

Designing Information Provision Experiments[†]

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Information provision experiments allow researchers to test economic theories and answer policy-relevant questions by varying the information set available to respondents. We survey the emerging literature using information provision experiments in economics and discuss applications in macroeconomics, finance, political economy, public economics, labor economics, and health economics. We also discuss design considerations and provide best-practice recommendations on how to (i) measure beliefs; (ii) design the information intervention; (iii) measure belief updating; (iv) deal with potential confounds, such as experimenter demand effects; and (v) recruit respondents using online panels. We finally discuss typical effect sizes and provide sample size recommendations. (JEL C90, D83, D91)

1. Motivation

Standard economic theories usually understand choices as a combination of three factors: preferences, constraints, and beliefs. Information enters the choice environment indirectly by affecting beliefs and (perceived) constraints. For instance, in the context of a firm owner choosing how much to invest, information about historical returns could

affect beliefs about the return on investment, while information about loan opportunities could affect borrowing constraints. The goal of economic experiments is typically to change some features of the choice environment to study how choices are made. Information experiments achieve this by varying the information set available to economic agents.

Information provision experiments allow researchers to provide cleanly identified evidence by only varying one feature of

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Philipp Lergetporer, Michel Marechal, David McKenzie, Brendan Nyhan, Ricardo Perez-Truglia, Peter Schwardmann, Gaurav Sood, Diego Ubfal, Pierre-Luc Vautrey, and Basit Zafar for very useful suggestions. Cecilie Christensen, Hrishikesh Iyengar, Apoorv Kanoongo, and Florian Schoner provided excellent research assistance. Roth: Funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy–EXC 2126/1–390838866.

[†] Go to <https://doi.org/10.1257/jel.20211658> to visit the article page and view author disclosure statement(s).

the information set. This in turn provides researchers with the tools to test basic assumptions of models or differentiate between different theoretical mechanisms. For example, Bursztyń et al. (2014) use information treatments to study the relative importance of “social learning” and “social utility” in the context of peer effects in financial decisions, while Allcott and Taubinsky (2015) use information treatments to identify the importance of different behavioral biases in the lightbulb market.

One powerful possibility opened up by information experiments is to generate exogenous variation in perceptions of real-world environments, which allows answering policy-relevant questions. Information provision experiments allow changing perceptions of real-world phenomena which themselves cannot be directly changed. For instance, in labor economics, an important policy question is to what extent students internalize market returns to education when making educational choices. While researchers cannot manipulate the returns to education, they can provide information to generate exogenous variation in the perceived returns to education (Jensen 2010). When studying attitudes toward immigration, it is impossible to change the characteristics of immigrants, but researchers can vary perceptions of the immigrant population by correcting people’s misperceptions (Grigorieff, Roth, and Ubfal 2020). Similarly, researchers cannot manipulate intergenerational mobility or influence the state of the macroeconomy, but it is possible to change perceptions of intergenerational mobility (Alesina, Stantcheva, and Teso 2018) or the perceived likelihood of a recession (Roth and Wohlfart 2020). Finally, researchers cannot manipulate social norms, but information provision experiments can be used to study the causal effect of perceived social norms on behavior (Bursztyń, González, and Yanagizawa-Drott 2020).

The opportunities provided by information experiments to test economic theories and answer policy-relevant questions have made them popular in economics over the last few years. As shown in figure 1, the number of information provision experiments published in leading economics journals has strongly increased over the last ten years. This growth demonstrates the increasing importance of information provision in experimental work. In this article, we review the growing literature on information experiments in economics with a particular focus on methodological questions. Many of the methodological questions discussed in this review also extend beyond information provision experiments, and have relevance for the design of other types of experiments or for collecting non-experimental survey data.

The paper proceeds as follows: section 2 summarizes areas in which information experiments have been widely applied. Section 3 outlines best-practice recommendations for the measurement of beliefs. Section 4 discusses the design of the information intervention. Section 5 outlines important aspects of the measurement of belief updating. Section 6 discusses best-practice recommendations for mitigating concerns about experimenter demand effects. Section 7 describes online samples that are commonly used for information provision experiments. Section 8 discusses typical effect sizes and provides sample size recommendations. Finally, section 9 offers concluding remarks.

2. *Major Applications*

In this section, we provide an overview of areas in economics in which information provision experiments have been widely applied. This review is necessarily incomplete, and focuses on applications in public economics, political economy, macroeconomics,

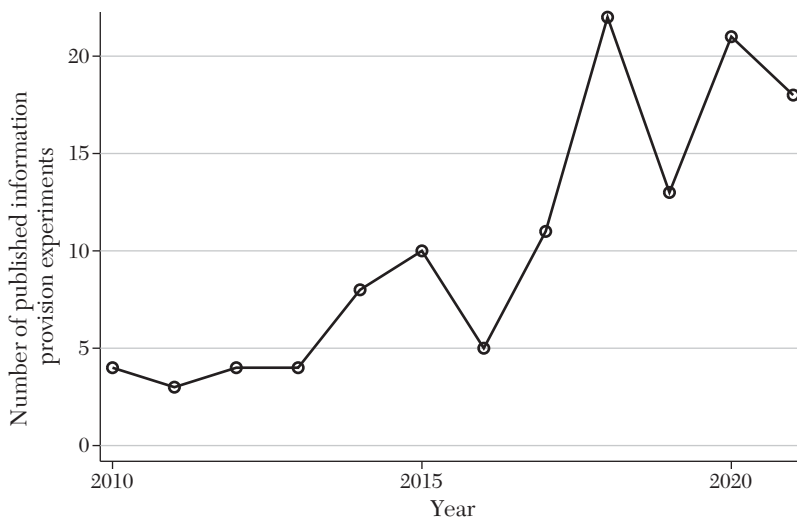


Figure 1. Number of Information Provision Experiments Published in Leading Journals Since 2010

Notes: This figure shows the number of information provision experiments published in leading journals since 2010. For 2021, publications and forthcoming papers as of mid-April are included. The figure is based on publications in the following journals: the *American Economic Review*, the *American Economic Journal: Applied Economics*, the *American Economic Journal: Economic Policy*, the *American Economic Journal: Macroeconomics*, *Econometrica*, the *Economic Journal*, the *Journal of Development Economics*, the *Journal of Political Economy*, the *Journal of Public Economics*, the *Journal of the European Economic Association*, the *Review of Economics and Statistics*, and the *Review of Economic Studies*. To identify articles, we used Google Scholar to search for all articles published in these journals since 2010 containing the words information, treatment, beliefs or expectations, and experiment, and then verified which of the search results featured an information provision experiment. We also supplemented with papers covered in our review that were not captured using this search algorithm. This figure does not include information provision experiments operating in a laboratory setting in which respondents receive information about features of the laboratory environment or the behavior of other participants in the lab. The figure also does not include information provision experiments in which respondents are informed about product characteristics.

household finance, and labor, education, and health economics.¹

Public Economics.—Information provision experiments are used in many areas of public economics. Chetty and Saez (2013)

¹Our review does not include information provision experiments operating in a laboratory setting in which respondents receive information about features of the laboratory environment or the behavior of other participants in the lab. The review also does not feature work studying the role of the media in shaping beliefs and behavior.

conduct an experiment with 43,000 Earned Income Tax Credit (EITC) recipients, in which a random subset received personalized information about the EITC schedule. Chen et al. (2010) find that personalized social information can significantly increase digital public goods provision, in the form of user moving ratings and community database maintenance. Chen, Lu, and Zhang (2017) conduct a field experiment with 75,000 drivers, establishing that social information can reduce traffic violations. Dörrenberg

and Peichl (2018) examine how social norms affect tax morale, and Blesse, Buhlmann, and Dörrenberg (2019) study how beliefs shape preferences for tax simplification.

Bérgolo et al. (forthcoming) and Dörrenberg and Schmitz (2015) examine how firms respond to information about audit probabilities, and Bott et al. (2020) study whether people's tendency to evade taxes responds to information about detection probability and moral appeals. Similarly, Perez-Truglia and Troiano (2018) examine how information on financial penalties, shaming penalties, and peer comparisons shape tax delinquents' future repayment rates. De Neve et al. (2021) study the impact of deterrence, tax morale, and simplifying information on tax compliance. Jessoe and Rapson (2014) show that information about residential electricity usage makes households more responsive to temporary price increases. Finally, a literature in behavioral public economics has studied how misperceptions regarding the fuel economy affect consumers' purchasing decisions (Allcott and Knittel 2019, Allcott and Taubinsky 2015, Allcott 2013).

Political Economy.—Information experiments are also commonly used to study how information affects policy attitudes, such as people's demand for redistribution (Kuziemko et al. 2015; Alesina, Stantcheva, and Teso 2018; Karadja, Mollerstrom, and Seim 2017; Cruces, Perez-Truglia, and Tetaz 2013; Fehr, Mollerstrom, and Perez-Truglia 2022; Gärtner, Mollerstrom, and Seim 2019; Chen, Wang, and Yang 2016; Fehr, Müller, and Preuß 2020; Hoy and Mager 2018), their support for government spending (Lergetporer et al. 2018; Roth, Settele, and Wohlfart 2022a), their views on educational inequality (Lergetporer, Werner, and Woessmann 2020) and tuition fees (Lergetporer and Woessmann 2021), their support for immigration (Alesina,

Miano, and Stantcheva 2023; Haaland and Roth 2020; Grigorieff, Roth, and Ubfal 2020; Facchini, Margalit, and Nakata 2022; Lergetporer, Piopiunuk, and Simon 2021; Barrera et al. 2020; Bansak, Hainmueller, and Hangartner 2016; Hopkins, Sides, and Citrin 2019), their tendency to discriminate against immigrants (Alesina et al. 2018), their support for affirmative action (Haaland and Roth 2023, Settele 2022), or affective party polarization (Ahler and Sood 2018). In the context of the coronavirus pandemic, Settele and Shupe (forthcoming) study the role of beliefs for supporting lockdown measures and Rafkin, Shree Kumar, and Vautrey (2021) study determinants of inference from official government projections.

