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ERRORS IN PROBABILISTIC REASONING AND JUDGMENT BIASES

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ABSTRACT

Errors in probabilistic reasoning have been the focus of much psychology research and are among the original topics of modern behavioral economics. This chapter reviews theory and evidence on this topic, with the goal of facilitating more systematic study of belief biases and their integration into economics. The chapter discusses biases in beliefs about random processes, biases in belief updating, the representativeness heuristic as a possible unifying theory, and interactions between biased belief updating and other features of the updating situation. Throughout, I aim to convey how much evidence there is for (and against) each putative bias, and I highlight when and how different biases may be related to each other. The chapter ends by drawing general lessons for when people update too much or too little, reflecting on modeling challenges, pointing to areas of economics to which the biases are relevant, and highlighting some possible directions for future work.

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Section 1. Introduction

Probabilistic beliefs are central to decision-making under risk. Therefore, systematic errors in probabilistic reasoning can matter for the many economic decisions that involve risk, including investing for retirement, purchasing insurance, starting a business, and searching for goods, jobs, or workers. This chapter reviews what psychologists and economists have learned about such systematic errors. At the cost of some precision, throughout this chapter I will use the term "belief biases" as shorthand for "errors in probabilistic reasoning." By "bias," in this chapter I will mean any deviation from correct reasoning about probabilities or Bayesian updating.[1]

This chapter's area of research—which is often called "judgment under uncertainty" or "heuristics and biases" in psychology—was introduced by the psychologist Ward Edwards and his students and colleagues in the 1960s (e.g., Phillips and Edwards, 1966). This topic was the starting point of the collaboration between Daniel Kahneman and Amos Tversky. Their seminal early papers (e.g., Tversky and Kahneman, 1971, 1974) jumpstarted an enormous literature in psychology and influenced thinking in many other disciplines, including economics.

Despite so much work by psychologists and despite being one of the original topics of modern behavioral economics, to date belief biases have received less attention from behavioral economists than time, risk, and social preferences. Belief biases have also made

[1] My use of the same term "bias" for all of these deviations is not meant to obscure the distinctions between them in terms of their psychological origins. For example, the gambler's fallacy (the belief that heads is likely to be followed by tails; Section 2.A) is a mistaken mental model of independent random processes, while Non-Belief in the Law of Large Numbers (the belief that the distribution of a sample mean is independent of sample size; Section 3.B) is a failure to understand or apply a deep statistical principle. These differences can matter, for example, for who makes the errors, under what circumstances, and the likelihood that interventions could reduce the bias.

few inroads in applied economic research, with the important exception of behavioral finance (see Chapters XXX (by Barberis) and XXX (by Malmendier) of this Handbook). I suspect that is because in many available datasets, beliefs have been unobserved. But today, datasets are becoming much more plentiful, and it is easier than ever to collect one's own data. Therefore in my view, the relative lack of attention paid to belief biases makes them an especially exciting area of research, rife with opportunities for innovative work. For some topics in this chapter, particularly beliefs about random sequences (Section 2) and prior-biased updating (Section 8), the body of evidence and theory is relatively mature. For these topics, the biases could be fairly straightforwardly incorporated into applied economic models or explored in new empirical settings. For other topics, such as many aspects of beliefs about sample distributions (Section 3) and features of biased inference (Section 5), there are basic questions about what the facts are and how to model them that remain poorly addressed. For those topics, careful experimental work and modeling could fundamentally reshape how these biases are understood.

This chapter has three specific goals. First, I have tried to organize the topics in a natural way for economists. For example, I review biased beliefs about random samples before discussing biased inferences because, according to the standard model in economics, beliefs about random samples are a building block for inference. I hope that this organization will facilitate more systematic study of the biases and integration into economics.

Second and relatedly, I have tried to highlight when and how different biases may be related to each other. For example, some of the biases about random samples may underlie some of the biases about inferences. Sometimes, belief biases are presented in a

way that makes them seem like an unmanageable laundry list of unrelated items. By emphasizing possible connections, I hope to point researchers in the direction of a smaller number of unifying principles. At the same time, I have tried to highlight when different biases may push in opposite directions or even jointly imply logically inconsistent beliefs, cases which raise interesting challenges for modeling and applications.

Third, I have tried to convey how much evidence there is for (and against) each putative bias. Often, papers focused on a particular bias review existing evidence somewhat selectively. While it is impossible to be comprehensive, and while I have surely missed papers inadvertently, for each topic I attempted to find as many papers as I could that provide relevant evidence from both economics and psychology. In some cases, I was surprised by what I learned. For example, as discussed in Section 4, the evidence overwhelmingly indicates that people tend to infer too little from signals rather than too much, even from small samples of signals. Another example is discussed in Section 9: while discussions of the literature often take for granted that people update their beliefs more in response to good news than bad news, and while the psychology research is nearly unanimously supportive, the evidence from experimental economics taken as a whole is actually rather muddy, and it leaves me puzzled as to whether and under what circumstances there is an asymmetry.

For each bias, in addition to discussing the most compelling evidence for and against it, which is usually from laboratory experiments, I also try to highlight the most persuasive field evidence and existing models of the bias. While I mention modeling challenges as they arise, I return in Section 10 to briefly discuss some of the challenges common to many of the belief biases.

Due to space constraints, I cannot cover all belief biases, or even most of them.[2] The biases I focus on all relate to beliefs about random samples and belief updating. I chose these topics because they are core issues for most applications of decision making under risk, they allow the chapter to tell a fairly coherent narrative, and some of them have not been well covered in other recent reviews. In addition, admittedly, this chapter is tilted toward topics I am more familiar with.

An especially major omission from this chapter is "overconfidence," which is probably the most widely studied belief distortion in economics to date and is discussed at some length in Chapters XXX (by Barberis) and XXX (by Malmendier) of this Handbook. The term "overconfidence" is unfortunately used to refer to several distinct biases—and for the sake of clarity, I advocate adopting terminology that distinguishes between distinct meanings. One meaning is overprecision, a bias toward beliefs that are too certain (for reviews, see Lichtenstein, Fischhoff, and Phillips, 1982, and Moore, Tenney, and Haran, 2015). Relatedly, the biased belief that one's own signal is more precise than others' signals has been argued to be important for understanding trading in financial markets (e.g., Daniel, Hirshleifer, and Subrahmanyam, 1998), as well as for social learning and voting; this bias is discussed in Chapter XXX (by Eyster) of this Handbook, which addresses biases in beliefs about other people.[3] Another meaning is overoptimism, a bias toward beliefs that

[2] Moreover, because this chapter is organized around specific biases, it omits discussion of related work that is less tightly connected to the psychological evidence. For example, Barberis, Shleifer, and Vishny (1998) is among the seminal papers that incorporated belief biases into an economic model. Yet it is only barely mentioned in this chapter because its core assumption—that stocks switch between a mean-reverting state and a positively autocorrelated state—does not fit neatly with the evidence on people's general beliefs about i.i.d. processes (described in Section 2).

[3] While the key feature of this bias is the *relative* precision of one's own versus others' signals, models of the bias typically assume that agents believe that their own signal is more precise than it is, and therefore agents overinfer from their own signal. Relevantly for such models, the evidence reviewed in Section 4 of this chapter indicates that people generally *underinfer* rather than overinfer (see also Section 10.A). Therefore, it would be more realistic to assume that agents underinfer from their own signal, even if they believe that others observe less precise signals (and thus infer even less than they do).

are too favorable to oneself (a classic early paper is Weinstein, 1980; for a review, see Windschitl and O'Rourke, 2015). Although I do not discuss overoptimism in this chapter, biases in belief updating, in particular those reviewed in Sections 5.A, 6, and 8, are relevant to how overoptimistic beliefs are maintained in the face of evidence. A closely related omission is motivated beliefs, an important class of biases related to having preferences over beliefs (a classic review is Kunda, 1990; for a recent review, see Bénabou and Tirole, 2016). While I do not discuss the broad literature on motivated beliefs, preference-biased updating (reviewed in Section 9) is considered to be one potential mechanism that helps people end up with the beliefs they want.

Other omissions from this chapter include: vividness bias, according to which hearing an experience described more vividly, or experiencing it oneself, may cause it to have a greater impact on one's beliefs (e.g., Nisbett and Ross, 1980; an early review is Taylor and Thompson, 1982, which concludes that the evidence is not strong; for a recent meta-analysis, see Blondé and Girandola, 2016); and hindsight bias, according to which, ex post, people overestimate how much they and others knew ex ante (Fischhoff, 1975; for a recent review, see Roese and Vohs, 2012, and for an economic model, see Madarász, 2012). I do not review the evidence on how people draw inferences from samples about population means, proportions, variances, and correlations (for reviews, see Peterson and Beach, 1967; Juslin, Winman, and Hansson, 2007).[4] I also do not cover the availability heuristic, according to which judgments about the likelihood of an event is influenced by

[4] Recent work in this vein has concluded that people tend to overlook selection biases and treat sample statistics as unbiased estimators of population statistics (Juslin, Winman, and Hansson, 2007). Much of the economics research on errors in strategic reasoning has focused on such failure to account for selection bias (see Chapter XXX (by Eyster) of this Handbook). In the experimental economics literature, Enke (2017) recently explored this error in a non-strategic setting.

how easily examples or instances come to mind (Tversky and Kahneman, 1974; for a review, see Schwarz and Vaughn, 2002), but Gennaioli and Shleifer's (2010) model of representativeness, discussed in Section 7.C of this chapter, is related to it.

Although some of the biases in this chapter might be understood as people not paying attention to relevant aspects of a judgment problem, I do not review the literature on inattention since that is the focus of Chapter XXX (by Gabaix) of this Handbook. I also do not at all address biases in probabilistic beliefs about other people or their behavior. Many of those biases are covered in Chapter XXX (by Eyster) of this Handbook. However, in Section 10 of this chapter, I briefly mention some of the modeling challenges that arise when applying the biases discussed here in environments with strategic interaction.

I will also not separately discuss the sprawling literature on "debiasing"—which refers to interventions designed to reduce biases—although some of this work will come up in the context of specific biases. Debiasing strategies come in three forms (Roy and Lerch, 1996): (i) modifying the presentation of a problem to elicit the appropriate mental procedure; (ii) training people to think correctly about a problem; and (iii) doing the calculations for people, so that they merely need to provide the inputs to the calculations. The classic review is Fischhoff (1982), and a more recent review is Ludolph and Schulz (2017). Some recent work has suggested that instructional games may be more effective than traditional training methods at persistent debiasing that generalizes across decision making contexts (Morewedge et al., 2015).

