Who Said or What Said? Estimating Ideological Bias in Views Among Economists*

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Abstract

There exists a long-standing debate about the influence of ideological views in economics. Surprisingly, however, there is no concrete empirical evidence to examine this critical issue. Using a randomized controlled online experiment involving economists in nineteen countries, we estimate the effect of ideological bias on views among economists. Participants were asked to evaluate statements from prominent economists on different topics. However, source attribution for each statement was randomized without participants' knowledge. For each statement, participants either received the actual source, an altered ideologically-different source, or no source. We find that changing source attributions from mainstream to less-/non-mainstream, or removing them, significantly reduces economists' reported agreement with statements. We suggest that these results are consistent with the existence of ideological bias among economists. We find additional evidence that supports this conclusion. First, changing/removing sources has no impact on economists' reported confidence with their evaluations. Second, majority of survey participants report that arguments should be evaluated based on their content only, which is in contrast with how they actually evaluate statements. This suggests that part of the ideological bias evident in our results operates through implicit or unconscious modes. Third, switching source attributions to less-/non-mainstream (who are typically politically to the left of mainstream sources) affects those at the far right of the political spectrum much more significantly than those at the far left. Finally, the heterogeneity patterns in our results by gender, country, PhD completion country, research area, and undergraduate major are also consistent with the existence of ideological bias.

Keywords: Ideology, ideological bias, authority bias, views among economists, randomized controlled experiment.

JEL Codes: A11, A14.

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1. Introduction

Mainstream (Neoclassical) economists put a strong emphasis on the positivist conception of their discipline and have always claimed to be objective and non-ideological. Friedman (1953) describes in his famous essay that "positive economics is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences." Similarly, Alchian asserts that "[i]n economics, we have a positive science, one completely devoid of ethics or normative propositions or implications. It is as amoral and non-ethical as mathematics, chemistry, or physics.".[1] There exists, however, a long-standing debate about the role of ideology in economics, which some argue has resulted in rigidity in the discipline, rejection and isolation of alternative views, and narrow pedagogy in economic training (e.g. Backhouse 2010, Colander 2005, Fine and Mikonakis 2009, Fullbrook 2008, Frankfurter and McGoun 1999, Galbraith 1989, Krugman 2009, Morgan 2015, Romer 2015, Rubinstein 2006, Samauels 1992, Stiglitz 2002, Thompson 1997, Wiles 1979, and others).

Unfortunately, there is not much direct and concrete empirical evidence to evaluate the claims made by different groups about the existence and the role of ideological bias in economics. This is partly because different sides of the argument have not been engaged in any constructive conversation about this critical topic, which has led to an increasing divide between them. Mainstream economists simply ignore and dismiss these arguments without engaging in a conversation with the critics, while some critics have taken the ideological bias in mainstream economics as a given and used it to denigrate neoclassical economics. We argue that the absence of an engaging conversation in this area is clearly not a good sign. We take the words of Alice Rivlin in her 1987 American Economic Association presidential address to heart that "economists need to be more careful to sort out, for ourselves and others, what we really know from our ideological biases".

In this study, we are not directly investigating the credibility of the different arguments about the influence of ideological bias in economics by checking the validity of their evidence and the consistency of the conclusions drawn. We will instead take an agnostic view on these discussions, and rather take them as alarming signs that invoke important questions which require further investigation. We believe that the answer to whether there is a (hidden) ideological bias

[1] Letter from Armen Alchian to Glenn Campbell, January 20, 1969. See Freedman (2016).

among economists has important intellectual implications, both theoretical and practical. Theoretically, it will help us investigate the extent to which the theoretical arguments behind the positivist methodology of neoclassical economics are consistent with the empirical evidence. In terms of practical implications, the answer to this question will inform the debate about the ideological views in economics as it relates to the evolution of the mainstream economics discourse, economic training, and economists' presentation of themselves in policy debates.

In order to examine the effect of ideological bias on views among economists, we use an online randomized controlled experiment involving economists in nineteen countries.[2] More specifically, we ask participants in our online survey to evaluate statements from prominent (mainly mainstream) economists on a wide range of topics (e.g. fairness, inequality, role of government, intellectual property, globalization, free market pros and cons, economic methodology, women in economics, etc.). All participants receive identical statements in the same sequence. However, source attribution provided for each statement is randomized without participants' knowledge. For each statement, participants randomly receive either a mainstream source (Control Group), or a relatively less-/none-mainstream source (Treatment 1), or no source attribution at all (Treatment 2). We then measure whether economists agree/disagree with identical statements to different degrees when statements are attributed to authors (researchers) with different views (ideologies) which put them at different distances to mainstream economics. Traditional norms of modern science, such as organized skepticism and disinterestedness, are intended to make the identity of the source of an argument irrelevant (Merton 1973). Therefore, any evidence showing that changing the identity of sources affects how economists evaluate arguments could cast doubt on the notion that economists are objective scientists and are not influenced by ideological biases.

We find clear evidence that changing or removing source attributions and ideologies attached to them significantly affects economists level of agreement with statements. More specifically, we find that changing source attributions from mainstream to less-/non-mainstream sources reduces the agreement level by around one-fourth of a standard deviation. Similarly, removing mainstream source attributions reduces the agreement level by more than one-third of a

[2] By economists we mean those with a graduate degree in economics who are either academics, or work in government agencies, independent research institutions or think tanks. Majority of economists in our sample are academics with a PhD degree in economics. See the data section and Table A1 in our online appendix for more details.

standard deviation. These results hold for almost all 15 statements evaluated by participants, using a wide range of topics and ideological distances between sources.

We suggest that these results are consistent with the existence of a strong ideological bias in views among economists. We also find additional evidence that supports this conclusion. First, our results suggest that changing or removing source attributions, while significantly affects economists' level of agreement with statements, has no impact on their reported confidence with their assessment. This is consistent with the existence of ideological bias, which usually affects one's opinion or judgment without affecting his/her confidence level. Second, the professional standard that economists in our survey set for themselves, while consistent with the image that economists are objective and non-ideological, diverges significantly from their actual behaviour. More specifically, a majority of participants in our survey (82 percent) report that arguments should be evaluated based on their content only, as opposed to their author (0.5 percent), or a combination of both (around 18 percent), which is in sharp contrast with how they actually evaluate statements. This suggests that perhaps part of the ideological bias evident in our results operates through implicit or unconscious modes. Third, we find clear evidence that switching source attributions to less-/non-mainstream sources (who are typically politically to the left of mainstream sources) affects those at the far right of the political spectrum much more significantly than those at the far left. More specifically, the reduction in agreement level resulting from changing sources from mainstream to less-/non-mainstream is almost 6 times larger at the far right relative to the far left.

Using background information collected from participants, we also examine whether our results vary by characteristics such as gender, country, area of research, country where PhD was completed, and undergraduate major. We find that the estimated ideological bias among female economists is around 40 percent less than their male counterparts. Interestingly, on one statement in our survey which examines the issue of gender gap in economics, there is clear disagreement between male and female economists, with women much more strongly agreeing with the existence of a serious and persisting gender gap in the discipline. In addition, on this specific statement, while men still exhibit strong ideological bias, women display no signs of ideological bias. This is perhaps due to the fact that when it comes to the important issue of gender gap in economics which involves female economists at a personal level, women put aside ideology and merely focus on the content of the statement as opposed to its source.

We also find systematic and significant heterogeneity in ideological bias by country, area of research, country where PhD was completed, and undergraduate major with some economists exhibiting no ideological bias on average, and some others showing very strong bias. In addition, the heterogeneity found in our results remains consistent with the existence of ideological bias (see Section 5.2).

The remainder of the paper proceeds as follows. Section 2 provides a brief overview of the discussion about economics and ideology. Section 3 explains in more detail what we mean by ideological bias and how we measure it. It also describes our experimental design. Section 4 describes our data and empirical methodology. Section 5 presents and discusses our results. Section 6 concludes.

2. Economics and ideology: a brief overview

Milberg (1998) elegantly summarizes the long-standing debate about the influence of ideology in economics by stating that "the history of economic thought can in fact be read as a series of efforts to distance knowledge claims from the taint of ideology, a continuing struggle to establish the field's scientific merit." About a century ago, Irving Fisher, in his presidential address to the American Economic Association, raised his concern about ideological bias in economics by stating that, "academic economists, from their very open-mindedness, are apt to be carried off, unawares, by the bias of the community in which they live." (Fisher 1919). Other prominent economists such as Joseph Schumpeter and George Stigler also made substantial contributions to this discussion over the next few decades (see Schumpeter (1949) and Stigler (1959, 1960, 1965) for examples). However, the change in the nature of economic discourse, the increasing use of mathematics and statistics, and the increasing dominance of the positivist methodology, represented by Friedman's 'Methodology of Positive Economics', have reduced the concern with ideological bias in economics, which has gradually given way to a consensus that "economics is, or can be, an objective science".[3]

Due to this prevailing consensus, the issue of ideological bias has been largely ignored within mainstream economics in the last few decades. Critics, however, argue that the increasing reliance of economics on mathematics and statistics has not freed the discipline from ideological bias; it has simply made it easier to disregard it (e.g. Lawson 2012, Myrdal 1954). There also exists

[3] See Friedman (1953).

evidence that could suggest that economics has not successfully rid itself of ideological bias. For example, Hodgson and Jiang (2007) argue that due to ideological bias in economics, the study of corruption has been mainly limited to the public sector, when there is abundant evidence of corruption in the private sector (sometimes in its relation to the public sector but also internally). Jelveh et al. (2015) point to ideological overtones that could be identified in public debates between prominent economists over public policy during the last financial crisis as an example of ideological bias in economics. They also point out that these perceptions of ideological bias among economists have even affected the selection of economists as experts for different government positions.[4] Other prominent manifestations of this ideological bias include the so-called fresh-water/salt-water divide in macroeconomics (Gordon and Dahl 2013), the conflicts between liberal/conservative camps in economics (especially regarding the possible distribution-efficiency trade-off), and the ideologically charged debates over the controversial book by Thomas Piketty (2014) or over Paul Romer (2015) and his criticism that "mathiness lets academic politics masquerade as science".

There also exists a long-standing charge laid mainly by non-neoclassical economists regarding the prevalence of ideological bias among neoclassical economists (e.g. Backhouse 2010, Fine and Mikonakis 2009, Fullbrook 2008, Frankfurter and McGoun 1999, Morgan 2015, Smauels 1992, Thompson 1997, Wiles 1979). For example, summarizing the views of the Post-Autistic economics movement in France, Fullbrook (2003) argues that economic profession is the "opposite of pluralistic" and is "dogmatically tied to value-laden neoclassical orthodoxy." Samuels (1980) suggests that economics is much more a "system of belief than it is a corpus of verified logical positivist knowledge" and that many uses of economics "may represent only the clothing of normativism with the garments of science". Rothbarb (1960) criticizes what Hayek calls "scientism" in economics and argues that it is a "profoundly unscientific attempt to transfer uncritically the methodology of the physical sciences to the study of human action."

There are also studies that point to the ideological biases in economic training. Based on a survey of graduate students in economics, Colander (2005) raises concerns regarding how graduate

[4] They point out to the following two examples: "The rejection of Peter Diamond, a Nobel laureate in economics, by Senate Republicans, as the nominee to the Federal Reserve Board, with one of the top Republicans on the Banking Committee calling him "an old-fashioned, big government, Keynesian" at the nomination hearing (see here). And, the withdrawal of Larry Summers from his candidacy for the chairmanship of the Federal Reserve Board due to strong opposition from a coalition group over several issues related to ideology, including his role to "push to deregulate Wall Street"" (see here).

training in economics may lead to biases in students' views. For example, he argues that graduate training in economics induces conservative political beliefs in students. Similarly, Allgood et al. (2012) find evidence that suggests "undergraduate coursework in economics is strongly associated with political party affiliation and with donations to candidates or parties". Surveying undergraduate students in several disciplines including economics, Rubinstein (2006) finds "sharp differences between the groups in dealing with the dilemma of profit maximization vs. worker layoffs." He argues that studying economics influences students' views on economic issues without them even realizing. Stiglitz (2002) also argues that "[Economics as taught] in America's graduate schools … bears testimony to a triumph of ideology over science."

These discussions have not been limited to academic circles and have received significant attention in the press in recent years with titles such as "Time to stop this pretense – Economics is not a science" (Halligan 2013), "Yes, Economics is a science" (Chetty 2013), "Maybe Economics is a science, but many economists are not scientists" (Krugman 2013), "Is Economics a science?" (Shiller 2013), "Don't let the Nobel prize fool you. Economics is not a science" (Luyendijk 2015), and "How Economics became a religion" (Rapley 2017).

Surprisingly, however, there is very thin empirical evidence to rule out or establish these claims. We are only aware of one study that examines this issue to some extent. Gordon and Dahl (2013) use data from a series of questions from IGM Economic Expert Panel to examine to what extent prominent economists (41 economists) from the top seven economics departments disagree about key economic issues. Their results suggest that "there is close to full consensus among these panel members when the past economic literature on the question is large. When past evidence is less extensive, differences in opinions do show up."[5] They also find that "there is no evidence to support a conservative versus liberal divide among these panel members, at least on the types of questions included so far in the surveys."[6]

[5] The variable that measures the size of the economic literature related to a certain question is constructed based on judgment calls by Gordon and Dahl (2013).

[6] They use two approaches here. First, they use different distance-based clustering methods to examine whether panel members are clustered into "two or even a few roughly equal-sized camps" based on their responses. As their second approach, they identify a subset of questions that are likely to generate disagreement among panel members, and then classify answers to these questions as either consistent with "Chicago price theory" or consistent with concerns regarding distributional implications or market failures. They then test whether participants' responses are homogenous as a panel or are divided into two groups. They find evidence that supports the former.

3. Measuring Ideological Bias and Experimental Design

As Eagleton (1991) suggests, the term "ideology" has been used in different ways by different social scientists. This is partly due to the complex and multi-dimensional nature of the concept, which does not yield itself very easily to a neat definition. We therefore see little advantage in providing a narrow definition by singling out one trait among a complex of traits. It is the complex itself that we are interested in and in this paper we measure one clear manifestation of this complex notion, that is, someone agreeing/disagreeing with an argument not based on its content but rather based on his/her perception of the differences between the views of the author of the argument and his/her own views, even when those perceived differences are unrelated to the content of the argument.

There exists significant evidence of such manifestation of prejudice and ideological bias in psychology where participants, often undergraduate students in the social sciences, tend to agree more with findings that are in line with their previously reported political views (see McCoun (1998) for a review). There also exists evidence of such bias among regular citizens. For example, MacCoun and Paletz (2009) conduct an experiment to examine how ordinary citizens evaluate hypothetical research findings on controversial topics. They find that when findings challenge people's prior beliefs, they are more skeptical of the findings. Their results also suggest that "citizens, especially those holding conservative beliefs, tended to attribute studies with liberal findings to the liberalism of the researcher, but citizens were less likely to attribute conservative findings to the conservatism of the researcher." They interpret this as effects of "partisanship and ideology".

It is well-understood that examining issues such as the impact of bias, prejudice, or discrimination on individual views and decisions is very challenging given the complex nature of these types of behaviour. For example, the issue of discrimination in the labour market has long been an issue of importance and interest to labour economists. However, as Bertrand and Duflo (2017) put it, "it has proven elusive to produce convincing evidence of discrimination using standard regression analysis methods and observational data". This has given rise to a field experimentation literature in economics that has relied on the use of deception, for example through sending out fictitious resumes and applications, to examine the prevalence and consequences of discrimination against different groups in the labour market (See Bertrand and

Duflo (2017) and Riach and Rich (2002) for a review. Also see Currie et al. (2014) as another example of experimental audit studies).

Given that answering our question of interest is subjected to the same challenges, we take a similar approach, namely using fictitious source attributions, in order to produce reliable results (see Section 3 in our online appendix for a more detailed discussion). More specifically, we employ a randomized controlled experiment that involves an online survey. Participants are asked to evaluate a series of statements presented to them by choosing one of the following options: strongly agree, agree, neutral, disagree, and strongly disagree. They are also asked to choose a confidence level on a scale from 1 to 5 for their selected answer. These statements are on a wide range of topics in economics and while they are mainly from prominent (mainstream) economists, most of them are critical, to different extents, of certain aspects of mainstream economics.

All participants receive identical statements in the same sequence. However, source attribution for each statement is randomized without participants' knowledge. For each statement, participants randomly receive either a mainstream source (Control Group), or a relatively less-/none-mainstream source (Treatment 1), or no source attribution (Treatment 2).[7] See Table A8 in our online appendix for a complete list of statements and sources. Participants who are randomized into treatment 2 for the first statement remain there for the entire survey. However, those who are randomized into control group or treatment 1 are subsequently re-randomized into one of these two groups for each following statement.

