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DISPELLING MISCONCEPTIONS ABOUT ECONOMICS

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Abstract

Some popular views about the workings of the economy are completely at odds with solid empirical evidence and congruent theoretical explanations and therefore can be qualified as misconceptions. Such beliefs lead to support for harmful policies. Cognitive biases may contribute to explaining why misconceptions persist even when scientific information is provided to people. We conduct two experimental studies to investigate, for the first time in economics, whether presenting information in a refutational way affects people's beliefs about an important socio-economic issue on which expert consensus is very strong: the harmful effects of rent controls. In the laboratory (Study 1) both our refutational and non-refutational messages induce a belief change in the direction of expert knowledge. The refutational message, however, does not improve significantly on the non-refutational one. In the field (Study 2), where participants are college students receiving economic training, the refutational text improves, subject to some caveats, on standard instruction but not on the non-refutational message.

JEL: A12, A2, D9, I2.
Keywords: misconceptions; biases; rent control; economic communication; persuasion

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1. Introduction

Misconceptions about natural, health, economic and social issues are pervasive in society. Denying climate change, attributing autism to vaccines, and the idea that humans only use a small part of their brain are some illustrative examples of beliefs that are contradicted by scientific evidence. Bensley and Lilienfeld (2017) define misconceptions as "claims about behavior and mental processes that are unsupported or contradicted by high-quality psychological research, that is, they are assertions inconsistent with well-established scientific research (p. 378)". This definition can be generalized to any field of science. Studies in cognitive psychology suggest that cognitive biases –such as confirmation bias, blind-spot bias, self-serving bias and causal illusions– predispose people to hold on to misinformation and ignore scientific evidence when it contradicts a particular pre-existing belief. As a result, misconceptions are very entrenched and hard to eradicate (Kahneman, 2011; Lewandowsky et al., 2012).

In economics, some popular views about the workings of the economy are completely at odds with solid empirical evidence and congruent theoretical explanations, and therefore can be qualified as misconceptions. Work by Caplan (2002), Jacob, Christandl, and Fetechenhauer (2011) and Sapienza and Zingales (2013), among others, provides many examples of the divergence between economic researchers' consensus and lay people beliefs. Some divergences relate to the public's perceptions of factual data, i.e., the actual inflation rate, or the unemployment rate (Runge and Hudson, 2020). Others relate to causal relations, such as to whether buying domestic products increases home country employment, and whether rent control is an effective policy to increase the availability of affordable housing.

Misconceptions about economics are of concern especially because they may lead citizens to demand and support policies that have harmful net effects. Although possibly

well-intended, such misconceptions may have a negative impact on welfare, as, for example, when people underappreciate policies' equilibrium effects (Dal Bo, Dal Bo and Eyster, 2017). Communicating factual evidence and theory may not be enough to offset misconceptions. Experiments show that when people are exposed to factual information they only partially revise their beliefs. This is a manifestation of overweighting the initial opinion, a common mistake made in many situations that deviates from rational processing of information. The neural basis of this phenomenon has been studied for instance by Achtziger et al. (2014). The proportion of people who revise beliefs about topics sensitive to political views, as is often the case with economic policies, is lower than for topics that appear to be more technical (Johnston and Ballard, 2016; Nyhan and Reifler, 2010; Nyhan, 2020). It is thus pertinent to explore interventions aimed at reducing misconceptions about economics.

We analyze, to the best of our knowledge for the first time in economics, the effectiveness of a particular type of intervention, the refutation text, to dispel a misconception about an economic issue. This misconception is the highly popular opinion that rent control is an effective policy to increase the availability of affordable housing.[1] It is very hard to eliminate, even after individuals take a formal course in economics where they are exposed to theory and factual information (Busom, Lopez-Mayan and Panadés, 2017).

The refutation text (RT hereafter) is a communication tool designed to help people revise their false beliefs through slow, analytical processing of information. We design our RT using findings from research in psychology, where the problem of how to dispel misconceptions has been studied for some time (Tippett, 2010; Lewandowsky, 2021). The key feature of a RT is how arguments contradicting the misconception are presented.

[1] We substantiate this statement in the next section.

Essentially, the RT must first explicitly state the belief and assert it is a misconception. It then should emphasize the negative consequences of the belief and refute it explaining the arguments and evidence obtained through scientific research. In this way the RT intends to connect this new information to the incorrect information pre-existing in a person's memory. In addition, the RT should acknowledge the motivation for the misconceived belief. We discuss the characteristics of our RT in detail in Section 2.

Refutational texts have been used to dispel misconceptions in other scientific fields. For instance, in psychology and STEM education (Kowalski and Taylor, 2009; Masson et al. 2014; Lucariello, Tine and Ganley, 2014); in public health, where these refutational strategies are used to reduce vaccine hesitancy (World Health Organization, 2017); and in natural sciences, especially regarding climate change denial (Druckman, 2015; Jamieson, Kahan and Scheufele, 2017; Nussbaum, Cordoba and Rehmat, 2017; the Yale Program on Climate Change Communication).

We contribute to the literature by adding novel insights about the effectiveness of using a written refutational message to communicate results from research in economics to the public and reduce the prevalence of the misconception on rent controls. Existing research has explored the implications of entrenched beliefs and cognitive biases for some economic decisions, such as financial decisions (Lusardi and Mitchell, 2014), education decisions (Lavecchia, Liu and Oreopoulos, 2016; Levitt et al., 2016), and labor market decisions (Cardoso, Loviglio and Piemontese, 2016). All this research owes a lot to the seminal work of Kahneman and Tversky (2000), Kahneman (2011) and Thaler (2015). However, the analysis of strategies to communicate economic policies to the public has only very recently become the focus of research, mainly regarding monetary policy (Haldane and McMahon, 2018; Coibion, Gorodnichenko and Weber, 2019). Even as the need to investigate how to communicate findings of economic research to the public is

increasingly acknowledged (see, for instance, Royal Economic Society (2021)), no previous work has analyzed the effectiveness of a refutational message to eradicate misconceptions about economics.

We conduct two experimental studies. Study 1 is a laboratory experiment where participants are college students who are not enrolled in economics courses or majors. Participants are randomly allocated to one of three conditions: individual exposure to the RT; team exposure to the RT; and exposure to a non-refutational text (NRT hereafter), which is the benchmark. The effect of the RT on beliefs is estimated by comparing the change in beliefs –before and after the intervention– across conditions.

Study 2 is a field (quasi) experiment conducted in the setting of a college-level course, where participants are first-year students in principles of economics from three cohorts (2015, 2017, 2019). The first cohort is exposed to a standard lecture on price controls and to a standard practice session where problems about supply, demand and price controls are solved; the second cohort is exposed to the standard lecture and to a practice session with the RT; and the third cohort is exposed to the standard lecture and to a practice session with the NRT. The effect of the RT is estimated by comparing the change in beliefs between the beginning and the end of the semester across cohorts.

The rest of the paper is organized as follows. Section 2 presents the refutational approach and our research questions. Section 3 and Section 4 describe and present the results of Study 1 and Study 2, respectively. Section 5 discusses findings and limitations. Section 6 concludes.

2. Refutational communication and research questions

2.1. Background

Providing information about economic experts' views to lay people may correct people's beliefs when these are of technical nature, but when symbolic and politically salient issues are involved, opinion change is more unlikely (Johnston and Ballard, 2016). We may conjecture that exposure to economic arguments and evidence in a detailed way would be more effective in dispelling misconceptions. There is, however, scant evidence that speaks to this issue.[2] One exception is Busom, Lopez-Mayan and Panadés (2017) who show that exposure to standard economic instruction for a semester hardly affects initial false beliefs.

In contrast to economics, scholars in natural sciences and in psychology have long been concerned about misconceptions (Nakhleh, 1992; CUSE, 1997; Lilienfeld et al., 2009; Kowalski and Taylor, 2009; Lilienfeld, 2010; Lucariello, Tine and Ganley, 2014; Masson et al., 2014; Nussbaum, Cordova and Nehmat, 2017). Research suggests that when people holding views about how the world works are presented with contradicting information in a purely expository way, the brain classifies it as errors, allowing them to stick to their original views (Dunbar, Fugelsang and Stein, 2007; Masson et al., 2014; Byrnes and Dunbar, 2014). Studies in psychology suggest that several cognitive biases may be at the root of the failure of direct expository communication to reduce misconceptions (Bensley and Lilienfeld, 2017). Cognitive factors, such as confirmation bias and the propensity to engage in intuitive thinking, may contribute to explaining the prevalence and persistence of misinformation among the public in general (Pennycook and Rand, 2019). Therefore, when exposure to theoretical reasoning and empirical

[2] For studies about the impact of a variety of instructional methods on college students' academic performance, see List (2014) and Allgood, Walstad and Siegfried (2015). These studies however do not inquire about students' beliefs.

evidence by itself is not sufficient to dispel misconceptions; it is necessary to design appropriate communication strategies.

We decide to use the refutational approach based on its previous success in countering misconceptions in fields such as climate change (Nussbaum, Cordoba and Rehmat, 2017), psychology (Lilienfeld et al., 2009; Kowalski and Taylor, 2009; Lilienfeld, 2010) and mathematics (Lucariello, Tine and Ganley, 2014). In our case, we use this approach to dispel the belief about rent controls being an effective policy to increase access to housing. This belief can be qualified as a misconception because extensive empirical evidence contradicts it across time and countries (Glaeser and Luttmer (2003); Gyourko, Saiz and Summers (2008); Mora-Sanguinetti (2011); Andrews, Caldera Sanchez and Johansson (2011); Andersson and Söderberg (2012); Hilber and Vermeulen (2016); Gyourko and Molloy (2015) and Molloy (2020), and Diamond and McQuade (2019)).[3] As a result, researchers' consensus about the negative effects of the rent control policy is very strong.