While I mention throughout the chapter when belief elicitation was incentivized, I do not discuss the literature on how to elicit beliefs in an incentive-compatible way. For a recent review, see Schotter and Trevino (2014).

There are a number of literature reviews that partially overlap the material covered in this chapter. Some of these are oriented around belief updating and are therefore similar to this chapter in terms of topics covered (Peterson and Beach, 1967; Edwards, 1968; DuCharme, 1969; Slovic and Lichtenstein, 1971; Grether, 1978; Fischhoff and Beyth-Marom, 1983). Others are reviews of the behavioral decision research literature more broadly that have substantial sections devoted to biases in probabilistic beliefs (Rapoport and Wallsten, 1972; Camerer, 1995; Rabin, 1998; DellaVigna, 2009). Relative to this chapter, Dhami (2017, Part VII, Chapter 1) is a textbook-style treatment that covers a much broader range of judgment biases but in less depth. This chapter builds on and updates these earlier reviews. For the biases it addresses, this chapter aims to broadly cover the available evidence from both psychology and economics with an eye toward formal modeling and incorporation into economic analyses.

The chapter has five parts and is organized as follows. The first part examines biased beliefs about random processes: Section 2 is about sequences (e.g., a sequence of coin flips), and Section 3 is about sampling distributions (e.g., the number of heads out of ten flips). An overarching theme is that, while some biases about sampling-distribution beliefs seem to result from biases in beliefs about sequences, there are additional biases that are specific to sampling-distribution beliefs. The second part of the chapter examines biases in belief updating. On the basis of a review and meta-analysis of the experimental evidence, Section 4 lays out a set of stylized facts. The central lesson is that people underweight *both* the information from signals *and* their priors—errors that I refer to as underinference and base-rate neglect, respectively. Section 5 discusses the three main theories of underinference, and Section 6 discusses base-rate neglect. The third part of the

chapter is Section 7, which focuses on the representativeness heuristic, generally considered to be a unifying theory for many of the biases discussed earlier in the chapter. I highlight that the representativeness heuristic has several distinct components and that efforts to formalize it have focused on one component at a time. At the end of the section, I reflect on the merits of modeling the representativeness heuristic as opposed to specific biases. The fourth part of the chapter examines interactions between biased updating and other features of the updating situation. Section 8 focuses on a type of confirmation bias I call "prior-biased updating," according to which people update less when the signal points toward the opposite hypothesis as their prior. Section 9 reviews the evidence on what I call "preference-biased updating," which posits that people update less when the signal favors their less-preferred hypothesis. The final part of the chapter is Section 10, which draws general lessons from the chapter as a whole, reflects on challenges in this area of research, advocates for connecting better to field evidence and other areas of economics, and highlights some possible directions for future work.

Section 2. Biased Beliefs About Random Sequences

2.A. The Gambler's Fallacy and the Law of Small Numbers

The gambler's fallacy (GF) refers to the mistaken belief that, in a sequence of signals known to be i.i.d., observing one signal reduces the likelihood of next observing that same signal. For example, people think that when a coin flip comes up heads, the next flip is more likely to come up tails.

The GF has long been observed among gamblers and is one of the oldest documented biases. Laplace (1814), who anticipated much of the literature on errors in probabilistic reasoning (Miller and Gelman, 2018), described people's belief that the fraction of boys and girls born each month must be roughly balanced, so that if more of one sex has been born, the other sex becomes more likely. The first systematic study of the GF was Alberoni (1962a,b), who reported many experiments showing that, with i.i.d. binomial signals, people think a streak of *a* signals is less likely than a sequence with a mix of *a* and *b* signals.[5]

Rabin (2002) and Oskarsson, Van Boven, McClelland, and Hastie (2009) provided reviews of the extensive literature documenting the GF in surveys and experiments. While most of this evidence comes from undergraduate samples, Dohmen, Falk, Huffman, Marklein, and Sunde (2009) surveyed a representative sample of the German population, asking about the probability of a head following the sequence TTTHTHHH. While 60% of

[5] Laplace (1814) and Alberoni (1962a,b) both provided explanations of the GF that anticipated Tversky and Kahneman's (1971) theory, the Law of Small Numbers, which is discussed below. Specifically, Laplace conjectured that the GF results from misapplying the logic of sampling without replacement, which is exactly the intuition captured by Rabin's (2002) model of the Law of Small Numbers, also discussed below. Alberoni's "Principle of the Best Sample" is essentially a restatement of Tversky and Kahneman's description of the Law of Small Numbers: "[People believe that the most likely] sample is that which, without presenting a cyclic structure, reflects the composition of the system of expectations in the whole and in each of its parts" (Alberoni, 1962a, p. 253).

the sample gave the correct answer of 50%, the GF was the dominant direction of bias, with 21% of the sample giving answers less than 50% and 9% of the sample giving answers greater than 50%.

Rabin (2002) pointed out ways in which some of the laboratory evidence is not fully compelling. For example, in experiments involving coin flips (or other 50-50 binomial signals) that ask participants to guess the next flip in a sequence, either guess has an equal chance of being correct. Moreover, many of the experiments are unincentivized. However, there have been experiments that address these concerns. For example, Benjamin, Moore, and Rabin (2018) conducted two incentivized experiments in which they elicited participants' beliefs about the probability of a head following streaks of heads of each possible length up to 9. Like Dohmen et al., they found that the majority of reported beliefs were the correct answer of 50%, but the incorrect answers predominantly exhibited the GF. On average, their participants (undergraduates and a convenience sample of adults) assessed a 44% to 50% chance that a first flip would be a head but only a 32% to 37% chance that a flip following 9 heads would be a head.[6]

Most field evidence of behavior consistent with the GF is from gambling settings, such as dog- and horse-race betting (Metzger, 1985; Terrell and Farmer, 1996), roulette playing in casinos (Croson and Sundali, 2005), and lottery-ticket purchasing (e.g.,

[6] Miller and Sanjurjo (2018) pointed out conditions under which GF-like beliefs are actually *correct* rather than being a bias. Specifically, fixing an i.i.d. sequence, say, a sequence of coin flips, and any streak length, they show that the (true) frequency of a head following a streak of heads *within that sequence* is less than 50%. Moreover, this frequency is decreasing in the streak length. Roughly speaking, the reason is that the expected frequency of heads in the entire sequence is 50%, so knowing that some of the flips are heads makes it more likely that the others are tails. Miller and Sanjurjo's result, however, is not relevant for much of the evidence on the GF. For example, Dohmen et al. and Benjamin, Moore, and Rabin asked about the probability of a head following a specific sequence of flips, questions for which the correct answer is always 50%. Miller and Sanjurjo's result *is* relevant for evidence of the hot-hand bias, however, as discussed in Section 2.B.

Clotfelter and Cook, 1993; Terrell, 1994). For example, using individual-level administrative data from the Danish national lottery, Suetens, Galbo-Jørgensen, and Tyran (2016) found that players placed roughly 2% fewer bets on numbers that won in the previous week.

Chen, Moskowitz, and Shue (2016) examined three other field settings: judges' decisions in refugee asylum court, reviews of loan applications, and umpires' calls on baseball pitches. In all three settings, they found that decision making is negatively autocorrelated, controlling for case quality. For example, even though the quality of referee asylum cases appears to be serially uncorrelated conditional on observables, Chen et al. estimated that a judge is up to 3.3% more likely to deny asylum in the current case if she approved it in the previous case. To explain their findings, Chen et al. theorized that judges think of underlying case quality as an i.i.d. process and thus, due to the GF, when the previous case was (say) positive, the decision maker's prior belief about underlying case quality is negative for the next case. This prior belief then influences the decision in the next case. While Chen et al. persuasively ruled out a number of alternative explanations, they acknowledged that they cannot rule out "sequential contrast effects" (e.g., Pepitone, and DiNubile, 1976; Simonsohn, 2006; Bhargava and Fisman, 2014), in which the decision maker's perception of (rather than belief about) case quality is influenced by the previous case.

A related literature in economics examines whether people randomize when playing a game that has a unique Nash equilibrium in mixed strategies. Equilibrium play requires that the sequence of actions be unpredictable and hence serially independent, but in laboratory games, experimental participants often alternate actions more often than they

should (for a review, see Rapoport and Budescu, 1997). In the largest field study to date, Gauriot, Page, and Wooders (2016) analyzed data on half a million serves made by professional tennis players and find that players switch their direction too often (see also Walker and Wooders, 2001; Hsu, Huang, and Tang, 2007). This excessive switching could reflect the mistaken GF intuition for what random sequences look like.

As an explanation of the GF, Tversky and Kahneman (1971) proposed that "people view a sample randomly drawn from a population as highly representative, that is, similar to the population in all essential characteristics" (p. 105). They called this mistaken intuition a belief in the "Law of Small Numbers" (LSN), a tongue-in-cheek name which conveys the idea that people believe that the Law of *Large* Numbers applies also to small samples.[7] Tversky and Kahneman highlighted two implications of the LSN. First, it generates the GF: after (say) a streak of heads, a tail is needed to ensure that the overall sequence reflects the unbiasedness of the coin. Second, belief in the LSN should cause people to infer too much from small samples.

There is very little evidence in support of the latter prediction. The evidence Tversky and Kahneman presented was from surveys of academic psychologists showing that they underestimate sampling variation and expect statistically significant results obtained in small samples to replicate at unrealistically high rates. For example, they described to their survey respondents an experiment with 15 participants that obtained a statistically significant result ($p < 0.05$) with $t = 2.46$. If a subsequent experiment with 15 more participants obtained a statistically insignificant result in the same direction with $t =$

[7] To help flesh out the LSN, Bar-Hillel (1982) directly asked experimental participants to judge the "representativeness" of different samples. She found that their judgments were influenced by a variety of factors. For example, a sample was judged to be more representative if its mean matched the population mean and if none of the sample observations were repeats.