Information experiments are also conducted to better understand the demand for news and the implications of media exposure for behavior. Chopra, Haaland, and Roth (2022) study how perceived informativeness affects people's demand for economic and political news. Bursztyrn et al. (2023) study how the common knowledge of rationales (which are usually supplied by the media) affects the public expression of xenophobia through the lens of a signaling model.

In the context of natural field experiments, researchers have used information treatments to study voting behavior (Kendall, Nannicini, and Trebbi 2015; Orkin 2019; Cruz, Keefer, and Labonne 2021; Cruz et al. 2018; Gerber et al. 2020; Aker, Collier, and Vicente 2017) or to study strategic behavior of political activists (Hager et al. 2020, 2021) and protesters (Cantoni et al. 2019, Hager et al. 2022).

Experiments studying the effects of information campaigns in the context of political behavior have also been widely applied in developing-country settings. Armand et al. (2020) test whether information can counteract the political resource curse in Mozambique. Acemoglu et al. (2020) study whether information about improved public

services can help build trust in state institutions and move people away from non-state actors in Pakistan. Khan et al. (2021) document that information about past state effectiveness has a limited impact on support for policy, perceptions of state capacity, and trust in the state in Pakistan. Finally, Banerjee et al. (2018) show that mailing cards with program information to targeted beneficiaries reduces “leakage” in redistribution programs due to local officials not implementing government programs.

Macroeconomics.—Information provision experiments have been widely used in macroeconomics to study how households and firms form expectations about inflation (Binder and Rodrigue 2018; Armantier et al. 2016; Coibion, Gorodnichenko, and Weber 2021; Coibion, Gorodnichenko et al. forthcoming; Coibion, Gorodnichenko, and Kumar 2018; Cavallo, Cruces, and Perez-Truglia 2017; Coibion, Gorodnichenko et al. 2021), house prices (Fuster et al. 2022; Armona, Fuster, and Zafar 2019), interest rates (Coibion, Georgarakos et al. 2023; Link et al. 2023), or the broader economy (Coibion, Gorodnichenko, and Weber 2020b). Another set of studies have applied information provision experiments to examine the causal effect of macroeconomic expectations on behavior. For instance, these papers have studied how households’ spending decisions are affected by expectations about GDP growth (Roth and Wohlfart 2020; Coibion, Georgarakos et al. 2021), house prices (Qian 2019), or inflation (Coibion, Georgarakos et al. 2023), and how inflation expectations influence firms’ decisions (Coibion, Gorodnichenko, and Ropele 2020). In a developing-country context, Galashin, Kanz, and Perez-Truglia (2020) examine how information about inflation or the exchange rate affects consumer spending as measured in administrative credit card data. Roth, Settele, and Wohlfart (2022b) study how households’ percep-

tions of their exposure to macroeconomic risk causally affect information acquisition. Finally, information experiments have been used to study the effectiveness of different forms of policy communication (Coibion, Gorodnichenko, and Weber 2022; D’Acunto et al. 2020). In the context of the COVID-19 pandemic, Coibion, Gorodnichenko, and Weber (2020a) and Binder (2020) study how provision of information about policy responses shapes households’ macroeconomic expectations and spending plans.

Household Finance.—Research in household finance has studied the effects of information provision on retirement savings (Beshears et al. 2015, Dolls et al. 2018). Moreover, Bursztyn et al. (2019) examine how moral appeals affect debt repayment. Bursztyn et al. (2014a) study the mechanisms underlying peer effects in financial decisions. Bontan and Perez-Truglia (2020) study the causal effect of home price expectations on the timing of home sales using a large-scale field experiment featuring administrative data. Laudenbach et al. (2021) use an information experiment to study the causal effect of subjective beliefs about stock returns on investment choices measured in administrative account data. In the context of the coronavirus pandemic, Hanspal, Weber and Wohlfart (2021) provide experimental evidence that beliefs about the duration of the stock market recovery shape households’ expectations about their own wealth and their planned investment decisions and labor market activity.

Labor and Education Economics.—Information provision experiments have been applied to study job search (Belot, Kircher, and Muller 2019, 2022; Abebe et al. 2021; Franklin 2018; Carranza et al. 2020; Altmann et al. 2018), social norms (Bursztyn, González, and Yanagizawa-Drott 2020), educational aspirations (Lergetporer,

Werner, and Woessmann 2021), schooling decisions (Jensen 2010), major choice (Wiswall and Zafar 2014, Bleemer and Zafar 2018, Conlon 2021), postgraduate enrolment (Berkes et al. 2022), as well as school choice (Andrabi, Das, and Khwaja 2017; Ajayi, Friedman, and Lucas 2017). Researchers have shown that information about school quality affects parental investment decisions (Greaves et al. 2019), and that parents' beliefs about children's abilities affect their educational investments (Dizon-Ross 2019). Coffman, Featherstone, and Kessler (2017) highlight that information about peers' choices can affect job choice. Researchers in behavioral labor economics have also studied how information provision about peers affects people's work morale and labor market behavior (Cullen and Perez-Truglia 2022, Card et al. 2012). In agricultural economics, information provision experiments are also widely applied; for example, Hanna, Mullainathan, and Schwartzstein (2014) study the effects of information on farmers' behavior. In the context of migration, Baseler (2021) studies how perceived returns to migration shape migration decisions in Kenya, while Shrestha (2020) studies how information about the potential risks of dying and potential wages from working abroad affects actual migration decisions in Nepal. Humphries, Neilson, and Ulyssea (2020) study the role of information frictions for access to the Paycheck Protection Program in the context of the coronavirus pandemic.

Health Economics.—Information provision experiments have also been widely used to study information relevant for health behaviors. In the context of the United States, Nyhan and Reifler (2015) and Nyhan et al. (2014) study the effects of information campaigns about vaccines. Alsan and Eichmeyer (2021) study persuasion regarding the medical benefits of influenza vaccination with a particular focus on racial identity. Barari

et al. (2020) study public health messaging and social distancing in the context of the coronavirus pandemic, while Fetzer et al. (2021) and Akesson et al. (2022) study perceptions of pandemic risk factors. Faia et al. (2022) use an information experiment to study biases in information selection and processing in the context of the pandemic.

In developing-country settings, Kremer and Miguel (2007) document muted effects of information on how to avoid worm infections.² Fitzsimons et al. (2016) find that information provision to mothers in Malawi increases children's food consumption. Carneiro et al. (2020) study an intervention targeting early life nutrition, which also provides nutritional information. Banerjee, Barnhardt, and Duflo (2015) examine how information affects take-up of double-fortified salt. Benneer et al. (2013) examine how household drinking-water choices were affected by two different messages about risk from naturally occurring groundwater arsenic. Madajewicz et al. (2007) show that information about well safety regarding arsenic levels results in large switches from unsafe to safe water sources. Levine and Kinder (2004) discuss the success of an oral rehydration information campaign for diarrhea treatment.

A substantial amount of research has been conducted on the role of beliefs on sexual behaviors. Dupas (2011) and Kerwin (2018) examine how information about HIV risks affects sexual behaviors. Ciancio et al. (2020) study how mortality risk information changes survival expectations and sexual behaviors in Malawi. Chong et al. (2013) study the impact of an online sexual health education course provided through schools in Colombia. Jamison, Karlan, and Rafterl (2013) examine how information about sexual and reproductive health affects

²For a review of this literature in developing-country settings, see Dupas and Miguel (2017).

knowledge and sexual behavior in a general population sample in Uganda. Shacham, Godlonton, and Thornton (2014) study how information about the relationship between circumcision and HIV status affects sexual behavior in Malawi. Miller, De Paula, and Valente (2020) find that simply informing women about pregnancy risk increases stated intentions to use contraception substantially. Researchers have also studied how learning about one's HIV status affects sexual behavior (Delavande and Kohler 2012, Gong 2015).

As Kremer, Rao, and Schilbach (2019) note, most of the studies in the context of developing countries focus on the effects of information on behavior and not beliefs. We think that the elicitation of beliefs would be helpful to more clearly understand why certain information interventions are more successful in changing behavior than others. This is especially important in the context of health behaviors, where trust in information may vary substantially depending on prior belief and the identity of the sender.

3. *Measuring Beliefs*

Information provision experiments aim to study the effect of information on people's beliefs or to generate exogenous variation in beliefs to study the effects on other outcomes. This section discusses whether one should measure prior beliefs before the information provision and posterior beliefs after the information provision. It also discusses issues related to the measurement of beliefs, including advantages and disadvantages of measuring qualitative or quantitative point beliefs versus probabilistic beliefs, the use of external benchmarks for the elicitation of beliefs, the framing of belief elicitation, and techniques on how to deal with measurement error. Finally, we review the measurement of beliefs using hypothetical vignettes.

3.1 *Eliciting Prior and Posterior Beliefs?*

There are several advantages to eliciting prior beliefs in information provision experiments. First, eliciting prior beliefs about the provided piece of information allows researchers to estimate heterogeneous treatment effects by prior beliefs. This is particularly relevant in designs with a pure control group (that is, a control group that does not receive any information). Depending on their priors, groups of participants may update their beliefs in different directions in response to the information, leading to a muted average treatment effect. For instance, consider the experiment by Cruces, Perez-Truglia, and Tetaz (2013), which gives people information about their relative position in the income distribution. Since some people overestimate their position while other people underestimate their position, providing accurate information will lead overestimators and underestimators to update their beliefs in opposite directions. Eliciting prior beliefs is, therefore, necessary to make a directional prediction about how different groups should update their beliefs and change their behavior in response to the information. Furthermore, even if all respondents update their beliefs in the same direction, analyzing heterogeneity by prior beliefs allows the researcher to assess whether treatment effects on the outcome of interest are larger for respondents who received a larger information shock. Such heterogeneous effects by prior beliefs are often viewed as evidence that treatment effects are driven by genuine changes in beliefs rather than priming (see section 4.5). Second, when the outcome of interest is an expectation, eliciting prior beliefs about the information allows the researcher to estimate learning rates (see section 8). Third, eliciting prior beliefs about the information or prior beliefs about the outcome of interest increases statistical

power for detecting treatment effects (Clifford, Sheagley, and Piston 2020).