Although the public may hold other misconceptions about economics, we focus here on the one on rent controls for two reasons. First, this is an issue of great social importance where researchers' and popular opinion exhibit a remarkable disparity. According to the IGM Economic Experts Panel, 95% of panel members in 2012 disagree with the idea that rent controls lead to an increase in the quantity of affordable housing.[4] In contrast, according to a poll conducted by the Institute of Governmental Studies (IGS) of UC Berkeley, 60% of the state's registered voters favor rent control, while 26% oppose them. Support for rent controls is also found in other countries. For instance, in the UK, a survey

[3] See also the website of the Stockholm Housing Agency, https://bostad.stockholm.se/english/.
[4] The IGM statement reads: "*Local ordinances that limit rent increases for some rental housing units, such as in New York and San Francisco, have had a positive impact over the past three decades on the amount and quality of broadly affordable rental housing in cities that have used them*". See http://www.igmchicago.org/surveys/rent-control.

published by Survation reports that 59% of polled people in December 2014 backed rent controls, and only 7% opposed them. In December 2018 in a YouGov/Mayor of London Survey, 68% of the sample supported rent controls while 16% opposed them.[5]

Second, Busom, Lopez-Mayan and Panadés (2017) document that this misconception is hard to dispel even among students taking a principles of economics course. This incorrect belief persists after receiving standard instruction on price controls based on economic theory lectures and problem-solving sessions. Notably, many students who stick to the misconception do well in mid-term or final tests that include a question on this topic.

2.2. Designing a refutational text on rent controls

The refutational approach consists in writing a text that "engages, challenges, and remediates common misconceptions" (Kowalski and Taylor, 2009; Tippett, 2010; Braasch, Goldman and Wiley, 2013; Kendeou, Braasch and Braten, 2016; Lassonde, Kendeou and O'Brien, 2016).

In writing the basic content of our RT we start with the neutral and standard expository approach that a textbook such as Krugman et al. (2015) provides, and which is very similar to the corresponding chapter in other introductory economics manuals. This expository approach is the standard way of communicating economic knowledge in

[5] The question in the IGS poll was: "*Some people believe rent control laws that give local governments the ability to set limits on how much rents can be increased are a way to help middle and lower income people remain in their communities. Others say rent control leads to fewer rental units being built and this makes the problem worse in the long run. What is your opinion? Do you favor or oppose rent control laws in your area?*" The question in the Survation survey was: "*Would you support or oppose proposals for the government in introduce a "rent control" system in the UK*?". See http://survation.com/public-back-introduction-of-rent-control-survation-for-generation-rent/. The question in YouGov was: "*Thinking about the rents private landlords charge to people renting their homes, do you think...a) The government should introduce rent controls, limiting the amount that landlords can charge people renting their property; b) The government should not introduce rent controls, and should leave landlords to set the amount they charge people renting their properties; c) Do not know*".

universities. To incorporate the refutational elements, we follow findings in psychology (Tippett, 2010; Lewandowsky, 2021). According to these, the RT must: (i) state, or activate, the misconception, (ii) affirm explicitly that it is incorrect, (iii) provide arguments and evidence as to why this is the case, stressing the negative consequences of the belief and explaining the scientific conception as simply and clearly as possible. Druckman (2015) points out two additional elements: (iv) capturing the attention of the audience by making clear that the issue is relevant to the individual and her values, and (v) facilitating interactions by asking individuals to explain their opinions to others.

Our RT, detailed in Appendix A.1 (English translation), incorporates the features listed above as follows. In blocks 5 and 8 of the text, we activate the misconception and state that it is incorrect (elements (i) and (ii) above). In blocks 14 to 16, we explain the negative, unintended consequences of a rent control policy as shown in empirical studies and congruent with economic theory (element (iii) above). In particular, we point out the negative effects that a rent control policy –queuing, black market– has had in Stockholm (Andersson and Söderberg, 2012). We choose Stockholm to illustrate that the negative consequences of rent controls arise even in a country especially known for its welfare-oriented policies (related to element (iv) above).

Our inclusion of element (iv) is also reflected in us providing information about rental prices in participants' metropolitan area (see blocks 11 to 13 in the text). The intention here is to emphasize that the issue is relevant to the reader. Fairness concerns are likely to motivate the belief that rent control is an easy solution to the housing problem. We therefore write the text placing the rent control discussion in the context of searching for policies that are effective in improving social welfare and fairness (see blocks 16 and 17). To allow interactions among participants (element (v)) the text is discussed in teams in one of the conditions in Study 1 and in Study 2.

In addition to the five elements above, we include in the text some alternative, research-based policies that would increase affordable housing without the negative effects of rent controls. Our intention is that participants learn that scientific knowledge makes it possible to find effective policies that are aligned with their values and legitimate fairness concerns (blocks 17 and 18 in the text). In the RT we use simple, non-technical language, and provide some cues to induce critical thinking about own beliefs. For instance, we include sentences like: "*how things are often more complex than they seem"; "thinking slowly [...]", "ask ourselves the following questions".* The RT is about 1200 words long.[6]

2.3. Research questions

We elicit participants' beliefs about rent controls through a set of questionnaires. We measure the impact of the RT on the misconception through the change in beliefs pre and post exposure to the RT compared to the change in beliefs in the benchmark (the NRT in Study 1 and 2, and an additional benchmark in Study 2). Therefore, a positive impact of the RT refers to a change in beliefs towards abandoning the misconception compared to the change in beliefs in the corresponding benchmark.

Our main research question pertains directly to the impact of the RT and is the following:

Research question 1 (RQ1): Does the RT have a positive impact on the misconception about the effect of rent controls?

Our second research question asks whether team discussion may reinforce the effect of the RT. It is inspired by work by Cooper and Kagel (2005), Charness and Sutter (2012), and Druckman (2015), who find that in many cases groups make better decisions.

[6] As an illustration, Nussbaum, Cordova and Rehmat (2017) use a 1009 word long RT on the greenhouse effect.

Research question 2 (RQ2): Does the RT with team discussion have a higher positive impact than without it?

An important issue is the duration of the potential effect of the RT. Previous work has studied the effects of refutation texts at different points in time (Broughton, Sinatra and Reynolds, 2010; Kowalski and Taylor, 2017; Lassonde, Kendeou and O'Brien, 2016; Nussbaum, Cordova and Rehmat, 2017). This literature suggests that the effect of the RT decays over time. Our third research question is thus the following:

Research question 3 (RQ3): Is the delayed impact of the RT smaller than the immediate impact?

Previous work by Sunstein et al. (2017) finds that the effect of providing information about climate change on reconsidering beliefs depends on subjects' initial beliefs. This inspires our fourth research question, which is the following:

Research question 4 (RQ4): Does the impact of the RT depend on initial beliefs?

To better understand some of the mechanisms driving the potential change in beliefs we investigate whether confirmation bias and propensity to analytical thinking may be correlated with this change. Confirmation bias is the tendency to search for, interpret, favor, and recall information that confirms or supports one's initial beliefs or values. The Wason selection task (WT hereafter) proposed in Wason (1960, 1977) is a measure of confirmation bias that has been used in experiments about how people process or seek information individually (Charness, Oprea and Yuksel, 2021) or make decisions in teams (Charness and Sutter, 2012).

To measure the dominance of reflective versus intuitive thinking processes we use a nine-item Cognitive Reflection Test (CRT hereafter). The initial test proposed by Frederick (2005) contained three items, but it has been expanded after some revisions (Toplak, West and Stanovich, 2014; Thomson and Oppenheimer, 2016). The CRT has

been used in economics to study decision-making, strategic behavior and social preferences (see, for instance, Brañas-Garza, Kujal and Lenkei, 2015). We posit the following research questions with respect to the WT and the CRT:

Research question 5 (RQ5): Does the RT have a higher positive impact the higher participants' score on the WT (low confirmation bias)?

Research question 6 (RQ6): Does the RT have a higher positive impact the higher participants' score on the CRT (more reflective)?

In Study 1 we address all six research questions *RQ1* to *RQ6*. In Study 2 we address *RQ1* and *RQ4*. In both studies we use the same RT, so we can compare its effects with respect to *RQ1* and *RQ4* with different types of participants in different environments.

3. Study 1: The laboratory experiment

3.1. Method

Participants. The experiment was conducted at the laboratory LINEEX of Universitat de Valencia (Spain) on October 16 and 17, and November 8 and 9, 2018.[7] The experiment complied with ethical regulations from the Universitat de Valencia. We recruited 180 participants from a pool of 40,000 students from Universitat de Valencia and Universitat Politecnica de Valencia in Fall 2018. Participants are chosen from majors that do not include any courses in economics in order to avoid a potential confounding factor: having received or receiving economic instruction at the time of the experiment. If included and beliefs changed, we would not be able to disentangle the effect of uncontrolled instruction from the effect of the RT. In addition, we consider that it is of

[7] LINEEX is a laboratory specialized in Experimental Social and Behavioural Economics at Universitat de Valencia, Spain. It implements projects developed by international research groups, European institutions and private firms.

interest to explore the impact of the RT on people who have not received economic instruction, as is the case of the general population.

Sample size was based on a statistical power analysis, and constrained by our budget. Since no previous paper has analyzed the refutational approach to dispel misconceptions about economics, we could not rely on previous estimates to compute the power. Our best guess was to build on the available estimated RT effect from Study 2 (0.64 in column (4), Table 9), which was ongoing when Study 1 was conducted. We set the total initial sample size for the laboratory equal to 180 individuals, 60 per each of the three conditions (control, individual and team), giving a power of around 75% (computed using the simulation method for STATA proposed by Campos-Mercade, 2018). While perhaps a little low, we faced a trade-off between power and the cost of Study 1. Payments to participants were set relatively high in order to minimize attrition between the two sessions each participant is asked to attend, as shown in Figure 1 below.

Materials. We design a questionnaire to find out participants' belief about the effectiveness of rent controls. The statement reads as follows: *"Establishing rent controls, such that rents do not exceed a certain amount of money, would increase the number of people who have access to housing facilities."* Participants are asked to indicate their agreement with this statement on a five-point scale: totally disagree; disagree; do not know; agree; totally agree.

This statement is included in the three opinion questionnaires that participants have to fill in at the three time points of Study 1 (see Figure 1 below). The statement on rent controls is introduced jointly with statements on other economic issues and attitudes in order to blur the focus on rent controls and to minimize potential experimenter demand

effects. Table 1 shows all the statements included in the three questionnaires.[8] All questionnaires are the same and are administered in the same order across control, individual and team conditions. They vary somewhat across the three time points in order to avoid memorization of answers. As shown in Table 1, the variation introduced in the questionnaires across time is rather small since nine out of thirteen statements are common across the three questionnaires. We also collect, for each participant, at the time points shown in Figure 1, socio-demographic information and answers to the WT and CRT (WT and CRT are available in Appendix A.5).

