 4 | 5 |
| T1: Efficiency | -0.852*** (0.161) | -1.096*** (0.140) | -0.974*** (0.174) | -0.872*** (0.150) | -0.623*** (0.181) | -0.228 (0.290) |
| Observations | 1396 | 1613 | 853 | 1075 | 728 | 336 |
| R^2 | 0.020 | 0.037 | 0.035 | 0.030 | 0.016 | 0.002 |

Notes: This table reports results from Equation (1) for each category of misperceptions (from 0 = no misperceptions to 5 = severe misperceptions), separately for the group of respondents in the control group and the Efficiency Treatment group.

Table B.5 – Effect Heterogeneity of the Distribution Treatment with Respect to Misperceptions

| | Support for Rent Cap |
|----------------------|----------------------|
| T2 (Distribution) | 0.285 (0.210) |
| Misperception 1 | -0.0310 (0.167) |
| Misperception 2 | -0.568*** (0.193) |
| Misperception 3 | -0.846*** (0.227) |
| T2 × Misperception 1 | -0.290 (0.229) |
| T2 × Misperception 2 | -0.508* (0.264) |
| T2 × Misperception 3 | -0.378 (0.305) |
| Observations | 6063 |
| R^2 | 0.015 |

Notes: This table reports results from Equation (2) for the sample of respondents in the control group and the Distribution Treatment group. Misperceptions take the values 0 (omitted category), 1, 2 and 3 (0 = correct but overestimated the income gap between landlords and tenants, 1 = correct but underestimated the gap, 2 = slightly wrong (both earn the same), 3 = very wrong (tenants earn more)).

Table B.6 – Effect Heterogeneity of the Landlords Treatment with Respect to Misperceptions

| | Support for the Rent Cap |
|----------------------|--------------------------|
| T3: Landlords | -0.0192 (0.120) |
| Misperception 1 | -0.441 (0.541) |
| Misperception 2 | -0.314*** (0.110) |
| T3 x Misperception 2 | -0.116 (0.627) |
| T3 x Misperception 2 | -0.216 (0.148) |
| Observations | 6101 |
| R^2 | 0.007 |

Notes: This table reports results from Equation (2) for the sample of respondents in the control group and the Landlords Treatment group. Labelling of misperceptions: 0 if correct but overestimated the share of private-sector companies among all landlords in Berlin, 1 if correct but underestimated the share, 2 if wrong. Note that category 1 applies to only 71 respondents (1.15% of respondents).

Table B.7 – Effect Heterogeneity of the Affordable Housing Treatment with Respect to Respondents’ Misperceptions

| | Support for the Rent Cap |
|------------------------|--------------------------|
| T4: Affordable Housing | -0.324** (0.129) |
| Misperception 1 | -0.110 (0.163) |
| Misperception 2 | 0.181 (0.111) |
| T4 x Misperception 1 | 0.586*** (0.226) |
| T4 x Misperception 2 | 0.227 (0.158) |
| Observations | 6010 |
| R^2 | 0.003 |

Notes: This table reports results from Equation (2) for the sample of respondents in the control group and the Affordable Housing Treatment group. Misperceptions take the values 0, 1 and 2 (0 = no misperceptions, 1 = respondents think share of income spent on housing has decreased for tenants in Berlin, 2 = respondents think share of income spent on housing has increased for tenants in Berlin).

Table B.8 – Effect Heterogeneity of the Affordable Housing Treatment with Respect to Respondents’ Misperceptions—Sample Split Regressions

| | Support for Rent Cap | | |
|-------------------------|----------------------|------------------|---------------------|
| Misperception Category: | 0 | 1 | 2 |
| T4: Affordable Housing | -0.324** (0.129) | 0.262 (0.185) | -0.0970 (0.0916) |
| Observations | 1531 | 700 | 3779 |
| R^2 | 0.004 | 0.003 | 0.000 |

Notes: This table reports results from Equation (1) for each category of misperceptions (0 = no misperceptions, 1 = respondents think income share spent on housing has decreased for tenants in Berlin, 2 = respondents think income share spent on housing has increased for tenants in Berlin), separately for the group of respondents in the control group and the Affordable Housing Treatment group.