1.70, most of Tversky and Kahneman's respondents said they would view that result as a "failure to replicate"—even though the second result is more plausibly viewed as supportive. However, as Oakes (1986) discussed, the interpretation of this evidence in terms of the LSN is confounded by other errors in understanding statistics, including a heuristic of treating results that cross the statistical significance threshold as much more likely to reflect "true" effects than they do. In additional surveys of academic psychologists, Oakes found that his respondents exhibited similar overinference from statistically significant results obtained in larger samples, indicating that the misinterpretations are not specific to small samples. Moreover, as discussed in Section 4 of this chapter, the experimental evidence on inference taken as a whole suggests that even in small samples, people generally *underinfer* rather than overinfer.

Rabin (2002) proposed a formal model of the LSN (see also Rapoport and Budescu, 1997, for a model of the belief that i.i.d. processes tend to alternate). Signals are known to be drawn i.i.d., with *a* signals having rate θ and *b* signals having rate 1-θ. Because the agent is a believer in the LSN, she forms beliefs as if the signals are drawn *without replacement* from an urn of finite size *M* containing θM *a* signals (where θM is assumed to be an integer). The model directly generates the GF: after (say) an *a* signal is drawn, there is one fewer *a* signal in the urn, so the probability that the next signal is *a* is $\frac{\theta M - 1}{M - 1}$, which is smaller than θ.

When the true rate is unknown and must be inferred by the agent, the model implies that the agent will err in the direction of overinference, ending up with a posterior belief that is too extreme. For example, suppose there are two states of the world: in state *A*, the rate of *a* signals is high (θ_A), whereas in state *B*, it is low ($\theta_B < \theta_A$). The agent thinks the

probability of *aa* is $\pi(aa \mid A) = \theta_A \cdot \left(\frac{\theta_A M - 1}{M - 1} \right)$ if the state is *A* and $\pi(aa \mid B) = \theta_B \cdot \left(\frac{\theta_B M - 1}{M - 1} \right)$ if the state is *B*. While the agent thinks a streak such as *aa* is less likely than it is regardless of the state, the agent thinks it is especially unlikely in state *B* since $\frac{\pi(aa \mid B)}{\pi(aa \mid A)} < \left(\frac{\theta_B}{\theta_A} \right)^2$. Consequently, the agent interprets *aa* as stronger evidence in favor of state *A* than it truly is. In Rabin's example, if the agent thinks an average fund manager has a 50% chance of success in each year, then he thinks a manager with two consecutive successful years is unusually good.[8]

This overinference in turn implies that, when the agent observes a small number of signals from many sources, she exaggerates the amount of variation in rates across sources. For example, suppose all fund managers are average, and the agent observes the last two years of performance for many managers. Because the agent underestimates how often average managers will have consecutive good or bad years, she will think the number of fund managers with such consecutive years is inconsistent with all managers being average and will instead conclude that there must be a mix of good and bad managers.

This model is useful for straightforwardly elucidating this and other basic implications of belief in the LSN. However, Rabin highlights that the model has artificial features that limit its suitability for many applications; for example, since the urn only contains *M* signals, the urn must be "renewed" at some point in order for the model to make predictions about sequences longer than length *M*. To address these limitations, Rabin and

[8] Although Rabin's (2002) model generates both the GF and overinference, given the lack of evidence for the latter, it is worth noting that overinference does not necessarily follow from the belief in the GF. The GF for *a* signals entails that $\pi(a \mid a, A) < \pi(a \mid A)$ and $\pi(a \mid a, B) < \pi(a \mid B)$. Overinference after two *a* signals entails that $\frac{\pi(a \mid a, A)}{\pi(a \mid a, B)} < \frac{\pi(a \mid A)}{\pi(a \mid B)}$, but this is not implied by the GF inequalities.

Vayanos (2010) introduced a more generally applicable model of belief in the LSN (see also Teguia, 2017, for a related model in a portfolio-choice setting).

While both the Rabin (2002) and Rabin and Vayanos (2010) models describe the GF, they do not fully capture the psychology of the LSN that *any* sample should be representative of the population. Benjamin, Moore, and Rabin (2018) illustrated this point in an experiment regarding beliefs about coin flips. They generated a million sequences of a million coin flips and had participants make incentivized guesses about how often different outcomes occurred. In some questions, they randomly chose a location in the sequence (e.g., the 239,672nd flip out of the 1 million) and asked participants to guess how often, when there had been a streak of 1, 2, or 5 consecutive heads at that location, the next flip was a head. Participants' mean probabilities were 44%, 41%, and 39%, consistent with the GF. In other questions, Benjamin, Moore, and Rabin randomly chose 1, 2, or 5 *non-consecutive* flip locations in the sequence at random and asked participants to guess how often, when all of these flips had been heads, another randomly chosen flip would be a head. Participants' mean probabilities—45%, 42%, and 41%—were nearly the same as those for consecutive flips. Since these flips are non-consecutive, the Rabin (2002) and Rabin and Vayanos (2010) models do not predict any GF. In fact, Benjamin et al. proved that whenever a sequence of flip *locations* is chosen i.i.d., the resulting sequence of flips must be i.i.d. regardless of whether the flips themselves are serially dependent. Therefore, *no* model of the LSN in which an agent's beliefs are internally consistent could explain why people expect negative autocorrelation in flips from random locations. Section 10.B of this chapter contains a brief general discussion of some of the conceptual and modeling challenges raised by belief biases that generate internally inconsistent beliefs.

2.B. The Hot-Hand Bias

The term "hot hand" comes from basketball. A basketball player is said to have a hot hand when she is temporarily better than usual at making her shots. The term has come to be used more generally to describe a random process in which outcomes sometimes enter a "hot" state and have temporarily higher probability than normal. Regardless of whether a process actually has a hot hand, the "hot-hand bias" is when people believe the process has more of a hot hand than it does. An agent with the bias will have an exaggerated expectation that a streak of an outcome will continue because a streak is indicative that the outcome is hot.

The cleanest evidence for hot-hand bias comes from settings where people believe in a hot hand even though the outcomes are known to be i.i.d. (a case sometimes called the "hot-hand fallacy"). For example, as pointed out originally by Laplace (1814), lottery players place more bets on numbers that have won repeatedly in the recent past, implying that they mistakenly believe in a hot hand (e.g., Suetens, Galbo-Jørgensen, and Tyran, 2016; see Croson and Sundali, 2005, for evidence from roulette, and Camerer, 1989, and Brown and Sauer, 1993, for evidence from sports betting markets). This bias appears prima facie to be the opposite of the GF because the GF says that numbers that won recently are believed to be *less* likely to win again. Empirically, Suetens, Galbo-Jørgensen, and Tyran (2016) found evidence for both: after a lottery number won once, players bet less on it, but when a streak of two or more wins occurred, players bet more the longer the streak. Theoretically, Gilovich, Vallone, and Tversky (1985) and others have argued not only that the two biases co-exist but that the hot-hand bias is a consequence of the GF: to someone

who suffers from the GF, an i.i.d. process looks like it has too many streaks, so a belief in the hot hand arises to explain the apparent excess of streaks.

Rabin and Vayanos (2010) formally developed this argument that hand-hand bias can arise from belief in the GF. Rabin and Vayanos assumed that an agent dogmatically believes that one component of the process is negatively correlated, as per the GF, but puts positive probability (even if very small) on the possibility that the process has a hot hand. After observing an i.i.d. process for a sufficiently long time and updating Bayesianly about the probability of a hot state, the agent will come to believe with certainty that there is a hot state. With the resulting combined GF/hot-hand beliefs, the agent will expect high-frequency negative autocorrelation, but will expect positive autocorrelation once a long enough streak has occurred. Applying their model to investors' beliefs about i.i.d. stock returns, Rabin and Vayanos argued that it explains several puzzles in finance, such as why investors believe that stock returns are partially predictable and hence active mutual fund managers can outperform the stock market.

This theory of hot-hand bias coexisting with and arising from the GF is consistent with several observations. First, Suetens, Galbo-Jørgensen, and Tyran's (2016) evidence mentioned above—that lottery players bet less on a number after it comes up once but more after a streak—fits the theory nicely. Moreover, Suetens, Galbo-Jørgensen, and Tyran (2016) found that the lottery players exhibiting the hot-hand bias also tend to be those exhibiting the GF. Second, Asparouhova, Hertzel, and Lemmon (2009) found that when experimental participants are asked to predict the next outcome of a process and are not informed that the process is i.i.d., they predict reversals of single outcomes and continuation of streaks, again the pattern implied by the theory. Finally, for random

processes whose i.i.d. nature is arguably well understood by people (such as coin flips and roulette spins), the GF is by far the dominant belief. For example, as mentioned in Section 2.A, Benjamin, Moore, and Rabin (2018) asked participants the probability of a head following streaks of different lengths up to 9 heads and found that the perceived likelihood of a head is declining monotonically in the length of the streak. The theory of hot-hand bias arising from the GF implies that for a random process where people put near-zero prior probability on the existence of the hot hand, the hot-hand bias should *not* arise—unless people observe the process for a very long time. Consistent with this, over 1000 draws of binary i.i.d. processes, Edwards (1961a) found that experimental participants predicted reversals of streaks for the first 200 draws (see also Lindman and Edwards, 1961) but continuation of streaks for the last 600 draws.

On the other hand, Guryan and Kearney's (2008) finding of a "lucky store effect" may be a challenging observation for the theory. In data on weekly lottery drawings from Texas, they found that stores that sold a winning ticket sold substantially more tickets in subsequent weeks, with the effect persisting for up to 40 weeks. This seems to be a case of hot-hand bias without the GF. As a possible reconciliation with the theory, Guryan and Kearney speculated that in this context, lottery players might have a strong prior on a hot hand, for example, because of a belief in the store clerk's karma.

In the psychology literature, a variety of factors have been proposed to explain when the GF versus hot-hand bias occurs (Oskarsson, Van Boven, McClelland, and Hastie, 2009). For example, Ayton and Fischer (2004) found that experimental participants anticipated negative autocorrelation in roulette spins but positive autocorrelation for successes in human prediction of the outcomes of roulette spins. They proposed that the

GF dominates for natural processes, whereas the hot-hand bias dominates when human performance is involved (see also Caruso, Waytz, and Epley, 2010). While this theory cannot explain evidence of the GF after a single outcome and the hot-hand bias after a streak as in Suetens, Galbo-Jørgensen, and Tyran (2016), it is complementary with Rabin and Vayanos's model insofar as it provides a theory to explain people's prior probability of a hot hand, which is taken as exogenous in Rabin and Vayanos's model.