We use the RT described in Section 2.2 in the individual and team conditions, and design the NRT for participants in the control condition (see Appendix A.2, English translation). To write the NRT we take as reference point the RT, eliminate the refutational elements and elaborate an expository text that essentially reflects the content of standard introductory textbooks, such as Krugman et al. (2015). This NRT closely captures the expository communication of economic knowledge in universities. Introductory textbooks explain the negative effects of price controls (price ceilings and minimum prices) on demand and supply in general. They provide the reasoning that can be applied to any good or service in competitive markets; it is a general theory. The cases of gasoline, rents and other goods are presented as illustrations. Finally, both the NRT and the RT have a short introduction to the concepts of supply and demand (see Appendix A.3) to clarify these basic economic concepts.

Comprehension checks. To make sure that participants pay attention to their respective text (NRT or RT), we include some questions at the end of each text (see Appendix A.1 and A.2 for questions in the RT and in the NRT, respectively).

[8] For all statements, participants are asked to indicate their agreement on the 5-point scale, except for *Innate ability vs Effort* where the responses are: only effort, more on effort than on innate ability; equally on both; less on effort than on innate ability; only innate ability. Table 1 shows the English translation of the statements from the original questionnaires in Spanish.

Table 1. Statements in Opinion Questionnaires

	First quest.	Second quest.	Third quest.
Rent control[a]			
"Establishing rent controls, such that rents did not exceed a certain amount of money, would increase the number of people who have access to housing facilities"	*	*	*
Minimum wage[a]			
"Raising the minimum wage from 650€ to 800€ per month would increase employment"	*	*	*
Mistrust Statistics			
"Economic statistics do not reflect, in general, the true economic situation"	*	*	*
Fare evasion			
"Skipping public transportation fares may be justified in some cases"	*	*	*
Tax evasion			
"Prosecution of tax evasion should be increased, since taxes finance health, education, pensions and other social expenditures"	*	*	*
Buying home country[a]			
"If the home government bought only home products, employment would increase"	*	*	*
Innate ability vs Effort			
"To understand and master a subject, how much depends on effort and how much on innate ability?"		*	
Value of personal experiences			
"I believe my personal experiences allow me to understand many economic issues of our society"	*		*
Methods in Social Sciences			
"Social sciences are not based on the same scientific method as natural sciences"	*		*
Concern about use of math			
"I am worried about the weight math may have in my degree"		*	
Outrage with respect to housing			
"I feel outraged at the high price of rental housing in metropolitan areas"	*	*	*
Differences in income[b]			
"Nowadays in Spain the differences in people's incomes are too great"	*	*	*
Equal opportunities[b]			
"Nowadays in Spain I have equal opportunities for getting ahead in life, like everyone else"	*	*	*

Note: [a] These statements are adapted from the IGM Forum (https://www.igmchicago.org/) and Sapienza and Zingales (2013). Explicit references to the US in the statements were replaced by appropriate references in Spain. [b] These statements were taken from the Special Eurobarometer 471, "Fairness, inequality and intergenerational mobility", December 2017.

Procedure. We have three conditions with 60 different randomly-allocated participants in each; we thus use a between-subjects design. For each condition we conduct two sessions. Figure 1 summarizes the timeline of the experiment and the sequence of tasks.

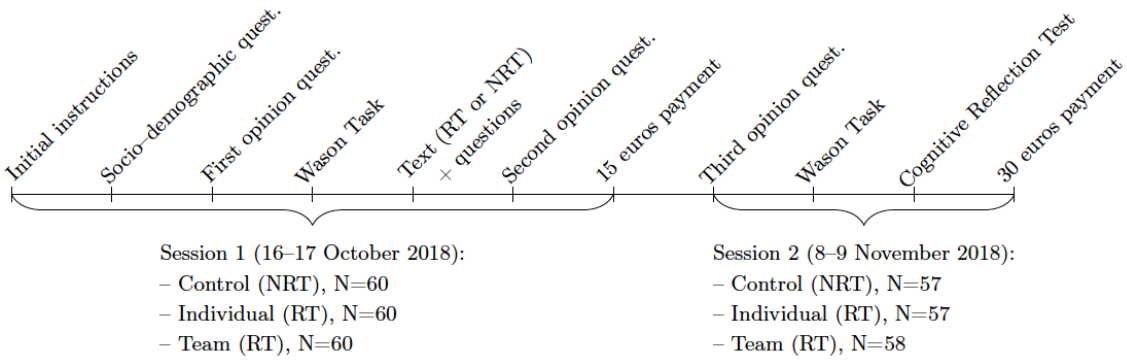

Figure 1: Procedure description of Study 1. Note: RT: Refutational text; NRT: Non-refutational text.

As shown in Figure 1, the final sample of participants attending both sessions is 172 because of eight no-shows to the second session. Attrition is practically equally distributed across conditions (see Table B.1 in Appendix B).

In the control condition, participants read the NRT and answer the questions individually. In the individual condition, participants read the RT and answer the follow-up questions individually. In the team condition, participants read the RT in randomly formed three-member teams. Team members move to the desk of one of them to discuss and answer the questions. One team member is asked to type the team's answers and afterwards they all go back to their desks. Note that only these questions are discussed and answered by the team; the opinion questionnaires, the WT, the CRT and personal information questions are always answered individually.

In the first session participants sign consent forms, are thanked for their participation and are given instructions (see Figure 1). Participants are informed that the experiment consists of two sessions, the one they are attending at the moment and a second one that will take place three weeks later. They are told that after the first session they will be paid 15 euros (5 euros show-up fee and 10 euros for completing all the tasks) and after the

second session they will be paid another 30 euros for completing the tasks. Participants are also told that the second session will be shorter than the first.

Participants are informed that they will be asked to give their sincere opinions about a number of social and economic issues and without the answers having any influence on payments. Next, they are told that they have to read a text and answer a number of related questions. Lastly, they will have to answer again a number of questions on economic and social issues.

After listening to these instructions, a set of demographic questions appear on participants' screens. The first opinion questionnaire and the WT follow. The statements and the WT appear in a particular order, but participants can move back and forth between them. Then, each participant is given a text (the RT or the NRT) on paper, is asked to read it and answer the questions individually or with her team depending on the condition. Appendix A.4 shows the instructions provided to participants just before they are given their respective text. After reading the text and answering the questions included, participants individually fill out a second opinion questionnaire. Then, participants are paid 15 euros and are told the dates for the second session.

In the second session participants are first thanked for coming back and then are asked to answer the third opinion questionnaire, followed by the WT and the CRT. Then participants are paid 30 euros and leave.[9]

Analysis. We measure the change in beliefs about rent controls at two points in time. First, the change at the end of the first session (*immediate post-intervention*), defined as the difference between the participant's response to the statement in the second opinion

[9] The first session was planned to last 120 minutes and the second session about 50 minutes. The first session effectively took on average 114 minutes (maximum time is 126 minutes), and the second session took on average 50 minutes (maximum is 64 minutes). To avoid excessive duration of the first session, we only include the CRT in the second session.

questionnaire and her response in the first opinion questionnaire. Second, the change at the end of the second session (*delayed post-intervention*), defined as the difference between the participant's response to the statement in the third opinion questionnaire and her response in the first opinion questionnaire. The original responses are transformed into numerical values as follows: 5 (fully disagree), 4 (disagree), 3 (do not know), 2 (agree), and 1 (fully agree).

For each of the two points in time, we estimate the following linear regression:

$$y_i = \alpha + \beta D_i + \varepsilon_i \quad (1)$$

where y_i is the corresponding change in beliefs of participant i, which takes values between −4 (a change from fully disagree to fully agree) and 4 (a change from fully agree to fully disagree). A positive value thus obtains when the response varies from agreement towards disagreement, that is, towards abandoning the misconception. D_i is a dummy variable equal to one if a participant is in the RT condition of interest, and zero otherwise. β measures the impact of D_i on y_i. That is, it measures the average change in beliefs of participants in the RT condition of interest relative to the average change in beliefs of participants in the comparison condition.

3.2 Results

***Description of outcomes*.** Table 2 shows the distribution of participants' beliefs about rent controls from the three opinion questionnaires. The last two columns add up the two "agree", and the two "disagree" categories.

The prevalence of the misconception is high, ranging from 75 to 84 percent of participants (Panel A). These figures are similar to those of the opinion polls cited in Section 2.1. Immediately after the intervention (Panel B), the percentage of participants disagreeing increases substantially in all three conditions, by 16 to 22 percent points (pp),

indicating a fall in the misconception. Three weeks after the intervention, the percentage disagreeing is still notably high (Panel C) in all three conditions, but the percentage of those who agree is higher in the control condition.

Table 2. Beliefs about rent controls (% participants)

	A. Initial Beliefs (First questionnaire)						
	Totally disagree	Disagree	Agree	Totally agree	Do not know	Sum disagree	Sum agree
Control condition	0.0	10.5	50.9	33.3	5.3	10.5	84.2
Individual condition	1.8	10.5	52.6	22.8	12.3	12.3	75.4
Team condition	0.0	8.6	44.8	37.9	8.6	8.6	82.8
	B. Immediate post-intervention (Second questionnaire)						
	Totally disagree	Disagree	Agree	Totally agree	Do not know	Sum disagree	Sum agree
Control condition	7.0	24.6	45.6	17.5	5.3	31.6	63.2
Individual condition	5.3	22.8	42.1	12.3	17.5	28.1	54.4
Team condition	5.2	25.9	48.3	12.1	8.6	31.0	60.4
	C. Delayed post-intervention (Third questionnaire)						
	Totally disagree	Disagree	Agree	Totally agree	Do not know	Sum disagree	Sum agree
Control condition	3.5	21.1	47.4	21.1	7.0	24.6	68.4
Individual condition	1.8	22.8	49.1	7.0	19.3	24.6	56.1
Team condition	0.0	25.9	48.3	12.1	13.8	25.9	60.4

Note: Sample sizes (participants who attended both sessions) are, respectively 57, 57 and 58 in control, individual and team conditions.