Table B.9 – Effect Heterogeneity of the Displacement Treatment with Respect to Misperceptions

| | Support for Rent Cap |
|--------------------|----------------------|
| T5: Displacement | 0.545*** (0.072) |
| Misperception | -2.063*** (0.105) |
| T5 x Misperception | -0.548*** (0.144) |
| Observations | 6072 |
| R^2 | 0.169 |

Notes: This table reports results from regression (2) for the sample of respondents in the control group and the Displacement Treatment group. Misperceptions are coded as a dummy variable (1 if the respondent has misperceptions, 0 if not).

Table B.10 – Effect Heterogeneity of the Displacement Treatment with Respect to Misperceptions: Sample Split Regressions

| | Support for Rent Cap | |
|------------------|----------------------|---------------------|
| Misperception: | 0 | 1 |
| T5: Displacement | 0.545*** (0.0724) | -0.00294 (0.124) |
| Observations | 4178 | 1894 |
| R^2 | 0.013 | 0.000 |

Notes: This table reports results from Equation (1) for each category of misperceptions (0 = no misperceptions and 1 = misperceptions), separately for the group of respondents in the control group and the Displacement Treatment group.

B.4 Robustness Misperception Results

Table B.11 – Robustness Misperceptions: Efficiency Treatment

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| T1: Efficiency | -1.346*** (0.409) | -1.046*** (0.158) | -1.002*** (0.204) | -1.143*** (0.220) | -1.220*** (0.288) |
| Misperception | 0.174*** (0.034) | 0.178*** (0.034) | 0.187*** (0.033) | 0.174*** (0.036) | 0.173*** (0.034) |
| T1 \times Misperception | 0.0924** (0.046) | 0.0993** (0.046) | 0.103** (0.045) | 0.122** (0.049) | 0.0967** (0.046) |
| Int. Ideological | YES | - | - | - | - |
| Int. Attention Check | - | YES | - | - | - |
| Int. How Sure | - | - | YES | - | - |
| Int. Pol. Orientation | - | - | - | YES | - |
| Int. News Consumption | - | - | - | - | YES |
| Observations | 6001 | 6001 | 6001 | 5077 | 6001 |

Notes: This table reports for the sample of respondents in T1 and the control group results from an expanded regression (2), where we add the full interaction of the treatment dummy and further controls. In Column (1), we add a variable indicating to what extent (if at all) participants perceive the survey as ideologically biased, and the interaction of this variable with the treatment dummy. In Column (2), we add a variable indicating whether respondents passed the attention check and the interaction of this variable with the treatment dummy. In column (3), we add a variable eliciting how sure respondents are w.r.t. their prior beliefs and the interaction of this variable with the treatment dummy. In column (4), we additionally control for respondents' political orientation and an interaction of this variable with the treatment dummy. In column (5), we additionally control for the frequency of respondents' economics news consumption and an interaction of this variable with the treatment dummy. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

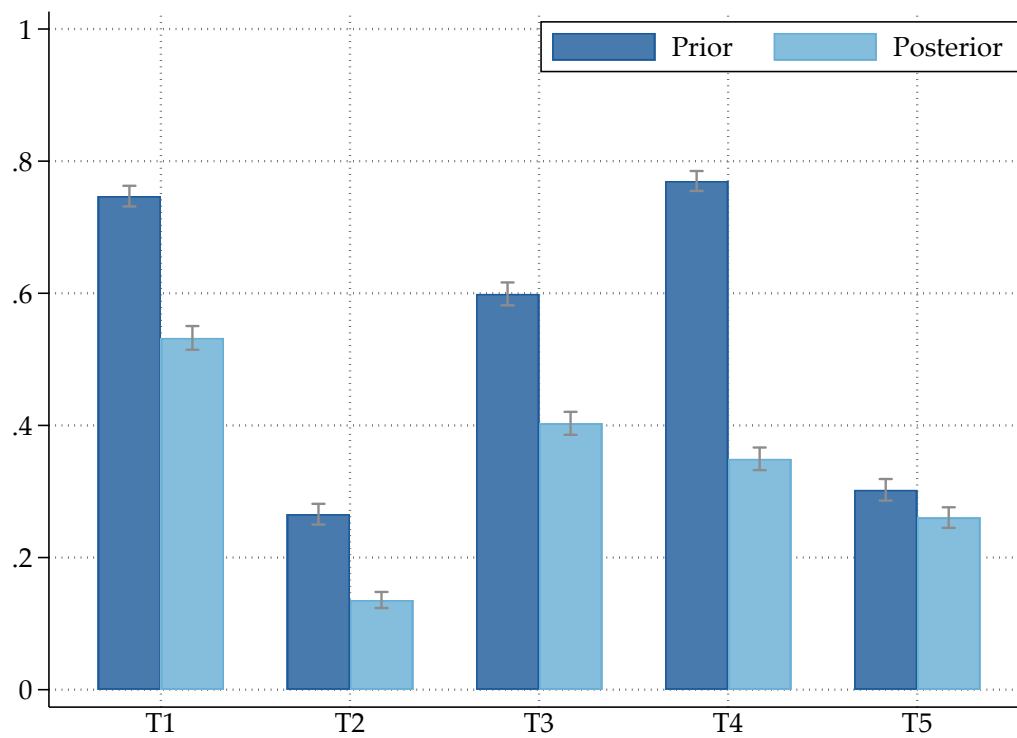
Table B.12 – Robustness Misperceptions: Displacement