Three points are worth highlighting here. First, the actual and the altered sources for each statement were carefully paired such that they can be associated to commonly known but different views (such as different schools of thoughts, political leanings, disciplines, attitudes towards mainstream economics, etc.), so that there is an ideological distance between them and switching the source can potentially induce an ideological bias. For each source, we also provide information on their discipline, their affiliation, and the title of one of their publications. This is to further

[7] We are aware that in economics the notions of mainstream and less-/non-mainstream have changed a lot over time. Similar to many other classifications, this is just an attempt to summarize a complex notion and provide an easy-to-understand relative comparison, even though we believe these differences are more appropriately understood as an spectrum rather than a black and white distinction. For the lack of better classifications, therefore, we categorize our sources into these two groups to clearly summarize and represent the relative ideological distance between them to the reader. Of course, it is well-understood that this classification does not readily apply to some sources, such as older ones (e.g. Karl Marx or Sigmund Freud) or sources from other disciplines (e.g. Michael Sandel) in the same way it applies to others. However, to remain consistent and avoid confusion for the reader, we stick to the same naming convention for all sources.

pronounce the ideological differences between the two sources for each statement, especially in cases where sources might be less known by economists.[8] Second, statements were carefully selected so that their attribution to altered sources is believable by participants. Third, all selected statements were relatively obscure so the misattribution would not be easily noticed by participants.[9]

4. Data

The target population for this study were economists from nineteen different countries.[10] We used Economics Departments, Institutes and Research Centers in the World (EDIRC) website, which is provided by the Research Division of the Federal Reserve Bank of St. Louis, to identify economic institutions (economics departments, government agencies, independent research institutions and think tanks) in each target country. We then used the website of each institution (provided on EDIRC website) to manually extract the email addresses of economists in each institution. The extracted email addresses were then used to send out invitations and reminders to ask economists to participate in the survey. The survey was conducted between October 2017 and April 2018. While the survey's opening and closing dates were different for different countries, the survey was open in each country for approximately two months.

In many cases during email extraction, especially in case of multidisciplinary departments (e.g. school of business, management, and economics), research institutions, and government agencies, it was not clear from the institution's website which listed faculty members or researchers were economists and which ones had a degree from other disciplines. In these cases we asked our team of research assistants to extract all listed email addresses. Our rationale was

[8] For example, while some economists might not know Richard Wolf or Anwar Shaikh, knowing that they are affiliated with the University of Massachusetts Amherst or the New School for Social Research, the two famous heterodox schools in economics, makes it more likely to induce an ideological reaction. Similarly, titles of selected publication for each source, such as "*Rethinking Marxism*", "*The Crisis of Vision in Modern Economic Thought*", or "*What Money Can't Buy: The Moral Limits of Markets*", serve the same purpose.

[9] We received less than a dozen emails from people who had recognized the misattribution of an statement to a source. In all but one of these cases, the statement identified as being misattributed was statement 13 (see Table A8 in the online appendix), which is perhaps the least obscure statement used in our survey. This statement is from Adam Smith's Wealth of Nations, which was misattributed to Karl Marx. All the emails we received, however, made it clear that this was perceived as a mistake in our survey and not part of our survey design. Nevertheless, identifying misattributions, which seems to be quite rare based on emails we received, would only lead to an underestimation of the true bias effect.

[10] These countries include Australia, Austria, Brazil, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, South Africa, Sweden, Switzerland, UK, US. The entire survey was translated into French, Italian, Japanese and Brazilian Portuguese to allow participants from corresponding countries to complete the survey in their own native language if they choose to.

that sending email invitations to some non-economists was clearly better than risking to exclude some economists, especially since this exclusion could be systemically related to the type of institution and lead to sample selection. We made sure however that non-economists who received the survey invitation were self-filtered out later on by making it clear in our email invitation as well as on the first page of the survey that the target population of the survey are economists.[11]

As a result, we are not able to provide a reasonable and reliable estimate of the participation rate in our survey since that would require the total number of economists in the target population, which is considerably smaller than the total number of email addresses we extracted online, for the reason discussed above. In addition, this calculation is further complicated by the fact that upon sending email invitations we received a considerable number of auto-replies from people who had left their institution, were on sabbatical, parental, or sick leave, or temporarily had no access to their email.

Participants in our survey were required to complete each page in order to proceed to the next page. As a result, they could not skip evaluating some statements. However, participation in the survey was entirely voluntary and participants could choose to withdraw at any point during the survey, without providing any reason, by simply closing the window or quitting browser. Participants were assured that any responses collected up until the point of withdrawal will not be included in the study. For this reason, we are not allowed, by the terms of our ethics approval, to use data collected from people who did not complete the entire survey. As a result, we have restricted our sample to participants who have completed the entire survey.[12] We run several tests to ensure that our focus on participants who completed the entire survey does not introduce sample selection bias in our results and we find no evidence of such a bias. See Section 1 in our online appendix for more details.

Our final sample includes 2,425 economists from nineteen different countries. Summary statistics for our sample are reported in Table A1 in our online appendix. While we cannot measure a reliable participation rate for our survey for the reasons discussed above, the summary statistics suggest that we have a very diverse group of economists in our final sample. We also report the

[11] As expected, we received many emails from faculty members who were not economists (historians, statisticians, sociologists, political scientists, engineers, etc.) asking us to remove them from the email list.

[12] A total of 3,288 economists participated in our survey. There were 454 participants who quit the survey at the very beginning (in the questionnaire section where they were asked to provide background information). Another 409 people withdrew from the survey at some point after they started evaluating the statements. See Table A3 in our online appendix for more details.

percentage of responses that come from different institutions in the US, Canada, and the UK in figures A3 to A5 in our online appendix as examples to show that participants in our survey come from a very diverse group if institutions in each country.

The primary dependent variable in our analysis is the reported agreement level with each statement. In our baseline analysis, we estimate linear regression models in which the agreement variable is coded as 1 for "strongly disagree", 2 for "disagree", 3 for "neutral", 4 for "agree", and 5 for "strongly agree". We also estimate ordered logit models for robustness check. The agreement level of participant i with statement j is represented by the variable y_{ij} and is modeled as:

$$y_{ij} = \gamma_1 S1_{ij} + \gamma_2 S2_{ij} + X_i\beta + \epsilon_{ij} \quad (1)$$

Where $S1_{ij}$ and $S2_{ij}$ are indicators that are equal to one if for statement j participant i received a less-/none-mainstream source, or no source, respectively. The estimated coefficients of interest are $\gamma_1$and γ_2 and measure average difference in agreement level between those who randomly received a less-/non-mainstream source or no source, respectively, compared to those who received a mainstream source. We also include several individual-level control variables (X_i) in some of our specifications.[13] However, if our randomization is carried out properly, including these control variables should not affect our results (and as reported later on we find that they don't).

5. Results

5.1. Main Results

Table 1 displays the results from estimating linear models. Column (1) uses a simplified model with no additional control variables, while columns (2) to (4) add personal and job characteristics as well as individual fixed effects.[14] We find clear evidence that changing source attributions from mainstream to less-/non-mainstream significantly reduces the agreement level by 0.26 points. This is around one-fourth of a standard deviation or a 7 percent reduction in an average agreement level of 3.6 in our sample. Our results also suggest that removing mainstream sources (i.e. providing no source) also significantly reduces the agreement level by 0.41 points (an 1l.3

[13] Our primary control variables include: gender, PhD completion cohort (15 categories), Current Status (8 categories), Country (19 categories), Research Area (18 categories). Additional control variables used in some specifications include age cohort (13 categories), country/region of birth (17 categories), English proficiency (5 categories), department of affiliation (8 categories), country/region where PhD was completed (16 categories). See Table A1 in the online appendix for detail on different categories.

[14] Refer to Table A2 in our online appendix for estimated coefficients of control variables.

percent reduction which is equal to 35% of a standard deviation). As estimates reported in Columns (2), (3), and (4) suggest, controlling for different individual characteristics and individual fixed effects does not change our results, which provides further support that our randomization protocol was implemented properly. In addition, results from our specification with individual fixed effects suggests that our estimates of treatment 1 are unlikely to suffer from sample selection bias due to non-random attrition across treatment groups.

We also estimate the effect of our two treatments on agreement level separately for each statement. These results are summarized in Figure 1. We find that for all but three statements, changing source attributions to a less-/non-mainstream source significantly reduces the agreement level. The estimated reductions range from around one-tenth of a standard deviation to around half of a standard deviation. Interestingly, we find that the largest reduction in agreement level for treatment 1 occurs for Statement 6, which is arguably the statement that is most critical of mainstream economics and its methods, and also brings up the issue of ideological bias in mainstream economics.[15]

Regarding the three statements with no reduction in agreement level for treatment 1 (i.e. statements 1, 3, and 7), one potential explanation is that the (perceived) ideological distance between the sources for each statement is not large enough and therefore switching the source attribution does not affect the agreement level. Taking a closer look at the sources for each statement seems to suggest that this is a plausible explanation. The sources for these statements are Dani Rodrick Vs. Paul Krugman, Hayek Vs. Freud, and Irving Fisher Vs. Kenneth Galbraith. Interestingly and consistent with this explanation, we find that for the same three statements, removing the source attribution (i.e. treatment 2) significantly reduces the agreement level. Perhaps by removing the source participants perceive the source of each statement as someone with strong (ideological) views against mainstream economics (stronger than our altered sources), which makes them more likely to reject the statement.

[15] The statement reads: Economic discourse of any sort - verbal, mathematical, econometric-is rhetoric; that is, an effort to persuade. None of these discursive forms should necessarily be privileged over the others unless it is agreed by the community of scholars to be more compelling. Only when economists move away from the pursuit of universal knowledge of 'the economy' and towards an acceptance of the necessity of vision and the historical and spatial contingency of knowledge will the concern over ideological 'bias' begin to fade. Such a turn would have important implications for economic method as well, as knowledge claims would increasingly find support, not in models of constrained optimization, but with such techniques as case studies and historical analyses of social institutions and politics. Increasing reliance of economics on mathematics and statistics has not freed the discipline from ideological bias, it has simply made it easier to disregard.

Results displayed in the right panel of Figure 1 suggest that removing the source attributions significantly reduces the agreement level for all statements. Similar to our results reported in Table 1, the estimated effects of treatment 2 are larger than treatment 1 in all but two of the statements. The two statements are Statement 8 (Amartya Sen Vs. Michael Sandel) and Statement 10 (Larry Summers Vs. Yanis Varoufakis). It seems that in these two cases the (perceived) ideological distance between sources is so large that induces a reduction in agreement level larger than removing the source.

While OLS estimates are perhaps easier to summarize and report, given the discrete ordered nature of our dependent variable, a more appropriate model to use in this context is an ordered logit model. Another advantage of using ordered logit is that it allows us to examine whether our treatments have heterogeneous effects. Changing a source from mainstream to less-/non-mainstream might have opposite effects on those who (strongly) agree/(strongly) disagree with a statement. For example, on the one hand, those who strongly agree with a statement that is critical of a certain aspect of mainstream economics are more likely to be less-/non-mainstream, and therefore treatment 1 might induce higher agreement among them. On the other hand, those who strongly disagree with the same statement are more likely to be mainstream, and therefore treatment 1 might induce higher disagreement among them.[16] These potential heterogeneous effects will not be captured by our OLS model.

Table 2 reports the estimates from our ordered logit model. Overall, we find results similar to those reported in Table 1 using OLS.[17] Using ordered logit model we find that changing sources from mainstream to less-/non-mainstream, or to no source, significantly increases (decreases) the probability of disagreement (agreement) with statements. More specifically, we find that providing a less-/non-mainstream source on average increases the probability of "strong disagreement" by 2.2 percentage points or 44 percent, increases the probability of "disagreement" by 5 percentage points or 30 percent, increases the probability of reporting "neutral" by 2.1 percentage points or 12.6 percent, reduces the probability of "agreement" by 3.6 percentage points or 9 percent, and

[16] This potential heterogeneous effect might be also responsible for the larger effect estimated for treatment 2. For example, providing a less-/non-mainstream source might generate biases in two directions, with the positive effect on agreement partially canceling out the negative effect on agreement. Removing the source however might only introduce a negative effect on agreement. As a result, our treatment 2 will have a larger effect than treatment 1. Ordered logit model will allow us to examine this possibility as well.

[17] We also estimate multinomial logit models for robustness check. Results from these models are very similar to those from ordered logit models.

reduces the probability of "strong agreement" by 5.7 percentage points or 27 percent. This suggests that regardless of the extent to which people agree or disagree with an statement, changing the source to less-/non-mainstream significantly decreases (increases) their agreement (disagreement) level. In addition, while the effect of treatment 1 on probability of (strong) disagreement is larger relative to its impact on the probability of strong (agreement), we find no evidence of a heterogeneous treatment effect.

Moreover, similar to our linear estimates, we also find larger effects in the same direction when no sources are provided for the statements. Also, in line with our linear estimates, we get almost identical results when we include control variables in our specification. Several other robustness checks performed (see Section 2 in our online appendix) also fully confirm the robustness of our results.

We suggest that the results discussed above are consistent the existence of a ideological bias among economists. The significant reduction in agreement level we find is distinctly driven by changing the source attributions, which influences the way economists perceive the statements. In addition, we suggest that the larger reduction in agreement level induced by removing the source attribution (treatment 2) is potentially driven by a combination of "authority" bias (Milgram 1963) and ideological bias. First of all, the lack of source is likely to induce authority bias against the statement. Then, given that most of evaluated statements are critical of mainstream economics, the participant is likely to guess that the actual source is someone who is considered hostile or strongly critical of mainstream economics. This could induce an ideological bias. When the two biases are added, it leads to an even larger reduction in agreement than when there is only ideological bias (treatment 1). It is important to mention at this point that those randomized into treatment 2 were clearly informed, before starting to evaluate the statements, that "All the statements that you are going to evaluate are made by scholars in economics, and do not necessarily reflect the views of the researchers. We have not provided the actual sources of these statements to make sure they are evaluated based on their content only."

We would also like to highlight that the results reported in Figure 1 suggest that it is not just extreme differences in views (e.g. Smith Vs. Marx) that invoke ideological bias by economists. Even smaller ideological differences (e.g. Deaton Vs. Piketty or Sen Vs. Sandel) seem to invoke strong ideological reactions by economists. In addition, while for most of the statements the estimated effect of treatment 2 is larger than treatment 1, for the two statements that induce the

largest ideological bias (i.e. statements 6 and 9) the estimated effects of treatment 1 and treatment 2 are almost identical. This is consistent with the idea that in cases where the effect of ideological bias is very strong, it dwarfs the effect of authority bias, rendering the effects of both treatments similar in magnitude.

An alternative explanation for our findings is that the reduction in agreement level induced by altering the sources could be driven by differences in the credibility or the reliability of the sources rather than ideological bias induced by differences in their views. We argue, however, that this alternative explanation is not valid. To begin with, our less-/non-mainstream sources are not random people but rather prominent scholars in the field with views that put them at different distances, sometimes relatively close and sometimes rather far, to mainstream economics. Therefore, credibility or reliability of the sources cannot be the explanation for the results.[18] After all, there are no objective scientific measures that could be used to assess the credibility or reliability of these sources. Arguing that those who adhere to the same views as our own are more credible or reliable is a typical manifestation of ideological bias. In addition, several other findings we present below do not support this alternative explanation and further affirm the existence of a strong ideological bias among economists.

Frist, we examine how changing or removing attributed sources influences the participants' reported level of confidence of their evaluations. More specifically, we estimate models similar to those reported in Table 1 but instead use as our dependent variable the level of confidence reported for one's evaluation of a statement (on a scale from 1 to 5, with 1 being least confident and 5 being most confident). If our results are driven by differences in credibility/reliability of the sources, then changing source attributions is expected to affect one's confidence in one's evaluation. In contrast, ideological bias is expected to affect the level of agreement with little or no impact on confidence level. Consistent with the latter hypothesis, we find clear evidence that altering or removing source attributions does not influence participants' confidence in their evaluations. As results reported in Table 3 suggest, the estimated coefficients are quantitatively very small and statistically insignificant.[19]

[18] In addition, given that our participants are economists themselves, they are equipped with the knowledge and training to evaluate the content of most of the statements without depending on an expert's opinion. This further weakens the validity of the credibility/reliability hypothesis.

[19] Results from ordered logit (not reported here) also suggest that our treatments have no impact on confidence level. Our estimated differences in predicted probabilities are all small and statistically insignificant for all five categories.

Second, significant reduction in agreement level induced by changing source attributions is in sharp contrast with the academic standard that participants in our survey hold themselves to. More specifically, as part of the questionnaire that appears at the end of the survey, we ask participants to express their own personal views regarding several issues including how they believe "a claim or argument should be rejected?" Interestingly, a strong majority of participants (around 82%) report that "a claim or argument should be rejected only on the basis of the substance of the argument itself." Around 18% of participants report that "a claim or argument should be rejected based on what we know about the views of the author or the person presenting the argument as well as the substance of the argument." There exists only a tiny minority (around 0.5%) who report "a claim or argument should be rejected based on what we know about the views of the author or the person presenting the argument."