Eliciting posterior beliefs is important in settings where there is a direct interest in studying the effect of information on these beliefs. Moreover, the measurement of posterior beliefs is necessary to learn about the size of the first stage in settings where information provision experiments are used to study the causal effects of beliefs on other outcomes. In settings where respondents are provided with information about facts (see, e.g., Roth, Settele, and Wohlfart 2022a), eliciting posteriors primarily serves to measure trust in or attention to the information. As such, eliciting posteriors is less strictly needed than in designs where respondents receive a potentially noisy signal about a variable (e.g., Roth and Wohlfart 2020), where posteriors are used to assess how informative respondents find the provided signal.

A potential downside of designs measuring both priors and posteriors about the same object is that such within-designs potentially lead to stronger experimenter demand effects (see section 6). Alternatively, respondents may be subject to consistency bias in their survey responses (Falk and Zimmermann 2013), leading to a muted effect of information in within-designs. However, Roth and Wohlfart (2020) do not find any significant effect of eliciting priors on the estimated average learning rate in the context of information about macroeconomic risk. Similarly, Clifford, Sheagley, and Piston (2020) find little evidence of bias in estimated treatment effects due to the elicitation of prior beliefs in survey experiments on political attitudes. Moreover, in designs with a pure control group, being asked the same question twice might confuse respondents in the control group. One remedy is to use a different elicitation mode for the posteriors compared to the priors (Coibion,

Gorodnichenko, and Weber 2022) or to elicit posttreatment beliefs about a related but different outcome (Haaland and Roth 2023).

3.2 *Qualitative, Quantitative, or Probabilistic Beliefs?*

How exactly should one measure beliefs? Should one measure beliefs using qualitative or quantitative survey questions? Should one measure point estimates on quantities or probabilistic beliefs in which people attach probabilities to different states of the world occurring?³

Qualitative Beliefs: Verbal Response Scales.—One way to measure beliefs is to present respondents with verbal response scales, for example, reaching from “very low” to “very high,” or from “strongly agree” to “strongly disagree.” Such belief measures have the simple advantage that the response options are easy to understand for respondents, but a clear disadvantage is that they are not easily interpersonally comparable, which can result in severe identification challenges (Bond and Lang 2019). For instance, in the context of measuring beliefs about the size of the immigrant population, people might hold systematically different views on whether a given fraction of immigrants in the population is “very low” or “very high.” Such differences in the interpretation of qualitative response options can be driven by partisanship, as shown by Gaines et al. (2007) for the case of beliefs about the Iraq war. Moreover, verbal response scales are relatively crude and therefore limit the extent of information that can be conveyed (Manski 2017). Furthermore, with qualitative

³See Delavande, Giné, and McKenzie (2011) and Delavande (2014) for excellent overviews on the measurement of subjective beliefs with a particular focus on developing-country settings.

beliefs, it is often theoretically ambiguous in which direction people should update their beliefs in response to an information treatment. For instance, to manipulate perceptions about the size of the immigration population in the United States, one could inform treated respondents that 12 percent of the US population is made up of immigrants (Hopkins, Sides, and Citrin 2019; Grigorieff, Roth, and Ubfal 2020). Without a quantitative pre-treatment beliefs measure, it is not clear whether treated respondents should revise their beliefs about the size of the immigration population upward or downward in response to this information. At the same time, given their advantages in terms of simplicity, including qualitative belief measures can at least serve as a robustness check for results based on quantitative belief data.

Qualitative Beliefs: Open-Ended Questions.—It is also possible to use open-ended questions to measure beliefs (Bursztyn et al. 2023; Stantcheva 2020; Andre et al. 2019). The key advantage of such open-ended questions is that respondents are not primed by the available response options. In other words, open-ended questions enable researchers to directly measure what “comes to mind.” They therefore allow researchers to shed light on people’s attention allocation. For example, Andre et al. (2022) study which propagation mechanisms come to households’ and experts’ minds when thinking about canonical macroeconomic shocks. In the context of macroeconomic expectation formation, Leiser and Drori (2005) and D’Acunto et al. (forthcoming) study people’s associations with inflation using open-ended text questions. Stantcheva (2020) examines what considerations people have in mind when thinking about a given policy. Bursztyn et al. (2023) use such an open-ended elicitation to study inference about the motives for xenophobic expression. Using a preregistered

text analysis procedure and hand coding of the qualitative responses, they use this data for studying inference. They validate their open-ended question with a structured belief measure and establish strong correlations. In the context of information provision experiments, open-ended questions have two main purposes. First, they can be used as a validation check for the quantitative belief data (as in Bursztyn et al. 2023). Second, they can be used to study whether providing information changes people’s attention to a given topic.

Quantitative Point Beliefs.—Quantitative point beliefs, where respondents are asked to state their beliefs on a numerical scale, have the advantage of interpersonal comparability while still being relatively straightforward for respondents to understand, but they do not allow for individuals to express their uncertainty about outcomes. It is therefore a good practice to add a second qualitative question on how sure or confident people were in their previous answer. For instance, such qualitative measures of confidence can be used for tests of Bayesian updating (Armona, Fuster, and Zafar 2019; Roth and Wohlfart 2020)⁴ or to examine whether subjective measures of confidence are related to the accuracy of people’s beliefs (Graham 2018). Furthermore, eliciting confidence allows the researcher to differentiate between strong misperceptions and a lack of knowledge. A second disadvantage of point beliefs is that it is unclear which feature of their subjective belief distribution over potential future outcomes respondents report. While researchers often implicitly or explicitly interpret point beliefs as the mean over the respondent’s subjective distribution, respondents may report their median

⁴Since confidence in priors is not randomly assigned and is likely correlated with other variables, such tests are often only suggestive.

or mode belief.⁵ Lastly, people's point beliefs might be sensitive to question framing (Eriksson and Simpson 2012).

Probabilistic Beliefs.—In probabilistic belief elicitation, respondents state probabilities for the occurrence of different mutually exclusive events. Such probabilistic elicitation has the advantage that there is a well-defined absolute numerical scale that is interpersonally and intrapersonally comparable (Manski 2017). Probabilistic elicitation was pioneered by Manski (2004) and has been widely and successfully applied in some areas in economics such as labor economics and the economics of education (Attanasio, Boneva, and Rauh 2022; Boneva and Rauh 2017; Boneva, Golin, and Rauh 2022; Wiswall and Zafar 2018, 2021) as well as health economics (Delavande and Kohler 2009). These measures allow researchers to directly compute a measure of uncertainty as well as the mode, the median, and the mean. Probabilistic data also enable researchers to characterize the nature of updating beyond showing average learning rates, or to document biases in updating. For instance, Barron (2021) uses a probabilistic elicitation to test whether people update their beliefs in the financial domain in line with a Bayesian benchmark. Directly measuring people's uncertainty has recently received additional attention in the context of abstract choice and updating tasks as well as survey expectations. Enke and Graeber (2019) propose a measurement of cognitive uncertainty and show that people who are cognitively uncertain implicitly compress

probabilities toward a cognitive default of 50:50 in binary state spaces.

One drawback of probabilistic scales is that a large fraction of the population has difficulties in understanding and interpreting probabilities (Tversky and Kahneman 1974). A second drawback is that people's stated beliefs are typically influenced by how the outcomes are categorized (Benjamin, Moore, and Rabin 2017). A third drawback is that probabilistic questions are more time consuming and taxing for respondents, which makes the experiment longer and potentially induces higher attrition or a higher fraction of missing responses. Some survey providers might also object to the use of probabilistic questions as they might confuse respondents. However, best-practice recommendations, such as the use of visual interfaces, have been shown to mitigate some of these problems, even when working with low-literacy populations (Delavande, Giné, and McKenzie 2011; Delavande 2014).

3.3 *Benchmarks*

One approach measures beliefs about objects of interest for which there is an objective external benchmark. For instance, in the context of income inequality, one can elicit beliefs about the income share going to the top 1 percent of income earners rather than a general question about whether income inequality is "high" or "low." Measuring subjective beliefs about quantities with well-defined benchmarks has several advantages. First, by eliciting beliefs about a well-defined benchmark the experimenter fixes beliefs about the environment and imposes additional structure on the responses. This in turn may lower heterogeneity in how the question is interpreted and thereby reduce measurement error and make responses across participants more comparable. Second, it allows one to characterize the extent of biases in beliefs compared to the benchmark. Third,

⁵For instance, De Bruin et al. (2011) show that survey respondents' point forecasts about future inflation or future wage growth are not consistently associated with means constructed from individual-level subjective probability distributions over future inflation or wage growth, but are often associated with the median or other measures of central tendency of respondents' reported distribution.

it enables one to incentivize the belief elicitation in a transparent way. Fourth, the availability of benchmarks allows for the provision of information treatments that are tightly connected with the belief elicitation. Recent applications of belief elicitation reliant on benchmarks are studies on social norm elicitation (Krupka and Weber 2013), racial discrimination (Haaland and Roth 2023), intergenerational mobility (Alesina, Stantcheva, and Teso 2018), immigration (Haaland and Roth 2020; Grigorieff, Roth, and Ubfal 2020; Alesina, Miano, and Stantcheva 2023), or infectious disease spread (Fetzer et al. 2021, Akesson et al. 2022).

3.4 Framing of Belief Elicitations

In settings in which respondents are relatively experienced, they are capable of accurately assessing economic quantities. For example, respondents are relatively good at assessing the price of gas (Ansolabehere, Meredith, and Snowberg 2013). However, in settings in which respondents are relatively unfamiliar, there will be higher levels of measurement error especially when respondents are unsure about the response scale, for example in the context of unemployment estimates. However, careful framing of questions can reduce measurement error. For example, the provision of anchors that convey information about the response scale can reduce measurement error (Ansolabehere, Meredith, and Snowberg 2013). For instance, Roth, Settele, and Wohlfart (2022a) measure beliefs about the debt-to-GDP ratio in the United States using different historical or cross-country anchors, and show that the provision of an anchor reduces the dispersion of beliefs and rounding.