Much of the field evidence on the hot hand comes from professional sports. Identifying a hot-hand bias in such settings is tricky because sports performance is typically *not* i.i.d. Since confidence, anxiety, focus, and fatigue vary over time, a true hot hand is plausible, as is its opposite, a cold hand. Yet accurately estimating the magnitude of a true hot hand in performance is itself challenging for several reasons, including that performance affects outcomes only probabilistically (Stone, 2012) and that endogenous responses by the other team may counteract positive autocorrelation in a player's performance (e.g., Rao, 2009). Bar-Eli, Avugos, and Raab (2006) reviewed the sizeable literature testing for a true hot hand in a variety of sports.

Gilovich, Vallone, and Tversky's (1985) seminal paper introducing the hot-hand bias focused on the context of basketball. The paper attracted a lot of attention because it made a surprising empirical claim: contrary to strongly held beliefs of fans, players, and coaches, there is *not* a hot hand in basketball. Gilovich et al. made this claim on the basis of evidence from three studies. First, they analyzed the shot records of 9 players from a National Basketball Association (NBA) team over a season and found no evidence of positive autocorrelation for any of the players. Second, they analyzed the free-throw records of 9 players from another NBA team and, again, found no evidence of

autocorrelation. Finally, they ran a shooting experiment with 26 collegiate basketball players and found evidence of positive autocorrelation for only one player. They also found, in incentivized bets, that both shooters and observers expected positive autocorrelation, but in fact neither shooters nor observers could predict the shooters' performance better than chance. From the contrast between the widespread belief in the hot hand and the absence of it in the data, Gilovich et al. inferred that beliefs are biased. Subsequent work replicated and extended Gilovich et al.'s findings (e.g., Koehler and Conley, 2003; Avugos, Bar-Eli, Ritov, and Sher, 2013).

Miller and Sanjurjo (2014, 2017) recently identified a subtle statistical bias in earlier analyses that overturns the conclusion of no hot hand in basketball. Put simply, Gilovich et al. and others had inferred that there is no true hot hand because the empirical frequency of making a second shot in a row, $\hat{p}(\text{hit}|\text{hit})$, is roughly equal to the unconditional frequency of making a shot, $\hat{p}(\text{hit})$. While the details vary with the statistical method, roughly speaking, $\hat{p}(\text{hit}|\text{hit})$ is estimated as the ratio of two empirical frequencies: $\hat{p}(\text{hit then hit}) / \hat{p}(\text{hit})$. But when making shots is i.i.d., $\hat{p}(\text{hit then hit})$ and $\hat{p}(\text{hit})$ are positively correlated in a finite sample. Consequently, $\hat{p}(\text{hit}|\text{hit})$ is biased downward relative to the true conditional probability, $p(\text{hit}|\text{hit})$ (Rinott and Bar-Hillel, 2015). Thus, the evidence that $\hat{p}(\text{hit}|\text{hit})$ is roughly equal to $\hat{p}(\text{hit})$ implies that the *true* probability $p(\text{hit}|\text{hit})$ is actually greater than $p(\text{hit})$. In re-analyses of earlier data, Miller and Sanjurjo (2014, 2017) found that this bias is substantial. Correcting for the bias, they concluded that there is evidence for a hot hand in basketball. In a new shooting experiment with many more shots per participant, Miller and Sanjurjo (2014) again concluded that many players have a hot hand. Miller and Sanjurjo (2017) re-analyzed Gilovich et al.'s betting data,

pooling across bettors to increase power, and concluded that overall, the bettors *did* predict shooters' performance better than chance. By showing that there is a hot hand, these new analyses and evidence re-opens—but does not answer—the key question of whether there is a hot-hand *bias* in basketball, i.e., a belief in a stronger hot hand than there really is.

In two other sports, recent papers found both a true hot hand and evidence for a bias. Among Major League Baseball players, Green and Zwiebel (2017) found that recent performance predicts subsequent performance for both batters and pitchers, and the magnitudes are substantial (although the analysis did not control for player-ballpark interaction effects, which can be important in baseball). However, pitchers overreact to recent good performance by batters, indicating that they believe that the hot hand is stronger than it is. For example, they walk batters who have recently been hitting home runs more than can be justified based on the batters' hot hand. Among players in the World Darts Championship, Jin (2018) found a substantial hot hand but also found that players' willingness to take a high-risk/high-reward shot increases by more than it should in light of their hot hand.

2.C. Additional Biases in Beliefs About Random Sequences

Almost all research on beliefs about random sequences have focused on the LSN and the hot-hand bias, and as discussed in Section 2.B above, for purely mechanical random processes such as coin flips, the LSN is the relevant bias. Kleinberg, Liang, and Mullainathan (2017) have found, however, that (current models of) the LSN provides far from a complete theory of people's perceptions about random sequences. Kleinberg et al. asked 471 online experimental participants to generate 25 random sequences of 8 coin flips.

Using the empirical frequencies calculated from this large number ($471 \times 25 = 11{,}775$) of 8-flip sequences, Kleinberg et al. generated the (approximately) optimal prediction of the probability that participants will generate a head on the next flip after any given sequence of fewer than 8 flips. They also used the experimental data to estimate the parameters of the Rabin (2002) and Rabin and Vayanos (2010) models of the LSN, and then they generated predictions from the estimated models. In an independent validation sample, they compared the predictive success of the models with that of the optimal prediction. They found that the models achieved no more than 15% of the reduction in mean squared error (relative to random guessing) attained by the optimal prediction. This finding implies that there are additional systematic biases in people's beliefs about coin flips beyond what is captured in current models of the LSN.[9]

This intriguing result raises two further questions that remain largely unresolved. First, is the remainder of the potentially attainable predictive power (the other 85%) comprised of biases that are as predictive or more predictive of people's beliefs as the LSN, or is it comprised of many "minor" biases, each of which individually has very little predictive power? If the latter, then the benefit from identifying and modeling any given additional bias may not be worth the opportunity cost of investing research resources elsewhere.

[9] Is 15% of the way toward the optimal prediction large or small? The performance of other economic models provide a natural benchmark. While Kleinberg et al.'s analysis has not yet been carried out for other models, related exercises have been conducted. Using laboratory data on choices under risk and ambiguity, Peysakhovich and Naecker (2017) compared the mean squared error of predictions made by existing economic models with that of predictions made by machine learning algorithms (trained on the same laboratory data used to estimate the models). They found that the probability-weighting model achieved *all* of the predictive gains of the machine learning algorithms, whereas models of ambiguity aversion fell far short of the predictive power of the algorithms. Fudenberg and Liang (2018) used a related approach to study initial play in strategic-form games and found that models of level-*k* thinking (see Chapter XXX (by Eyster) of this Handbook) achieved ~50-80% of the attainable predictive power, depending on specification.

Second, are these other biases generalizable across domains—as the LSN is—or are they specific to this setting (e.g., to coin flips)? If the latter, then again, the benefit from identifying the biases may be small. Kleinberg et al. provide some evidence on the generalizability question, showing that the optimal predictions from the 8-flip data continue to perform well when applied to 7-flip data and to i.i.d. sequences using a different alphabet than H and T.

Despite the open questions, Kleinberg et al.'s results nonetheless should make us humble about our current state of knowledge and raise the possibility that the payoffs to discovering the nature of the additional biases could be substantial.

Section 3. Biased Beliefs About Sampling Distributions

Throughout this chapter, I will use the term "sampling distribution" to refer the distribution of the *number* of *a* and *b* signals. For example, for a sample of size 2, the sampling distribution specifies the probabilities of three events: 0 *a*'s and 2 *b*'s, 1 *a* and 1 *b*, and 2 *a*'s and 0 *b*'s.

Whereas the previous section reviewed research on people's beliefs about the likelihood of particular random sequences, this section focuses on people's sampling-distribution beliefs. At the end of the section, I discuss the extent to which people's beliefs about sampling distributions may or may not be consistent with their beliefs about the sequences that must logically underlie the distributions.

3.A. Partition Dependence

Bayesian beliefs satisfy a normative principle called extensionality: if two events correspond to the same set of states, then the probabilities of the two events must be equal. In this section, I discuss a bias in which people's beliefs violate this principle: people assign greater total probability to an event when it is described as the union of subevents rather than as a single event. Following Fox and Rottenstreich (2003), I refer to this bias as "partition dependence" because beliefs depend on how the state space is partitioned into events. Partition dependence is not only an important bias in itself, but it is also a potential confound for evidence on other belief biases, and for that reason, it comes up throughout this section and later in this chapter.

Partition dependence was first systematically studied by Tversky and Koehler (1994). Drawing on extensive existing evidence (e.g., Teigen, 1974a; Olson, 1976;

Fischhoff, Slovic, and Lichtenstein, 1978) and new experiments, Tversky and Koehler found that people assign greater total probability to an event when it is "unpacked" into subevents. For example, when Tversky and Koehler asked undergraduates to estimate the frequency of death by natural causes, the mean estimate was 56%. When they instead asked about three mutually exclusive subcategories—heart disease, cancer, and other natural causes—the mean estimates were 18%, 20%, and 29%, which add up to 67%. Even for decision-theory experts, unpacking an event has been found to increase the probability assigned to it, although typically less dramatically than for non-experts (e.g., Fox and Clemen, 2005). Similarly for subject-matter experts; for example, in several surveys of physicians, Redelmeier, Koehler, Liberman, and Tversky (1995) described a patient exam and asked the physicians to assign probabilities to various possible diagnoses or prognoses. As in the results with other samples, unpacked events were assigned higher total probabilities.

Sonnemann, Camerer, Fox, and Langer (2013) found evidence that partition dependence is reflected in behavior in a range of experimental markets and naturally occurring betting markets. For example, in an experimental market, students traded contingent claims on professional basketball and soccer outcomes. For some participants, an interval of outcomes comprised a single contingent claim (e.g., an NBA team will win from 4 to 11 games during the playoffs), while for other participants, that same interval was unpacked into two contingent claims (e.g., 4-7 and 8-11). To combat the worry that participants might infer that the market designer chose the intervals to be equally probable, each group of participants was informed about the contingent claims that other groups traded. Sonnemann et al. found higher sum-total prices for unpacked contingent claims

than for their corresponding packed contingent claims, and the differences persisted over the 8 weeks of the experiment.