Regression results. Table 3 reports the results of estimating equation (1) by OLS with robust standard errors. Columns (1) to (4) refer to the impact of the RT in the individual condition relative to the NRT in the control condition. They partially answer *RQ1* and *RQ3*. The estimated immediate effect is negative (column (1)), although with a high standard error. Adding a set of demographic control variables changes the absolute magnitude of the effect, but it remains not significant. The delayed impact (column (4)) turns out to be positive, but again with a large standard error. These results suggest that the RT, when participants read it individually, does not do significantly better than the NRT, whether immediately or with a delay.

Columns (5) to (8) show the effect of the RT in the team condition relative to the impact of the RT in the individual condition. These regressions assess whether discussing the RT in teams has an additional effect on the change in beliefs, answering thus *RQ2.* The estimated effects are positive but small and not significant, which suggests that discussing the RT in teams does not add to the refutational content.[10]

Columns (9) to (12) in Table 3 show the impact of the RT in the team condition relative to the NRT. These results complete the evidence about *RQ1* and *RQ3*, and they are also interesting to assess consistency of results from columns (1) to (8). Since the RT in the individual condition does not significantly add to the NRT and then, the RT in the team condition does not significantly add to the RT in the individual condition, we would expect that the RT in the team condition does not significantly add to the NRT either, which is what we observe. In addition, results in columns (9) to (12) will allow a comparison with Study 2 below, where we only have a team condition.

To sum up, in Study 1 we do not find evidence for a positive answer to *RQ1.* The RT does not significantly reduce the misconception when compared to a text without refutational content, the NRT. Regarding *RQ2*, we do not find that the impact of the RT with team discussion is higher than without it. Finally, we do not obtain evidence that the delayed impact is smaller than the immediate effect (*RQ3*).

[10] We run two additional regressions, one for the immediate change in beliefs and another for the delayed change, estimating the effect of the RT jointly for the three conditions, including dummy variables for RT individual and team. Results do not change relative to the separate estimations shown in Table 3. We test the equality of the RT estimates for individual and team conditions in both regressions and cannot reject the null hypotheses. Results are available upon request.

Table 3. Estimation Results

	Individual (RT) vs Control (NRT)				Team (RT) vs Individual (RT)				Team (RT) vs Control (NRT)			
	Immediate		Delayed		Immediate		Delayed		Immediate		Delayed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RT	-0.14	-0.32	0.02	0.11	0.25	0.05	0.18	0.05	0.11	0.09	0.20	0.29
	(0.26)	(0.31)	(0.24)	(0.28)	(0.25)	(0.28)	(0.22)	(0.23)	(0.24)	(0.24)	(0.24)	(0.23)
Age		0.12		0.17		0.27		0.36**		0.10		0.19
		(0.12)		(0.12)		(0.15)		(0.12)		(0.09)		(0.10)
Female		0.60*		0.18		-0.01		-0.24		0.56*		0.21
		(0.29)		(0.28)		(0.26)		(0.28)		(0.25)		(0.26)
CAT grade		0.09		-0.01		-0.00		0.02		0.08*		0.03
		(0.05)		(0.06)		(0.05)		(0.05)		(0.05)		(0.05)
Scholarship		0.37		0.16		-0.11		0.08		0.12		0.10
		(0.28)		(0.28)		(0.28)		(0.25)		(0.26)		(0.27)
Nonspanish		0.18		-0.72		-0.57		-0.37		-1.77**		-1.01
		(0.94)		(0.63)		(0.66)		(0.41)		(0.65)		(0.79)
Health		0.37		0.09		0.41		0.68*		0.59*		0.55
		(0.36)		(0.28)		(0.35)		(0.29)		(0.28)		(0.28)
Engineering		1.27**		0.80		0.21		0.58		0.92		1.15**
		(0.47)		(0.44)		(0.42)		(0.37)		(0.49)		(0.42)
Sciences		1.29*		0.83		1.25**		1.10**		1.27***		0.68*
		(0.56)		(0.49)		(0.42)		(0.39)		(0.36)		(0.29)
Humanities		0.54		0.40		0.15		0.66		0.97*		1.04
		(0.40)		(0.41)		(0.42)		(0.43)		(0.38)		(0.54)
Constant	0.65***	-3.46	0.46*	-3.25	0.51*	-4.93	0.47**	-6.96**	0.65***	-3.02	0.46*	-4.19*
	(0.17)	(2.46)	(0.18)	(2.25)	(0.20)	(2.95)	(0.16)	(2.45)	(0.17)	(1.95)	(0.18)	(1.95)
N	114	114	114	114	115	115	115	115	115	115	115	115
R^2	0.00	0.14	0.00	0.09	0.01	0.11	0.01	0.16	0.00	0.23	0.01	0.15

Note: Dependent variable is belief change. It takes values between -4 and 4; positive values indicate a change away from the misconception. RT: Refutation text. Robust standard errors are reported in parentheses. Significance levels: *p < 0.05, **p < 0.01, ***p < 0.001.

Who changes her mind? Results reported above measure the average effect of the RT on the change in beliefs. However, it is plausible that the estimated effect varies depending on the initial belief. As descriptive evidence, Table B.4 in Appendix B shows the transition matrices among the three categories: agree, do not know, disagree. Persistence of the misconception among participants who initially agree with it is high in all conditions. Panel A of Table B.4 also shows that among those participants who

initially disagree, an important percentage has the misconception immediately after the treatment (whether individual or team). In the second session (Panel B), though, while about two thirds of participants in the control condition have the misconception, the rate is much smaller in individual and team conditions, which are exposed to the RT.

Table 4 shows the results of estimating the causal effect of the RT conditional on initial beliefs. They answer *RQ4*. Columns refer to the initial belief, while rows refer to the specification for the immediate and the delayed effect. Each cell is the result of a separate estimation. Panel A and B show that both the estimated immediate and delayed effects of the RT do not significantly depend on the initial belief both for participants in the individual condition relative to the control, and for participants in the team condition relative to the individual one. Finally, Panel C presents the estimates of the effect of the RT in teams relative to the control. In this case the delayed effect of the RT is positive and significant for participants who initially disagree with the misconception. This suggests that the RT prevents that beliefs change wrongly towards the misconception. Overall, results from Table 4 do not show strong evidence that the effect of the RT varies according to initial beliefs (*RQ4*).

Table 4. RT effect conditional on initial belief

	(1) Agree	(2) Do not know	(3) Disagree
	A. Individual (RT) vs Control (NRT)		
Immediate RT effect	0.04	-0.37	-0.80
	(0.18)	(0.38)	(0.64)
R^2	0.07	0.42	0.22
Delayed RT effect	-0.01	0.15	0.37
	(0.18)	(0.41)	(0.61)
R^2	0.05	0.09	0.52
N	91	10	13
	B. Team (RT) vs Individual (RT)		
Immediate RT effect	-0.02	0.60	-0.23
	(0.18)	(0.81)	(0.70)
R^2	0.01	0.18	0.34
Delayed RT effect	0.01	0.23	-0.22
	(0.16)	(0.77)	(0.56)
R^2	0.03	0.11	0.11
N	91	12	12
	C. Team (RT) vs Control (NRT)		
Immediate RT effect	0.01	0.28	-0.42
	(0.19)	(0.82)	(1.02)
R^2	0.04	0.33	0.09
Delayed RT effect	-0.05	0.23	1.85^{***}
	(0.17)	(0.79)	(0.28)
R^2	0.01	0.21	0.50
N	96	8	11

Note: Dependent variable is belief change, taking values between -2 and 2. We aggregate totally disagree and disagree into one category, totally agree and agree into another. Positive values indicate a change away from the misconception. RT: Refutation text. NRT: Non-refutational text. Regressions include only CAT grade and female as explanatory variables because of the small sample sizes. Robust standard errors are in parentheses. Significance levels: *p < 0.05, **p < 0.01, ***p < 0.001.

Change in beliefs and cognitive tests. We explore whether confirmation bias (*RQ5*) and reflective thinking (*RQ6*) play a role in the change of beliefs. Starting with confirmation bias, we compute WT scores as the percentage of correct responses to the three questions collected in the first session. A higher score means lower confirmation bias. Most participants (about 80%) did not provide a correct answer to any of the questions. The mean score is 9% for the control and individual conditions, and 6% for the team condition.[11] Panels A and B in Table 5 show the partial correlation between the WT

[11] This is in line with the outcome reported in Wason (1977).

score and the immediate and delayed change in beliefs, respectively. Each column represents a separate regression, without and with control variables, for each condition. The WT is not significantly correlated with either the immediate or the delayed change in beliefs.

Next, we analyze the importance of reflective versus intuitive thinking processes as measured by the CRT. Panel C in Table 5 shows the correlation between the delayed change in beliefs and the CRT score for each condition.[12] We compute the score as the percentage of correct answers over the nine questions. The mean scores are 40%, 43% and 45%, respectively, for the control, individual and team conditions. A higher propensity to reflection is significantly related to the change of beliefs, in the direction of decreasing the misconception, only within the control condition (column (2) in Panel C).

The pairwise correlation between CRT scores and WT scores is close to zero (-0.02) and not significant, suggesting that both measures capture different cognitive traits. Panel D in Table 5 shows that when including both measures jointly none of them is significant.

[12] Recall that the CRT was only used in the second session.

Table 5. Change in beliefs within condition and cognitive performance

	Control condition		Individual condition		Team condition	
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Immediate change					
WT	1.32	1.01	-1.34	-1.44	-0.08	-0.34
	(0.87)	(0.60)	(1.08)	(1.21)	(1.09)	(0.87)
R^2	0.04	0.56	0.03	0.17	0.00	0.26
	B. Delayed change					
WT	-0.66	-1.21	-1.07	-1.05	-0.98	-0.70
	(1.14)	(1.15)	(0.90)	(1.01)	(0.60)	(0.53)
R^2	0.01	0.26	0.03	0.19	0.02	0.24
	C. Delayed change					
CRT	1.56	1.79*	0.23	-0.80	1.13	1.17
	(0.88)	(0.89)	(0.91)	(1.02)	(0.63)	(0.85)
R^2	0.05	0.27	0.00	0.18	0.05	0.27
	D. Delayed change					
WT	-0.90	-1.13	-1.07	-1.41	-1.03	-0.56
	(1.02)	(1.11)	(0.90)	(1.03)	(0.55)	(0.61)
CRT	1.68	1.71	0.02	-1.28	1.16	1.12
	(0.90)	(0.96)	(0.91)	(1.07)	(0.62)	(0.86)
Explanatory variables	No	Yes	No	Yes	No	Yes
N	57	57	57	57	58	58
R^2	0.07	0.30	0.03	0.22	0.07	0.27

Note: Each column in each panel represents a separate regression. The dependent variable is belief change and takes values between -4 and 4; positive values indicate a change away from the misconception. Results with control variables include the same set of explanatory variables as in Table 3. CRT: Cognitive Reflection Test. WT: Wason Task. Robust standard errors are in parentheses. Significance levels: *p < 0.05, **p < 0.01, ***p < 0.001.