While these views are consistent with the standards one would expect from economists who claim to be objective, solely driven by data and hard evidence, and free from prejudice and ideology, they are in sharp contrast with how economists in our sample actually have evaluated different statements presented to them.[20] This suggests that perhaps part of the ideological bias evident in our results operates in implicit or unconscious modes. Bertrand and Duflo (2017) highlight the importance of implicit bias by pointing out that "[m]odern social psychologists believe that attitudes can occur in implicit modes and that people can behave in ways that are unrelated or even sometimes opposed to their explicit views or self-interests."

Third, we examine how our results vary by self-reported political orientation of participants. Our less-/non-mainstream sources often represent views or ideologies that are (politically) to the left of mainstream sources. Therefore, if our results are driven by ideological bias, then reduction in agreement level should be larger among those more to the right of the political spectrum since altering the sources should potentially induce a larger ideological reaction among this group. At the same time, in the absence of ideological bias, the perception of scientific credibility/reliability of different sources should not produce systematically different results across economists with different political leanings. Therefore, our estimates should not vary systematically by political orientation under this alternative hypothesis. In fact, if anything, those

[20] We also examine differences in our estimates between those who claim statements should be evaluated only based on their content relative to those who claim both the content and the views of the author matter. We find almost identical estimates for treatment 1 (-0.22 versus -0.20) and treatment 2 (-0.34 versus -0.35) for both groups. This suggests that our results are not driven by one group.

on the right should be less affected by changing the source attributions since they are significantly more likely to report that a statement should be evaluated based on its content only. More specifically, among those on the far right, 86.7 percent of participants report that in evaluating an statement only its content matters, while 13.3 percent report that both content as well as what we know about the views of the author matter. In contrast, among those on the far left, these numbers are 73.8 percent and 25.1 percent, respectively.

We estimate linear models similar to Equation (1) above where we allow the effect of each treatment to vary by political orientation. Political orientation is reported by participants on a scale from -10 (far left) to 10 (far right). We use the reported values to group people into 5 categories.[21] Results from this model are reported in the first three columns of Table 4 and suggest that, for those on the far left, altering the source only reduces the average agreement level by 0.05 points of a standard deviation, which is less than one-fourth of the overall effect we reported in Table 1 (0.223). Moving from the far left to the far right of the political orientation distribution, however, consistently increases this effect, with the effect of altering the source being 6 times larger at the far right compared to the far left (-0.31 versus -0.050, respectively). We reject, at 0.1% confidence level, both the null hypothesis that the effect at the far left is equal to the effect at the far right, and that the effects are equal across all five groups (F-statistics are 39.7 and 14.57, respectively). As estimates reported in columns (4) to (6) suggest, our results remain the same if we categorize people based on quintiles of the political orientation distribution. Moreover, including additional control variables do not affect our results.[22]

It is reasonable to argue, however, that the self-reported measure of political orientation used to categorize people depends on political environments and contexts that could vary significantly from one country to another. For example, someone who is considered a centrist or centre-right in the UK could be perhaps categorized as left in the US. This could complicate the interpretation of these results. To address this issue, we use participants' answers to a series of questions that they were presented with at the end of our survey to help us identify their political typology.[23] More specifically, we regress our political orientation measure on a series of indicators created based on answers to these questions. We then use predicted values from this regression to

[21] Far left = [-10 -7], Left = [-6 -2], Centre = [-1 1], Right = [2 6], Far Right = [7 10].
[22] These results are not reported here, but are available upon request.
[23] Participants were asked to read a series of binary statements and for each pair pick one that comes closest to their view. See Table A9 in our online appendix for a list of these statements.

categorize people into five groups based on its distribution quintiles. Results reported in Columns (7) to (9) of Table 4 are based on this alternative categorization and remain similar to those reported in other columns.

Fourth, as it is discussed in the next section, we find evidence of significant and systematic heterogeneity in our estimated effects by gender, country of residence, country where PhD was completed, undergraduate major, and main research area. We cannot square these results with the credibility/reliability hypothesis since we cannot think of compelling reasons to explain why people from a certain gender or from certain countries would care more about scientific credibility/reliability of sources and therefore be more strongly affected by it. However, as we discuss in more detail in the following section, these differences by personal characteristics remain consistent with the existence of ideological bias among economists.

In light of our discussion above, and for the remainder of the paper, we refer to estimated effect of treatment 1 as the effect of ideological bias. We also refer to the estimated effect of treatment 2 as the effect of authority bias. We recognize however that the estimated effect of treatment 2 could be driven by a combination of authority bias and ideological bias, and use the term authority bias as a simple proxy.

5.2. Heterogeneity Analysis

It is interesting and important to understand how the biases we find in our analysis and discussed above vary across different groups. This could help to shed more light on the driving forces behind these biases. Therefore, In this section, we examine how our results vary by different characteristics including gender, country, country/region where PhD was completed, main research area, and undergraduate major. We use OLS for our heterogeneity analysis since OLS estimates are easier to summarize and present, and since they are similar to estimates from ordered logit models as reported before.

First, we examine gender differences in our estimates. These results are reported in Table 5 and suggest that on average female economists who are randomized into control group agree less with our statements compared to their male counterparts in the control group. The estimated difference in agreement level is around 6 percent of a standard deviation. In addition, we find that the estimated ideological bias is almost twice as large among male economists compared to their female counterparts (24% of a standard deviation reduction in agreement level versus 14%, respectively), a difference that is statistically significant at 0.1% confidence level. Gordon an Dahl

(2013) also find evidence that suggests male economists are less cautious to express an opinion. This seems to be consistent with stronger ideological bias among male economists found in our results since ideological bias and assigning high levels of certainly to our own views usually work hand in hand.

We find, however, that the gender difference in authority bias is much smaller (33% of a standard deviation reduction for males versus 36% for females) and statistically insignificant. This is consistent with our hypothesis above that the reduction in agreement level resulting from removing source attributions does not seem to be solely driven by ideological bias, and there are other factors such as authority bias that could play a role in this reduction. For example, although we find that females are less ideologically biased, if they are more strongly influenced by authority bias, then the combination of the two effects resulting from removing source attributions could lead to small differences between men and women.

We find similar results when we estimate gender differences in treatment effects separately for each statement. In 9 out of 15 statements, the estimated ideological bias is larger for men than for women, while the results are more mixed for our estimates of authority bias (see Figure A2 in our online appendix). We would like, however, to highlight the estimated gender difference in ideological bias for Statement 5 which involves the issue of gender gap in economics.[24] Overall, and without considering group assignment, there exists a very large difference in the level of agreement with this statement between male and female economists. More specifically, conditional on observed characteristics, the average agreement level among male economists is 0.78 points lower than female economists, a very large difference that is around 2/3rd of a standard deviation and statistically significant at 0.1%. Taking group assignment into account, female economists who randomly receive *Carmen Reinhart* as the statement source (i.e. control group) report an agreement level that is on average 0.73 points higher compared to their male counterparts in the control group. Moreover, while switching the source from *Carmen Reinhart* to *Diane Elson* does not affect the agreement level among female economists (estimated effect is 0.006), it significantly

[24] The statement reads: "Unlike most other science and social science disciplines, economics has made little progress in closing its gender gap over the last several decades. Given the field's prominence in determining public policy, this is a serious issue. Whether explicit or more subtle, intentional or not, the hurdles that women face in economics are very real." The actual (mainstream) source of the statement is *Carmen Reinhart, Professor of the International Financial System at Harvard Kennedy School and the author of This Time is Different: Eight Centuries of Financial Folly (2011)*. The altered (less-/non-mainstream) source of the statement is *Diane Elson, British Economist and Sociologist, Professor Emerita at the University of Essex, and the author of Male bias in the development process (1995)*.

decreases the agreement level among male economists by 0.175 points (around 15% of a standard deviation). We believe this difference highlights a large gap between male and female economists in their perception and concerns regarding the gender gap in economics.[25] We believe this is of critical importance since approaching and solving the issue of gender gap requires similar understanding of the problem by both men and women. Moreover, it seems that when it comes to the important issue of gender gap in economics which involves female economists at a personal level, women put aside ideology and merely focus on the content of the statement as opposed to its source.

Next, we examine how our estimated effects vary by country of residence. These results are reported in Table 6. For both treatments, we find that the estimated effects vary significantly across countries, ranging from around half of a standard deviation to zero. We also reject the null that the estimated effects of treatment 1 (treatment 2) are the same across countries at 0.1% confidence level. More specifically, we find that economists in Austria, Brazil, and Italy exhibit the smallest ideological bias (for Brazil and Austria the estimated effects are also statistically insignificant). On the other side of the spectrum we find economists in Ireland, Japan, Australia, and Scandinavia who exhibit the largest ideological bias. Economists in countries such as Canada, the UK, France, and the US stand in the middle in terms of the magnitude of the estimated ideological bias. In addition, when we examining the effect of authority bias, these countries maintain their positions in the distribution, although the estimated effects of authority bias remain larger than estimated effects of ideological bias for most of the countries. This suggests again that while ideological bias is a driving force behind estimated effect of treatment 2, there are other factors such as authority bias that also come into play when we remove source attributions.

Table 7 reports results that examine heterogeneity by country/region where PhD was completed. We find that economists who completed their PhD in Asia, Canada, Scandinavia, and the US exhibit the strongest ideological bias, ranging from 0.43 to 0.24 of a standard deviation. On the opposite end we find economists with PhD from South America, Africa, Italy, Spain and Portugal exhibiting the smallest ideological bias (Statistically insignificant for South America and Africa). These results are somehow consistent with those reported in Table 6 and suggest that some of the countries where economists exhibit the largest/smallest ideological bias are also those that

[25] Gender differences in perception of gender discrimination is not limited to economics and has been documented in other studies and in various contexts (e.g. Fisman and O'neill 2008, Miller and Katz 2018, Raggins et al. 1998).

induce strongest/weakest ideological bias in their PhD students (e.g. Brazil, Italy, Scandinavia). In addition, we find that our estimated effects of authority bias, while larger in size, largely follow the same patterns as our estimates of ideological bias.

In Table 8 we take up the issue of heterogeneity by the main area of research. Not surprisingly, we find that economists whose main area of research is history of thought, methodology, heterodox approaches; cultural economics, economic sociology, economic anthropology; or business administration, marketing, accounting exhibit the smallest ideological bias, as well as authority bias.[26] We find however that economists whose main area of research is macroeconomics, public economics, international economics, or financial economics are among those with the largest ideological bias. Another interesting point to highlight is that while for economists in all research areas the estimated effect of ideological bias is (significantly) smaller than the estimated effect of authority bias, macroeconomists are the only group for whom the estimated ideological bias is significantly larger than the estimated authority bias (1/3rd versus 1/5th of a standard deviation). This is potentially driven by the fact that our altered sources induce a stronger ideological reaction in macroeconomists than when we remove the sources altogether.

Lastly, we examine heterogeneity by undergraduate major. As we discussed before, there exists evidence that suggests economic training, either directly or indirectly, could induces ideological views in students (e.g. Allgood et al. 2012, Colander and Klamer 1987, Colander 2005, Rubinstein 2006). Consistent with these studies, we find that economists whose undergraduate major was economics or business/management exhibit the strongest ideological bias (around 1/4th of a standard deviation). However, we find that economists with undergraduate major in law; history, language, and literature; or anthropology, sociology, psychology, exhibit the smallest ideological bias (statistically insignificant in all three cases).[27]

6. Conclusion

Using a randomized controlled online experiment involving economists in nineteen countries, we find clear evidence of a strong ideological bias among economists. Our results suggest that economists' reported level of agreement with statements is significantly reduced when statements are randomly (mis)attributed to authors with views or ideologies that put them at

[26] For the latter group, this could be driven by the fact that they are not very familiar with where different sources stand in relation to mainstream economics and their ideology.

[27] Of course, this systematic difference could be driven by self-selection of individuals into different undergraduate majors and is not necessarily causal.

different distances to mainstream economics, even when this distance is relatively small. In addition, we find that removing the source attribution also significantly reduces the agreement level with statements. We attribute this reduction to a combination of ideological bias and authority bias. We also find that in most cases this latter effect is larger than the effect of altering the source.

Three additional findings in our analysis provide further evidence that our results are driven by ideological bias. First, despite the significant negative effect on agreement level, we find that changing or removing source attributions does not affect economists' level of confidence with their evaluations of the statements. Second, we also find that our results are in sharp contrast with the scientific standard that a dominant majority of our participants (82 percent) set for themselves, which is "a claim or argument should be rejected only on the basis of the substance of the argument itself." This suggests that perhaps part of the ideological bias evident in our results operates in implicit or unconscious modes. Third, we find that the reduction in agreement level resulting from altering the source attributions is systematically and significantly affected by political orientation of participants in a way that is consistent with ideological bias.

We also find systematic and significant heterogeneity in our estimates of ideological bias by gender, country, country/region where PhD was completed, area of research, and undergraduate major. Our results suggest that while economists with certain characteristics exhibit very strong ideological bias, others display very little or no ideological bias.

We believe whether or not one believes that economics can be a 'science' in the strict sense and free from ideological underpinnings, the type of ideological bias that we have found in this study, namely people endorsing or denouncing an argument on the basis of its author's identity rather than its substance, is unhealthy for the integrity of any academic discipline. This type of ideological bias could impede the engagement with alternative views, narrow the pedagogy, and bias and delineate research parameters. We believe finding out about our own biases, especially when there exists evidence that suggests they could operate through implicit or unconscious modes, is certainly a positive outcome and the first step for economists who strive to be objective and ideology-free. This is also consistent with the standard most economists in our study hold themselves to. We believe that the persistent denial of these biases could be more harmful than being aware of their presence and influence. To echo again the words of Alice Rivlin in her 1987 American Economic Association presidential address, "economists need to be more careful to sort out, for ourselves and others, what we really know from our ideological biases."

Another important step to minimize the influence of our ideological biases is to understand their roots. As argued by prominent social scientists (e.g. Althusser 1976, Foucault 1969, Popper 1955, Thompson 1997), the main source of ideological bias is knowledge-based, influenced by the institutions that produce discourse. For example, Colander and Klamer (1987) and Colander (2005) survey graduate students at top-ranking graduate economic programs in the US and find that according to these students, techniques are the key to success in graduate school, while understanding the economy and knowledge about economic literature only help a little. Similarly, Rubinstein (2006) suggests that "students who come to us to 'study economics' instead become experts in mathematical manipulations." This lack of depth in knowledge acquired, not only in economics but also in any discipline or among any group of people, make individuals to lean more on ideology. This highlights the importance of economic training as perhaps the most influential factor in shaping ideological views among economists. It forms economists' views, and affects the way they process information, identify problems, and approach these problems in their research. In addition, not surprisingly, this training may also affect the policies they favour, and the ideologies they adhere to. Moreover, as Colandar (2005) points out, over time these influences are passed on from one cohort of graduate students to the next. He argues that "In many ways, the replicator dynamics of graduate school play a larger role in determining economists' methodology and approach than all the myriad papers written about methodology."