3.5 Multiple Measurement

Many belief elicitation experiments involve asking difficult questions to respondents. The

cognitive strain in turn may induce measurement error. How can researchers mitigate the extent of measurement error? Gillen, Snowberg, and Yariv (2019) propose an instrumental variables (IV) approach, which leverages multiple measurements to deal with classical measurement error. We believe that this is particularly important in the context of (quantitative) belief measurement. When reducing classical measurement error is important, researchers ideally should measure their belief of interest using (i) a qualitative survey question, (ii) a quantitative point estimate, and (iii) a probabilistic question in which respondents attach probabilities to mutually exclusive states of the world. These multiple measurements in turn can be leveraged to employ the IV methods that help to deal with measurement error (Gillen, Snowberg, and Yariv 2019). For instance, Giglio et al. (2021) apply such an IV approach in the context of survey expectations about stock returns, using both point beliefs and subjective probability distributions. However, since multiple measurements might be cognitively taxing for respondents, their benefits must be weighed against the risk of increasing survey fatigue or higher attrition rates. Moreover, this approach cannot be used in the case of nonclassical measurement error. Finally, randomly assigned information treatments can also be used to instrument beliefs, and thereby deal with measurement error to some extent (for a discussion of such an IV approach, see section 8).

3.6 Incentives

When eliciting beliefs with an objective external benchmark, it is possible to provide accuracy incentives to encourage higher effort and more truthful responses. For instance, with a discrete outcome, one can promise the respondents a monetary reward if they answer the question correctly. Similarly, with a continuous outcome, one

can offer the respondents a monetary reward if their answer is within some percentage range of the correct answer. The advantages of these mechanisms are that they are simple to explain to respondents and provide stark incentives to provide correct answers. The disadvantage of these mechanisms is that they are only incentive compatible for eliciting the mode of a respondent's belief distribution. While respondents in some situations might be perfectly willing to provide truthful responses even in the absence of monetary incentives, incentives might be especially important in political settings where respondents might form motivated beliefs or receive expressive utility from stating beliefs that are consistent with their partisan leanings (e.g., Democrats stating a low unemployment rate under a Democratic president). Consistent with survey respondents stating beliefs in a motivated way, incentives have been shown to reduce partisan bias in people's stated beliefs about economics and politics (Prior, Sood, and Khanna 2015; Bullock et al. 2015). For example, the partisan gap in beliefs about the current unemployment rate shrinks when respondents receive prediction incentives.⁶ Relatedly, Settele (2022) shows that gender differences in reported beliefs about the gender wage gap shrink in the presence of incentives. Peterson and Iyengar (2020) find a moderate reduction in partisan differences in beliefs on topics such as climate change, immigration, or firearms when survey respondents are provided with incentives, and Berinsky (2018) finds small effects of incentives on respondents' tendencies to endorse political rumors. Allcott et al. (2020), on the other hand, find no effect of incentives on partisan differences in beliefs about the coronavirus pandemic. Trautmann and van de Kuilen

(2014) find that incentives do not improve the accuracy of people's predictions about the behavior of others in lab games. In the context of macroeconomic forecasting, it has been shown that non-incentivized survey reports strongly correlate with incentivized belief measures (Armantier et al. 2015) and that incentives do not have any statistically significant effects on reported beliefs (Roth and Wohlfart 2020). Similarly, Andre et al. (2022) find muted effects of incentives on the accuracy of macroeconomic beliefs, even though response time significantly increases. Moreover, Hoffman and Burks (2020) find no effect of incentives on workers' tendency to overestimate their productivity. Finally, Grewenig et al. (2022) provide mixed evidence on the relevance of incentives in shaping accuracy. Their evidence highlights that incentives have similar effects as a prompt to google the statistic of interest.⁷ This highlights the potential undesirable side-effects of incentives when the information of interest is publicly available.

Eliciting Probabilistic Beliefs with Incentives.—If the goal is to elicit probabilistic beliefs in an incentive-compatible way, one needs to use a proper scoring rule. A commonly used method is the quadratic scoring rule (QSR) which provides incentive compatibility for risk-neutral agents (Brier 1950). Palfrey and Wang (2009) and Wang (2011) provide evidence that the QSR induces more accurate and better-calibrated forecasts than improper scoring rules or

⁶This evidence suggests that prediction incentives can lower motivated errors, thereby reducing nonclassical measurement error.

⁷This issue might be mitigated by using software that prevents people from going outside of their current browser. Alternatively, one may ask respondents not to use outside information, but it is also conceivable that such messages might themselves have backfiring effects. In cases where it is very important to have tight control over the information environment, information provisions experiments can also be conducted in the laboratory (see e.g., Bursztyn, González, and Yanagizawa-Drott 2020 for an example of an information provision experiment performed in a laboratory setting).

non-incentivized belief elicitation, respectively. More recently, the binarized scoring rule (BSR) by Hossain and Okui (2013) has become a popular alternative to the QSR.⁸ Compared to the QSR, in which the size of the reward depends on the accuracy of people's estimates, the BSR offers a fixed reward in which the chance of winning the reward increases in the accuracy of people's estimates. This makes the BSR incentive compatible irrespective of risk preferences. A clear disadvantage of both mechanisms is that they suffer from flat incentive structures in which relatively large deviations from truthful reporting generate only modest changes in the expected rewards. Furthermore, Danz, Vesterlund, and Wilson (2022) provide evidence that complex incentive schemes could lead to less truthful reporting by making respondents misunderstand the incentive scheme. Specifically, they find a strong increase in truthful reporting in a treatment without any information about how incentives are determined compared to a baseline condition providing full information about how incentives are determined by a BSR. Based on this, we consider it best practice to simply inform respondents that it is in their best interests to provide truthful responses when using a proper scoring rule and only provide the mathematical details to respondents who express an explicit interest, for instance, in the form of a clickable pop-up box.

Taken together, while incentives seem to be important when eliciting beliefs in the political domain, incentives seem to have little effect on stated beliefs in nonpolitical domains.⁹ Furthermore, incentives could

backfire when the true answer can easily be googled or when the complexity of the incentive structure makes respondents misunderstand the payoff structure.

3.7 Hypothetical Vignettes

Another approach to measuring beliefs is to ask respondents to make predictions about an outcome under different hypothetical scenarios. The use of such hypothetical vignettes is an increasingly popular approach to measuring beliefs in contexts that are difficult to study in a real-world setting, such as education and human capital (Attanasio, Boneva, and Rauh 2022; Delavande and Zafar 2019; Wiswall and Zafar 2018; Boneva and Rauh 2017, 2018; Kiessling 2021), preferences over wealth taxation (Fisman et al. 2017), and beliefs about the effects of macroeconomic shocks (Andre et al. 2022). Hypothetical vignettes, in the form of conjoint experiments where many different attributes are simultaneously randomized, are widely used to study preferences over different types of immigration (Hainmueller and Hiscox 2010; Hainmueller and Hopkins 2014; Bansak, Hainmueller, and Hangartner 2016). Hainmueller, Hangartner, and Yamamoto (2015) show that the responses in the vignettes are highly predictive of real-world behaviors.

Hypothetical vignettes have the advantage of allowing the researcher more control over the context specified to respondents. Potential disadvantages of hypothetical vignettes include that the hypothetical nature may lower respondents' effort or induce experimenter demand effects. Furthermore, when designing hypothetical vignettes, it is important to consider whether experimentally manipulating an attribute also changes beliefs about other background characteristics (Dafoe, Zhang, and Caughey 2018). For

⁸Eyting and Schmidt (2021) propose a modified version of the BSR based on multiple point predictions.

⁹However, most of this evidence is based on relatively small stakes and it is an open question whether high-stakes incentives would change stated beliefs more strongly. Furthermore, there is some evidence suggesting that incentives improve the accuracy of forecasts in the context of economic games (Wang 2011) and incentives have also

been shown to increase performance in memory and recall tasks (Camerer and Hogarth 1999).

instance, manipulating whether an immigrant is described as “motivated to find work” or “not motivated to find work” might not isolate economic concerns about immigration if the manipulation also changes beliefs about how likely they are to fit in culturally. Finally, it may be cognitively challenging for respondents to think in hypotheticals, which could in turn increase measurement error and reduce external validity.

4. *Designing the Information Intervention*

In this section, we discuss issues related to the design of the information intervention. First, we highlight different types of information that have been provided in prior work. Second, we discuss which sources of information are commonly used. Third, the section reviews issues related to the presentation of the information. Fourth, we lay out ways that researchers can more credibly identify the effects of information rather than the effects of priming individuals on an issue. Finally, we review commonly used methods that employ probabilistic information treatments.

4.1 *Types of Information*

Quantitative Information.—Many survey experiments provide respondents with quantitative information, such as statistics based on official census data (Kuziemko et al. 2015; Grigorieff, Roth, and Ubfal 2020; Bontan and Perez-Truglia 2022; Roth, Settele, and Wohlfart 2022b) or expert forecasts about the future of the economy (Roth and Wohlfart 2020; Armantier et al. 2016; Coibion, Gorodnichenko, and Weber 2022). While quantitative information may be hard to understand for a large fraction of the population, it often facilitates the interpretation of experimental findings in the context of a theoretical framework. Moreover, together with elicited priors and posteriors, numerical information allows for the calculation of learning

rates (see section 8). Many times researchers provide statistical information about the behavior of others (Coffman, Featherstone, and Kessler 2017; Duflo and Saez 2003; Allcott 2011). A commonly used strategy provides a random subset of respondents with information about others’ effort choices (Cantoni et al. 2019; Hager et al. 2020, 2021) or others’ beliefs, preferences, and actions (Bursztyn, González, and Yanagizawa-Drott 2020; Bursztyn, Egorov, and Fiorin 2020; Coibion, Gorodnichenko et al. 2021).

Anecdotal Evidence, Stories, and Narratives.—Another highly relevant and important, but different, type of information relies on qualitative anecdotes, stories, or narratives.¹⁰ This information is not based on statistics, but instead provides qualitative information that closely resembles case studies. Experiments systematically studying the role of stories, anecdotal evidence, and narratives are still very scarce, and we believe a fruitful area for future research. Anecdotal information can also be communicated via pictures and videos, which may be more effective in conveying information. A literature in development economics has studied how inspirational videos change people’s beliefs and economic behavior (Bernard et al. 2014, Riley 2022).¹¹

Tailored versus General Information.—One key difference across information treatments is whether the treatments are tailored to individuals or whether they concern more general pieces of information. For example,

¹⁰Bénabou, Falk, and Tirole (2018) study the role of narratives from a theoretical perspective.