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| T5: Displacement | 0.541*** (0.073) | 0.545*** (0.072) | 0.503*** (0.068) | 0.530*** (0.077) | 0.544*** (0.073) |
| Misperception | -2.053*** (0.105) | -2.062*** (0.105) | -1.865*** (0.110) | -2.024*** (0.115) | -2.051*** (0.106) |
| T5 \times Misperception | -0.522*** (0.143) | -0.549*** (0.144) | -0.694*** (0.147) | -0.463*** (0.155) | -0.549*** (0.144) |
| Int. Ideological | YES | - | - | - | - |
| Int. Attention Check | - | YES | - | - | - |
| Int. How Sure | - | - | YES | - | - |
| Int. Pol. Orientation | - | - | - | YES | - |
| Int. News Consumption | - | - | - | - | YES |
| Observations | 6072 | 6072 | 6072 | 5145 | 6072 |

Notes: This table reports for the sample of respondents in T5 and the control group results from an expanded Equation (2), where we add the full interaction of the treatment dummy and further controls. In Column (1), we add a variable indicating to what extent (if at all) participants perceive the survey as ideologically biased, and the interaction of this variable with the treatment dummy. In Column (2), we add a variable indicating whether respondents passed the attention check and the interaction of this variable with the treatment dummy. In column (3), we add a variable eliciting how sure respondents are w.r.t. their prior beliefs and the interaction of this variable with the treatment dummy. In column (4), we additionally control for respondents' political orientation and an interaction of this variable with the treatment dummy. In column (5), we additionally control for the frequency of respondents' economics news consumption and an interaction of this variable with the treatment dummy. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.5 Belief Updating