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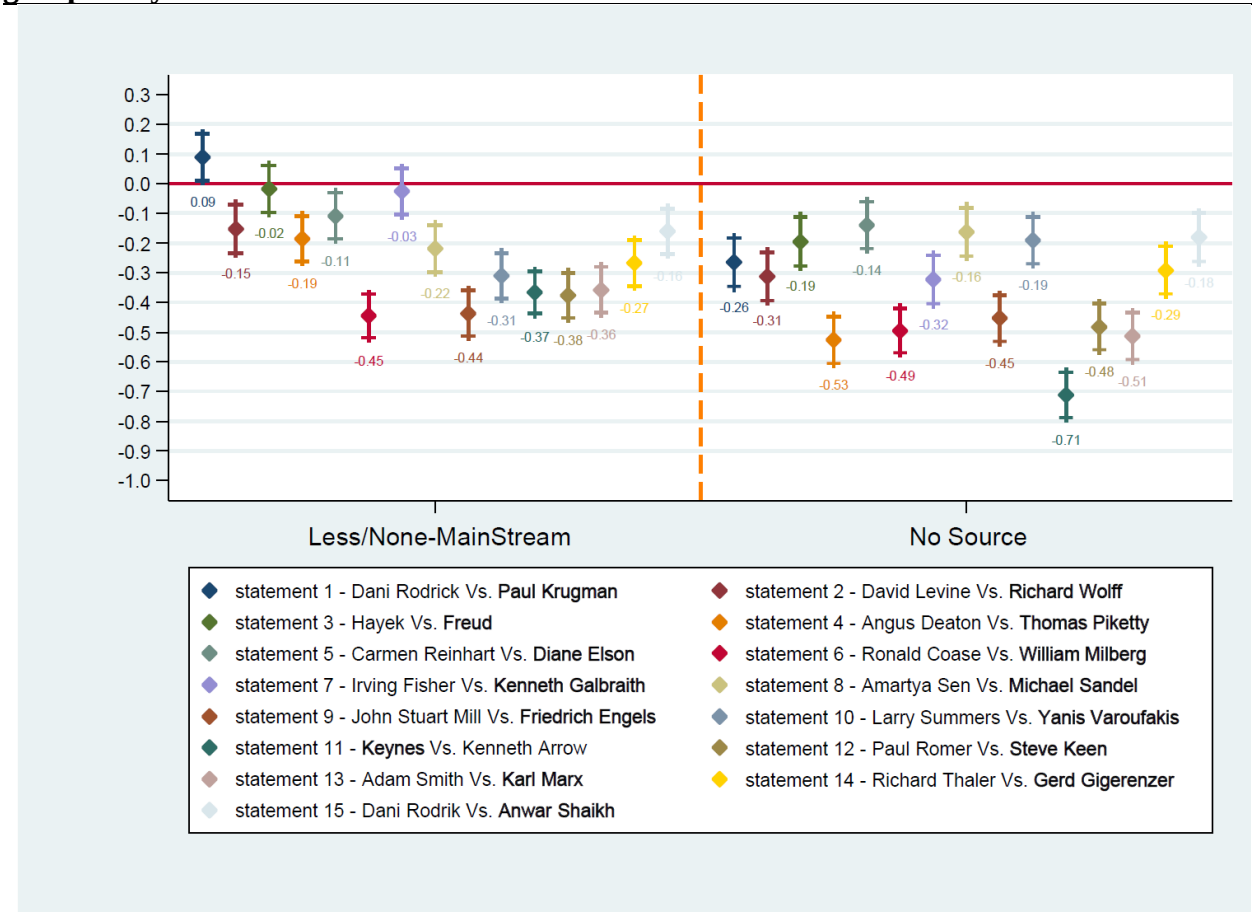
Tables and Figures

Figure 1: OLS estimates of differences in agreement level between control and treatment groups – By statement



Note: Agreement levels is z-normalized for each statement. Control variables include: gender, PhD completion cohort, current status, country, research area. Both 90% and 95% confidence intervals are displayed for each estimate. The two horizontal lines on each confidence interval band represent where the 90% confidence interval ends.
First (second) listed source for each statement is the actual (altered) source. Bold source for each pair refers to the less-/non-mainstream source. See Table A8 in our online appendix for more details.

Table 1: OLS Estimates of differences in agreement level between control and treatment groups

A: In Units of Agreement Level	(1)	(2)	(3)	(4)
Treatment 1 (none-/less-mainstream source)	-0.264***	-0.261***	-0.262***	-0.268***
	(0.014)	(0.014)	(0.014)	(0.014)
Treatment 2 (no source)	-0.415***	-0.404***	-0.406***	†
	(0.015)	(0.015)	(0.015)	
B: In Units of Standard Deviation				
Treatment 1 (none-/less-mainstream source)	-0.223***	-0.220***	-0.221***	-0.226***
	(0.012)	(0.012)	(0.012)	(0.012)
Treatment 2 (no source)	-0.350***	-0.341***	-0.343***	†
	(0.012)	(0.012)	(0.012)	
P-value: Treatment 1 = Treatment 2	0.000	0.000	0.000	NA
Controls	No	Yes	No	No
More Control	No	No	Yes	No
Fixed Person Effects	No	No	No	Yes
Number of observations	36375	36375	36375	25185

Note: Omitted category is receiving a mainstream source . Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree). For panel (B), the dependent variable is standardized to have mean zero and standard deviation of one. The average agreement level in our sample is 3.35 with standard deviation of 1.185. Significance levels: *** < 1%, ** < 5%, * < 10%.

Controls include: gender, PhD completion cohort, current status, country, research area. More Controls include all the previously listed variables as well as age cohort, country/region of birth, English proficiency, department of affiliation, country where PhD was completed.

† We cannot identify the effect of treatment 2 in models with individual fixed effects since those who are sorted into treatment 2 receive all statements without a source and therefore there is no variation in treatment within a person and across statements. We therefore exclude these participants from the fixed effects model.

Table 2: Ordered logit estimates of differences in agreement level between control and treatment groups

Panel A: Without Controls	Outcome: Strongly disagree	Disagree	Neutral	Agree	Strongly agree
Predicted probability of outcome	0.050***	0.168***	0.166***	0.403***	0.212***
Control Group (mainstream source)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
Difference in predicted probability	0.022***	0.050***	0.021***	-0.036***	-0.057***
mainstream Vs. less-/non-mainstream	(0.001)	(0.003)	(0.001)	(0.002)	(0.003)
Difference in predicted probability	0.039***	0.083***	0.029***	-0.067***	-0.085***
mainstream Vs. no source	(0.001)	(0.003)	(0.001)	(0.002)	(0.003)
Panel B: With Controls	**Outcome: Strongly disagree**	**Disagree**	**Neutral**	**Agree**	**Strongly agree**
Predicted probability of outcome	0.048***	0.166***	0.169***	0.411***	0.206***
Control Group (mainstream source)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Difference in predicted probability	0.021***	0.051***	0.022***	-0.038***	-0.056***
mainstream Vs. less-/non-mainstream	(0.001)	(0.003)	(0.001)	(0.002)	(0.003)
Difference in predicted probability	0.037***	0.083***	0.030***	-0.068***	-0.082***
mainstream Vs. no source	(0.001)	(0.003)	(0.001)	(0.002)	(0.003)
Number of observations	36375	36375	36375	36375	36375

Note: Robust standard errors are reported in parentheses. Significance levels: *** < 1%, ** < 5%, * < 10%.
The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree).
Controls include: gender, PhD completion cohort, current status, country, research area.

Table 3: OLS estimates of differences in confidence level

A: In Units of Confidence Level	(2)	(4)
Treatment 1 (none-/less-mainstream source)	0.005	0.008
	(0.011)	(0.010)
Treatment 2 (no source)	-0.019	†
	(0.012)	
B: In Units of Standard Deviation		
Treatment 1 (none-/less-mainstream source)	0.006	0.009
	(0.012)	(0.011)
Treatment 2 (no source)	-0.020	†
	(0.013)	
P-value: treatment 1 = treatment 2	0.037	NA
Controls	Yes	No
Fixed Person Effects	No	Yes
Number of observations	36088	24984

Note: Omitted category is Control Group (i.e. mainstream source). Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is confidence level with evaluation on a scale from 1 (least confident) to 5 (most confident). For panel (B), the dependent variable is standardized to have mean zero and standard deviation of one. The average confidence level in our sample is 3.93 with standard deviation of 0.928. Since confidence level was voluntary to report in our survey, compared to agreement level regressions we lose a small number of observations were confidence level is not reported. Significance levels: *** < 1%, ** < 5%, * < 10%.
Controls include: gender, PhD completion cohort, current status, country, research area.
† We cannot identify the effect of treatment2 in fixed effects model since those who are sorted into this group receive all statements without a source and therefore there is no variation in treatment within a person and across statements. We therefore exclude these participants from the fixed effects model.

Table 4: OLS Estimates of differences in agreement level between control and treatment groups – By political orientation

	Main Results			Robustness 1			Robustness 2		
	Author-created categories			Categories by quintiles of political orientation			Categories by quintiles of *adjusted* political orientation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Control group	Treatment 1	Treatment 2	Control group	Treatment 1	Treatment 2	Control group	Treatment 1	Treatment 2
Far Left		-0.050*	-0.355***		-0.073***	-0.357***		-0.080***	-0.261***
		(0.027)	(0.030)		(0.023)	(0.025)		(0.025)	(0.027)
Left	0.109***	-0.241***	-0.346***	0.044*	-0.244***	-0.282***	0.098***	-0.237***	-0.427***
	(0.023)	(0.019)	(0.019)	(0.026)	(0.030)	(0.030)	(0.024)	(0.025)	(0.027)
Center	0.130***	-0.286***	-0.411***	0.095***	-0.291***	-0.413***	0.113***	-0.262***	-0.389***
	(0.026)	(0.026)	(0.029)	(0.024)	(0.026)	(0.027)	(0.028)	(0.032)	(0.031)
Right	0.136***	-0.315***	-0.333***	0.092***	-0.283***	-0.426***	0.098***	-0.274***	-0.381***
	(0.029)	(0.031)	(0.032)	(0.024)	(0.027)	(0.029)	(0.025)	(0.026)	(0.029)
Far Right	0.138***	-0.319***	-0.349***	0.074***	-0.310***	-0.333***	0.113***	-0.312***	-0.362***
	(0.043)	(0.054)	(0.057)	(0.026)	(0.030)	(0.031)	(0.026)	(0.027)	(0.028)
P-value of equality	0.57	0.000	0.372	0.238	0.000	0.003	0.907	0.000	0.000
F-statistic of equality	0.637	14.57	1.06	1.41	14.76	3.96	0.18	11.49	5.10
# observations		36315			36315			36315	

Note: Control group refers to receiving a mainstream source. Treatment 1 refers to receiving a less-/non-mainstream source. Treatment 2 refers to receiving no source. Omitted category is Far Left & control group. Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree) and is z-normalized for each group. Political orientation is self-reported by participants on a scale from -10 (far left) to 10 (far right). Significance levels: *** < 1%, ** < 5%, * < 10%. Controls include: gender, PhD completion cohort, current status, country, research area.

For Columns (1) to (3), we use self-reported political orientation to group participants into 5 categories: Far left = [-10 -7], Left = [-6 -2], Centre = [-1 1], Right = [2 6], Far Right = [7 10]. Results reported in Columns (4) to (9) are for robustness check. For results reported in columns (4) to (6), we create the five political groups using the quintiles of political orientation distribution. For results reported in columns (7) to (9), we create the five political groups using the quintiles of the adjusted political orientation distribution. Adjusted political orientation measure is created by running a regression of self-reported political orientation on a series of indicators based on questions asked from participants to identify their political typology. See Table A9 in our online appendix for more details.

Table 5: OLS Estimates of gender differences in agreement level between control and treatment groups

	(1)	(2)	(3)
	Control group	Treatment 1	Treatment 2
Male		-0.242***	-0.337***
		(0.013)	(0.014)
Female	-0.063***	-0.140***	-0.363***
	(0.020)	(0.025)	(0.027)
P-value: equality of coefficients		0.000	0.395
F-statistic: equality of coefficients		12.32	0.72
Number of observations		36375	

Note: Control group refers to receiving a mainstream source. Treatment 1 refers to receiving a less-/non-mainstream source. Treatment 2 refers to receiving no source. Omitted category is male & control group. Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree) and is z-normalized for each gender. Significance levels: *** < 1%, ** < 5%, * < 10%.
Controls include: PhD completion cohort, current status, country, research area.

Table 6: OLS Estimates of differences in agreement level between control and treatment groups – By country

	(1) Control Group	(2) Treatment 1	(3) Treatment 2
Australia		-0.327***	-0.539***
		(0.056)	(0.059)
Austria	-0.266***	0.022	-0.077
	(0.082)	(0.104)	(0.102)
Brazil	-0.333***	0.016	0.015
	(0.072)	(0.084)	(0.105)
Canada	-0.026	-0.284***	-0.399***
	(0.045)	(0.035)	(0.037)
France	-0.099**	-0.223***	-0.366***
	(0.047)	(0.041)	(0.042)
Germany	-0.130**	-0.181***	-0.240***
	(0.055)	(0.061)	(0.065)
Ireland	0.009	-0.458***	-0.445***
	(0.116)	(0.154)	(0.157)
Italy	-0.195***	-0.124***	-0.261***
	(0.048)	(0.042)	(0.045)
Japan	-0.012	-0.397***	-0.402***
	(0.065)	(0.081)	(0.081)
Netherlands	-0.074	-0.264***	-0.134*
	(0.068)	(0.081)	(0.079)
New Zealand	-0.054	-0.237***	-0.355***
	(0.071)	(0.082)	(0.087)
Scandinavia	-0.051	-0.321***	-0.427***
	(0.050)	(0.047)	(0.053)
South Africa	-0.049	-0.127	-0.359***
	(0.087)	(0.116)	(0.106)
Switzerland	0.042	-0.298***	-0.464***
	(0.078)	(0.101)	(0.098)
UK	-0.032	-0.229***	-0.392***
	(0.052)	(0.051)	(0.052)
US	-0.076*	-0.206***	-0.337***
	(0.040)	(0.020)	(0.020)
P-value: equality of coefficients	0.000	0.000	0.000
F-statistic: equality of coefficients	3.80	2.63	3.34
Number of observations		36375	

Note: Control group refers to receiving a mainstream source. Treatment 1 refers to receiving a less-/non-mainstream source. Treatment 2 refers to receiving no source. Omitted category is Australia & control group. Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree) and is z-normalized for each country. Significance levels: *** < 1%, ** < 5%, * < 10%.
Controls include: gender, PhD completion cohort, current status, research area.

Table 7: OLS Estimates of differences in agreement level between control and treatment groups – By country/region where PhD was completed

	(1) Control Group	(2) Treatment 1	(3) Treatment 2
Africa		-0.103	-0.305**
		(0.129)	(0.128)
Asia	0.297**	-0.433***	-0.392***
	(0.125)	(0.108)	(0.101)
Canada	0.333***	-0.321***	-0.471***
	(0.107)	(0.046)	(0.052)
Europe 1 (France, Belgium)	0.118	-0.166***	-0.265***
	(0.109)	(0.040)	(0.042)
Europe 2 (Germany, Austria, Netherlands,	0.243**	-0.208***	-0.278***
Switzerland, Luxembourg)	(0.108)	(0.044)	(0.042)
Europe 3 (Italy, Spain, Portugal)	0.012	-0.116**	-0.233***
	(0.108)	(0.048)	(0.055)
Europe 4 (Denmark, Finland, Norway, Sweden)	0.338***	-0.314***	-0.465***
	(0.110)	(0.058)	(0.061)
Europe 5 (UK, Ireland)	0.219**	-0.181***	-0.338***
	(0.106)	(0.045)	(0.046)
Not Applicable	0.020	-0.188***	-0.384***
	(0.115)	(0.044)	(0.046)
Oceania	0.255**	-0.188**	-0.331***
	(0.116)	(0.081)	(0.079)
Other	0.286*	-0.090	-0.884***
	(0.150)	(0.182)	(0.210)
South America	0.134	0.013	-0.040
	(0.146)	(0.112)	(0.126)
United States	0.275***	-0.245***	-0.362***
	(0.102)	(0.018)	(0.019)
P-value: equality of coefficients	0.000	0.008	0.000
F-statistic: equality of coefficients	2.83	2.23	6.41
Number of observations		36375	

Note: Control group refers to receiving a mainstream source. Treatment 1 refers to receiving a less-/non-mainstream source. Treatment 2 refers to receiving no source. Omitted category is Africa & control group. Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree) and is z-normalized for each country/region. Significance levels: *** < 1%, ** < 5%, * < 10%. Controls include: gender, PhD completion cohort, current status, country, research area. "Other" category includes Central America, Eastern Europe, Rest of Europe, Middle East, The Caribbean. Due to very small cell size for these countries/regions (135 observations in total), we have put them all in one category.

Table 8: OLS Estimates of differences in agreement level between control and treatment groups – By research area

	(1) Control Group	(2) Treatment 1	(3) Treatment 2
Teaching		-0.140**	-0.404***
		(0.060)	(0.060)
History of Thought, Methodology,	-0.115**	-0.111**	-0.236***
Heterodox Approaches	(0.053)	(0.049)	(0.053)
Mathematical and Quantitative Methods	-0.011	-0.265***	-0.308***
	(0.053)	(0.047)	(0.049)
Microeconomics	0.005	-0.223***	-0.368***
	(0.050)	(0.042)	(0.042)
Macroeconomics and Monetary Economics	-0.034	-0.334***	-0.198***
	(0.048)	(0.036)	(0.037)
International Economics	0.032	-0.269***	-0.495***
	(0.051)	(0.045)	(0.049)
Financial Economics	-0.037	-0.265***	-0.274***
	(0.059)	(0.063)	(0.061)
Public Economics	0.005	-0.301***	-0.315***
	(0.052)	(0.045)	(0.048)
Health, Education, and Welfare	0.010	-0.233***	-0.496***
	(0.053)	(0.049)	(0.056)
Labor and Demographic Economics	-0.022	-0.212***	-0.366***
	(0.048)	(0.037)	(0.040)
Law and Economics	-0.063	-0.218**	-0.367***
	(0.078)	(0.102)	(0.111)
Industrial Organization	-0.012	-0.255***	-0.338***
	(0.056)	(0.055)	(0.060)
Economic Development, Innovation,	-0.010	-0.149***	-0.495***
Technological Change	(0.050)	(0.042)	(0.043)
Agricultural and Natural Resource Economics	-0.053	-0.171***	-0.372***
	(0.051)	(0.043)	(0.045)
Urban, Rural, Regional, Real Estate,	-0.054	-0.126	-0.335***
and Transportation Economics	(0.069)	(0.079)	(0.080)
Cultural Economics, Economic Sociology,	-0.297***	-0.0736	0.0885
Economic Anthropology	(0.113)	(0.164)	(0.173)
Business Administration, Marketing, Accounting	-0.024	-0.0736	0.0885
	(0.079)	(0.164)	(0.173)
Other	-0.288***	-0.237**	-0.496***
	(0.065)	(0.103)	(0.119)
P-value: equality of coefficients	0.000	0.006	0.000
F-statistic: equality of coefficients	2.81	2.04	4.64
Number of observations		36375	

Note: Control group refers to receiving a mainstream source. Treatment 1 refers to receiving a less-/non-mainstream source. Treatment 2 refers to receiving no source. Omitted category is Teaching & control group. Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree) and is z-normalized for each research area. Significance levels: *** < 1%, ** < 5%, * < 10%.
Controls include: gender, PhD completion cohort, current status, country.