¹¹This is also related to a literature studying how the media affects people’s beliefs and their behavior (Banerjee, La Ferrara, and Orozco-Olvera 2019; Bursztyn, Cantoni et al. 2020; La Ferrara, Chong, and Duryea 2012; DellaVigna and Kaplan 2007; Martinez-Bravo and Stegmann 2022; Bursztyn et al. 2022; Yanagizawa-Drott 2014).

while Roth and Wohlfart (2020) provide all respondents with information about general economic conditions, Roth, Settele, and Wohlfart (2022b) provide individuals with economic information based on their personal characteristics. In the context of health behaviors, Prina and Royer (2014) study the impact of providing tailored information to parents about the body weight of their own school-aged children.

4.2 Sources of Information

There are many different sources for information that prior research has used to exogenously vary respondents' beliefs and expectations. Researchers commonly provide respondents with official government statistics (for instance, about the unemployment rate among immigrants [Grigorieff, Roth, and Ubfal 2020] and research evidence, for instance, about the labor market effects of immigration [Haaland and Roth 2020], racial discrimination [Haaland and Roth 2023], intergenerational mobility [Alesina, Stantcheva, and Teso 2018], or the economic cost of pandemic response measures [Settele and Shupe forthcoming]). In the context of forward-looking expectations, one method to exogenously vary expectations is the provision of expert forecasts. In the context of macroeconomic forecasts, Roth and Wohlfart (2020) provide respondents with different forecasts about the likelihood of a recession and Hager et al. (2022) provide different expert forecasts about the anticipated turnout to different protests. In experiments that aim to change perceptions of social norms, researchers provide respondents with information about the views of respondents as measured in other surveys (Bursztyń, González, and Yanagizawa-Drott 2020). Moreover, researchers have also explored the effects of randomly providing news articles or statements from policy makers on people's beliefs and expectations (Coibion, Gorodnichenko, and Weber 2022).

In general, it is important to consider how credible respondents find the source of information. Rafkin, Shree Kumar, and Vautrey (2021) randomize exposure to information that highlights the government's inconsistency in the context of the coronavirus epidemic. They show that when inconsistency is salient, participants have a reduced propensity to revise prior beliefs about death counts and lower self-reported trust in the government.

Generating a First Stage on Beliefs.— Sometimes, the researcher can choose between several different truthful sources of information that might differ in how closely aligned they are likely to be with people's prior beliefs. If the goal of the information provision experiment is to generate the largest possible first-stage effect on beliefs, one needs to provide information that is sufficiently different from people's prior beliefs to generate an effect. However, if the information provided is too extreme, respondents might find the information less credible (Gentzkow and Shapiro 2006), making it necessary to strike a trade-off between providing a large information shock while retaining trust in the information provided.

4.3 Presentation of the Information

How should researchers present the information in order to maximize the effectiveness of the information intervention? To minimize concerns about demand effects, the treatment should ideally be short and neutrally framed. At the same time, to generate a strong first-stage effect on beliefs, it is important to present the information in a way that maximizes understanding among respondents. One way to increase the understanding of the treatment message is to supplement the text with a graphical illustration of the information. In designs in which researchers elicit prior beliefs, an intuitive way of presenting the information graphi-

cally contrasts prior beliefs with the value from the information treatment (see, for instance, Roth and Wohlfart 2020).

4.4 *Identity of the Sender*

A key question in the design of the information intervention concerns the identity of the sender of the information. The identity of the sender plays a particularly important role in domains in which trust in the information is essential. For example, Alsan and Eichmeyer (2021) study persuasion regarding the medical benefits of influenza vaccination by experimentally varying race concordance between sender and receiver. They find that race concordance improves ratings of the sender and signal, but only among Black respondents. Banerjee et al. (2020) study messaging about the coronavirus pandemic using a prominent Nobel laureate as the messenger. Alatas et al. (2020) study why messaging from celebrities affects the effectiveness of information dissemination on social media. Korlyakova (2021) varies whether people receive information about ethnic discrimination from experts or from ordinary people and finds larger belief updating from information provided by experts. D'Acunto, Fuster, and Weber (2021) show that diverse policy committees are more effective in managing expectations of underrepresented groups.

In the political domain, it is possible that recipients of information will think that the information source is biased (for example, households that expect the government to manipulate official inflation statistics). Cavallo, Cruces, and Perez-Truglia (2016) show that households update their beliefs taking the perceived bias of the information source into account. In the context of macroeconomic expectation formation, Coibion, Gorodnichenko, and Weber (2022) study how varying the source of information about monetary policy affects updating of inflation expectations. In general, it is good practice

to include direct questions on how credible and accurate people found the provided information at the end of the survey.

4.5 *Priming versus Information*

One key challenge in information experiments is to disentangle the effects of priming from genuine belief updating.¹² Common methods to mitigate concerns about priming include (i) eliciting prior beliefs of respondents in both the treatment and the control group, (ii) separating the information provision from the main outcomes with follow-up studies, and (iii) including an active control group (that is, the control group also receives differential information). The first approach guarantees that respondents in both the treatment and the control groups are primed on the issue of interest. Moreover, eliciting priors allows researchers to examine whether treatment effects are stronger among respondents whose priors are less aligned with the information, which is often interpreted as evidence of genuine changes in beliefs (Lergetporer et al. 2018; Roth, Settele, and Wohlfart 2022a; Armantier et al. 2016). The second approach ensures that any short-lived priming effects are no longer relevant when the main outcomes are elicited. The third approach ensures that respondents across all conditions receive information on the issue of interest, but the information differs in terms of its content. In the following, we discuss the use of active control groups in more depth.

Active versus Passive Control.—Many information provision experiments measure prior beliefs on an issue and then provide the treatment group with information on that issue, while a pure control group receives no information at all. An alternative design would measure prior beliefs and then

¹²For an excellent review on priming in economics, see Cohn and Maréchal (2016).

provide the treatment and control groups with different information (this approach of using an active control group was pioneered by Bottan and Perez-Truglia 2022; for other recent examples of papers implementing active control groups in information provision experiments, see Roth and Wohlfart 2020; Hager et al. 2022; Roth, Settele, and Wohlfart 2022b; Settele 2022; Akesson et al. 2022; Link et al. 2023).

Providing the control group with information has several advantages for studying the causal effect of expectations on behavior. In a design with a pure control group, the variation hinges on prior beliefs. The identification mostly comes from individuals with larger misperceptions *ex ante*. An active control group design generates variation in the relevant belief also among individuals with more accurate priors, and therefore identifies average causal effects of beliefs on outcomes for a broader population. Moreover, receiving an information treatment may have side effects, such as uncertainty reduction, attention, and emotional responses (especially in designs where respondents have been corrected). Such side effects should arguably be similar across groups that receive different pieces of information. Finally, since prior beliefs may be measured with error and correlated with both observables and unobservables, causal identification and the interpretation of heterogeneous treatment effects are more difficult in designs with a pure control group.

There are also some advantages to having a pure control group. First, having a pure control group makes it easier to interpret correlations between the pretreatment beliefs and the outcome of interest, as beliefs among control group respondents are not affected by the treatment. Second, sometimes the policy-relevant question of interest is concerned with the effect of providing a particular piece of information compared to not providing this information. See a discussion of these issues in Roth and Wohlfart

(2020) in the context of experiments on macroeconomic expectations or in Hager et al. (2022) in the context of strategic interactions in political movements. Furthermore, sometimes it is not possible to have an active control group without deceiving respondents, in which case it is better to have a pure control group or employ a probabilistic design as discussed below.

4.6 Probabilistic Information Treatments

Researchers have started to use probabilistic information treatments to compare belief updating to Bayesian benchmarks (Eil and Rao 2011, Möbius et al. 2022, Zimmermann 2020, Thaler 2020). In probabilistic information treatments, respondents are told that with a probability p they will learn the truth about a fact, and with probability $(1 - p)$ they will learn the opposite of the truth. Employing probabilistic information treatments provides researchers with fully exogenous variation in beliefs in settings where only one piece of truthful information about a benchmark is available, and otherwise one would have to revert to a design with one treatment group and a control group. It also provides researchers with a Bayesian benchmark for belief updating. However, probabilistic signals introduce more scope for motivated beliefs into the updating process, which could in turn lower the effectiveness of the information treatment (Eil and Rao 2011, Möbius et al. 2022, Thaler 2020). Probabilistic information treatments are usually applied to study motivated reasoning in belief updating, rather than studying causal effects of information and beliefs on behaviors. A downside of probabilistic information treatments is that they are more artificial and less natural for respondents.

Finally, Schwarzmann and van der Weele (2019) use probabilistic information treatments to study how self-confidence affects the persuasiveness of the messenger and find

that higher self-confidence causally increases the persuasiveness of the messenger.

5. *Measuring Belief Updating*

In order to understand the mechanisms through which an information treatment operates, it is essential to measure a rich set of beliefs that capture the theoretical mechanisms that may be at play. We first discuss how to circumvent issues related to numerical anchoring. Second, we argue that measurement of beliefs about the provided information should be more commonly used to better understand and interpret the effects of information.

Numerical Anchoring.—An additional methodological concern for quantitative outcome measures elicited after the information provision, such as posterior beliefs about the statistic, is unconscious numerical anchoring. There are several best practices for alleviating concerns about numerical anchoring. First, one can provide irrelevant numerical anchors and test their effects on the posterior belief of interest in order to gauge the importance of such anchoring (Coibion, Gorodnichenko, Kumar et al. 2020; Cavallo, Cruces, and Perez-Truglia 2017; Roth and Wohlfart 2020). Second, one should measure at least some quantitative beliefs on a scale that differs from the scale on which the information is communicated. Third, one should also employ qualitative measures of beliefs, which are naturally immune to numerical anchoring.

Follow-Up Surveys.—Follow-up surveys, conducted a few weeks after the initial information intervention, are an important tool used to mitigate concerns about numerical anchoring, which is a short-lived phenomenon. Follow-up surveys also alleviate concerns about consistency bias in survey responses (Falk and Zimmermann 2013).