Tversky and Koehler (1994) proposed a formal model of partition dependence called "support theory" (see also Rottenstreich and Tversky, 1997). To establish notation, Ω is the set of all possible states of the world. A subset of Ω is called an *event* and is denoted $E \subseteq \Omega$. A *partition* of Ω is a set of mutually exclusive events that jointly cover the state space Ω. In the above example from Tversky and Koehler, heart disease, cancer, and other natural causes are three events. In support theory, there exists a function $s(\cdot)$, defined independent of the partition, that maps any event into a strictly positive number. The function $s(\cdot)$, which is called the *support function*, captures the strength of belief in each possible event. In particular, if the agent's beliefs are elicited using partition ε, then the agent's belief about any event $E \subseteq \Omega$ is:

$$\pi(E \mid \varepsilon) = \frac{s(E)}{\Sigma_{F \in \varepsilon} s(F)}. \tag{3.1}$$

The key property of the support function is: For any mutually exclusive events E' and E'',

$$s(E') + s(E'') \geq s(E' \bigcup E''). \tag{3.2}$$

If equation (3.2) always holds with equality, then $s(\cdot)$ represents a standard subjective probability (and equals a subjective probability if rescaled so that $\Sigma_{F \in \varepsilon} s(F) = 1$). Whenever

equation (3.2) holds with strict inequality, the support function is said to be *subadditive*. Subadditivity is the central feature of support theory because it captures the evidence that unpacking an event generates a higher total probability than asking about it as a single event. Tversky and Koehler provided properties on the observed subjective probabilities that imply equations (3.1)-(3.2), and Ahn and Ergin (2010) provided a decision-theoretic axiomatization.

The vast majority of evidence on partition dependence is consistent with subadditivity, and the few studies that found the opposite identified mechanisms generating those results that may not be relevant more generally (Macchi, Osherson, and Krantz, 1999; Sloman, Rottenstreich, Wisniewski, Hadjichristidis, and Fox, 2004). For example, Sloman et al. (2004) argued that when an event is unpacked into subevents that are atypical, attention is directed away from the typical members, which may reduce the event's perceived likelihood. For instance, they found that death by "pneumonia, diabetes, cirrhosis, or any other disease" was judged as less likely than death by "any disease" (40% versus 55%).

As Tversky and Koehler and others pointed out, depending on the setting, subadditivity could result from a variety of psychological mechanisms, including imperfect memory for unmentioned events, salience of mentioned events, ambiguity in the way packed events are described, and an implicit suggestion that mentioned events are more likely than unmentioned ones. Fox and Rottenstreich (2003) provided evidence that subadditivity can also result from a bias toward assigning equal probability to each category, i.e., the reported probabilities are compressed toward a uniform distribution

("ignorance prior") across categories.[10] In a series of studies, Fox and Clemen (2005) found that subadditivity persists in settings where other mechanisms are unlikely to be at play. For example, in one study, MBA students were asked to rate the probabilities that particular business schools would be ranked #1 in the next *Business Week* rankings. Some participants assigned probabilities to six categories: (i) Chicago, (ii) Harvard, (iii) Kellogg, (iv) Stanford, (v) Wharton, and (vi) None of the above. Other participants assigned probabilities to two categories: (i) Chicago, Harvard, Kellogg, Stanford, or another school other than Wharton, and (ii) Wharton. This design rules out many possible mechanisms for subadditivity because the same set of schools was mentioned to both groups of participants, and yet subadditivity was observed: the median probability assigned to Wharton was 30% in the first group but 60% in the second group. Fox and Clemen concluded that compression accounts for the robust evidence of subadditivity across settings.

Of particular relevance for discussion later in this section, Teigen (1974a), Olson (1976), and Benjamin, Moore, and Rabin (2018) reported evidence of partition dependence in sampling-distribution beliefs for binomial signals that is consistent with Fox and Clemen's compression mechanism. For example, Benjamin, Moore, and Rabin elicited from each participant the probability distribution of outcomes of ten flips of a fair coin. This distribution was elicited with four different ways of partitioning the outcomes:

(A) 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10 heads (11-bin partition)

[10] Fox and Rottenstreich suggested that this psychological mechanism may also underlie the "1/*n* heuristic" (Benartzi and Thaler, 2001), in which people allocate their money equally across the investment options offered to them. The same mechanisms that generate subadditivity in probability judgments might also underlie what has been called the "part-whole bias" in the contingent valuation literature (e.g., Bateman et al., 1997), in which the sum of people's valuations of the components of a good add up to more than people's valuation of the whole.

(B) 0-3, 4, 5, 6, 7-10 heads (5-bin partition)

(C) 0-4, 5, 6-10 heads (3-bin partition)

(D) Each possible number of heads (0-10) elicited separately (eleven 2-bin partitions)

In partitions A-C, the outcome categories were presented together on the same screen, and participants' probabilities were restricted to sum to 100%. For D, each possible number of heads was asked about on a separate screen, and there was no requirement that the total sum to 100%. Questions in D, such as "What percentage of ten-flip sets include exactly 4 HEADS and 6 TAILS?", are believed to induce 2-bin partitions because they effectively ask about the probability of a given outcome as opposed to any other outcome (e.g., Fox and Rottenstreich, 2003). Each participant provided sampling-distribution beliefs in response to each of A-D, which were presented in a random order and interspersed with other questions.

Table 1 shows participants' mean beliefs for each of these partitions, in each of two experiments. Two patterns are clear. First, there is subadditivity. For example, across partitions A-C, the total probability assigned to 0-4 heads is smallest when it is described as a single event, higher when unpacked to the two events 0-3 heads and 4 heads, and highest when further unpacked to five events: 0, 1, 2, 3, and 4 heads. Second, relative to the correct probability distribution, participants' mean beliefs are compressed toward a uniform distribution in all partitions. One consequence is that the probabilities sum to more than 100% in D (where they were not constrained to sum to 100%), consistent with similar evidence from previous work (e.g., Teigen, 1974a, 1974b; Redelmeier et al., 1995).

Partition dependence raises fundamental issues about interpreting and measuring beliefs. For example, if reported beliefs depend on the partition, then does it make sense to talk about a person's "true" beliefs? Within the subjective expected utility tradition, a natural approach would be to define a person's true beliefs as those implied by the person's behavior, but the evidence from Sonnemann, Camerer, Fox, and Langer (2013) mentioned above indicates that doing so would not uniquely pin down beliefs because behavior is also partition dependent. Indeed, in Ahn and Ergin's (2010) decision-theoretic framework, the beliefs implied by behavior depend on the partition relevant to the decision problem. A related question is whether there are better and worse partitions to use when eliciting beliefs, when the purpose is to aid someone in decision making. The answer to this question presumably depends on the psychological mechanism that generates partition dependence. For example, if a particular description of events causes people to forget about some of the states of the world, then that description is suspect. On the other hand, if subadditivity is due to people compressing beliefs toward a uniform distribution, then beliefs are biased regardless of which partition is used to elicit them. These normative issues have been largely unaddressed in the context of belief elicitation, but they are analogous to issues that have been raised for framing effects in general; for discussion, see Chapter XXX (by Bernheim and Taubinsky) of this Handbook.

Related to the issue of "true" beliefs, partition dependence raises a thorny conceptual problem that needs to be addressed before proceeding with the rest of this section: since reported beliefs depend on the partition, which partition should be used for the purpose of defining other sampling-distribution biases? For example, when a coin is

flipped 10 times, do people overestimate the probability of 4 heads as in partition D of Table 1, or underestimate it as in partition A?

One way to define and study other belief biases separately from partition dependence is to write down a model of how beliefs are affected by partition dependence, use the model to undo its effects, and then examine the resulting beliefs. Such an approach posits the existence of latent "root beliefs," which are what the beliefs would be if they were purged of partition dependence. The root beliefs are never directly observed but may be inferred using the model, and then other belief biases can be defined in terms of how the root beliefs deviate from the correct probabilities. This approach has been taken by Clemen and Ulu (2008) and Prava, Clemen, Hobbs, and Kenney (2016). For example, Clemen and Ulu proposed a model that extends support theory by assuming that observed beliefs are a mixture of the root beliefs with a uniform distribution over the events in a partition. Using their model, Clemen and Ulu proposed a method of inferring root beliefs from observed beliefs, demonstrated their method in an experiment, and found that the inferred root beliefs exhibited little or no partition dependence.

In later parts of this section, when attempting to disentangle other biases in sampling-distribution beliefs from partition dependence, I will refer back to a similar approach taken by Benjamin, Moore, and Rabin (2018). Benjamin, Moore, and Rabin proposed a quite general framework that does not make functional form assumptions, and they proved some results regarding inferences that can be drawn about the root beliefs in this framework. Specifically, denoting the root belief about event E as $r(E)$, they assumed that the support of an event is a continuous, positive-valued function of the agent's root belief:

$$s(E) = g(r(E)) \tag{3.3}$$

for all $E \subseteq \Omega$. The function g has two key properties. First, it is strictly increasing. This assumption means that one event has greater support than another if and only if the root beliefs assign it greater probability. The assumption implies that there is a special situation in which root beliefs can be inferred: when the reported beliefs are equal to each other. That is, if there is some partition in which the agent reports that each event has equal probability, then the agent's root beliefs also assign equal probability to each event.

Second, g is weakly concave. Given the other assumptions, this assumption is essentially equivalent to inequality (3.2). It ensures that the reported beliefs are a compressed version of the root beliefs. It implies that there is another special situation in which inferences can be drawn about the root beliefs: when the *correct* probabilities of each event in a partition are equal to each other. In that case, we know that, relative to the root beliefs, the reported beliefs are biased toward the correct probabilities. Therefore, in whatever direction the reported beliefs are biased relative to the correct probabilities, the root beliefs are biased in the same direction (and are even further away from correct). Partition dependence is problematic for the growing literatures in many areas of economics that rely on survey elicitations of people's beliefs (for a review, see Manski, 2018). An early example is Viscusi (1990), who asked a representative sample "Among 100 cigarette smokers, how many of them do you think will get lung cancer because they smoke?" The mean response was 42.6—surely a dramatic overestimate of the true probability. This finding is often interpreted as suggesting that, if people were better informed about the

health risks of smoking, they would smoke *more*. However, the partition of the state space of the consequences of smoking as {get lung cancer, not get lung cancer} would be expected, per compression, to lead people to assign an especially high probability to the event of getting lung cancer. Thus, unless the state space is partitioned this way when people are deciding whether to smoke, it is not clear how to relate the reported belief to the prevalence of smoking behavior.