Table 9: OLS Estimates of differences in agreement level between control and treatment groups – By undergraduate major

	(1) Control Group	(2) Treatment 1	(3) Treatment 2
Other Social Sciences		-0.062	-0.236*
(Anthropology, Sociology, Psychology)		(0.104)	(0.123)
Business, Management	0.147*	-0.223***	-0.262***
	(0.084)	(0.050)	(0.054)
Biology, Chemistry, Physics	0.206**	-0.137*	-0.328***
	(0.094)	(0.078)	(0.084)
Computer Science, Engineering	0.246***	-0.133*	-0.346***
	(0.093)	(0.074)	(0.077)
Earth and space sciences, Geography	0.301***	-0.195**	-0.438***
	(0.097)	(0.092)	(0.087)
Economics	0.224***	-0.254***	-0.348***
	(0.077)	(0.015)	(0.016)
History, Language and literature	-0.018	0.001	-0.058
	(0.102)	(0.092)	(0.093)
Law	-0.106	0.079	-0.078
	(0.143)	(0.186)	(0.310)
Mathematics, Statistics	0.216***	-0.154***	-0.272***
	(0.082)	(0.043)	(0.044)
Philosophy, Political Science, International Affairs	0.180**	-0.195***	-0.407***
	(0.086)	(0.059)	(0.066)
Agricultural/Environmental Sciences	0.192*	-0.118	-0.567***
	(0.104)	(0.097)	(0.103)
Other	-0.107	0.179	-0.044
	(0.161)	(0.192)	(0.175)
Not Reported	0.234***	-0.226***	-0.429***
	(0.083)	(0.048)	(0.047)
P-value: equality of coefficients	0.001	0.023	0.021
F-statistic: equality of coefficients	2.97	2.07	2.09
Number of observations		36375	

Note: Control group refers to receiving a mainstream source. Treatment 1 refers to receiving a less-/non-mainstream source. Treatment 2 refers to receiving no source. Omitted category is Other Social Sciences & control group. Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree) and is z-normalized for each undergraduate major. Significance levels: *** < 1%, ** < 5%, * < 10%. Controls include: gender, PhD completion cohort, current status, country, research area.

Online Appendix

Who Said or What Said? Estimating Ideological Bias in Views Among Economists

August 2018

1. Attrition and sample selection

As discussed in the main text, we exclude from our final sample participants who did not complete the entire survey. Table A3 reports the numbers of people who reached different levels of progress in the survey before withdrawing from the survey. If withdrawal from the survey was non-random across our three groups, our results could potentially suffer from sample selection bias. This could happen, for example, if people who are relatively more ideologically biased are also more likely to withdraw from the survey if they receive statements with no source attribution or less-/non-mainstream source attributions. We perform several tests to examine the possibility of sample selection bias in our results.

First, we estimate a discrete time proportional hazard model (complementary log-log), using a fully non-parametric baseline hazard to examine how survival rate (not quitting the survey) changes as participants progress through the survey and whether it differs for participants randomized into different groups. These results are presented in Figure A1 and each point estimates the rate of withdrawal from the survey at statement j given that it has not occurred before statement j. Our results clearly suggests that the withdrawal rate is almost identical at different stages of the survey for those who are randomized into control group (mainstream source) and treatment 1 (less-/non-mainstream source). This suggests that estimates comparing control group with treatment 1 (which we refer to in the main text as ideological bias) are unlikely to suffer from sample selection due to the sample restriction imposed. This is also consistent with our estimates from models with individual fixed effects discussed in the main text (Table 1, Column 4). These estimates are not affected by sample selection since they are identified by variations in treatment within an individual and across statements, and are identical to estimates from our OLS models.

Results reported in Figure 1 suggest however that those who were randomized into treatment 2 (no source) have a slightly higher withdrawal rate. This could be potentially due to the fact that the absence of a source or authority behind statements that are critical of mainstream economics could make participants more likely to stop responding to the survey. Assuming that those who are relatively more ideologically-biased and less tolerant of alternative views are more likely to withdraw in this situation, this higher withdrawal rate will result in an underestimation of the true difference between treatment 2 and control group (which we refer to in the main text as authority bias). In other words, our estimated effects of treatment 2 could be thought of as conservative estimates or lower-bounds of the true bias.

As our second test, we examine whether the number of mainstream versus less-/non-mainstream sources received by a participant affects the probability of withdrawal. As it was mentioned in the main text, participants who were randomized into control group or treatment 1 for the first statement were re-randomized between these two groups for each subsequent statement. Therefore, as they progress through the survey they will receive some of the statements with mainstream sources and some of the statements with less-/non-mainstream sources. Results from this second test are reported in Table A4. We find that receiving more mainstream or less-/non-mainstream sources reduces the probability of quitting. This is to be expected and consistent with our results in Figure A1 that as participants progress through the survey their probability of completing the entire survey (quitting the survey) increases (decreases). Our estimates in Table A4 clearly suggest, however, that there are no statistically significant differences in reduction in probability of withdrawal between control group and treatment 1. In other words, receiving more mainstream or non-mainstream sources does not make participants to withdraw at a higher rate. Therefore, similar to the first test, these results suggest that our estimated differences between control group and treatment 1 are unlikely to suffer from sample selection.

Alternatively, one could argue that, while people randomized into different groups might have similar withdrawal rates, the amount of cognitive effort participants put in to evaluate statements might be systematically different across groups. While we believe that this is a manifestation of ideological bias, nonetheless we run a test where we take the survey completion time as a proxy for effort invested for cognitive evaluation and examine whether it is different across different groups. These results are reported in Table A5 and suggest that there are no differences in average survey completion time between control group and treatment 1. We find those randomized into treatment 2 on average take less time to complete the survey, but the estimated difference is very small (less than a minute) and is to be expected since people in this group have less text to read given that there are no source attributions provided.

Finally, we examine whether including those who completed part of the survey in our analysis affects our results. While the terms of our ethics approval does not allow us to report these estimates here, our results stay almost identical to those reported in the main text, which is consistent with other tests discussed above.

2. Robustness checks

We perform several additional tests to examine the robustness of our results. First, we estimate a model similar to the one reported in Table 1 in the main text where we estimate average differences in agreement level between different groups using participants' reported confidence level as weights. In other words, we give more (less) weight to evaluations of participants who report more (less) confidence in their assessment. These estimates are reported in Column (1) of Table A6 and are identical to estimates without weights reported in Column (2). This is of course consistent with our results in Table 3, discussed in the main text, that on average there are no differences in the self-reported confidence level between different groups.

As our second robustness check, we examine whether restricting our sample to individuals with different survey completion times will affect our results. One might argue, for example, that our results could be driven by individuals who do not pay much attention to the statements, for instance, and very quickly go through the survey, mainly using sources as the basis for their evaluation. To examine this hypothesis, we estimate average differences in agreement level between different groups using different subsamples based on individual survey completion time. These results are reported in Columns (3) to (6) of Table A6. We find that restricting our sample to individuals who completed the survey in different time frames (5 to 20 minutes, 7 to 20 minutes, 10 to 20 minutes, 10 to 30 minutes) does not affect our estimates and our results stay robust across the board.

As our final robustness check we design a falsification test in which we re-estimate the specifications reported in Table 1 but where we replace our group assignment indicators for each statement with new indicators based on participant's group assignment from her next statement. Results from this falsification test are reported in Table A7 and lend additional support to our randomization and identification strategy. More specifically, since those who are randomized into control group or treatment 1 at the beginning of the survey are re-randomized between these two groups for each subsequent statement, identifying a person's group assignment using her next statement is similar to the outcome that would emerge from a coin flip. Therefore, there should not be any differences in average agreement level between control group and treatment 1 in the falsification test, which is exactly what we find in Table A7. Moreover, since those who are randomly selected into treatment 2 stay in that group for the entire survey, their group assignment measure will not change in the falsification scheme. Therefore, our falsification test should still

produce estimated gaps in agreement level with the control group. However, since the new control group is a random mix of individuals from the actual control group and actual treatment 1, the estimated gaps should be smaller than the ones reported in Table1. Results reported in Table A7 fully support this conclusion.

Altogether, using different robustness tests, we find no evidence that would cast doubt on our survey implementation, identification strategy, or estimated results.

3. Why deception?

While experiments with deception are commonly used in other social sciences such as psychology and political science, economists discourage the use of deception in certain contexts. As Wilson (2014) points out, economists' reluctance, or opposition, to the use of deception is not based on any deontic aversion to dishonesty, but rather due to their concern regarding the potential effect deception might have on participant's psychological reaction to suspected manipulation. This concern usually applies to experiments that involve some type of monetary reward that is supposed to induce incentive in participants.

For example, Cooper (2014) suggests that "the ban on deception stems primarily from the role of economic experiments in testing economic theory. The theoretical predictions in most settings are a function of the objective functions for individual decision makers. A test of these predictions is valid only if the experimenter has control over the incentives of the experimental participants. If participants believe they or others will be paid according to some rule other than what they have been told, control is lost and the test is no longer valid." Similarly, Davis and Holt (1993) argue that "Most economists are very concerned about developing and maintaining a reputation among the students by the induced monetary rewards rather than by psychological reactions to suspected manipulation."

Given this potential distortionary impact deception could have on participants, it is therefore argued by some economists that deception might impose negative externalities on experimenters in general over time by destroying the credibility of all economic experimenters. On the other hand, Cooper (2014) points out that "no one experiment using deception destroys the reputation of a researcher, a laboratory, or the field as a whole […]." Although he points out that "the cumulative effect could be quite severe", he also acknowledges that "there is little evidence of indirect effects akin to the poisoning of the well feared by many experimental economists." In

addition to this argument, we would also like to highlight that this potential cumulative negative effect relies on two important conditions:

(1) The extent to which the same target population will be asked to participate in other experiments that are also likely to suffer from participant's suspicion about deception. As Jamison et al. (2008) point out, "The primary concern with deception is that many experimental laboratories use a common pool of participants. Thus, a public goods problem exists in which experiencing deception in one experiment may cause participants to react differently (and uncertainly) in future games with other researchers." This concern does not seem to apply to our study since economists are rarely the participants of experimental studies.

(2) The extent to which future experiments are similar to the previous experiment(s) with deception that participants were exposed to. Results from Brock and Becker (1966) and Cook et al. (1970) suggest that past experiments with deception do not affect the behaviour of participants in future experiments when the experiments are dissimilar.

Nevertheless, one could argue that there might be situations in which the same target population (i.e. economists) is asked to participate in a study that does not involve deception and simply tries to evaluate economist's opinions and views. We argue however that under such scenario our experiment could affect the behaviour of participants in ways that could be only described as positive. More specifically, if we find that using altered sources with different views will induce ideological bias in responses, this could encourage people to avoid judgment based on sources and pay more attention to the content of an argument or idea when expressing their opinions on a subject. This is especially important given the fact that in many cases exhibited individual bias could be unconscious or unintentional, and gaining knowledge about this behaviour is potentially extremely valuable. Bertrand and Duflo (2017) highlight the importance of implicit bias by pointing out that "modern social psychologists believe that attitudes can occur in implicit modes and that people can behave in ways that are unrelated or even sometimes opposed to their explicit views or self-interests." We believe finding out about our own potential biases is certainly a positive outcome for economists who strive to be objective and ideology-free. This also seems to be consistent with the standard most participants in our study hold themselves to.[1]

[1] At the end of our survey, we ask our participants to answer a series of questions to identify their political typology. One of these questions is however not related to political typology and is included to find out how our participants think an argument should be evaluated. Around 82% of participants in our survey report that "a claim or argument

In light of these arguments, we suggest that categorical rejection of applying deception in experiments is an extreme position that is not supported by theory or empirical evidence. As Cooper (2014), the Editor-in-Chief for *Experimental Economics* puts it "only an extremist would claim that experimenters (or economists in general) should never use deception." He points out that "Labor economics, for example, has benefited from a long series of field experiments on discrimination that use deception." In essence, our study is not any different from these correspondence studies that are well-accepted in economics, evident by their publication in top economic journals (See Bertrand and Duflo (2017) and Riach and Rich (2002) for a review. Also see Currie et al. (2014) as another example). Given the important methodological challenges and limitations in measuring the existence of discrimination, Guryan and Charles (2013) argue that "correspondence studies represent a significant methodological advance."

Similar to these correspondence studies, unless we use a treatment that is unknown to participants (i.e. alteration of the true source of a statement), we cannot provide a convincing answer to the question we are interested in, especially given the fact that such biases are often exhibited in unconscious or unintentional ways. Clearly, if participants are aware of the true purpose of the study, which is examining whether their assessment of different statements is influenced by ideologically-different sources, this will affect how they express their views and renders our results practically useless. In our opinion, the main difference between our study and the correspondence studies is that our target population are economists as opposed to potential employers, or doctors in the case of Currie et al. (2014). One could even argue that our study lacks some of the potential disadvantages that could be associated with correspondence or audit studies. For example, flooding the market with fictitious resumes could negatively affect the chances of some real candidates for being reasonably evaluated by employers. Similarly, sending fake patients to hospitals (See Currie et al. 2014) clearly consumes public resources and affects real patients.

To summarize, based on arguments provided above, we believe the application of deception in our study is not associated with the potential negative outcomes feared by some

should be rejected only on the basis of the substance of the argument itself." Around 18% of participants report that "a claim or argument should be rejected based on what we know about the views of the author or the person presenting the argument as well as the substance of the argument." There exists only a tiny minority (around 0.5%) who report "a claim or argument should be rejected based on what we know about the views of the author or the person presenting the argument."

economists. Cooper (2014) suggests the following four rules as reasonable guidelines for when deception is allowable in experiments:

1. The deception does not harm subjects beyond what is typical for an economic experiment without deception.
2. The study would be prohibitively difficult to conduct without deception.
3. Subjects are adequately debriefed after the fact about the presence of deception.
4. The value of the study is sufficiently high to merit the potential costs associated with the use of deception.

We believe our discussion above clearly suggests that our experiment meets all these four criteria.