Follow-up surveys to study whether information provision has persistent effects on beliefs, preferences, and behaviors are increasingly common and were pioneered by Kuziemko et al. (2015), Cavallo, Cruces, and Perez-Truglia (2017), and Coppock (2016) in the context of survey experiments. Follow-ups in the context of information experiments usually take place one to eight weeks after the initial information provision. An exception is Fehr, Mollerstrom, and Perez-Truglia (2022) whose follow-up takes place one year after the initial information provision. In choosing the time between main and follow-up surveys, researchers often face a trade-off between testing for persistence and maximizing the recontact rate of respondents.

Measuring Beliefs about the Information.—Finally, in order to obtain a better understanding of the effects of the information treatment, we think that researchers should measure trust in, and other beliefs about, the provided information. For example, Haaland and Roth (2020) elicit a rich set of beliefs about the research evidence provided to respondents. Naturally, such explicit questions may induce significant experimenter demand effects. One way to mitigate concerns about such experimenter demand effects is to elicit incentivized measures of willingness to pay for the information of interest (Hjort et al. 2021; Haaland and Roth 2023; Fehr, Mollerstrom, and Perez-Truglia 2022; Hoffman 2016).

Cross-Learning.—Another recurring issue in information provision experiments is cross-learning. Specifically, respondents may not only update beliefs about the object of interest but at the same time change their beliefs about other variables. For instance, Coibion, Georgarakos et al. (2023) find that the provision of information about inflation not only changes respondents' inflation expectations but also their

beliefs about GDP growth. On the one hand, such cross-learning can be seen as a natural by-product of experimental changes in beliefs, as changes in beliefs due to natural variation are similarly often correlated across variables. On the other hand, cross-learning can complicate the interpretation of IV estimates exploiting randomized information provision, as such estimates are often compared to theoretical benchmarks that do not account for cross-learning. In other words, in the presence of substantial cross-learning it is less straightforward to interpret the effects of information on behavior through the lens of belief changes.

One way to overcome the issue of cross-learning is to hold fixed beliefs about other variables by providing identical information about the other variables to respondents in both the control and treatment groups. However, simultaneous provision of several pieces of information might arguably reduce attention to the main piece of information and lead to a weaker first stage. In any case, researchers should include measures for beliefs about other variables which could be shifted by the treatment in their survey in order to be able to detect cross-learning and to gauge its extent and implications. For instance, Link et al. (2023) provide information about future nominal interest rates and include measures of posterior inflation expectations to quantify updating of expectations about real interest rates.

6. *Dealing with Experimenter Demand Effects*

One concern with information provision experiments are demand effects (Zizzo 2010; de Quidt, Haushofer, and Roth 2018; Mummolo and Peterson 2019).¹³ While

recent empirical evidence suggests a limited quantitative importance of experimenter demand effects in online surveys in some domains (de Quidt, Haushofer, and Roth 2018; Mummolo and Peterson 2019), it is still possible that in other contexts treatment effects are confounded by experimenter demand effects as people in the different treatment arms may make differential inference about the experimenter's expectations.¹⁴ In this section, we outline best-practice recommendations to mitigate concerns about experimenter demand effects.

Obfuscated Follow-Ups.—Haaland and Roth (2020, 2023) propose the use of obfuscated follow-ups to mitigate concerns about experimenter demand effects. Obfuscated follow-up surveys are follow-up studies with the same respondents as in the initial experiment, which are presented as an independent study to participants. Since no treatment is administered in the follow-up study, differential experimenter demand between the treatment and control groups is unlikely to be a concern unless respondents nonetheless realize that the follow-up is connected to the main study. Haaland and Roth (2020, 2023) take several steps to hide the connection between their main study and their obfuscated follow-up study. First, they collaborated with a market research company where respondents regularly receive invitations to participate in surveys. The marketing company sent generic invitations that only reveal information about pay and expected completion time. Second, they employed two different consent forms for the two surveys. Third, to give the impression that the follow-up is an independent study, they

corrections from the perspectives of interviewer recruitment, survey design, and experiment setup.

¹⁴It is plausible that the willingness to please the experimenter could vary across different decision-making domains, which might increase the relevance of demand effects.

¹³In the case of surveys administered by enumerators, Kerwin and Reynoso (2021) show that reported beliefs are significantly related to interviewer knowledge and suggest

first ask respondents a series of questions about their demographics. Fourth, to further obfuscate the purpose of the follow-up, they pose questions about unrelated issues before asking any of the actual questions of interest. Following the approach proposed by Haaland and Roth (2020, 2023), Settele (2022) uses an obfuscated follow-up survey in the context of attitudes towards affirmative action.

Anonymity.—Anonymity has been argued to be a powerful tool against experimenter demand effects in experimental research (Hoffman et al. 1994). In the context of policy preference experiments, researchers have recently relied on the use of anonymous online petitions in order to mitigate concerns about experimenter demand effects (Grigorieff, Roth, and Ubfal 2020). A commonly used additional tool is “list methods,” which aims to veil the answers of individual respondents and is increasingly applied throughout the social sciences (Bursztyn, González, and Yanagizawa-Drott 2020; Chen and Yang 2019; Lergetporer, Piopiunik, and Simon 2021; Coffman, Coffman, and Ericson 2017).

Incentivized Outcomes.—Over the last few years, researchers have started using incentivized outcomes in the context of survey experiments. A commonly used approach is to elicit incentivized donations to political organizations that capture specific policy preferences (Grigorieff, Roth, and Ubfal 2020; Bursztyn, Egorov, and Fiorin 2020; Roth, Settele, and Wohlfart 2022a; Haaland and Roth 2023; Settele 2022). Presumably, demand effects should be lower in tasks in which real money is at stake.

Field Outcomes.—A small number of studies manage to link information provision with natural outcomes from the field, such as the take-up of job offers (Bursztyn, González,

and Yanagizawa-Drott 2020), the repayment of credit card debt (Bursztyn et al. 2019), welfare take-up (Finkelstein and Notowidigdo 2019), policy choices of politicians (Hjort et al. 2021), campaign donations (Perez-Truglia and Cruces 2017), voting behavior (Kendall, Nannicini, and Trebbi 2015; Cruz et al. 2018; Gerber et al. 2020), canvassing activity using an online application (Hager et al. 2020, 2021), home sales (Bottan and Perez-Truglia 2020), credit card spending (Galashin, Kanz, and Perez-Truglia 2020), or stock trading choices of retail investors (Laudenbach et al. 2021). The key advantage of these studies is that they provide unobtrusive behavioral outcome data from a natural setting. Experimenter demand effects are of no concern in many of these natural settings, as respondents are often not aware of the fact that they are part of an experiment. In general, given that decisions in the field involve much higher stakes than survey responses, it is unlikely that changes in these outcomes reflect demand effects.

Neutral Framing.—How should researchers frame the information treatments? One way to minimize the relevance of experimenter demand effects is to adopt a neutral framing of the experimental instructions. The neutral framing of instructions usually makes the purpose of the experiment less transparent and draws less attention of respondents to the expectations and wishes of the experimenter. For example, Bursztyn, Egorov, and Fiorin (2020) truthfully tell respondents that they will be assigned to decide whether to authorize a donation to either a pro-immigrant or an anti-immigrant organization. This reduces concerns that researchers are perceived as politically biased.

Obfuscated Information Treatments.—One way to mitigate experimenter demand effects is to obfuscate the information treatments. Specifically, researchers can try to

obfuscate the purpose of the study by providing respondents with additional pieces of information that are irrelevant, or by giving respondents tasks that give the impression that the purpose of the study is completely unrelated to the actual goal. One possibility is to give people an unrelated reason for why they receive the information of interest. For instance, researchers could tell respondents that they need to proofread or summarize pieces of information. For an example in the context of immigration attitudes, see Facchini, Margalit, and Nakata (2022). Furthermore, in experiments in which the researcher elicits incentivized prior beliefs, the purpose of the information treatment may be naturally concealed by framing the information treatment as feedback on whether the respondent's answer qualified for an extra payment.

Demand Treatments.—de Quidt, Haushofer and Roth (2018) propose the use of demand treatments in order to measure the sensitivity of behavior and self-reports with respect to explicit signals about the experimenter's expectations. For example, they tell respondents that they “expect that participants who are shown these instructions” will act in a particular way. The idea behind their approach is that one can use explicit signals of experimenters' wishes in order to bound the natural action. Roth and Wohlfart (2020) and Mummolo and Peterson (2019) apply demand treatments in the context of survey experiments on macroeconomic expectations and in political science, respectively, and confirm the finding that responsiveness to demand treatments is quite moderate.

Measuring Beliefs about the Study Purpose.—Many research studies in economics and psychology measure beliefs about the study purpose. Demand effects are less likely a concern in an experiment or survey if participants cannot identify the intent of the

study. Allcott and Taubinsky (2015) measure perceptions of study intent and show that there is strong dispersion in perceived intent within treatment groups, suggesting that it is unclear in which way demand effects might affect behavior. To test whether respondents across treatment arms hold different beliefs about the study purpose, Bursztyjn et al. (2023) use a machine-learning classifier to predict treatment status based on open-ended text responses about perceived study purpose.

Heterogeneity by Self-Monitoring Scale.—Allcott and Taubinsky (2015) argue that if demand effects are driving behavior in experiments, then they should be more pronounced for respondents who are more able to detect the intent of the study and are more willing to change their choices given the experimenter's intent. Allcott and Taubinsky (2015) employ the self-monitoring scale by Snyder (1974) and find no evidence that self-monitoring ability moderates the treatment effect.

Summary.—Overall, evidence suggests that demand effects may be of less limited quantitative importance in online experiments in some domains (de Quidt, Haushofer, and Roth 2018). However, the importance of demand effects could vary a lot across settings. We believe that they might be a concern particularly in sensitive domains in which participants care about pleasing the experimenter, while they are less of a concern in domains in which participants care about expressing their true preferences. Since it is ex ante unclear how relevant demand effects are, it is best practice to include some of the above outlined checks.

7. Samples

In this section, we first provide an overview of samples that are commonly used to

conduct information provision experiments, with a particular focus on the United States. We then then provide recommendations on how to measure attention in online surveys to ensure high-quality responses.

7.1 *Online Panels*

We now discuss the advantages and disadvantages of three different types of online samples that are commonly used for conducting information provision experiments: (i) probability-based samples, (ii) online panels representative in terms of observables, and (iii) online labor markets, such as Amazon Mechanical Turk.