To explore whether cognitive factors interact with the RT, we estimate equation (1) for the delayed change in beliefs, adding the interaction terms of the WT and the CRT with D_i:

$$y_i = \alpha + \beta_1 D_i + \beta_2 CRT_i + \beta_3 WT_i + \beta_4 D_i CRT_i + \beta_5 D_i WT_i + \varepsilon_i \qquad (2)$$

Table 6 reports the results. Columns (1), (3) and (5) add the WT and the CRT scores to the specification used in Table 3 with all explanatory variables. We do not find evidence that cognitive factors are directly related to the change in beliefs with the exception of the CRT in column (5). The positive estimate indicates that a higher CRT

score is related to a change in beliefs away from the misconception for the participants who read the RT in teams relative to the participants who individually read the NRT. Columns (2), (4) and (6) add the interaction terms to the previous specification. The coefficient of the RT is significant for participants in the individual condition relative to the control condition (column (2)). The corresponding estimated marginal effect, computed at the mean of the CRT and WT, gives a value of 0.14 (p-value = 0.62), in line with the estimated effect in column (1).[13] Analogously, the estimated marginal effect in column (4) is 0.03 (p-value = 0.91), close to the estimated effect in column (3), and 0.20 (p-value = 0.36) in column (6), close to the coefficient in column (5). Interactions of cognitive factors with the RT, however, are not significant, suggesting that the treatment effect does not vary across treated participants with different cognitive scores. The significant result in column (6) shows that a higher CRT is associated with a larger move away from the misconception, and is consistent with that of column (5). Interaction terms are not significant.

Table 6. Change in beliefs, RT and cognitive scores (delayed effect)

	Individual (RT) vs Control (NRT)		Team (RT) vs Individual (RT)		Team (RT) vs Control (NRT)	
	(1)	(2)	(3)	(4)	(5)	(6)
RT	0.08	1.26*	0.03	-0.43	0.20	0.63
	(0.28)	(0.60)	(0.23)	(0.54)	(0.22)	(0.50)
CRT	0.49	1.73	0.26	-0.33	1.34*	1.89*
	(0.75)	(0.91)	(0.60)	(0.99)	(0.63)	(0.87)
WT	-0.93	-1.08	-0.96	-1.15	-0.85	-0.80
	(0.83)	(1.10)	(0.55)	(0.90)	(0.79)	(1.15)
RT*CRT		-2.63		1.00		-0.97
		(1.39)		(1.21)		(1.10)
RT*WT		-0.34		0.21		-0.20
		(1.48)		(1.08)		(1.22)
Explanatory variables	Yes	Yes	Yes	Yes	Yes	Yes
N	114	114	115	115	115	115
R^2	0.12	0.15	0.18	0.18	0.20	0.20

Note: Dependent variable is belief change and takes values between -4 and 4; positive values indicate a change away from the misconception. RT: Refutation text. CRT: Cognitive Reflection Test. WT: Wason Task. Regressions include the same set of explanatory variables as in Table 3. Robust standard errors in parentheses. Significance levels: *p < 0.05, **p < 0.01, ***p < 0.001.

[13] The marginal effect with interactions computed at the means is: $y_{D=1} - y_{D=0} = \beta_1 + \beta_4 CRT_{mean} + \beta_5 WT_{mean}$. Standard errors of the marginal effects are obtained using the Delta method.

The bottom line is that our results do not allow us to answer positively to *RQ5* and *RQ6*. We do not find evidence that the positive effect of the RT is higher the higher the WT and CRT scores. In addition, confirmation bias is not significantly related to the change in beliefs. More reflective individuals move away from the misconception, but we only observe this among participants in the team condition when compared with participants in the control condition.

Following suggestions from an anonymous referee, we conduct an exploratory analysis of the correlation of the change in beliefs with outrage feelings (statement *Outrage with respect to housing* in Table 1). As Table C.2 in Appendix C shows this factor is only significantly correlated with the change in beliefs when the RT is discussed in teams relative to reading it individually. In this case, stronger outrage feelings (higher agreement with the statement) are correlated with changing beliefs away from the misconception. We also investigate whether the team composition of initial beliefs affects participants' final beliefs. We consider several possible measures of team composition, as we explain in Appendix C. In general, these measures are not significantly correlated with final beliefs, as shown in Table C.4 in Appendix C. Only the dummy variable measuring initial unanimity in sharing the misconception is negatively and significantly correlated with final beliefs a few weeks after the intervention. This indicates that the individual final belief is less likely to move away from the misconception when there is within-team unanimity relative to when there are discrepancies between team members.

4. Study 2: The field (quasi) experiment

4.1. Method

Participants. Study 2 is conducted in the setting of a college course at the Universitat Autonoma de Barcelona (UAB), Spain. Participants are first-year college students

enrolled in a compulsory principles of economics course, majoring in Business, Business and Law, Economics, and Law and Labor Relations.[14] The study was approved by the UAB in a call for research projects on innovation in teaching presented by three of the coauthors.[15] Two of them, in turn, are the coordinators of this course in the School of Economics and Business and in the School of Law at UAB. Therefore, we are able to introduce the intervention as part of the regular development of the course, and to avoid confounding factors from uncontrolled instruction.

It involves three cohorts of students, corresponding to three different enrolment years with a two-year gap between cohorts. Students enrolled in 2015 are the control cohort in the sense that in the practice session they solve textbook-type problems on competitive markets and price controls; those enrolled in 2017 work with the RT in the practice session; and those enrolled in 2019 work with the NRT. Thus, year of enrollment is the variable defining the assignment to the intervention. In this sense this is not a pure –contemporaneous– experimental design and this is why we refer to it as a quasi-experiment. Note, though, that students are blind to assignment.

We acknowledge that a between-cohort assignment may have some drawbacks. The two-year gap between cohorts may be a concern since it does not allow holding strictly everything constant between 2015, 2017 and 2019. However, in this five-year span the content of undergraduate programs did not suffer meaningful changes, nor did college admission rules. From an aggregate perspective all these years correspond to an economic expansion period. Monthly rents were increasing similarly in Barcelona (see figure B.1 in Appendix B). In addition, the issue of housing affordability, and rental prices in particular, had been a topic of public debate since 2011, as reflected in the media and

[14] In Spain, first-year college students enroll directly in specific majors; they can switch fields subsequently. Legal studies can be a student's major at the undergraduate level.
[15] Scientific and Evaluation Committees of the UAB for innovation projects in teaching, July 2017.

political parties' electoral programs. We are confident that the political and social environment was quite stable over this period. We discuss these potential limitations of Study 2 in more detail in Section 5.

Initial sample sizes consist of 508, 399, 344 participants, respectively, in the 2015, 2017 and 2019 cohorts. As our outcome of interest is the change in beliefs over the semester, we exclude from the sample participants who did not answer the end of semester questionnaire. This leaves us with a sample size of 340, 272 and 316 participants in the 2015, 2017 and 2019 cohorts (67%, 68% and 91% of the initial sample, respectively). Attrition obeys mostly to non-attendance at the end of the semester, as assignment deadlines lead some students to skip lectures.[16]

Materials. Participants in each cohort are asked to answer two questionnaires: one at the beginning of the semester (initial) and another at the end of the semester (final). All questionnaires include the same statement as Study 1 about the effectiveness of rent controls, as well as other statements on social and economic issues. The latter, as in Study 1, are intended to blur the focus on rent controls and to minimize potential experimenter demand effects. Final and initial questionnaires contain some different statements in order to avoid memorization of answers. Table 7 shows the statements included in the initial and final opinion questionnaires in each cohort.[17] The questionnaires in Study 1 are based on the statements included in questionnaires in the 2017 cohort adding the statements *Differences in income* and *Equal opportunities* and eliminating the statements related to the development of the course.

[16] In the 2019 cohort attrition was lower as we offered a small bonus for filling both the beginning and end of semester questionnaires (0.2 points in their final mark, on a 0-10 points scale).

[17] For all statements, participants are asked to indicate their agreement on the 5-point scale, except for *Innate ability vs Effort* where the responses are: only effort, more on effort than on innate ability; equally on both; less on effort than on innate ability; only innate ability. Table 1 shows the English translation of the statements from the original questionnaires in Spanish.

Table 7. Statements in Opinion Questionnaires

	Cohorts		
	2015	2017 and 2019	
	Initial and Final	Initial	Final
Rent control[a]			
"Establishing rent controls, such that rents did not exceed a certain amount of money, would increase the number of people who have access to housing facilities"	*	*	*
Minimum wage[a]			
"Raising the minimum wage from 650€ to 800€ per month would increase employment"	*	*	*
Mistrust Statistics			
"Economic statistics do not reflect, in general, the true economic situation"		*	*
Fare evasion			
"Skipping public transportation fares may be justified in some cases"		*	*
Tax evasion			
"Prosecution of tax evasion should be increased, since taxes finance health, education, pensions and other social expenditures"		*	*
Buying home country[a]			
"If the home government bought only home products, employment would increase"	*	*	*
Innate ability vs Effort			
"To understand and master a subject, how much depends on effort and how much on innate ability?"		*	**
Value of personal experiences			
"I believe my personal experiences allow me to understand many economic issues of our society"		*	**
Methods in Social Sciences			
"Social sciences are not based on the same scientific method as natural sciences"		*	**
Concern about use of math			
"I am worried about the weight math may have in my degree"		*	**
Outrage with respect to housing			
"I feel outraged at the high price of rental housing in metropolitan areas"		*	**
Differences in income[b]			
"Nowadays in Spain the differences in people's incomes are too great"		*	**
Equal opportunities[b]			
"Nowadays in Spain I have equal opportunities for getting ahead in life, like everyone else"		*	**
Lectures touch on real issues			
"I think that lectures touch on real issues about our society"			*
Learned unexpected content			
"This course (principles of economics) has taught me unexpected things"			*
Difficult course due to math			
"It is hard for me to understand this subject (economics) because of the use of math"			*

Note: [a] These statements are adapted from the IGM Forum (https://www.igmchicago.org/) and Sapienza and Zingales (2013). Explicit references to the US in the statements were replaced by appropriate references in Spain. [b] These statements were taken from the Special Eurobarometer 471, "Fairness, inequality and intergenerational mobility", December 2017.