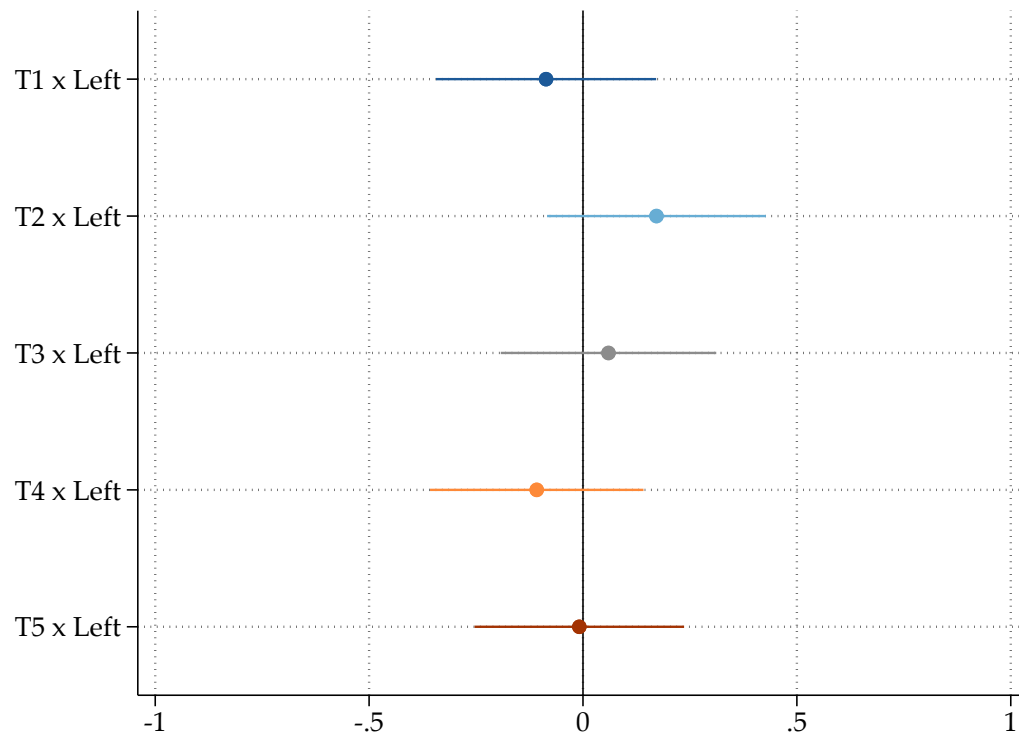
Figure B.8 – Belief Updating



Notes: This figure shows the share of participants with misperceptions by treatment group before and after the treatment, as well as corresponding 95% confidence intervals.

B.6 Further Treatment Effect Heterogeneity

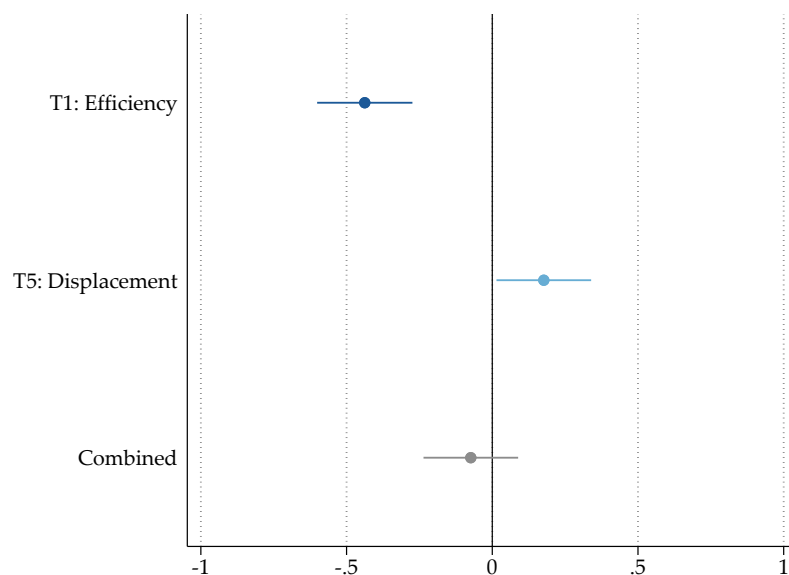
Figure B.9 – Treatment Effect Heterogeneity by Political Orientation



Notes: This figure shows the interaction effect across treatments with respect to political orientation. Left is an indicator indicating whether an individual has voted for either *SPD*, *Die Linke* or *Bündnis90/Grüne*, the three main parties on the left of the political spectrum in Germany.

B.7 Obfuscated Follow-up Survey

**Figure B.10 – Persistence of ATEs in an Obfuscated Follow-up Survey:
General Agreement with the Rent Cap**



Notes: This figure plots the estimated coefficients from Equation (1) (as well as 95% confidence intervals) per treatment for our main outcome variable: *In your opinion, how would you rate a rent control policy such as the Berlin rent cap?* (elicited on a 0-10 Likert scale) in an obfuscated follow-up survey 1-4 weeks after the October 2022 survey.

C INFORMATION TREATMENTS (ENGLISH TRANSLATION)

Introductory Statement (All Groups):

In the following, we would like to ask you some questions about the Berlin rent cap.

The Berlin rent cap was introduced by the Berlin Senate in February 2020 to put a ceiling on rents in the state of Berlin. The law set rent caps for re-rented apartments and froze rents from existing leases at the June 18, 2019, level. If rents from existing leases were 20% above the rent cap, they had to be reduced to the rent cap.

With few exceptions, the Berlin rent cap affected all rental apartments in the Berlin metropolitan area. Only new apartments that were ready for first-time occupancy after January 1, 2014, were exempt from the Berlin rent cap. On April 15, 2021, the Federal Constitutional Court declared the Berlin rent cap unconstitutional for formal reasons (lack of legislative authority on the part of the Berlin Senate) and thus suspended it.

T1 Efficiency Treatment:

How do you think the Berlin rent cap has affected the supply of rental housing in Berlin?
[response categories randomized]

- 1) It has increased the supply of rental housing.
- 2) It has reduced the supply of rental housing.
- 3) It has not affected the supply of rental housing.