Probability-Based Samples.—The most representative samples are probability-based panels. In a probability-based panel, the survey company recruits the sample by randomly selecting households from a representative sample frame. People cannot join the panel unless they have been randomly selected for participation. Random sampling from a representative sample gives these panels some desirable theoretical properties relating to unbiasedness and quantifiable margins of error. However, given that the nonresponse rate for probability-based panels is often quite high, there is still a strong element of self-selection into the panels.¹⁵ A clear advantage of probability-based samples is that they include more respondents who are typically underrepresented in non-probability-based samples, such as low-income and rural respondents as well as respondents from the non-Internet population. The disadvantages of probability-based samples are that they are typically much costlier than

convenience samples and they typically offer the least degree of flexibility in survey design and implementation.

In the United States, a widely used probability-based panel is AmeriSpeak by NORC at the University of Chicago. The panel uses NORC's National Frame, which is designed to provide at least 97 percent sample coverage of the US population. The NORC National Frame is used for several landmark studies in the United States, including the General Social Survey, which is one of the most frequently analyzed datasets in the social sciences. Other probability-based samples of the US population open to academic researchers include the RAND American Life Panel, the Understanding America Study at the University of Southern California, and the Ipsos KnowledgePanel (formerly administered by Growth from Knowledge).

Representative Online Panels.—Representative online panels are constructed to be representative of the general population in terms of observable characteristics, but do not use random sampling to recruit respondents. In a representative online panel, the survey company recruits respondents through, for instance, advertisements, and anyone who wants to join the panel is free to do so. The main advantage of these panels is that they are much more affordable than probability-based panels while retaining representativeness in terms of some important observable characteristics such as age, income, race, and gender. The main drawbacks of these panels are that the lack of random sampling makes it difficult to estimate the margin of error for the general population and that they do not include respondents from the non-Internet population. However, given that most probability-based panels have relatively high nonresponse rates, the differences in the extent of selection between probability-based samples and representative online panels might not be that large in practice.

¹⁵In the United States, a typical response rate for probability-based samples is between 5 and 15 percent; see <https://www.pewresearch.org/methods/u-s-survey-research/our-survey-methodology-in-detail/> (accessed April 9, 2021)

Three large providers that are widely used in the social sciences are Dynata (formerly Research Now and Survey Sampling International), Lucid, and YouGov. While some providers (such as YouGov) aim to match higher-dimensional cells of the population (such as age X gender), others (such as Lucid) approximate marginal distributions of basic demographics in the population. Furthermore, they allow for the use of obfuscated follow-up studies. The main disadvantage of these panels is that inferences may be less externally valid and there is a concern that respondents who self-select into online panels are very different from the broader population. However, using German data, Grewenig et al. (2018) show that the online and offline populations hardly differ in terms of survey responses in the context of political views and opinions, once one controls for the survey method and observable respondent characteristics.

Coppock and McClellan (2019) find that samples from Lucid score similarly to respondents in the American National Election Study (ANES) on the Big Five personality inventory, show similar levels of political knowledge, and recover framing effects similar to the ones observed in a probability-based sample (the General Social Survey). Haaland and Roth (2023) find similar experimental results using a sample from a representative online panel provider and a probability-based sample. Other comparable providers are Respondi, Prolific, and the Qualtrics panel.

Amazon Mechanical Turk.—The third type of available online sample is online labor markets, such as Amazon Mechanical Turk (MTurk), which are widely used in the social sciences and economics (Kuziemko et al. 2015). Coppock (2018) conducts 15 replication experiments and finds a very high degree of replicability of survey experiments in the field of political science with MTurk as compared to nationally representative

samples. Horton, Rand, and Zeckhauser (2011) replicate several well-known lab experiments using MTurk, concluding that online experiments on MTurk are just as valid as traditional physical lab experiments. However, recent studies suggest that data quality on MTurk has been declining over time, partly through the proliferation of bots (automated computer programs) and nonserious respondents, which threatens the data quality on the platform if sensible screening procedures are not implemented (Ahler, Roush, and Sood, 2021; Chmielewski and Kucker, 2020). To maximize data quality on MTurk, one should only allow workers that have completed a large number of previous tasks with a high completion rate. Furthermore, in the actual survey, one should include fraud detection tools such as a CAPTCHA to rule out bots at the beginning of the survey. While MTurk is less representative than most other survey platforms, the platform has some important advantages. First, data collection speed is typically very fast and it offers researchers maximum flexibility in terms of research design. Second, since users sign up for MTurk with their own credit cards, it is also possible to incentivize respondents with real money (respondents from more representative panel platforms are typically paid in panel currencies that can be converted into gift vouchers). Third, it is possible to conduct follow-up studies with low attrition rates (Grigorieff, Roth, and Ubfal, 2020).

7.2 Measuring Attention in Online Surveys

Screeners.—One concern in online surveys is that respondents are inattentive and speed through the surveys (Krosnick 1991). We recommend using multiple attention checks in online surveys. Recent research suggests that the inclusion of attention checks does not influence estimated treatment effects, but it allows researchers to study how measured attention affects behavior (Kane and Barabas 2019; Berinsky,

Margolis, and Sances 2014). One example of an attention screener is the following:

The next question is about the following problem. In questionnaires like ours, sometimes there are participants who do not carefully read the questions and just quickly click through the survey. This means that there are a lot of random answers which compromise the results of research studies. To show that you read our questions carefully, please enter turquoise as your answer to the next question. What is your favorite color?

There are at least two features of attention checks that we consider important: first, it is important that participants understand why researchers use these attention checks. This explanation can mitigate concerns about negative emotional reactions to the use of attention checks on the part of participants. Second, we think that attention checks should be simple to understand and should not be too cognitively demanding. Therefore, having an unambiguous and easy-to-understand question is important. For an excellent review on attention checks, see Berinsky, Margolis, and Sances (2014).

Open-Ended Questions.—Bots have been identified as a threat to online surveys. On top of standard bot protections, such as asking respondents to categorize distorted pictures that computers cannot easily recognize (CAPTCHAs), we recommend using at least two open-ended questions in the survey, for example, to inquire about feedback on the survey or to ask about the study purpose. These open-ended questions are a useful tool to assess data quality and to identify bots that may provide identical (and/or nonsensical) responses to different open-ended questions.

8. Typical Effect Sizes and Recommended Sample Sizes

In this section, we briefly discuss typical effect sizes from information provision experiments.

Learning Rates.—Information experiments usually measure belief updating using either qualitative or quantitative questions. In the context of quantitative beliefs, papers often include a calculation of learning rates. To calculate such learning rates, we require prior beliefs about the provided piece of information.¹⁶ Moreover, typically we observe both a treatment group, which receives information, and a control group, which does not receive any information. To quantify the extent to which the respondents update their beliefs toward the signal they receive during the information treatment, one can estimate the following specification:

$$\begin{aligned} \text{Updating}_i &= \beta_0 + \beta_1 \text{Treatment}_i \text{Perc.-gap}_i \\ &\quad + \beta_2 \text{Treatment}_i + \beta_3 \text{Perc.-gap}_i \\ &\quad + \varepsilon_i, \end{aligned}$$

where Updating_i is defined as the difference between the respondent's posterior and prior about the quantity of interest. Since priors about the quantity of interest should be balanced across treatment arms, one could alternatively directly use the posterior as a left-hand-side variable. The perception gap, Perc.-gap_i , is the difference between the true signal and the respondent's prior belief about the signal. The key coefficient of interest, β_1 , captures the extent of belief updating toward the provided signal among respondents in the treatment group, on top of any updating that also happens for respondents in the control group. β_2 captures the average treatment effect on respondents' beliefs to the extent it does not depend on individual priors. β_3 measures the extent to which changes in beliefs in the control group depend on the perception gap. It is essential to control for

¹⁶An exception are designs with an active control group, in which the average learning rate can be inferred from comparing the difference in posteriors between treatment groups with the difference in the provided signals.

Perc.-gap_i in a non-interacted form, as also respondents who were not provided with the information may change beliefs into the direction of the signal, for example because they have thought more carefully about the question once they are asked a second time or because they committed a typo the first time they stated their beliefs (Fuster et al. 2022).

Cavallo, Cruces, and Perez-Truglia (2017) and Cullen and Perez-Truglia (2022) show how the coefficient β_1 can be given a more structural interpretation. Specifically, under Bayesian updating with normally distributed priors and signals (where the moments of these distributions are independent of each other), and quadratic loss function, updating of beliefs will be linear in the perception gap. β_1 captures the weight respondents put on the signal, while putting a weight of $1 - \beta_1$ on their prior belief.

Table 1 gives an overview of estimated learning rates from a few select information experiments that provide quantitative information and calculate such learning rates. Many of the papers estimating quantitative learning rates focus on macroeconomic expectations. For instance, Armantier et al. (2016) find a learning rate of 0.39 for one-year inflation expectations in response to a professional forecast. Armona, Fuster, and Zafar (2019) estimate an instantaneous learning rate of 0.18 for house price growth in response to information about past house price growth. In a two-month follow-up, they estimate a learning rate of 0.13, indicating a high degree of persistence. Cavallo, Cruces, and Perez-Truglia (2016) estimate learning rates between 0.3 and 0.8 for inflation expectations in response to information about official inflation statistics or product price changes, which persist at about half of their initial values in a two-month follow-up. Roth and Wohlfart (2020) estimate a learning rate of 0.32 for recession expectations in response to a professional forecast. In a two-week follow-up, they document a learning

rate of 0.13, indicating a moderate degree of persistence. Taken together, these papers document that people persistently learn from the information provided, but that effects in most cases become weaker over time.