We use the RT described in Section 2.2 for the 2017 cohort and the NRT designed in Study 1 for the 2019 cohort. The only difference between the RT in Study 1 and the RT in Study 2 is that the latter provides data of rents in Barcelona, instead of Valencia, since participants from Study 2 live in that area. In addition, in Study 2 we do not include the short introduction that precedes the RT and the NRT in Study 1 (shown in Appendix A.3) because students have already had a lecture on supply and demand before the practice session.

Following the reading of RT and NRT, participants in the 2017 and in the 2019 cohort have to answer, respectively, the questions shown in Appendix A.1 and A.2. In the case of the NRT we add a second standard problem on price controls with respect to Study 1 because in practice sessions in college, students are expected to solve more than one. Finally, in Study 2 we do not include the WT and the CRT to avoid excessive questionnaire length, and taking too much class time.[18] We collect participants' socio-demographic information from administrative records.

Procedure. Figure 2 summarizes the timeline of Study 2 for each cohort, as well as the respective final sample sizes. In all cohorts, students are informed that the questionnaires are part of an opinion study for the School of Economics and Business, and that they will not be used for grading, so as not to make them feel pressured to respond in a way that does not reflect their sincere opinion.

Course content is essentially the same for all cohorts. Krugman et al. (2015) is the recommended textbook; class sessions include lectures and practice sessions. Course evaluation is based on in-class graded assignments, a mid-term and a final exam. About

[18] The beginning of semester questionnaire included some socio-demographic questions. When we were granted access to administrative records, we disregarded participants' answers to avoid potential measurement errors, especially in CAT grade.

the fourth week in the semester the topic of price controls is introduced within the context of supply and demand analysis. The lecture explains with graphical analysis their effects on the market and provides examples. This lecture follows essentially the content of the standard textbook by Krugman et al. (2015). Following the lecture, in the standard-teaching 2015 cohort, students are asked to solve textbook-type problems on market equilibrium and price controls that they can discuss among each other. In this cohort, students solve a graded quiz that included questions about competitive markets and price controls.

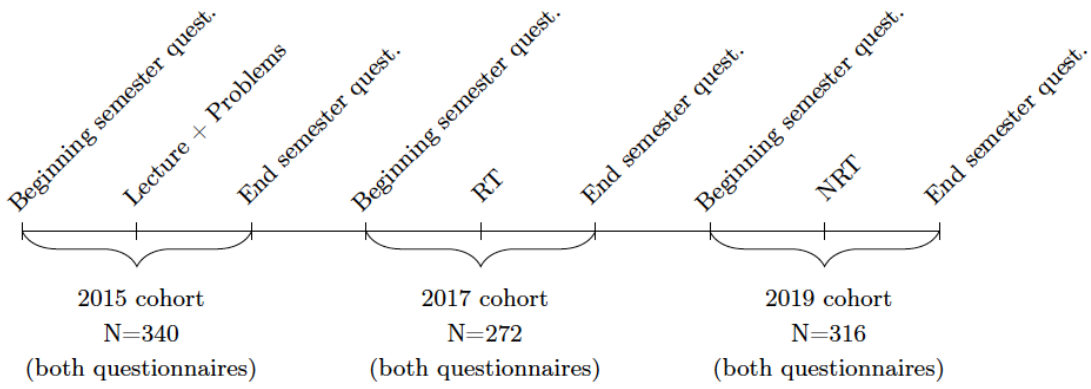

Figure 2: Procedure description of Study 2. Note: RT: Refutational text; NRT: Non-refutational text.

The main difference between the 2017 and 2019 cohorts with respect to the 2015 cohort is, by design, the content of the practice session. The practice session for the 2017 and 2019 cohorts consists, respectively, in reading and discussing in teams the RT and the NRT.[19] In parallel with Study 1, students in the 2017 and 2019 cohort are randomly allocated to a team of three or four members at the beginning of the practice session. Each student is given a copy of the corresponding text, asked to read it and then discuss and

[19] In Study 2 it was not possible for us to have both conditions with and without team discussion as in Study 1, due to university regulations that require all students in a cohort to be taught in the same way.

answer, on paper, the corresponding questions using the template provided. Each team has to hand in just one response template. The session lasts 90 minutes in all cohorts. This exercise is graded with a weight of around 10% of total grade. Since the practice session cannot exceed 90 minutes, participants do not have time to answer the second questionnaire as in Study 1 at the end of the practice session and thus we cannot measure the immediate effect of the intervention.

Analysis. We measure the change in beliefs about rent controls at one point in time: the change at the end of the semester, defined as the difference between the participant's response in the final and the initial questionnaire (*delayed post-intervention*). Original responses are transformed into numerical values as in Study 1 and we estimate the same specification (1) as in Study 1. In Study 2, we can compare the effect of the RT (2017 cohort) against two different benchmarks: the natural benchmark of standard teaching in the 2015 cohort and the NRT practice of the 2019 cohort. For completeness, we can also compare the NRT to standard teaching. Therefore, equation (1) is used to estimate three sets of regressions: (i) RT (2017 cohort) vs standard teaching (2015 cohort); (ii) RT (2017 cohort) vs NRT (2019 cohort), and (iii) NRT (2019 cohort) vs standard teaching (2015 cohort).

First, when comparing the RT (2017 cohort) to standard teaching (2015 cohort), consistent estimation of the parameter β (the effect of the RT relative to standard teaching) in equation (1) requires considering that 29 out of 272 participants in the 2017 cohort (11%) did not attend the RT practice session, that is, they are non-compliers. We deal with this selection problem using an instrumental variable (IV) approach. Following Angrist and Pischke (2009), intention-to-treat is a valid instrument, since assignment to the RT intervention and to standard teaching is based on an exogenous variable, year of

enrollment (2017 and 2015, respectively). Participants are thus blind to the corresponding intervention. We define a variable Z_i, which equals one if a participant i belongs to the 2017 cohort and zero if she belongs to the 2015 cohort. D_i, which equals one if i attended the RT session and zero otherwise, is instrumented with Zi. Hence, we use both compliers and non-compliers to estimate β. With a binary instrument the IV estimator of β is the Wald estimator.

Some participants may be more responsive than others to the RT because of differences in unobserved cognitive abilities. In the presence of heterogeneous treatment effects, the Wald estimator identifies the LATE (local average treatment effect) if the monotonicity assumption holds (Imbens and Angrist, 1994; Angrist, Imbens and Rubin, 1996).[20] The LATE is the effect of the intervention on compliers. Under the monotonicity assumption, the LATE is equal to the average treatment-on-the-treated (ATT) effect as proved in Bloom (1984) and discussed in Angrist and Pischke (2009).

A potential source of bias when estimating β in this specification is the difference in participants' characteristics across 2017 and 2015 cohorts as a result of attrition. As explained in section 4.2, we take these differences into account by including in specification (1) a set of observed characteristics as control variables. We cannot rule out completely, however, that attrition may generate systematic differences in unobserved factors across cohorts; therefore, the estimated effect should be interpreted with caution, as discussed in Section 5.

In the second set of regressions, we compare the RT (2017 cohort) with the NRT (2019 cohort). When using these cohorts to estimate equation (1), the parameter β is the effect of the RT relative to the NRT. The estimation of β here faces two potential sources

[20] Monotonicity implies that there are no defiers, that is, people who receive treatment even if they have been assigned to the control group. In our case, the monotonicity assumption is satisfied since no one from one cohort is enrolled in any of the other two cohorts.

of bias. One is selection into the corresponding intervention (RT and NRT) since both cohorts have non-compliers. Non-compliers in the 2019 cohort are individuals who do not attend the practice session with the NRT (only 11 out of 316, 3.5%). Non-compliers may have different characteristics across cohorts, thus potentially biasing β. Another potential source of bias is differences in attrition rates in 2017 and 2019 that may lead to differences in the composition of cohorts. The first source of bias, however, will play a limited role since the differences in the composition of compliers across the 2017 and 2019 cohorts (column (13) in Table B.2 in Appendix B) are almost equal to the differences in the composition across the whole cohorts, including non-compliers (column (10) in Table B.2). The main source of potential bias is, hence, attrition. As explained in section 4.2, OLS estimation of specification (1) includes a vector of observed characteristics in order to minimize this bias. Now D_i is a dummy variable equal to one if the individual is in the 2017 cohort and attends the practice session with the RT, and zero otherwise (that is, the individual is in the 2019 cohort and attends the practice session with the NRT). Thus, we estimate the effect of the RT relative to the NRT using compliers.

In the third set of regressions, when comparing the NRT (2019 cohort) to standard teaching (2015 cohort), consistent estimation of the parameter β (the effect of the NRT relative to standard teaching) in equation (1) requires considering the 3.5% of non-compliers in the 2019 cohort. We do this by using the same IV method described above. We define a variable *Zi*, which equals one if a participant *i* belongs to the 2019 cohort and zero if she belongs to the 2015 cohort. *Di*, which equals one if *i* attended the NRT session and zero otherwise, is instrumented with *Zi*. Again, we use here the whole sample (both compliers and non-compliers) to estimate β.

4.2 Results

Description of outcomes. In Table 8 we describe participants' distributions of beliefs about rent controls at the beginning of the semester (initial beliefs) and at the end of the semester (delayed post-intervention beliefs). The initial prevalence of the misconception is high, ranging 69 to 78 percent. These figures are similar to those in Study 1 and to the opinion polls cited in Section 2.1.