More generally, partition dependence implies that in order to elicit the beliefs that are relevant for decision making, the beliefs must be elicited using the same partition that people use when making the decision. This in turn means that economists will need to study what partitions people use. This is an important direction for research that, as far as I am aware, has not been explored.

3.B. Sample-Size Neglect and Non-Belief in the Law of Large Numbers

A striking regularity regarding sampling-distribution beliefs is *sample-size neglect*. It was first documented by Kahneman and Tversky (1972a). In an initial demonstration, they told one group of participants that 1000 babies are born a day in a certain region, and they asked,

> On what percentage of days will the number of boys among 1000 babies be as follows:
>
> Up to 50 boys
>
> 50 to 150 boys
>
> 150 to 250 boys

…

850 to 950 boys

More than 950 boys

Note that the categories include all possibilities, so your answers should add up to about 100%.

They asked another group of participants the analogous question about 100 babies, and they asked a third group about 10 babies (with the outcomes 0, 1, 2, …, 9, and 10 boys). As per the Law of Large Numbers, the correct sampling distribution puts more mass on the mean as the sample size gets larger. However, as shown in Figure 1a, all three groups reported the same distribution over sample proportions. Kahneman and Tversky called this distribution the "universal distribution" for a binomial with rate 50%. With the same three sample sizes, Kahneman and Tversky similarly elicited beliefs about two other distributions: a binomial with rate 80% (Figure 1b) and a normal distribution (not shown). For both, participants' subjective sampling distributions for the sample mean were again invariant to sample size. Kahneman and Tversky did not investigate sample sizes smaller than 10 but noted that they did not expect sample-size neglect to hold "…when the sample is small enough to permit enumeration of possibilities" (p. 441); as mentioned in Section 3.E below, it seems likely that people hold correct beliefs about sample sizes of 1 (although I am not aware of any evidence).[11]

[11] The idea that people may find it easier to reason correctly about small samples than large samples may be consistent with research in numerical cognition, which has found that people (as well as infants and non-human animals) have different cognitive systems for perceiving and thinking intuitively about small versus large numbers (for reviews, see, e.g., Feigenson, Dehaene, and Spelke, 2004; Anobile, Chicchini, and Burr, 2016). In the so-called "subitizing" range of numbers (up to about four), people precisely keep track of the individual objects, whereas for larger numbers, people rely on an approximate representation of magnitude. Research on these different systems has focused on performance on perception and arithmetic tasks, not

Despite pre-dating Tversky and Koehler (1994) by two decades, Kahneman and Tversky (1972a) anticipated the potentially confounding effect of partition dependence. They emphasized that "in contrast [to previous studies], subjects evaluate[d] the *same* number of categories for all sample sizes" (p. 441). Indeed, according to the model of partition dependence in equations (3.1) and (3.3), if the bins are held constant and if the function g is assumed to be the same across sample sizes, then the insensitivity of the reported-belief distributions to sample size implies that the root-belief distributions are also the same across sample sizes.

There have been several replications and extensions of Kahneman and Tversky's elicitation of full sampling-distributions beliefs. Recently, Benjamin, Moore, and Rabin (2018) elicited subjective sampling distributions about flips of a fair coin. They asked about samples of size 10, 1000, and 1 million, each with the same 11-bin partition used by Kahneman and Tversky. Despite incentivizing participants' responses and eliciting all three distributions from each participant, they found identical subjective sampling distributions across the three sample sizes. In an early replication, Olson (1976) reinforced Kahneman and Tversky's concern about the potentially confounding influence of partition dependence. Olson asked different groups of undergraduates to provide the sampling distribution for the percentage of boys born in regions with 100 and 1,000 babies born per day. When he used the same 11-bin partition as Kahneman and Tversky, he found identical distributions like they did. However, Olson also elicited the distributions using other partitions. For example, he asked another group of participants about the 100-baby distribution, but this time using an 11-bin partition with the outcomes <46, 46, 47, …, 53,

probabilistic reasoning. One might conjecture that intuitions for probabilistic reasoning are built in to the small-number system but not the large-number system.

54, and >54 boys. He found that the probabilities that participants assigned to these 11 bins were similar to those they assigned when the 11 bins corresponded to Kahneman and Tversky's partition. For instance, the median participant assigned only a slightly higher probability to the lowest category in the new partition—3% for <46 boys—than to the lowest category in Kahneman and Tversky's partition—1% for 0-5 boys—even though the true probability is much higher in the first case (18% versus roughly 0%).

Kahneman and Tversky interpreted sample-size neglect as showing that "The notion that sampling variance decreases in proportion to sample size is apparently not part of man's repertoire of intuitions" (p. 444). Sedlmeier and Gigerenzer (1997) proposed a more specific hypothesis: when asked about the distribution of means across samples, people instead give an answer about the distribution of outcomes within a sample. Among several pieces of evidence, the most telling comes from Sedlmeier (1994, Study 2, as described by Sedlmeier and Gigerenzer), who replicated and extended Kahneman and Tversky's elicitation of sampling-distribution beliefs about a normal distribution. Similar to prior work, Sedlmeier's experimental participants constructed distributions for the mean height of Israeli soldiers for sample sizes of 20 and 200—and, as in prior findings, the two distributions were identical. Another group of participants constructed distributions for height (as opposed to mean height) for these two sample sizes, i.e., distributions of heights for 20 soldiers and for 200 soldiers. These two distributions looked extremely similar, as they should, but they were also extremely similar to the distributions of mean height produced by the other participants, suggesting that the participants had no intuition that the two tasks were different.

Kahneman and Tversky reported further evidence of sample-size neglect from other questions that did not require participants to construct a distribution and are arguably less subject to confounding from partition dependence. For example, they asked whether a hospital with 45 births per day or one with 15 births per day would record more days with at least 60% of births being boys, or whether the two hospitals would have "About the same" number of days. Although the correct answer is the smaller hospital, more than half the participants chose "About the same," and roughly equal numbers chose the larger and smaller hospitals. This finding again points to people not understanding that the variance of the sampling distribution shrinks with sample size. It has been replicated in several dozen studies involving many variants of the judgment problem (for a review, see Lem, Dooren, Gillard, and Verschaffel, 2011).

Notwithstanding the evidence described above, people do seem to have two intuitions about the role of sample size, both originally identified by Bar-Hillel (1979). First, when asked directly, people expect the mean from a larger sample to be closer to the population mean. In a particularly clean demonstration, Well, Pollatsek, and Boyce (1990, Experiment 2) asked about the average height of the men registering at two conscription registration centers, one in which 25 men register per day and one in which 100 register per day, and told participants that the national average height in the population of men is 5 feet 9 inches. Similar to the hospital problem, one group of undergraduates was asked which center has more days when the average height exceeds 6 feet, and only 8% gave the correct answer of the smaller center. However, another group was asked which center will measure an average height closer to the national average on a particular day, and a third group was asked which will have more days when the average height is between 5 feet 6

inches and 6 feet (a 6-inch interval around the national average). In these latter two conditions, respectively 59% and 56% gave the correct answer. Well, Pollatsek, and Boyce concluded that although people have some basic understanding of the Law of Large Numbers, they do not understand its implications for the variance of the sampling distribution. I further discuss people's intuition that large samples are more likely to have means close to the population mean in Section 3.D.

Relatedly, Evans and Dusoir (1977, Experiment 2) hypothesized that when the question itself makes the logic clear to people, they can understand that extreme outcomes are less likely in large samples. Several studies have found evidence that has been interpreted as supporting this hypothesis (e.g., Bar-Hillel, 1979; Pelham and Neter, 1995, Study 1). For example, Bar-Hillel (1979) posed a version of the hospital problem with 15 and 5 births per day and asked which hospital recorded more days on which *all* the babies born were boys. In this problem, over half the participants correctly chose the smaller hospital and only a quarter chose "About the same." Bar-Hillel (1982) reported further evidence from versions of the problem that asked different groups of participants which hospital had more days in which the percentage of births being boys was over 60%, over 70%, over 80%, and 100%. She found that as the percentage became more extreme, more participants gave the correct answer. This seems to contradict the evidence from eliciting the full sampling distribution, discussed above, that people construct the same "universal distribution" regardless of sample size, even in the tails of the distribution, but constructing the distribution is arguably a more difficult task that does not give clues as to the correct intuition.

Second, people have an *incorrect* intuition that what matters for getting a sample mean close to the population mean is the *ratio* of the sample size to the population size, rather than the absolute sample size. Mathematically, as long as a sample is drawn with replacement, only the absolute sample size matters (and even if a sample is drawn without replacement, the ratio matters very little as long as the ratio is small). In one of Bar-Hillel's (1979, Experiment 4) studies, she described two urns, one containing 10 beads and one containing 100 beads, each with the same unknown proportions of red and green beads. She asked participants whether they would be more likely to correctly guess the majority color if they took 9 draws with replacement from the small urn or 15 draws with replacement from the large urn. 72 out of 110 participants erroneously chose the smaller number of draws from the small urn, presumably because it has a higher ratio of draws to urn size. Evans and Bradshaw (1986) also found evidence that experimental participants incorrectly believe they can draw stronger inferences when the ratio of sample size to population size is larger. I am not aware of any work that has explored the psychology underlying this intuition or its broader implications.

Benjamin, Rabin, and Raymond (2016) proposed a model to capture sample-size neglect. They called the bias that generates sample-size neglect *Non-Belief in the Law of Large Numbers (NBLLN)*. In the model, signals are drawn i.i.d. from a binomial distribution whose rate of *a* signals is θ. The agent, however, forms beliefs as if any particular sample is generated by a two-step process: (i) a "subjective rate" β is drawn from some distribution that has mean θ and full support on [0,1], called the "subjective-rate distribution"; and then (ii) the signals for the sample are drawn i.i.d. from a binomial distribution whose rate is β. This model directly generates sample-size neglect in large

samples: if β were the actual rate, the proportion of *a* signals in a large sample would be β (by the Law of Large Numbers). Therefore, in a large sample, the probability density that the agent assigns to any proportion of signals (say, 60% of babies are boys) is equal to the probability density that the subjective-rate distribution assigns to β equaling that value. In other words, as the sample size gets large, the agent's subjective sampling distribution for the mean converges to the subjective-rate distribution. Thus, in the model, the subjective-rate distribution is the "universal distribution" that the agent believes characterizes any large enough sample—and Kahneman and Tversky's evidence indicates that a sample size of 10 is already "large enough." For a sample size of one, the model implies that the agent has correct sampling-distribution beliefs. For any sample size larger than one, the agent's subjective sampling distribution is flatter than the correct distribution (due to the randomness of β), and the agent believes that tail events are more likely than they are.