Effect Sizes on Beliefs versus Behavior.—Effect sizes on self-reported attitudes and behavioral measures are typically much smaller in magnitude than effect sizes on belief updating in response to information treatments. For instance, Alesina, Stantcheva, and Teso (2018) employ an information treatment to generate exogenous variation in perceptions of social mobility. While perceptions about the probability of remaining in the bottom quintile of the income distribution increase by 9.7 percentage points—thus making treated respondents substantially more pessimistic about the social mobility process—the authors find essentially no average impact on policy preferences. Similarly, an experiment by Kuziemko et al. (2015) provides respondents with accurate information about the income distribution. They find a large effect on beliefs about income inequality: treated respondents are 12 percentage points more likely to believe that income inequality has increased. By contrast, policy preferences are largely unaffected by the treatment. Haaland and Roth (2020) report results from an experiment where effect sizes on beliefs and preferences are quite similar in magnitude. Specifically, they provide respondents with research evidence showing no adverse labor market impacts of low-skilled immigration. Treated respondents become 17.1 percent of a standard deviation more optimistic about the labor market impacts of low-skilled immigrants and 14.1 percent of a standard deviation more in favor of low-skilled immigration.¹⁷

¹⁷In some cases information interventions not only fail to correct, but even increase misperceptions among

TABLE 1
ESTIMATED LEARNING RATES FROM QUANTITATIVE INFORMATION

| Authors (year), journal, exhibit | Context | Type of information | Immediate LR | Time lapse | Follow-up LR | Persistence |
|--|---|---|---|----------------------------------|---|---------------------------|
| Armantier et al. (2016), <i>REStat</i> , table 2 | Inflation expectations | Average professional forecast (SPF) | 0.39 | No follow-up | NA | NA |
| Armona, Fuster, and Zafar (2019), <i>REStat</i> , table 9 | House price expectations | 1-year and 5-year past house price growth | 0.18 (returns) 0 (5-year) | 2 months | 0.13 | 72% |
| Bleemer and Zafar (2018), <i>JPubEc</i> , table 7 | Perceived returns/cost college education own child | Population-level returns/cost | 0.18 (returns) 0.35 (cost) | 2 months | No learning rates for follow-up | NA |
| Bottan and Perez-Truglia (2022), <i>REStat</i> , figure 2 | Perceived city-level earnings rank and costs of living | Tailored based on potential own income and statistics from ACS or CPS | Earnings rank: 0.87 Cost of living: 0.88 | 5 weeks | Earnings rank: 0.63 Cost of living: 0.75 | 76% 85% |
| Cavallo, Cruces, and Perez-Truglia (2017), <i>AEJ: Macro</i> , table 1 | Inflation expectations | Statistics such as the current inflation rate or price changes of selected products | Statistics: 0.84/0.43 (US/Arg.) Prices: 0.69/0.46 (US/Arg.) | 2 months/4 months (US/Argentina) | Statistics: 0.36/NA (US/Arg.) Prices: 0.34/0.21 (US/Arg.) | 43% 49% 46% |
| Coibion, Gorodnichenko et al. (2021), <i>QJE</i> , table 3 | Firms' first and higher-order inflation expectations | Avg. first- and higher-order exp. of other firms | 1st-ord. exp.: 0.31/0.58 (others' 1st-/higher-ord. exp.) 0.38/0.54 (others' 1st-/higher-ord. exp.) | 3 months | 1st-ord. exp.: 0.39/0.92 (others' 1st-/higher-ord. exp.) 0.28/0.79 (others' 1st-/higher-ord. exp.) | 126% 93% 74% 94% |
| Coibion and Gorodnichenko (2015), <i>AER</i> , tables 6 and 7 | Firms' expectations about inflation, unemployment, and GDP growth | Infl.: Prof. forecast or central bank target; Unempl. and GDP: Past 12 months | Infl.: 0.66 (pooled) Unempl.: 0.35 GDP: 0.44 | 6 months | 0 | 0% |

(Continued)

TABLE 1
ESTIMATED LEARNING RATES FROM QUANTITATIVE INFORMATION (Continued)

| Authors (year), journal, exhibit | Context | Type of information | Immediate LR | Time lapse | Follow-up LR | Persistence |
|--|--|--|---|--------------|--------------------------------|-------------|
| Fuster et al. (2022), <i>REStat</i> , figures 3 and A.2 | House price expectations | Choice btw. expert forecast, past 1 year and past 10 year house price growth | 0.38 (based on preferred source of information) | 4 months | 0.17 | 45% |
| Roth, Settele, and Wohlfart (2022a), <i>Journal of Econometrics</i> , table A.18 | Beliefs about the debt-to-GDP ratio | Factual information | 0.62 | 4 weeks | 0.21 | 34% |
| Roth, Settele, and Wohlfart <i>REStat</i> , table 1 (2022b), <i>AEJL</i> , table 2 | Perceived unemployment risk next recession | Change in unempl. rate in own demographic group | 0.49 | No follow-up | NA | NA |
| Roth and Wohlfart (2020), <i>REStat</i> , table 1 | Recession expectations | Individual professional forecasts (SPF) | 0.32 | 2 weeks | 0.13 | 41% |
| Wiswall and Zafar (2014), <i>REStat</i> , table 4 | Own earnings prospects by college major | Population-level earnings by college major | 0.08 | 2 years | No learning rate for follow-up | NA |
| Wiswall and Zafar (2015), <i>Journal of Human Capital</i> , table 7 | Own earnings prospects by college major | Population-level earnings by college major | 0.34 | No follow-up | NA | NA |

Notes: This table provides an overview of estimated learning rates in published papers that study how individuals update their beliefs in response to the provision of quantitative information and that calculate learning rates. The table shows both the immediate learning rate measured in the main survey, and the medium-run learning rate as measured with posterior beliefs elicited in a follow-up survey. Journal abbreviations are defined as follows: *AEJ*: *Macro* is the *American Economic Journals*; *Macroeconomics*; the *AER* is the *American Economic Review*; *AER-I* is the *American Economic Review: Insights*; *PubEc* is the *Journal of Public Economics*; *QJE* is the *Quarterly Journal of Economics*; *REStat* is the *Review of Economics and Statistics*; and *REStud* is the *Review of Economic Studies*. For Coibion, Gorodnichenko et al. (2021), the learning rate is calculated as one minus the weight put on the prior, scaled by the weight put on the prior in the control group. For Coibion and Gorodnichenko (2015), the learning rate is calculated as one minus the weight put on the prior.

Instrumental Variable Estimation and Behavioral Elasticities.—One way to illustrate effect sizes is to estimate two-stage least squares specifications, where the endogenous belief of interest is instrumented with the randomized information provision. For example, Bottan and Perez-Truglia (2020) find that a 1 percentage point increase in home price expectations reduces the probability of selling within six months by 2.5 percentage points. Roth and Wohlfart (2020) find that a 10 percentage point increase in the perceived likelihood of a recession leads to a decrease in planned consumption growth by 13 percent of a standard deviation. “Behavioral elasticities” are a simple measure of the effect of beliefs on behavior that is comparable across settings. Such elasticities can be calculated by regressing the log of the outcome of interest on the log of posterior beliefs instrumented by the treatment assignment. For example, Cullen and Perez-Truglia (2022) find that increasing the perceived manager salary by 10 percent would increase the number of hours worked by 1.5 percent. The key advantage of these approaches is that they make it easier to compare results across settings. The key disadvantage is that the exclusion restriction needed for an IV estimation may not hold, as the information provided may change several beliefs simultaneously (see our discussion on cross-learning).

Sample Sizes.—While information provision experiments often produce relatively large effects on beliefs, effect sizes on stated preferences or behavioral outcomes are typically much lower. Indeed, it is not uncommon to observe null effects on the main outcomes

of interest despite a large and significant first stage on beliefs (e.g., Kuziemko et al. 2015; Alesina, Stantcheva, and Teso 2018; Haaland and Roth 2023). Furthermore, how elastic different outcomes are with respect to changes in beliefs naturally varies a lot across different settings, making it difficult to make a generic recommendation about optimal sample sizes. For instance, an information provision experiment studying actual voting turnout—a sticky outcome where it is unrealistic to expect large effects of an information treatment—requires a larger sample size than a similar information provision experiment studying self-reported voting intentions.

While the optimal sample size depends on the context, null findings are not uncommon in information provision experiments, and it is important to have sufficient statistical power to be able to measure a null finding relatively precisely. As a minimum, we think information provision experiments should have at least 80 percent power to detect a treatment effect of 15 percent of a standard deviation. This requires a sample size of at least 700 respondents per treatment arm of interest. For studies including a follow-up study, one should take into account the likely attrition between the main study and the follow-up and adjust the power calculation accordingly. For instance, if one expects a 30 percent attrition between the main study and a follow-up, one needs an initial sample size of 1,000 respondents per treatment arm to have 80 percent power to detect a treatment effect of 15 percent of a standard deviation in the follow-up.

9. Concluding Remarks

Information provision experiments are a powerful method to test economic theories and answer policy-relevant questions. As shown in figure 1, the use of information provision experiments has grown

the targeted ideological group (Nyhan and Reifler 2010). However, while the evidence on the effectiveness of correction of misperceptions in the political domain is mixed, such “backfiring” effects seem to be the exception (Nyhan 2020, Guess et al. 2020, Nyhan 2021).

considerably in economics over the last decade. Furthermore, as our survey of the literature illustrates, they have become popular in most subfields of economics. Given the importance of generating exogenous variation in beliefs to test many influential economic theories, and given the potential of information provision experiments to address questions of high policy relevance, we believe that such experiments will continue to grow in popularity. We hope that the methodological considerations and best-practice recommendations discussed in this review will contribute to this growth by lowering the barriers to conducting information provision experiments for researchers previously unfamiliar with the methodology.

Common applications of information provision experiments include studying belief formation and how exogenous changes in beliefs affect economic behavior. The literature has demonstrated that the elasticity of beliefs and preferences with respect to information varies a lot between different domains and settings. Going forward, it will be important to provide systematic evidence on what determines the effectiveness of information in changing beliefs. Similarly, it will be important to better understand why exogenous changes in beliefs lead to large changes in behavior in some domains but not in others.

For instance, factors that are likely to be important for belief updating include the strength of prior beliefs, the complexity of the information, and people's experience in processing information. There could also be a key role for attention and memory in shaping the associations that come to mind when being presented with information, which may affect learning from the information.¹⁸ The relative importance of these different

factors is currently not well understood, and they are likely of key importance for understanding differential effects of information across contexts. Furthermore, to systematically assess the relative importance of different beliefs in shaping economic behavior, we believe that information experiments that are designed to allow for a structural interpretation of estimated elasticities between beliefs and behaviors will be especially valuable going forward.

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