Table 8. Beliefs about rent controls (% participants)

	A. Initial Beliefs (Beginning of semester)						
	Totally disagree	Disagree	Agree	Totally agree	Do not know	Sum disagree	Sum agree
2015 Cohort	3.2	16.2	56.2	12.9	11.5	19.4	69.1
2017 Compliers	1.7	9.9	57.2	20.6	10.7	11.6	77.8
2019 Compliers	1.9	7.2	42.9	34.4	13.44	9.2	77.4
	B. Delayed post-intervention (End of semester)						
	Totally disagree	Disagree	Agree	Totally agree	Do not know	Sum disagree	Sum agree
2015 Cohort	5	10.6	56.5	18.8	9.1	15.6	75.3
2017 Compliers	7.8	21.0	50.2	10.7	10.3	28.8	60.9
2019 Compliers	2.3	7.5	48.8	30.8	10.5	9.8	79.7

Note: Sample sizes (participants who answered both opinion questionnaires) are 340 in 2015 cohort, 243 in 2017 Compliers, and 305 in 2019 Compliers.

At the end of the semester (Panel B), the percentage of participants in the 2015 cohort holding the misconception increases by 6.2 pp, whereas the percentage disagreeing falls by 3.8 pp. In contrast, among compliers of the 2017 cohort (RT) the percentage agreeing drops by 16.9 pp while the percentage disagreeing increases by 17.3 pp.[21] Among the compliers of the 2019 cohort (NRT) the percentage disagreeing increases very slightly, less than the increase in the percentage agreeing. Hence among those exposed to the RT there is a change away from the misconception, while the change is weaker in the 2019 cohort, exposed to the NRT, and in the wrong direction in the 2015 cohort (exposed to standard teaching). [22]

[21] For the sake of brevity Table 8 does not report the distribution of responses of non-compliers but they are available upon request.
[22] In Appendix B we show the persuasion rates following Della Vigna and Gentzkow (2010).

In Table B.2 in Appendix B we show the distribution of characteristics across cohorts. The 2017 assigned-to-treatment and the 2015 cohorts, with comparable attrition rates, are very similar in terms of gender, share of non-Spanish students and retakers, and, importantly, in the College Admission Test (CAT) grade, which is a proxy for general academic ability at college entry (column (8) in Table B.2). There are some differences in the share of younger students, scholarship recipients and major composition. Columns (9) and (10) show that the 2019 cohort exhibits more differences with the 2015 and 2017 cohorts in several dimensions, due to the lower attrition rate. We take these differences into account by including in the regressions below the following observed characteristics as control variables: gender, a dummy of being 18 years old, retaker, CAT grade, receiving a scholarship, non-Spanish student, high school track, and major.[23] These variables account for the most important observed differences across cohorts. However, we cannot rule out completely that attrition generates differences in unobserved factors across cohorts and therefore results should be interpreted with caution.

Regression results. To answer *RQ1,* which in Study 2 pertains only to the delayed effect, Table 9 presents the results from estimating the three pairwise comparisons explained above. Standard errors need to be cluster-adjusted because of the design of the field experiment since in each cohort there are several clusters of individuals, that is, complete classes (Abadie et al., 2017). In each cohort all the students in the class are exposed to the same intervention. In addition, because of the small number of clusters (six in the 2015 cohort, and five in the 2017 and in the 2019 cohorts, see Table B.1 in

[23] The reason to include age as a dummy of being 18 is that in Study 2 participants are first-year college students while in Study 1 participants are college students enrolled at any year. The natural age of first-year students is 18 years old. Indeed, as shown in Table B.2 in Appendix B there is a large percentage of students at this age in all three cohorts: 62% in 2015 (column 1), 70% in 2017 (column 2), and 68% in 2019 (column 5). A dummy of being 18 is appropriate to capture this strong discontinuity in participants' age.

Appendix B) we use wild cluster bootstrap corrections. For OLS estimation we use the wild cluster bootstrap with the null imposed (or wild cluster restricted) proposed by Cameron, Gelbach and Miller (2008). For IV estimation we use wild restricted efficient bootstrap, which extends wild cluster bootstrap to IV linear models (Finlay and Magnusson, 2016). Bootstrap p-values are reported in brackets (see Appendix C for more details on the calculation of p-values).

Table 9. Estimation Results

	RT (2017 cohort) vs 2015 cohort				Compliers: RT (2017 cohort) vs NRT (2019 cohort)[a]		NRT (2019 cohort) vs 2015 cohort			
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	IV (9)	IV (10)
RT	0.63**	0.66**	0.61***	0.64***	0.48***	0.27				
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.07]				
NRT							0.14	0.14	0.19	0.21
							[0.44]	[0.24]	[0.29]	[0.06]
Age: 18 when enrolled			-0.24	-0.24		-0.24		-0.12		-0.12
			[0.08]	[0.08]		[0.09]		[0.37]		[0.35]
Female			0.06	0.07		-0.10		0.13		0.13
			[0.55]	[0.55]		[0.32]		[0.35]		[0.36]
CAT grade			0.13*	0.13*		0.10		-0.04		-0.03
			[0.02]	[0.02]		[0.09]		[0.33]		[0.47]
Scholarship			-0.07	-0.07		0.10		-0.14		-0.14
			[0.47]	[0.45]		[0.39]		[0.10]		[0.08]
Non-Spanish			-0.23	-0.23		-0.05		0.07		0.07
			[0.35]	[0.30]		[0.85]		[0.65]		[0.67]
Retaker			0.13	0.14		-0.64		-0.24		-0.25
			[0.17]	[0.18]		[0.14]		[0.33]		[0.33]
Law			0.47**	0.46*		0.29*		0.43		0.42
			[0.01]	[0.01]		[0.04]		[0.09]		[0.07]
Economics			0.29	0.29		0.07		0.02		0.03
			[0.12]	[0.11]		[0.66]		[0.91]		[0.85]
HS track			-0.02	-0.03		0.18		-0.14		-0.15
			[0.87]	[0.87]		[0.26]		[0.30]		[0.25]
Constant	-0.13	-0.14	-1.29*	-1.30*	0.02	-0.74	-0.12	0.37	-0.14	0.24
	[0.31]	[0.32]	[0.01]	[0.02]	[0.83]	[0.17]	[0.38]	[0.36]	[0.29]	[0.61]
N	612	612	610	610	548	547	656	653	656	653
R^2	0.05	0.05	0.08	0.08	0.04	0.07	0.00	0.04	0.00	0.04

Note: Dependent variable is belief change and takes values between -4 and 4; positive values indicate a change away from the misconception. RT: Refutation text. NRT: Non-refutational text. [a] In this estimation the sample includes only compliers in both cohorts. In brackets we report p-values obtained with wild cluster bootstrap restricted (with Webb weights for the auxiliary random variable). Significance levels: *p < 0.05, **p < 0.01, ***p < 0.001.

Columns (1) to (4) show the results of estimating the effect of the RT relative to standard teaching. In column (1), the OLS estimate of the RT without control variables,

0.63, is significant and has a positive sign, indicating that in the 2017 cohort beliefs change in the right direction (from agreeing with the statement towards disagreeing), compared with the 2015 control cohort. Column (2) shows that the IV estimate, interpreted as the ATT effect, is significant and slightly higher (0.66) than the OLS estimate. This indicates that not controlling for the bias from the non-compliance decision would underestimate the effect of the RT. Since compliers seem to be positively selected (as measured by the lower percentage of retakers, see column (11) in Table B.2) this would suggest that compliers might be harder to convince because they can think of better arguments to defend their initial view (Kahan et al., 2017). However, this bias has a limited role in driving the results since the difference between the IV and the OLS estimate is rather small.

The magnitude of the estimated coefficient falls only slightly after controlling for observed characteristics across cohorts (columns (3) and (4)). For the IV estimates the ATT effect is 0.65: the RT changes beliefs in the correct direction by 0.65 points.[24] This magnitude amounts to about 30% of the average response of the 2017 compliers at the beginning of the semester (2.15), and to almost one half of the standard deviation of the change in beliefs (the standard deviation of y_i is 1.36).[25]

Columns (5) and (6) in Table 9 show the estimated effect of the RT on the change in beliefs about rent controls relative to the NRT. In the specification that includes all the control variables we find a positive effect of the RT relative to the NRT. The magnitude of the estimated effect (0.27, with a p-value of 0.07) is similar to the magnitude of the

[24] Results from the first-stage estimation show that the instrument is strong. The estimated coefficient is 0.89, significant at 1% level, with an F-test = 1138.5 (p-value = 0.00), and partial $R^2 = 0.826$ ($R^2 = 0.829$).
[25] We run the IV estimation of equation (1) separately for Law, Economics, and Business, the latter including the double major in Law and Business. In all cases the effect of the RT is positive but it is only significant for Law and Business majors. The effect of the RT is lowest for economics (0.35) and highest for business majors (0.74). For law students the effect is in between, with a value of 0.63. Results are shown in Table C.1 in Appendix C.

effect of discussing the RT in teams compared with the NRT in Study 1 (0.29, column (12) in Table 3). Finally, column (10) shows the IV estimate of the NRT relative to standard teaching including control variables. The estimate is 0.21, with a p-value of 0.06. This suggests that the NRT may have a mild impact on the students' reconsideration of the misconception compared with standard instruction.

All in all, in Study 2 the evidence indicates that the RT has an edge over standard instruction in prompting a decline of the misconception. When compared with the NRT, the effect of the RT is however not significant at the 5% level. These findings suggest a partially positive answer to *RQ1*. However, the limitations of this study commend some caution, as we discuss below.

Who changes her mind? As in Study 1, we explore whether the effect of the RT may vary depending on the initial belief. As descriptive evidence, the transition matrix presented in Panel C in Table B.4 (Appendix B) shows a high persistence of the misconception among participants who initially hold it in the 2017 cohort, but less than among those in the 2015 cohort.

Table 10 presents the results of estimating the effect of the RT (2017 cohort) conditional on initial beliefs relative to standard teaching (2015 cohort) and to the NRT (2019 cohort). Columns refer to the initial belief and each cell is the result of a separate estimation. Panel A shows that compared with standard teaching, the RT significantly induces individuals who initially have the misconception to change their minds towards disagreeing with the statement, as intended. The RT does not have a significant effect on those who initially do not know or disagree. Panel B shows that the RT does not do significantly better than the NRT in inducing a change in beliefs conditional on initial beliefs. Thus, the answer to *RQ4* is that the RT has a positive significant effect among

participants who initially agree with the misconception, but only relative to standard teaching.