4. Reference

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5. Appendix tables and figures

Figure A1: Survival function by source assignment – Cloglog model

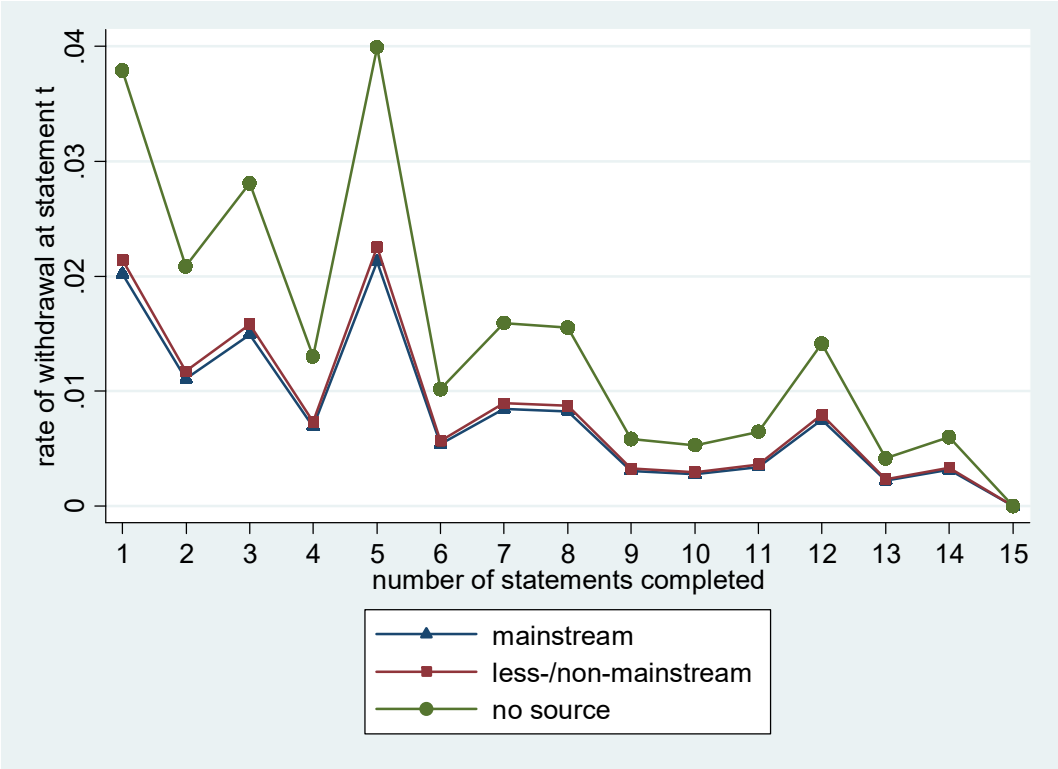
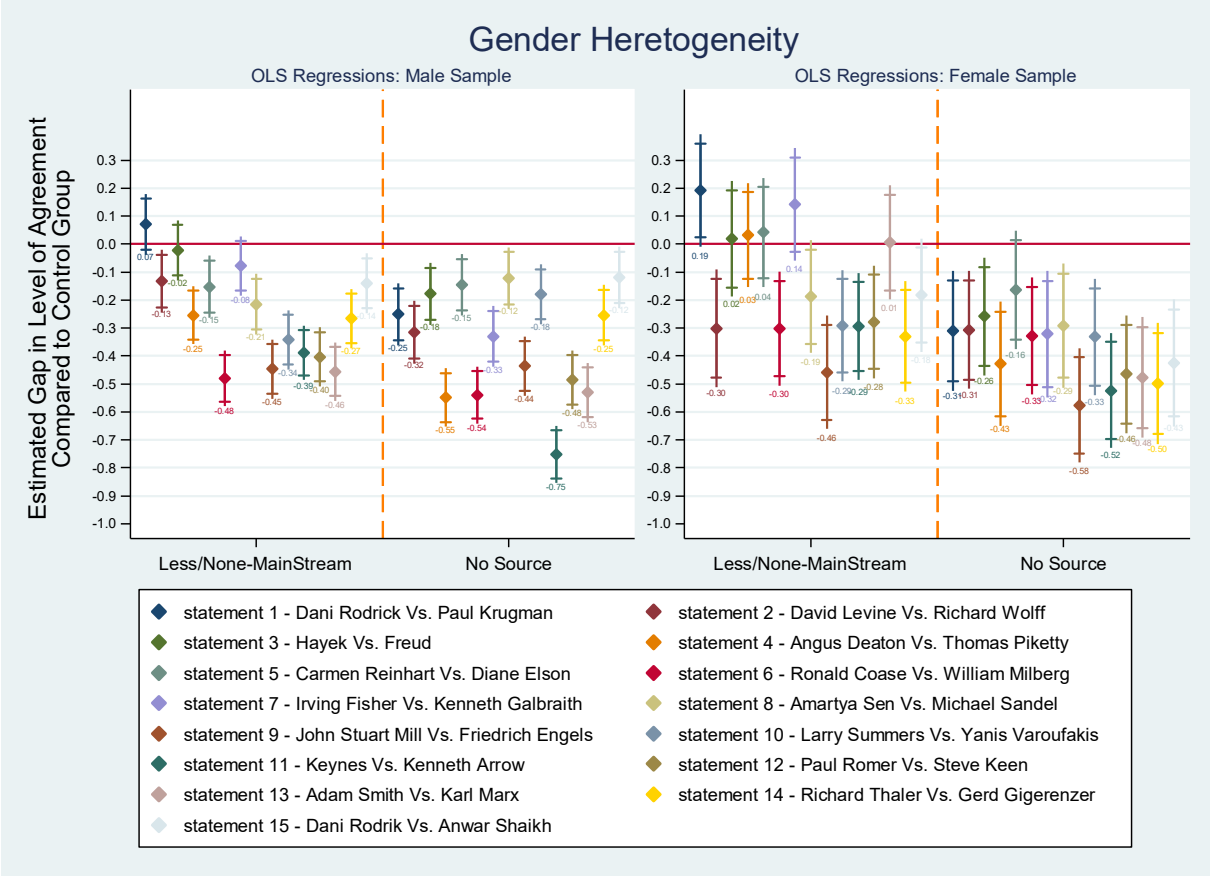


Figure A2: OLS estimates of gender differences in agreement level between control and treatment groups – By statement



Note: Agreement levels is z-normalized for each statement. Control variables include: PhD completion cohort, current status, country, research area. Both 90% and 95% confidence intervals are displayed for each estimate. The two horizontal lines on each confidence interval band represent where the 90% confidence interval ends.
First (second) listed source for each statement is the actual (altered) source.

Figure A3: Distribution of responses by institute of affiliation - US

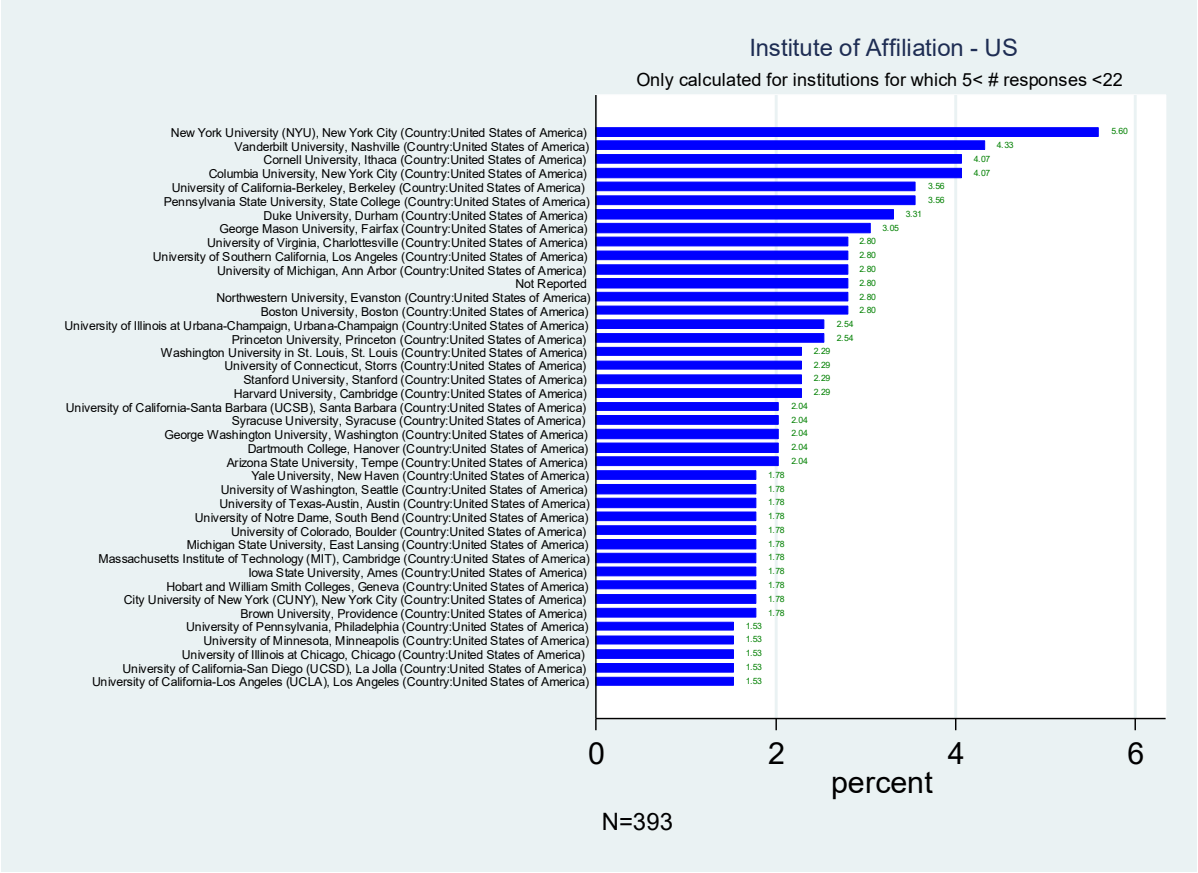


Figure A4: Distribution of responses by institute of affiliation - Canada

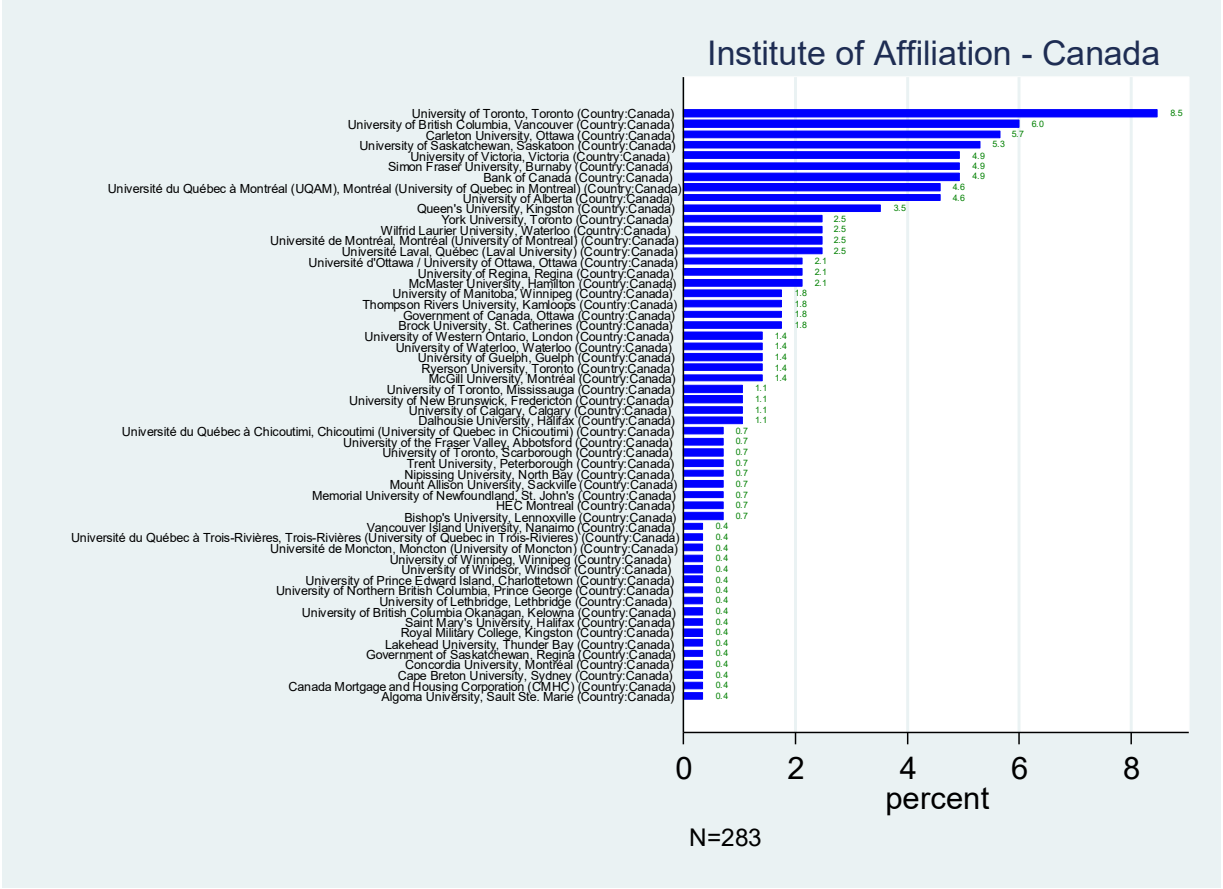


Figure A5: Distribution of responses by institute of affiliation – UK

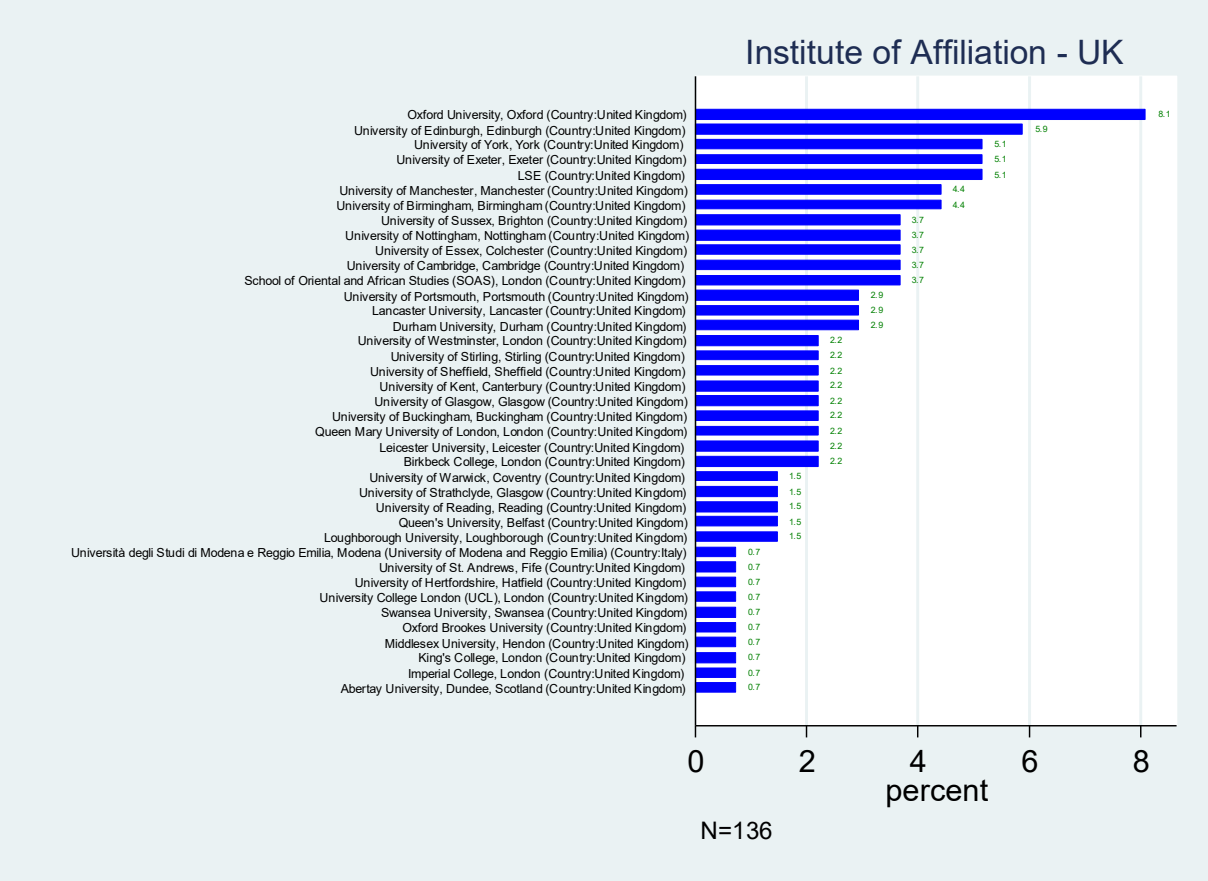


Table A1: Summary Statistics (All reported numbers are in percentages)

	Overall	Control	Treatment 1	Treatment 2
Current Country where job is located				
Australia	4.45	3.83	4.42	5.10
Austria	1.48	1.44	1.61	1.42
Brazil	1.94	1.91	1.34	2.49
Canada	11.70	11.00	11.50	12.50
France	8.74	8.49	9.92	7.95
Germany	3.92	3.83	3.62	4.27
Ireland	0.66	0.72	0.67	0.59
Italy	8.21	8.37	7.51	8.66
Japan	2.23	2.15	2.28	2.25
Netherlands	2.52	2.15	2.68	2.73
New Zealand	2.23	2.03	2.41	2.25
Scandinavia (Denmark, Finland, Norway, Sweden)	6.02	6.10	5.76	6.17
South Africa	1.32	1.08	1.47	1.42
Switzerland	1.57	1.67	1.74	1.30
United Kingdom	5.61	5.62	6.03	5.22
United States	37.40	39.60	37.00	35.70
Gender:				
Female	21.90	22.20	20.50	22.70
Age:				
18 to 25	0.08	0.00	0.00	0.24
26 to 30	2.14	1.67	2.28	2.49
31 to 35	10.20	10.50	10.60	9.61
36 to 40	11.90	11.20	10.60	13.60
41 to 45	11.40	10.90	11.10	12.20
46 to 50	10.20	11.60	8.71	10.20
51 to 55	10.80	11.50	10.50	10.40
56 to 60	9.81	8.97	11.50	9.13
61 to 65	9.77	9.69	9.52	10.10
66 to 70	7.71	8.25	8.04	6.88
71 to 75	5.28	5.02	6.30	4.63
76+	1.65	1.44	1.88	1.66
Not Reported	8.49	8.37	8.98	8.19
Country/Region of Birth:				
United States	26.80	28.10	25.70	26.60
Canada	7.96	7.42	8.58	7.95
Africa	2.02	2.27	1.74	2.02
Asia	6.56	6.58	6.43	6.64
Central America (including Mexico)	0.37	0.48	0.67	0.00
South America	3.26	3.47	3.08	3.20
Eastern Europe	2.23	2.39	1.74	2.49
Europe 1 (France, Belgium)	8.70	7.89	9.79	8.54
Europe 2 (Germany, Austria, Netherlands, Switzerland, Luxembourg)	9.24	8.25	9.52	9.96
Europe 3 (Italy, Spain, Portugal)	10.40	9.69	9.92	11.50
Europe 4 (Denmark, Finland, Norway, Sweden)	4.25	4.19	4.42	4.15
Europe 5 (UK, Ireland)	4.78	4.78	5.09	4.51
Europe 6 (Rest of Europe)	0.78	0.72	0.94	0.71
Middle East	1.32	1.08	1.21	1.66
Oceania	2.56	2.63	2.55	2.49
The Caribbean	0.21	0.24	0.27	0.12
Not Reported	8.54	9.81	8.31	7.47
Sample Size	2425	836	746	843