How confident are you in your answer? 0 (very uncertain)-10 (very certain)

As mentioned at the beginning, the unregulated segment comprised rental apartments that were ready for first-time occupancy since January 1, 2014. These apartments were not subject to the Berlin rent cap.

How do you think the Berlin rent cap has affected rents in the unregulated segment in Berlin? [response categories randomized]

- 1) It has led to higher rents in the unregulated segment.
- 2) It has led to lower rents in the unregulated segment.
- 3) It has not affected rents in the unregulated segment.

How confident are you in your answer? 0 (very uncertain)-10 (very certain)

Next page: Studies have shown that rent regulations such as the Berlin rent cap reduce the supply of rental housing, partly because rental apartments are converted into

condominiums. This leads to higher rents in the unregulated segment because apartment seekers have to switch to rental apartments that are not subject to rent regulation.

Initial study results indicate that the Berlin rent cap has led to similar effects. One year after the introduction of the rent cap, a decline in the supply of rental apartments in the regulated segment and an increase in rents in the unregulated segment could be observed.

T2 Distribution Treatment:

In your opinion, which group has a higher income on average, tenants or private landlords?
[response categories 1 and 2 randomized]

- 1) Tenants
- 2) Private landlords
- 3) Both about the same

How confident are you in your answer? 0 (very uncertain)-10 (very certain)

[FILTER: Answer = 1]

By how many percent do you think the income of tenants in Germany is on average higher than the income of private landlords? (field with input > 0%)

[FILTER: Answer = 2]

By how many percent do you think the income of private landlords in Germany is on average higher than the income of tenants? (field with input > 0%)

Next page: In 2018, private landlords had, on average, a 54% higher net income than tenants.

T3 Landlords Treatment:

In your opinion, which group of owners has the largest stock of rental apartments in Berlin? [response categories randomized]

- 1) Private-sector companies
- 2) Private individuals
- 3) Housing cooperatives
- 4) Public authorities
- 5) Communities of apartment owners

How confident are you in your answer? 0 (very uncertain)-10 (very certain)

[FILTER: Answer = 1]

What percentage of Berlin's rental housing market do you think is owned by private-sector companies (in percent)? (slider with input from 0 – 100%)

[FILTER: Answer = 2]

What percentage of Berlin's rental housing market do you think is owned by private individuals (in percent)? (slider with input from 0 – 100%)

[FILTER: Answer = 3]

What percentage of Berlin's rental housing market do you think is owned by housing cooperatives (in percent)? (slider with input from 0 – 100%)

[FILTER: Answer = 4]

What percentage of Berlin's rental housing market do you think is owned by public authorities (in percent)? (slider with input from 0 – 100%)

[FILTER: Answer = 5]

What percentage of Berlin's rental housing market do you think is owned by communities of apartment owners (in percent)? (slider with input from 0 – 100%)

Next page: In Berlin, private-sector companies own the largest stock of rental apartments. Their share of the rental housing market in Berlin is 29%. Private-sector companies include privately owned housing companies such as Deutsche Wohnen or Vonovia and other private-sector companies, for example banks, insurance companies and funds.

T4 Affordable Housing Treatment:

How do you think the average share of income used for housing rent has changed for Berlin renters in the period 2006 to 2018? [response categories randomized]

- 1) The share has increased.
- 2) The share has decreased.
- 3) The share has remained approximately the same.

How confident are you in your answer? 0 (very uncertain)-10 (very certain)

[FILTER: Answer = 1 or 2]

According to data from the Berlin-Brandenburg Statistics Office, the average share of income used by Berlin tenants for housing rent was 28 percent in 2006. In your opinion, what percentage of income did Berlin tenants spend on housing rent on average in 2018? (Field with input from 0 to 100)

Next page: According to data from the Berlin-Brandenburg Statistics Office, the average share of income used by Berlin tenants for housing rent was 28 percent in 2006 and 28.2 percent in 2018. This means that the share has remained roughly the same.

T5 Displacement Treatment:

Do you think that a rent regulation like the Berlin rent cap can protect lower-income households from being forced out of the city due to high rent costs? [response categories randomized]

- 1) Yes
- 2) No

How confident are you in your answer? 0 (very uncertain)-10 (very certain)

Next page: Studies show that rent regulations such as the Berlin rent cap can help protect lower-income households from displacement. For example, a comparable rent regulation in San Francisco resulted in renters in price-regulated apartments being less likely to leave San Francisco than renters who did not live in price-regulated apartments. Rent regulation acts like insurance against unaffordable rents.