Benjamin, Rabin, and Raymond used the model as a tool to explore the implications of sample-size neglect in a number of settings, including risky decision making. A number of implications follow from the agent's belief that the tails of the sampling distribution—such as all *a*'s or all *b*'s—are more likely than they are. To give some examples, if winning a lottery requires matching all numbers, and matching each number has probability θ, then the agent will overestimate his chance of winning and be too willing to play. If success at a job fair requires getting at least one job offer, and getting any job offer has probability θ, then the agent will overestimate his chance of getting no offers and will undervalue attending. If each of many stocks has positive expected value and earns money with

independent probability θ, then the agent overestimates the variance of payoffs in a diversified portfolio and hence will undervalue diversification.

Similarly, the model predicts that people will undervalue a repeated, positive-expected-value gamble. Benartzi and Thaler (1999) reported evidence from several studies on attitudes toward repeated gambles and long-term investing that they interpreted as consistent with sample-size neglect (related evidence is reported in Keren and Wagenaar, 1987, Keren, 1991, and Redelmeier and Tversky, 1992). For example, when undergraduate experimental participants were asked the probability of a net loss after 150 repetitions of a 90%/10% bet to gain \$0.10/lose \$0.50, participants' mean estimate was 24%—a dramatic overestimate relative to the correct probability of 0.3%. When actually offered this repeated gamble, only 49% accepted it. Yet 90% said they would accept a single-play bet that had the true distribution of money outcomes implied by the repeated bet, suggesting that they would have accepted the repeated bet if they had correctly understood the distribution of outcomes.

NBLLN also has implications for how people draw inferences. I will defer discussion of these implications until Section 5.A.

3.C. Sampling-Distribution-Tails Diminishing Sensitivity

As discussed above, NBLLN implies sample-size neglect: for large enough sample sizes, people's subjective sampling distribution is determined by a "universal distribution" that is invariant to sample size. This in turn implies that for large sample sizes, the tails of the subjective sampling distribution are fat relative to the true tails. There is also some evidence that the tails of the "universal distribution" are *flat* relative to the true tails.

NBLLN implies some flatness, but the amount of flatness is greater than can be explained by Benjamin, Rabin, and Raymond's (2016) model of NBLLN. Benjamin, Rabin, and Raymond (2016, Appendix C) conjectured that this excess flatness is due to another bias, which they called sampling-distribution-tails diminishing sensitivity (SDTDS): people think of unlikely outcomes as similar to each other.

Apparent flatness of the tails is evident in Figure 1b, which shows Kahneman and Tversky's survey data for the binomial with rate 0.8. In the true distribution for a sample size of 100, as one goes from 45-55% to 35-45% to 25-35% heads, the probability declines at an exponential rate, from 0.73 to 0.14 to 0.001. In contrast, the median participant's estimate declines much more slowly, from 0.22 to 0.15 to 0.10. Much of the other evidence from experimental participants' constructed sampling distributions also features flat tails (e.g., Wheeler and Beach, 1968; Peterson, DuCharme, and Edwards, 1968, Study 2; Teigen, 1974b). All of this evidence, however, is confounded by partition dependence, which would compress participants' estimates relative to their root beliefs.

In experiments designed to identify sampling-distribution beliefs separately from compression, Benjamin, Moore, and Rabin (2018) found evidence of flat tails for sample sizes of 1000 and 1 million. For example, experimental participants' sampling distribution for 1000 coin flips was elicited using the 5-bin partition: 0-487, 488-496, 497-503, 504-512, and 513-1000 heads. This partition was chosen because each bin has roughly equal true probability. Consequently, as discussed in Section 3.A, the deviation of beliefs away from equality indicates the direction of bias in root beliefs net of partition dependence. Mean beliefs had a "W" shape, overweighting the middle bin and extreme-tail bins but underweighting the intermediate-tail bins: mean beliefs were 26%, 13%, 21%, 14%, and

27%, compared with the true probabilities of 21.5%, 19.8%, 16.5%, 19.8%, and 21.5%, respectively. The combination of overweighting extreme tails but underweighting intermediate tails implies that the tail beliefs are too flat.

3.D. Overweighting the Mean and the Fallacy of Large Numbers

As discussed in Section 3.B, when people construct sampling distributions for samples of different sizes, they do not assign higher probability to the population mean in the larger sample size. Yet, as also discussed there, there is much evidence that when people are asked directly, they do have an intuition that when the sample is larger, the sample mean is likely to be closer to the population mean. Moreover, from experiments that control for confounding from partition dependence, there is some evidence that when people construct sampling distributions, they assign *too much* weight to the population mean. For example (as mentioned in Section 3.C), in five-bin elicitations of beliefs about samples of 1000 and 1 million coin flips, Benjamin, Moore, and Rabin (2018) found that relative to the true probabilities, experimental participants overweighted both the extreme-tail bins and the middle bin. Olson (1976) also found evidence that points to overweighting the mean, net of partition dependence. For instance (as also discussed in Section 3.C), some of his experimental participants constructed sampling distributions for how often a 100-baby sample would have different percentages of boys. Among participants where the middle bin in an 11-bin partition was 45-55 boys, participants' median estimate was 40% (the true probability is 68%). Among a different group of participants where the middle bin in an 11-bin partition was exactly 50 boys, participants' median estimate was actually slightly *higher*: 45% (the true probability is 8%).

Further evidence comes from Klos, Weber, and Weber (2005), who asked their experimental participants a set of questions about four repeated gambles. For example, one gamble was a 50-50 chance to win 200 euros or lose 100 euros. When participants were asked about the standard deviation of payoffs or about probability of a loss, it was clear that participants assigned too much probability mass to the tails, replicating Benartzi and Thaler's (1999) evidence of NBLLN. But participants were also asked the probability that the outcome would fall within +/- 100 euros of the expected value in 5 or 50 repetitions of the gamble. While the true probability is 21% for 5 repetitions and 7% for 50 repetitions, participants' mean estimates were dramatically too high: 47% and 58%.

This evidence is consistent with people having some correct Law of Large Numbers intuition. Yet the *overestimation* of the probability that the sample mean will match the population mean is more suggestive of the Law of Small Numbers (LSN) bias discussed in Section 2.A. The (incorrect) LSN intuition is that extreme sample realizations tend to be counteracted by additional signals (as opposed to the correct Law of Large Numbers intuition, which is that the effect of extreme sample realizations on the sample mean is diluted by additional signals). However, the LSN bias by itself does not explain why, in Klos, Weber, and Weber's experiment, participants' estimates—contrary to the true probabilities—are *higher* for 50 repetitions than for 5 repetitions. Klos, Weber, and Weber's comparison between 50 and 5 repetitions was motivated as a test of Paul Samuelson's (1963) hypothesis that people suffer from a "fallacy of large numbers." Samuelson had hypothesized that people have a specific misunderstanding of the Law of Large Numbers: while the correct idea is that for fixed $\epsilon > 0$, $p\left(\frac{1}{N}\Sigma_{i=1}^{N} s_i - \theta < \epsilon\right) \to 1$ as

$N \to \infty$, he argued people incorrectly think that $p\left(\Sigma_{i=1}^{N} s_i - N\theta < \epsilon\right) \to 1$. In words, the Law of Large Numbers states that the *mean* of the signals in the sample becomes arbitrarily close to the population rate. The fallacy states incorrectly that the *sum total* of the signals becomes arbitrarily close to its expected value.[12]

Psychologically, the fallacy of large numbers is closely related to the LSN: it is the belief that the GF is stronger in larger samples. More precisely, the GF is the belief that below-average realizations and above-average realizations tend to cancel out in any sample, whereas the fallacy of large numbers states that below-average and above-average realizations will *perfectly* cancel out in an arbitrarily large sample.

The fallacy-of-large-numbers hypothesis is plausible but logically contradicts sample-size neglect / NBLLN: if people's sampling-distribution beliefs are pinned down by a "universal distribution" over proportions regardless of sample size, then they would believe that the probability of the outcome $\Sigma_{i=1}^{N} s_i$ ending up in any fixed interval converges to zero as $N \to \infty$. Because of this contradiction, Benartzi and Thaler (1999) interpreted their evidence of NBLLN (see Section 3.B) as casting doubt on the fallacy-of-large-numbers hypothesis. But this internal inconsistency between biases could be a case where which bias occurs depends on which question a person is asked; for related discussion, see Sections 3.B, 3.F, and 10.B. Another possibility is that the fallacy-of-large-numbers

[12] For readers unfamiliar with the "fallacy of large numbers" hypothesis, some orientation regarding its history may be helpful. Samuelson noted that an MIT colleague said he would turn down a single gamble like the one studied by Klos, Weber, and Weber (a 50-50 chance to win 200 euros or lose 100 euros) but accept many repetitions of the gamble. Samuelson argued that his colleague's willingness to accept many repetitions was a mistake, and he proposed the fallacy of large numbers to explain the supposed mistake. Benartzi and Thaler (1999) documented behavior like that of Samuelson's colleague in surveys and experiments (see Section 3.B), but they argued that people's error is turning down the single gamble (due to loss aversion; see Chapter XXX (by O'Donoghue and Sprenger) in this Handbook), rather than accepting the repeated gamble. Moreover, as noted below, Benartzi and Thaler interpreted their evidence of NBLLN as evidence *against* the fallacy-of-large-numbers hypothesis.

hypothesis is not the correct explanation of Klos, Weber, and Weber's evidence. I am not aware of other tests of the hypothesis.

Overall, my reading of the data is that NBLLN coexists with a sampling-distribution bias of overweighting the mean, which may be due to the LSN. At this point, there is not enough evidence for a confident judgment about whether there is also a fallacy-of-large-numbers bias.