Table A1 (continued 1): Summary Statistics

	Overall	Control	Treatment 1	Treatment 2
English Proficiency:				
Native speaker	46.80	47.20	47.30	46.00
Advanced	39.90	39.40	41.00	39.40
Early advanced	6.64	6.46	5.90	7.47
Intermediate	5.32	5.26	5.23	5.46
Early intermediate	1.32	1.67	0.54	1.66
Political Orientation (on a scale from -10 to 10)				
Far Left (-10 to -7)	18.90	18.80	17.80	20.00
Left (-6 to -2)	40.90	39.70	43.40	40.00
Centre (-1 to 1)	19.80	21.30	18.20	19.80
Right (2 to 6)	15.20	15.30	15.40	14.80
Far Right (7 to 10)	4.95	4.90	4.83	5.10
Current Status:				
Assistant professor	18.40	17.70	18.50	19.00
Associate professor	22.10	20.70	21.20	24.30
Full professor, Professor emeritus	43.00	43.30	44.20	41.60
Postdoctoral fellow, Visiting scholar	2.72	3.59	2.41	2.14
Instructor (tenure-track or not tenure-track)	6.43	6.22	6.97	6.17
Employee of a government agency or institution	4.12	4.55	3.89	3.91
Employee of an independent research institution or think tank	2.31	2.99	2.28	1.66
Other	0.91	0.96	0.54	1.19
Department of Affiliation:				
Economics	62.90	61.40	63.40	64.10
Economics, Business, Management	8.74	9.69	7.51	8.90
Business, Management	5.03	4.55	5.63	4.98
Public policy	2.80	2.99	2.68	2.73
Industrial relations	0.21	0.60	0.00	0.00
Government agency or institution	2.43	2.15	2.28	2.85
Independent research institution or think tank	1.40	1.56	1.61	1.07
Other	4.54	3.95	4.83	4.86
Research Area:				
Teaching	4.16	3.71	4.69	4.15
History of Thought, Methodology, Heterodox Approaches	5.98	5.86	5.90	6.17
Mathematical and Quantitative Methods	6.72	6.94	7.24	6.05
Microeconomics	8.82	8.01	9.38	9.13
Macroeconomics and Monetary Economics	11.40	11.60	11.40	11.30
International Economics	6.60	6.46	6.30	7.00
Financial Economics	4.04	4.31	4.56	3.32
Public Economics	7.09	6.46	7.37	7.47
Health, Education, and Welfare	5.65	5.74	4.69	6.41
Labor and Demographic Economics	10.20	11.60	9.38	9.49
Law and Economics	1.40	2.15	1.07	0.95
Industrial Organization	4.58	4.43	4.29	4.98
Business Administration, Marketing, Accounting	2.97	3.35	3.08	2.49
Economic Development, Innovation, Technological Change	8.37	8.37	8.58	8.19
Agricultural and Natural Resource Economics	7.79	7.30	8.04	8.07
Urban, Rural, Regional, Real Estate, and Transportation Economics	2.43	2.15	2.68	2.49
Cultural Economics, Economic Sociology, Economic Anthropology	0.54	0.48	0.40	0.71
Other	1.24	1.08	0.94	1.66
Sample Size	2425	836	746	843

Table A1 (continued 2): Summary Statistics

	Overall	Control	Treatment 1	Treatment 2
PhD Completion Year:				
1951 to 1955	0.08	0.00	0.00	0.24
1956 to 1960	0.12	0.12	0.00	0.24
1961 to 1965	0.95	0.84	0.80	1.19
1966 to 1970	1.81	2.27	1.88	1.30
1971 to 1975	4.04	3.95	5.09	3.20
1976 to 1980	5.69	5.50	5.23	6.29
1981 to 1985	7.22	8.13	7.10	6.41
1986 to 1990	8.58	7.42	10.20	8.30
1991 to 1995	10.30	11.60	10.10	9.13
1996 to 2000	10.40	12.10	8.98	9.85
2001 to 2005	10.80	9.93	11.30	11.40
2006 to 2010	13.30	12.10	12.70	14.90
2011 to 2017	18.30	18.30	18.10	18.50
Expected in the future	0.95	1.08	0.67	1.07
No PhD Degree	7.46	6.58	7.91	7.95
Country/Region Completed PhD:				
United States	45.00	48.00	43.00	43.80
Canada	6.31	5.98	5.90	7.00
Africa	0.99	1.08	0.94	0.95
Asia	1.32	1.32	1.74	0.95
Central America (including Mexico)	0.04	0.12	0.00	0.00
South America	1.20	1.20	1.07	1.30
Eastern Europe	0.04	0.12	0.00	0.00
Europe 1 (France, Belgium)	9.15	8.37	10.10	9.13
Europe 2 (Germany, Austria, Netherlands, Switzerland, Luxembourg)	8.49	7.18	9.79	8.66
Europe 3 (Italy, Spain, Portugal)	5.98	6.94	4.83	6.05
Europe 4 (Denmark, Finland, Norway, Sweden)	4.16	4.43	4.56	3.56
Europe 5 (UK, Ireland)	7.13	6.34	7.24	7.83
Europe 6 (Rest of Europe)	0.08	0.00	0.13	0.12
Oceania	2.43	1.91	2.68	2.73
The Caribbean	0.04	0.00	0.13	0.00
Other	0.12	0.36	0.00	0.00
Not Applicable	7.46	6.58	7.91	7.95
Undergraduate Major:				
Economics	63.10	62.70	62.30	64.20
Other Social Sciences (Anthropology, Sociology, Psychology)	1.20	0.96	0.94	1.66
Business, Management	5.57	6.22	5.36	5.10
Biology, Chemistry, Physics	2.39	2.87	2.01	2.25
Computer Science, Engineering	2.68	2.51	2.68	2.85
Earth and space sciences, Geography	1.86	2.03	2.41	1.19
History, Language and literature	1.86	1.91	2.14	1.54
Law	0.33	0.48	0.13	0.36
Mathematics, Statistics	8.25	7.30	8.71	8.78
Medicine	0.08	0.00	0.13	0.12
Performing arts, Visual arts	0.04	0.12	0.00	0.00
Philosophy, Political Science	3.96	4.31	3.35	4.15
Agricultural Sciences	1.48	1.32	1.47	1.66
Other	0.50	0.60	0.54	0.36
Not Reported	6.72	6.70	7.77	5.81
Sample Size	2425	836	746	843

Table A2: Estimates of Control Variables – Agreement Level OLS Regression

Variable	(1)	(2)	Variable	(1)	(2)
Female	0.096***	0.099***	Ireland	-0.035	-0.039
	(0.012)	(0.012)		(0.063)	(0.066)
PhD: 1951-1955	0.294	0.365	Italy	0.248***	0.237***
	(0.246)	(0.241)		(0.024)	(0.040)
PhD: 1956-1960	0.461***	0.427***	Japan	0.047	-0.017
	(0.113)	(0.121)		(0.034)	(0.053)
PhD: 1966-1970	-0.152**	-0.241***	Netherlands	0.034	0.011
	(0.061)	(0.064)		(0.035)	(0.044)
PhD: 1971-1975	-0.177***	-0.259***	New Zealand	-0.031	-0.008
	(0.053)	(0.058)		(0.038)	(0.041)
PhD: 1976-1980	-0.127**	-0.188***	South Africa	0.354***	0.270***
	(0.052)	(0.057)		(0.045)	(0.070)
PhD: 1981-1985	-0.105**	-0.140**	Switzerland	0.072	0.080
	(0.051)	(0.058)		(0.044)	(0.050)
PhD: 1986-1990	-0.089*	-0.094	UK	0.061**	0.030
	(0.050)	(0.057)		(0.026)	(0.032)
PhD: 1991-1995	-0.098*	-0.084	US	-0.021	0.053**
	(0.050)	(0.058)		(0.018)	(0.027)
PhD: 1996-2000	-0.114**	-0.091	**Research**		
	(0.050)	(0.058)	Teaching	-0.226***	-0.202***
PhD: 2001-2005	-0.157***	-0.108*		(0.033)	(0.033)
	(0.050)	(0.059)	Math & Quant.	-0.347***	-0.332***
PhD: 2006-2010	-0.190***	-0.105*		(0.028)	(0.028)
	(0.051)	(0.059)	Micro	-0.411***	-0.389***
PhD: 2011-2017	-0.168***	-0.115*		(0.027)	(0.027)
	(0.051)	(0.060)	Macro	-0.356***	-0.337***
No PhD Yet	-0.102	-0.074		(0.026)	(0.026)
	(0.074)	(0.080)	International Econ	-0.325***	-0.305***
No PhD	-0.118**	-0.082		(0.028)	(0.028)
	(0.052)	(0.065)	Financial Econ	-0.367***	-0.342***
Associate Prof	-0.003	-0.007		(0.032)	(0.033)
	(0.019)	(0.020)	Public Econ	-0.347***	-0.336***
Full/Emeritus Prof	-0.046**	-0.049**		(0.029)	(0.029)
	(0.020)	(0.021)	Health, Education	-0.247***	-0.231***
Post Doc/Visiting	0.133***	0.117***	Welfare	(0.029)	(0.030)
	(0.032)	(0.034)	Labour	-0.264***	-0.252***
Instructor	-0.071***	-0.085***		(0.026)	(0.026)
	(0.026)	(0.026)	Law & Econ	-0.249***	-0.255***
Government Emp.	0.053*	0.090***		(0.052)	(0.053)
	(0.028)	(0.034)	IO	-0.334***	-0.329***
Independent Inst.	0.016	0.034		(0.031)	(0.031)
	(0.037)	(0.044)	Business administration	-0.074**	-0.071*
Status: Other	0.106*	0.120**	Marketing, Accounting	(0.037)	(0.038)
	(0.057)	(0.058)	Development	-0.183***	-0.186***
Austria	0.025	0.046		(0.027)	(0.027)
	(0.045)	(0.055)	Environmental	-0.222***	-0.215***
Brazil	0.148***	0.086	Natural Resource Econ	(0.027)	(0.028)
	(0.042)	(0.077)	Urban Econ.	-0.252***	-0.252***
Canada	0.011	0.061*		(0.038)	(0.039)
	(0.021)	(0.031)	Economic Socio/Anth.	0.024	0.023
France	0.251***	0.067		(0.072)	(0.073)
	(0.023)	(0.044)	Other	0.026	-0.012
Germany	0.111***	0.106**		(0.047)	(0.048)
	(0.030)	(0.043)			

Table A2 (Continued): Estimates of Control Variables – Agreement Level OLS Regression

	(2)		(2)		(2)
Age		Italy, Spain, Portugal	-0.031	**Country PhD**	
26-30	-0.230***		(0.039)	US	-0.036
	(0.086)	Scandinavia	-0.006		(0.026)
31-35	-0.146*		(0.045)	Africa	0.063
	(0.080)	UK, Ireland	0.124***		(0.079)
36-40	-0.213***		(0.034)	Asia	-0.058
	(0.079)	Rest of Europe	0.152**		(0.062)
41-45	-0.192**		(0.064)	Central America	-0.811**
	(0.078)	Middle East	0.187***		(0.388)
46-50	-0.133*		(0.052)	South America	0.291***
	(0.078)	Oceania	-0.024		(0.084)
51-55	-0.140*		(0.046)	Eastern Europe	-0.205
	(0.078)	The Caribbean	0.267**		(0.261)
56-60	-0.137*		(0.114)	France, Belgium	0.157***
	(0.078)	Not Reported	-0.049		(0.041)
61-65	-0.063		(0.033)	Germany, Austria, Switz.	0.071*
	(0.077)	**English Proficiency**		Netherlands, Luxemb.	(0.041)
66-70	-0.025	Advanced	-0.004	Italy, Spain, Portugal	0.128***
	(0.077)		(0.022)		(0.038)
71-75	0.016	Early Advanced	0.005	Scandinavia	0.092**
	(0.078)		(0.030)		(0.044)
76+	-0.078	Intermediate	0.087***	UK, Ireland	0.069**
	(0.083)		(0.032)		(0.033)
Not Reported	-0.102	Early Intermediate	-0.105*	Rest of Europe	-0.466***
	(0.079)		(0.053)		(0.153)
Country of Birth		**Department of Affiliation**		Oceania	0.027
US	0.012	Business, Econ, Manag.	0.069***		(0.047)
	(0.027)		(0.019)	The Caribbean	-0.744**
Africa	0.151***	Business, Management	0.040*		(0.372)
	(0.053)		(0.024)	Other	-0.076
Asia	0.159***	Public Policy	0.156***		(0.122)
	(0.036)		(0.032)		
Central America	0.011	Industrial Relations	0.318***		
	(0.101)		(0.118)		
South America	-0.061	Government	-0.060		
	(0.048)		(0.042)		
Eastern Europe	-0.028	Independent Institution	0.017		
	(0.045)		(0.056)		
France, Belgium	0.132***	Other	0.074***		
	(0.040)		(0.025)		
Germany, Austria, Nether.	-0.001	Not Reported	0.006		
Switz., Luxemb.	(0.037)		(0.018)		
Number of Observations	36375		36375		36375

Note: Reported estimates are estimated coefficients of control variables from regression specifications reported in Table 1 in the main text. Column (1) reports estimates from a model with more limited control variables (gender, PhD completion cohort, current status, country, research area) while Column (2) reports estimates from a model with more control variables (those previously listed as well as age cohort, country/region of birth, English proficiency, department of affiliation, country where PhD was completed). The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree). It is standardized to have mean zero and standard deviation of one. Heteroskedasticity-robust standard errors are reported in parentheses. Omitted categories are: PhD: 1961-1965; Status: Assistant Professor; Current Country: Canada; Research Area: History of Economic Thought, Methodology, and Heterodox Approaches; Age: 18-25; Country of Birth: Canada; English Proficiency: Native Speaker; Department of Affiliation: Economics; Country PhD: Canada; Significance levels: *** < 1%, ** < 5%, * < 10%.

Table A3: Number of people quitting at different stages of the survey - By group assignment

	Short Questionnaire	Long Questionnaire	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Total
All	237	217	75	40	53	24	73	18	28	27	10	9	11	24	7	10	2425	3288
Mainstream Source	73	64	21	12	7	8	23	5	8	10	6	2	2	6	0	3	843	1093
Less-/Non-mainstream	105	66	19	9	14	5	23	5	5	7	3	3	4	5	1	4	836	1114
No Source	59	87	35	19	32	11	27	8	15	10	1	4	5	13	6	3	746	1081

Note: People randomized into Control Group (mainstream source) or Treatment 1 (less-/non-mainstream source) at the beginning of the survey do not remain there for the entire survey and could change group for each subsequent statement. At the beginning of the survey each participant has a one-third probability to get randomized into Control Group, Treatment 1 or Treatment 2. Those who are randomized into Treatment 2 (no source) remain there for the entire survey. Those who are randomized into Control Group or Treatment 1 for the first statement will face a 50% chance to stay in their initially assigned group or move to the other one for each subsequent statement. Our classification of Mainstream and Less-/Non-mainstream above is based on the initial assignment. Short questionnaire refers to the first questionnaire page in the survey where participants indicate their country, institution of affiliation, and whether they are a student or not. Long questionnaire refers to the second questionnaire page where participants provide more background information such as gender, age, PhD cohort, area of research, undergraduate major, political orientation, etc.

Table A4: The effect of number of mainstream/non-mainstream sources received on the probability of not completing the survey – Marginal effects from probit model

	Without Control	With Control
Regression 1: Number of Mainstream Sources	-0.022***	-0.016***
	(0.003)	(0.003)
Regression 2: Number of Non-Mainstream Sources	-0.026***	-0.020***
	(0.003)	(0.003)
P-value for the null hypothesis of equality	0.383	0.418
Number of observations	1899	1899

Notes: Those who are randomized into Treatment 2 (no source) are excluded from the regression above since they receive all statements with no source attribution. The unit of observation is a participant. The dependent variable is equal to one if survey was completed by a participant and zero otherwise. Controls include: gender, PhD cohort (15 categories), Current Status (8 categories), Country (19 categories), Research Area (18 categories).