Table 10. RT effect conditional on initial belief

	(1) Agree	(2) Do not know	(3) Disagree
	A. RT (2017 cohort) vs Control (2015 cohort); IV estimates		
RT	0.30**	0.19	0.50
	[0.01]	[0.59]	[0.08]
Explanatory variables	Yes	Yes	Yes
N	441	70	99
R^2	0.07	0.09	0.08
	B. Compliers: RT (2017 cohort) vs NRT (2019 cohort); OLS estimates		
RT	0.33	0.25	0.52
	[0.23]	[0.64]	[0.60]
Explanatory variables	Yes	Yes	Yes
N	425	67	55
R^2	0.11	0.07	0.17

Note: Dependent variable is belief change, taking values between -2 and 2. We aggregate totally disagree and disagree into one category, totally agree and agree into another. Positive values indicate a change away from the misconception. RT: Refutation text. Regressions control for the same set of explanatory variables included in Table 9. In brackets we report p-values obtained using wild bootstrap restricted (with Rademacher weights for the auxiliary random variable) in Panel A and using wild cluster bootstrap restricted (with Webb weights for the auxiliary random variable) in Panel B. Significance levels: *p < 0.05, **p < 0.01, ***p < 0.001.

As in Study 1, we conduct an exploratory analysis of the correlation of the change in beliefs with outrage feelings (statement *Outrage with respect to housing* in Table 7) and with lectures touching on real issues (statement *Lectures touch on real issues* in Table 7). As Table C.2 in Appendix C shows these factors are not significantly correlated with the change in beliefs.

5. Discussion of results, strengths and limitations

Regarding *RQ1*, when we compare the RT with the NRT as a benchmark, we find the same answer for Study 1 and Study 2: the RT does not significantly improve on the NRT. In contrast, when using in Study 2 the benchmark of standard teaching, the RT significantly induces a change away from the misconception, so that for this comparison

RQ1 can be answered in the affirmative. This finding is in line with results obtained in previous studies on misconceptions referring to non-economic issues, where the refutational approach also has a positive effect (Tipett, 2010; Nussbaum et al., 2017). The NRT however does not have a significant effect against the benchmark of standard teaching. A practical implication is that misconceptions should be explicitly elicited and confronted in economic courses, otherwise these misconceptions are likely to prevail. In other words, standard teaching would not promote integrating the new information in the participant's pre-existing worldview when scientific knowledge collides with false but popular beliefs. We stress, however, that we should interpret this finding with caution, because of the limitations of Study 2 discussed later.

A plausible explanation for the lack of a significant effect of the RT relative to the NRT in both studies is that the NRT provides a very complete argumentation about the negative effects of rent controls.[26] In both studies, the prevalence of the misconception falls after reading the NRT, especially in Study 1, where the change in beliefs is similar to the change after reading the RT (see Tables 2 and 8). In writing the NRT, we decided to err on the side of excess rather than on the side of lack of information. We were worried that a positive impact of the intervention could be put into question because of a relatively weak NRT. Another part of the explanation of the lack of significant results may relate to an attention effect. Having to read a text, naturally focuses participants' attention on only one economic issue, and this may be the reason why we find no significant difference of the RT against the NRT benchmark. However, the RT does have an impact on the change of beliefs against the benchmark of standard teaching, suggesting that the refutational elements enhance the attention effect that the NRT may have. With our data,

[26] Note that in Study 2 p-values are high because of the conservative bootstrap correction motivated by the small number of clusters.

unfortunately, we cannot disentangle the role of these two factors: thorough argumentation in the NRT on one hand and high attention capture on the other.

We respond negatively to *RQ2* and *RQ3*, using the data of Study 1, the only setting where we can analyze these questions. We find no difference between the RT and the NRT both for individual vs team discussion, and for immediate vs delayed effect.

Results from both studies show some evidence that the effect of the RT depends on initial beliefs (*RQ4*). In Study 1, the RT in teams significantly prevents that, compared with the NRT, participants who initially disagree change their beliefs wrongly towards the misconception. This is of interest since participants who initially disagree would have changed their minds towards the misconception had they not been assigned to the RT treatment, that is, if they do not receive information that reassures them on their correct belief. In Study 2, the RT induces, with respect to standard teaching, a significant change to disagreeing for those who initially agree with the misconception.

With respect to *RQ5* and *RQ6*, the results of Study 1 show that the impact of the RT is not higher for participants with either lower confirmation bias or higher tendency to reflective thinking. In addition, confirmation bias is not directly correlated with the change in beliefs. More reflective participants, by contrast, move significantly away from the misconception when the RT is discussed in teams. This suggests that team discussion may help more reflective participants make a higher effort to reconsider initial beliefs.

Both studies have some strengths. Study 1, in addition to being performed in a highly controlled environment, easily allows investigating issues such as the role of cognitive factors. This is important, because it is a reasonable ex-ante conjecture that the impact of the RT may depend on such factors. In turn, strengths of Study 2 are that it takes place in a natural environment, and provides an additional sensible benchmark (standard teaching). The intervention is unobtrusive and can be easily implemented.

Both studies also have some potential limitations. A common concern with respect to laboratory studies is the potential experimenter demand effect, as participants may try to guess the purpose of the experiment and change their behavior accordingly (Zizzo, 2010). In line with findings by De Quidt et al. (2018), in our case, the experimenter demand seems to be small. The percentage of participants who do not change their delayed opinion after reading the RT is similar and close to 60% in both the team condition of Study 1, where participants are put in a fabricated situation in a computer laboratory, and in the 2017 cohort in Study 2, where the intervention occurs in the natural environment of a college class.

With respect to Study 2 we have already acknowledged in Section 4 the limitations derived from the between-cohort randomization. At the university level, there were no meaningful changes in admission rules and undergraduate programs across cohorts. Moreover, potential differences across cohorts steaming from different students' decisions of delaying college entry after completing high school, for instance because of taking a gap year, can be dismissed as in Spain this is a rare choice. The main concern thus is the potential environmental differences across cohorts. We have checked the stability of the social environment across cohorts. All years 2015 to 2019 correspond to an economic expansion period. In addition, the problem of rental housing has been similarly prominent in the public debate over these years.[27] Moreover, we have looked into the statistics provided by Barcelona's City Hall website on rental prices. Figure B.1 in Appendix B depicts the annual growth rate of the average monthly rent in Barcelona. It shows that rents increased substantially at similar rates during the years we were

[27] The main newspapers (El País, La Vanguardia, among others) have been reporting on housing prices and policy debates continuously since 2014. Some research economists (see for example, Jose Garcia-Montalvo, Universitat Pompeu Fabra and Barcelona Graduate School of Economics: http://jgmontalvo.com/prensa-escrita-referencias/) have regularly participated in the public debate about housing policies in the media, in particular about rental housing, over the years 2015 to 2019.

conducting the Study 2 (2015 to 2019). We thus overall believe that we can be confident that both within and across cohorts the environment regarding rental housing has been quite stable. The fact that the initial opinion about the rent control policy of the three cohorts in Study 2 is quite similar (between 69 to 78% agree with the misconception), and not far from what we observe in Study 1 and in polls elsewhere, is reassuring. However, we cannot rule out completely differences in the distribution of unobserved factors across cohorts that may affect the results of Study 2.

Note that had within cohort randomization been allowed by university rules, we believe that it would have had an important limitation, as contagion –or spillover effects– would hardly be possible to avoid, because of students in different groups of the same cohort easily meet and communicate. This would be a source of bias. The across-cohort randomization, using three cohorts with a two-year gap, minimizes contagion effects.

A second factor of caution is that the questionnaires in 2015 included, in addition to the three statements indicated in Table 7, a number of statements taken from Sapienza and Zingales (2013)[28]. Since they were too technical or too remote for first-year students, we replaced them in 2017 and 2019 with the statements shown in Table 7 that are more pertinent to college students. As a result, questionnaires in 2015 are somewhat different from questionnaires in 2017 and 2019, which may affect participants' responses. However, we consider that this potential source of confounding factors plays a limited role since we do not observe large differences in the distribution of initial responses to the statement of interest (Panel A in Table 8). A final concern mentioned in Section 4 is the different attrition rates across cohorts, which may lead to unobserved systematic differences across cohorts, even though we include observed characteristics in the specification to account for them.

[28] See Busom, Lopez-Mayan and Panadés (2017).

Given all the limitations we point out that may create systematic unobserved differences across cohorts, we refrain from interpreting results from Study 2 as causal.

6. Concluding remarks

In several fields, the concern about effectively communicating scientific evidence and, in particular, dispelling misconceptions, has become an active area of research. However, in economics this research is still embryonic. Our work contributes new empirical evidence on the effectiveness of a particular communication tool, the RT, to help people revise an intuitive, incorrect, belief, in our case, about the effects of rent controls. This approach, although previously used to debunk misconceptions in other socially relevant fields, has not been studied in relation to misconceptions about economics. We thus contribute to an emerging research in economics on how people form and change their beliefs on economic and social policies, and on how communication can be designed so that the demand for intuitive but welfare-reducing policies can be scaled down. This may then encourage the adoption of evidence-based policies by policymakers, who may be reluctant to do so if voters do not support these policies because they hold misconceptions.

Our findings suggest that the RT is not more effective than the NRT in correcting the misconception about the effect of rent controls. But they also show that, subject to some caveats, the RT is significantly effective in a natural environment, such as the case of college-level economic instruction.

We believe that several research paths open up. First, since we observe that the proportion of participants who stick to the misconception is still high in both studies, there is room for identifying other formats that could improve the results, for instance, combining the refutational approach with visual tools. Second, understanding more

deeply why some people are reluctant to accept well documented scientific information about the workings of the economy, testing the role of other cognitive factors. Finally, a broader issue is to explore how to communicate social science research results when the majority of economic researchers are uncertain about the effect of a policy or when researchers' consensus is weaker, in the sense of their opinions being strongly divided.

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