Table A5: Average difference in survey completion time (effort invested in cognitive evaluation) between control and treatment groups

	Without Control	With Control
	(1)	(2)
Treatment 1	-0.006	0.002
Non-/Less-Mainstream Source	(0.061)	(0.059)
Treatment 2	-0.742***	-0.721***
No Source	(0.061)	(0.060)
Constant	9.953***	10.61***
	(0.0433)	(0.270)
Number of observations	33465	33465

Note: Omitted category is Control Group (mainstream source). Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is the number of minutes taken to complete the survey. The analysis sample includes all participants who completed the survey, excluding outliers (those who took more than one hour to complete the survey). The outliers are only 2% of our sample. The average (median) completion time for this sample is 10.7 (8.7) minutes. Significance levels: *** < 1%, ** < 5%, * < 10%. Control variables include: gender, PhD completion cohort, current status, country, research area.

Table A6: Robustness 1

	Weighted	Not Weighted	5 to 20 minutes	7 to 20 minutes	10 to 20 minutes	10 to 30 minutes
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment 1	-0.289***	-0.262***	-0.275***	-0.260***	-0.269***	-0.253***
Non-/Less-Mainstream Source	(0.015)	(0.014)	(0.016)	(0.019)	(0.025)	(0.024)
Treatment 2	-0.448***	-0.405***	-0.395***	-0.385***	-0.394***	-0.381***
No Source	(0.016)	(0.015)	(0.017)	(0.020)	(0.028)	(0.027)
Number of observations	36088	36088	28425	19995	11430	12390

Note: Omitted category is Control Group (i.e. mainstream source). Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree). The average agreement level in our sample is 3.35 with standard deviation of 1.185. Significance levels: *** < 1%, ** < 5%, * < 10%. All regressions include the following controls variables: gender, PhD completion cohort, current status, country, research area.

Column (1) reports estimates from a model where we use reported confidence level as weights in our regression. Column (2) reports results from a similar model but without weights for comparison. Column (3) to (6) are estimates from models where our sample is constrained to participants who completed the survey in 5 to 20 minutes, 7 to 20 minutes, 10 to 20 minutes, and 10 to 30 minutes, respectively.

Table A7: Falsification test – OLS estimates

	(1)	(2)	(3)
Treatment 1	-0.009	0.000	-0.004
Non-/Less-Mainstream Source	(0.014)	(0.014)	(0.014)
Treatment 2	-0.287***	-0.272***	-0.276***
No Source	(0.015)	(0.015)	(0.015)
Controls	No	**Yes**	No
More Control	No	No	**Yes**
Number of observations	36375	36375	36375

Note: Omitted category is Control Group (i.e. mainstream source). Heteroskedasticity-robust standard errors are reported in parentheses. The dependent variable is agreement level on a scale from 1 (strongly disagree) to 5 (strongly agree). The average agreement level in our sample is 3.35 with standard deviation of 1.185. Significance levels: *** < 1%, ** < 5%, * < 10%. Controls include: gender, PhD completion cohort, current status, country, research area. More Controls include all the previously listed variables as well as age cohort, country/region of birth, English proficiency, department of affiliation, country where PhD was completed.

For falsification test, a person's group assignment for a statement is not her actual group assignment but rather her group assignment from next statement. Keep in mind that this does not affect those who are randomized into Treatment 2 at the beginning of the survey because they remain in that group for the entire survey.

Table A8: List of Statements and Sources Used in the Survey

Statement 1:

"When we expect redistributive effects to even out in the long run, so that everyone eventually comes out ahead, we are more likely to overlook reshufflings of income. That is a key reason why we believe that technological progress should run its course, despite its short-run destructive effects on some. When, on the other hand, the forces of trade repeatedly hit the same people – less educated, blue-collar workers – we may feel less sanguine about globalization. Too many economists are tone-deaf to such distinctions. They are prone to attribute concerns about globalization to crass protectionist motives or ignorance, even when there are genuine ethical issues at stake."

Real Source: *By Dani Rodrik, professor of international political economy at Harvard University and the author of The Globalization Paradox: Democracy and the Future of the World Economy (2012).*
Altered Source (Less-/non-Mainstream): *By Paul Krugman, professor of economics at Princeton University, the 2008 recipient of the Nobel Prize in Economics, and the author of The Accidental Theorist and Other Dispatches from the Dismal Science (1999).*

Statement 2:

"A realistic view of intellectual monopoly [e.g. patent, copyright] is that it is a disease rather than a cure. It arises not from a principled effort to increase innovation, but from an evil combination of medieval institutions – guilds, royal licenses, trade restrictions – and the rent-seeking behaviour of would be monopolists seeking to fatten their purse at the expense of public prosperity."

Real Source: *By David Levine, professor of economics at Washington University in St. Louis and the author of Against Intellectual Monopoly (2008).*
Altered Source (Less-/non-Mainstream): *By Richard Wolff, professor of economics emeritus at the university of Massachusetts, Amherst, and the author of Rethinking Marxism (1985).*

Statement 3:

"It is only in combination with particular, non-rational impulses that reason can determine what to do…"

Real Source: *Friedrich von Hayek (1899-1992), professor of economics at University of Chicago and London School of Economics, and the 1974 recipient of the Nobel Prize in economics.*
Altered Source (Less-/non-Mainstream): *Sigmund Freud (1856-1939), the founder of psychoanalysis and the author of the book Civilization and Its Discontents (1929).*

Statement 4:

"The very wealthy have little need for state-provided education or health care; they have every reason to support cuts in Medicare and to fight any increase in taxes. They have even less reason to support health insurance for everyone, or to worry about the low quality of public schools that plagues much of the country. They will oppose any regulation of banks that restricts profits, even if it helps those who cannot cover their mortgages or protects the public against predatory lending, deceptive advertising, or even a repetition of the financial crash."

Real Source: *By Angus Deaton, professor of economics at Princeton University, the 2015 recipient of the Nobel Prize in Economics, and the author of The Great Escape: Health, Wealth, and the Origins of Inequality (2013).*
Altered Source Less-/non-Mainstream): *By Thomas Piketty, professor of economics at the Paris School of Economics and the author of capital in the twenty-first century (2013).*

Table A8 (continued 1): List of Statements and Sources Used in the Survey

Statement 5:

"Unlike most other science and social science disciplines, economics has made little progress in closing its gender gap over the last several decades. Given the field's prominence in determining public policy, this is a serious issue. Whether explicit or more subtle, intentional or not, the hurdles that women face in economics are very real."

Real Source: *By Carmen Reinhart, Professor of the International Financial System at Harvard Kennedy School and the author of This Time is Different: Eight Centuries of Financial Folly (2011)"*
Altered Source (Non-Mainstream): *By Diane Elson, British Economist and Sociologist, Professor Emerita at the University of Essex, and the author of Male bias in the development process (1995).*

Note: The pilot version of the survey which was run in Australia had a different statement in place of the one above. Based on some useful feedback we received from participants, we decided to provide a statement that addresses gender issues in economics. Since we already had a Smith/Marx pair (Statement 13), we decided to replace the following quote used in our pilot version with the one provided above.

"Civil government, so far as it is instituted for the security of property, is in reality instituted for the defense of the rich against the poor, or of those who have some property against those who have none at all."

Real Source: *Adam Smith*
Altered Source (*Less-/non-Mainstream*): *Karl Marx*

Statement 6:

Economic discourse of any sort - verbal, mathematical, econometric-is rhetoric; that is, an effort to persuade. None of these discursive forms should necessarily be privileged over the others unless it is agreed by the community of scholars to be more compelling. Only when economists move away from the pursuit of universal knowledge of 'the economy' and towards an acceptance of the necessity of vision and the historical and spatial contingency of knowledge will the concern over ideological 'bias' begin to fade. Such a turn would have important implications for economic method as well, as knowledge claims would increasingly find support, not in models of constrained optimization, but with such techniques as case studies and historical analyses of social institutions and politics. Increasing reliance of economics on mathematics and statistics has not freed the discipline from ideological bias, it has simply made it easier to disregard.

Real Source (Less-/non-Mainstream): *By William Milberg, dean and professor of economics at the New School for Social Research and the author of The Crisis of Vision in Modern Economic Thought (1996).*
Altered Source: *By Ronald Coase (1910-2013), professor of economics at the University of Chicago Law School and the 1991 recipient of the Nobel Prize in Economics.*

Statement 7:

"Academic economists, from their very open-mindedness, are apt to be carried off, unawares, by the bias of the community in which they live. Economists whose social world is Wall Street are very apt to take the Wall Street point of view, while economists at state universities situated in farming districts are apt to be partisans of the agricultural interests".

Real Source: *By Irving Fisher (1867-1947), professor of political economy at Yale University.*
Altered Source (Less-/non-Mainstream): *By John Kenneth Galbraith (1908-2006), professor of economics at Harvard University and the author of The New Industrial State (1947).*

Table A8 (continued 2): List of Statements and Sources Used in the Survey

Statement 8:

"The market economy has depended for its own working not only on maximizing profits but also on many other activities, such as maintaining public security and supplying public services—some of which have taken people well beyond an economy driven only by profit. The creditable performance of the so-called capitalist system, when things moved forward, drew on a combination of institutions that went much beyond relying only on a profit-maximizing market economy and on personal entitlements confined to private ownership."

Real Source: *By Amartya Sen, professor of economics and philosophy at Harvard University and the author of Development as Freedom (1999).*
Altered Source (Less-/non-Mainstream): *By Michael Sandel, American political philosopher and professor of government at Harvard University, and the author of What Money Can't Buy: The Moral Limits of Markets (2012).*

Statement 9:

"The laws of property have made property of things which never ought to be property, and absolute property where only a qualified property ought to exist. They have not held the balance fairly between human beings, but have heaped impediments upon some, to give advantage to others; they have purposely fostered inequalities, and prevented all from starting fair in the race."

Real Source: *By John Stuart Mill (1806-1873), an English philosopher, political economist, and the author of On Liberty (1859).*
Altered Source (Less-/non-Mainstream): *By Friedrich Engels (1820-1895), a German philosopher and the co-author of The Communist Manifesto (1848).*

Statement 10:

"Sharp increases in unemployment beyond the business cycle—one in six American men between 25 and 54 are likely to be out of work even after the U.S. economy recovers—along with dramatic rises in the share of income going to the top 1 and even the top .01 per cent of the population and declining social mobility do raise serious questions about the fairness of capitalism..."

Real Source: *By Larry Summers, professor of economics and president emeritus at Harvard University.*
Altered Source (Less-/non-Mainstream): *By Yanis Varoufakis, Greek economist who also served as the Greek Minister of Finance (from January to July 2015, when he resigned), and the author of And the Weak Suffer What They Must? Europe's crisis, America's economic future*

Statement 11:

"It is a great fault of symbolic pseudo-mathematical methods of formalizing a system of economic analysis…that they expressly assume strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed; … Too large a proportion of recent mathematical economics are mere concoctions, as imprecise as the initial assumptions they rest upon, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols."

Real Source (Less-/non-Mainstream): *By Joh Maynard Keynes (1883-1946), professor of economics at Cambridge and the author of The General Theory of Employment, Interest and Money (1936).*
Altered Source: *By Kenneth Arrow, professor of economics at Stanford University and the 1972 recipient of the Nobel Prize in Economics.*

Table A8 (continued 3): List of Statements and Sources Used in the Survey

Statement 12:

"From this failure to expunge the microeconomic foundations of neoclassical economics from post-Great Depression theory arose the "microfoundations of macroeconomics" debate, which ultimately led to a model in which the economy is viewed as a single utility-maximizing individual blessed with perfect knowledge of the future. Fortunately, behavioral economics provides the beginnings of an alternative vision of how individuals operate in a market environment, while multi-agent modelling and network theory give us foundations for understanding group dynamics in a complex society. […] These approaches should replace neoclassical microeconomics completely."

Real Source (Less-/non-Mainstream): *By Steve Keen, post-Keynesian professor of economics at Kingstone University (UK) and the author of Debunking economics: the naked emperor dethroned? (2011).*
Altered Source: *By Paul Romer, professor of economics at the New York University and the author of The Troubles with Macroeconomics (forthcoming in the American Economic Review).*

Statement 13:

"In the progress of the division of labour, the employment of the far greater part of those who live by labour, that is, of the great body of people, comes to be confined to a few very simple operations, frequently one or two. But the understandings of the greater part of men are necessarily formed by their ordinary employments. The man whose whole life is spent in performing a few simple operations, of which the effects too are, perhaps, always the same, or very nearly the same, has no occasion to exert his understanding, or to exercise his invention in finding out expedients for removing difficulties which never occur. He naturally loses, therefore, the habit of such exertion, and generally becomes as stupid and ignorant as it is possible for a human creature to become."

Real Source: *By Adam Smith.*
Altered Source (Less-/non-Mainstream): *By Karl Marx*

Statement 14:

"For four decades, since my time as a graduate student, I have been preoccupied by the kinds of stories about the myriad ways in which people depart from the fictional creatures that populate economic models […]. Compared to this fictional world of Econs, Humans do a lot of misbehaving, and that means that economic models make a lot of bad predictions, predictions that can have much more serious consequences than upsetting a group of students. Virtually no economists saw the financial crisis of 2007–08 coming, and worse, many thought that both the crash and its aftermath were things that simply could not happen."

Real Source: *By Richard Thaler, professor of behavioural science and economics at University of Chicago Booth School of Business and the author of Misbehaving: The Making of Behavioural Economics (2015)..*
Altered Source (Less-/non-Mainstream): *By Gerd Gigerenzer, Director at the Max Planck Institute for Human Development, former professor of psychology at the University of Chicago, and the author of Gut feelings: The intelligence of the unconscious (2007).*

Table A8 (continued 4): List of Statements and Sources Used in the Survey

Statement 15:

"There are powerful forces having to do with the sociology of the profession and the socialization process that tend to push economists to think alike. Most economists start graduate school not having spent much time thinking about social problems or having studied much else besides math and economics. The incentive and hierarchy systems tend to reward those with the technical skills rather than interesting questions or research agendas. An in-group versus out-group mentality develops rather early on that pits economists against other social scientists. […] [E]conomists tend to look down on other social scientists, as those distant, less competent cousins who may ask interesting questions sometimes but never get the answers right. Or, if their answers are right, they are so not for the methodologically correct reasons. Even economists who come from different intellectual traditions are typically treated as "not real economists" or "not serious economists."

Real Source: *By Dani Rodrik, professor of international political economy at Harvard University and the author of The Globalization Paradox: Democracy and the Future of the World Economy (2012)..*

Altered Source (Less-/non-Mainstream): By Anwar Shaikh, professor of economics at the New School for Social Research (New York) and the author of Capitalism: Competition, conflict, Crises (2016).

Note: For each pair of sources we have identified a source as less-mainstream or non-mainstream. Of course, in economics the notions of mainstream and non-mainstream have changed a lot over time. Similar to many other classifications, this is just an attempt to summarize a complex notion and provide an easy-to-understand relative comparison, even though we believe these differences are more appropriately understood as an spectrum rather than a black and white distinction. For the lack of better classifications, therefore, we categorize our sources into these two groups to clearly summarize and represent the relative ideological distance between them to the reader. Of course, it is well understood that this classification does not readily apply to some sources, such as older sources (e.g. Karl Marx or Sigmund Freud) or sources from other disciplines (e.g. Michael Sandel) in the same way it applies to others. However, to remain consistent and avoid confusion for the reader, we stick to the same naming convention for all sources. For example, by using these terms for sources such as Marx or Freud, we intend to highlight that the views they held would have been considered non-mainstream today. Similarly, in cases where both sources might be considered mainstream (e.g. Rodrick Vs. Krugman, or Deaton Vs. Piketty), our use of these terms is intended to be relative rather than absolute.

Table A9: Political typology statements

A. Government regulation of business is necessary to protect the public interest B. Government regulation of business usually does more harm than good
A. Most people who want to get ahead can make it if they're willing to work hard B. Hard work and determination are no guarantee of success for most people
A. Discrimination is the main reason why disadvantaged people (females, immigrants, blacks, etc.) can't get ahead these days B. Disadvantaged people who can't get ahead are mostly responsible for their own condition
A. Business corporations make too much profit B. Most corporations make a fair and reasonable amount of profit
A. Too much power is concentrated in the hands of a few large companies B. The largest companies do NOT have too much power
A. A claim or argument should be rejected only on the basis of the substance of the argument itself. B. A claim or argument should be rejected based on what we know about the views of the author or the person presenting the argument. C. A claim or argument should be rejected based on what we know about the views of the author or the person presenting the argument as well as the substance of the argument.
A. Rise in income inequality over the last few decades has significant socioeconomic consequences and requires serious attention. B. Focus on the rise in income inequality as a socioeconomic concern is wrongheaded. What matters is poverty, not income inequality.
A. It is NOT necessary to believe in God in order to be moral and have good values B. It is necessary to believe in God in order to be moral and have good values

Note: For each pair, participants were asked to pick the option that comes closest to their view.