3.E. Sampling-Distribution Beliefs for Small Samples

All of the evidence discussed so far has been from sample sizes of at least 10. There are two papers that elicited subjective sampling distributions for smaller sample sizes. Wheeler and Beach (1968) elicited two binomial sampling distributions, with rates $\theta = 0.6$ and 0.8 and both with a sample size of N = 8. They found that their participants' distributions were too flat.[13] Peterson, DuCharme, and Edwards (1968, Study 2) elicited nine binomial sampling distributions, with the three rates $\theta = 0.6$, 0.7, and 0.8 and the three sample sizes N = 3, 5, and 8. They found that participants' sampling distributions were roughly correct for N = 3 but were flatter than the correct distributions for N = 5 and especially for N = 8. In all cases, beliefs were elicited using a partition that binned each possible outcome separately (e.g., 0, 1, 2, and 3). Thus, the evidence from both papers confounds the root-belief distributions with compression due to partition dependence, which would also flatten reported-belief distributions. Taking compression into account,

[13] Wheeler and Beach's study had a sequence of stages, and the sampling distributions were elicited three times over the course of the study. In between, the participants observed realized samples, made bets about which distribution each sample was drawn from, and then received feedback about whether they were correct (see Section 4.A for further discussion). While participants' sampling distributions were too flat at the beginning of the experiment (prior to any feedback), by the end of the experiment the distributions were too peaked.

Peterson et al.'s results may suggest that people's root-belief distributions are *too peaked* for sample sizes of 3, rather than too flat.

Using an elicitation designed to control for compression, Benjamin, Moore, and Rabin (2018) studied beliefs about samples of 10 coin flips and found no evidence that participants' root-belief distribution was too flat. Specifically, they elicited beliefs using the 5-bin partition 0-3, 4, 5, 6, and 7-10 heads, which is the partition that comes closest to equal true probabilities in each bin (17%, 21%, 25%, 21%, and 17%). According to the model of compression effects in Section 2.C, with such a partition, the direction of bias of reported beliefs also indicates the direction of bias of root beliefs. In both their convenience sample of adults and their sample of undergraduates, Benjamin et al. found that mean beliefs were approximately correct (18%, 22%, 28%, 18%, and 14% for the adults and 16%, 18%, 32%, 18%, and 16% for the students), except with some overweighting of the middle bin. These results suggest that, for sample sizes of 10, people's root-belief distribution is roughly correct or too peaked.

Putting the scant evidence together, it suggests that for sample sizes between one and 10, people's root-belief sampling distributions may be too peaked. I am not aware of any evidence regarding beliefs about samples of size one, probably because such an elicitation would be weird for experimental participants. It seems likely that such beliefs are correct: people would believe that the probability of an *a* signal in a single draw when the rate is known to be θ is equal to θ.

3.F. Summary and Comparison of Sequence Beliefs Versus Sampling-Distribution Beliefs

Psychologists have identified two main biases in people's beliefs about sequences of random events: the GF and the hot-hand bias, both of which may be due to the LSN (Sections 2.A and 2.B). The LSN also appears to influence people's beliefs about sampling distributions, causing them to assign too much probability to the possibility that the sample mean will be close to the population rate (Section 3.D).

People's sampling-distribution beliefs, however, are also influenced by other biases: partition dependence (Section 3.A), NBLLN (Section 3.B), and perhaps SDTDS (Section 3.C). Summarizing all of the evidence from Section 3 and focusing on what can be inferred about root beliefs: for "small" sample sizes (say, smaller than 10), people think the sampling distribution is too peaked, while for non-small sample sizes, people think the sampling distribution has tails that are too fat and too flat but also that put too much weight at the mean. Most of this evidence can be rationalized by LSN dominating at the small sample sizes and by LSN, NBLLN, and SDTDS jointly influencing beliefs at the larger sample sizes.

People's sampling-distribution beliefs are internally inconsistent due to partition dependence. Even if we put this aside by focusing on root beliefs, people's sampling-distribution beliefs are inconsistent with their sequence beliefs because several biases (such as NBLLN and SDTDS) influence sampling-distribution but not sequence beliefs.

In some direct tests in which sampling-distribution beliefs and sequence beliefs were elicited from the same experimental participants, Benjamin, Moore, and Rabin (2018) reported evidence of such inconsistency (suggestive evidence of such inconsistency was

also reported by Teigen, 1974a). For example, experimental participants' root beliefs about the distribution of the number of heads out of 10 coin flips are roughly correct, as mentioned in Section 3.E. This would imply that people think 9 heads out of 10 flips is 10 times more likely than 10 heads out of 10 flips. But, as per the GF, they believe that heads is roughly half as likely as tails following a streak of 9 heads. And since, given the GF, participants surely think that the nine other ways to get 9 heads out of 10 are at least as likely as HHHHHHHHHT, their sequence beliefs imply that 9 out of 10 heads should be at least 20 times more likely than 10 out of 10 heads.

This internal inconsistency means that people's beliefs about a random sample will depend on whether they are thinking about the sequence of signals or the distribution generated by that sequence. Economic models have generally not drawn this distinction, and I am not aware of work that studies when people think about sequences versus distributions, but these will be important issues to work out. I briefly discuss some of the related modeling challenges in Section 10.B.

Section 4. Evidence on Belief Updating

Belief updating is the revision of beliefs upon receipt of new information. The core component of the neoclassical theory of probabilistic beliefs is the assumption that people update beliefs according to Bayes' Theorem. This section is about the evidence on deviations from Bayesian updating. The review in this section aims to be comprehensive, except that I focus on settings where people are motivated only to be accurate; I defer discussion of settings where people also have preferences over which state of the world is true until Section 9.

For simplicity, I will describe Bayesian updating (and deviations from it) in the case where there are two states of the world, A and B. Denote the agent's *prior* beliefs, before observing new signals, by $p(A)$ and $p(B)$. Bayes' Theorem prescribes how to update the prior beliefs to *posterior* beliefs after observing some set of signals, S:

$$p(A \mid S) = \frac{p(S \mid A)p(A)}{p(S \mid A)p(A) + p(S \mid B)p(B)} \tag{4.1}$$

$$p(B \mid S) = \frac{p(S \mid B)p(B)}{p(S \mid A)p(A) + p(S \mid B)p(B)} \tag{4.2}$$

where $p(S \mid A)$ is the likelihood of observing S in state A, and $p(S \mid B)$ is the likelihood of observing S in state B.[14] It is often useful to write Bayes' Theorem in its posterior-odds form, obtained by dividing equation (4.1) by equation (4.2):

[14] Bayes' Theorem is an immediate consequence of the definition of conditional probability, $p(X \mid Y) \equiv \frac{p(X \cap Y)}{p(Y)}$. Using this definition for the first and last equalities: $p(A \mid S) = \frac{p(A \cap S)}{p(S)} = \frac{p(S \cap A)}{p(S \cap A) + p(S \cap B)} = \frac{p(S \mid A)p(A)}{p(S \mid A)p(A) + p(S \mid B)p(B)}$, and $p(B \mid S)$ is derived analogously.

$$\frac{p(A|S)}{p(B|S)} = \frac{p(S|A)p(A)}{p(S|B)p(B)} \tag{4.3}$$

This equation states that the posterior odds of state A to state B, $\frac{p(A|S)}{p(B|S)}$, is equal to the likelihood ratio, $\frac{p(S|A)}{p(S|B)}$, times the prior odds, $\frac{p(A)}{p(B)}$.

Much of the evidence on how people update their beliefs comes from what I will refer to as *updating problems*. In an updating problem, experimental participants are given priors and a set of signals from which the likelihoods could be calculated, and then their posterior beliefs are elicited. To illustrate this type of problem, Edwards (1968, p. 20-21) gave a hypothetical example:

> Imagine two urns filled with millions of poker chips. In the first urn, 70 percent of the chips are red and 30 percent are blue. In the second urn, 70 percent are blue and 30 percent are red. Suppose one of the urns is chosen randomly and a dozen chips are drawn from it: eight red chips and four blue chips. What are the chances that the chips came from the urn with mostly red chips? (Give your answer as a percentage.)

Here, the two states are A = {mostly red urn} and B = {mostly blue urn}, the prior probabilities are $p(A) = p(B) = 0.5$, and assuming that the chips are drawn with replacement (as in most of the experiments), the likelihoods can be calculated using the binomial distribution.

Biased updating can be identified by comparing people's posteriors with the correct posteriors. For example, Edwards reports that in his example, the intuitive answer for most people is roughly 70% or 80%. The correct answer is calculated by plugging the likelihoods, $p(S \mid A) = \binom{12}{8}(0.7)^8(0.3)^4 = 0.231$ and $p(S \mid B) = \binom{12}{8}(0.3)^8(0.7)^4 = 0.008$, and the priors into equation (4.1). Doing so yields a correct answer of 97%—much larger than most people anticipate! In this example, people underinfer, meaning that they infer less from the evidence than they should.

This section reviews the evidence on such deviations of people's posterior beliefs from normatively correct posterior beliefs in updating problems. Although Edwards's example is hypothetical, there are many dozens of experiments that have been conducted in which poker chips are actually drawn out of urns in front of the participants (or balls are drawn out of bookbags, etc.). These are often called *bookbag-and-poker-chip experiments*. Most of the evidence reviewed in this section comes from bookbag-and-poker-chip experiments.

Most of these experiments were published in the psychology literature during 1964-1973 and are unfamiliar to economists.[15] Some historical context helps to understand why. The pioneers in studying deviations from Bayesian updating were Ward Edwards, a psychologist, and his student, Larry Phillips (Edwards and Phillips, 1964; Phillips and Edwards, 1966). Edwards had written two important early reviews of behavioral decision research (1954, 1961b) and a seminal paper introducing psychologists to Bayesian statistics

[15] This literature also included a number of experiments on deviations from the Bayesian model of demand for information (e.g., Green, Halbert, and Minas, 1964; Edwards and Slovic, 1965). For economists, this work is also unfamiliar but relevant. I do not review it here.

that remains a classic among statisticians (Edwards, Lindman, and Savage, 1963). It was thus natural for him and other psychologists at the time to ask how people's actual updating compares to Bayes' Theorem. The bookbag-and-poker-chip paradigm was the workhorse in this active literature.

As discussed in Section 7 of this Chapter, Daniel Kahneman and Amos Tversky's persuasive "heuristics and biases" research program, beginning with Tversky and Kahneman (1971) and Kahneman and Tversky (1972a), redirected psychologists' attention toward understanding the psychological processes underlying belief judgments. In the meantime, Edwards's interests shifted toward designing computer programs to aid people in applying Bayes' Theorem to their priors and likelihood judgments (Edwards, 1968). After 1973, the psychology literature on biased belief updating became dominated by the sort of hypothetical updating scenarios that Kahneman and Tversky employed (which more closely resembled real-world situations than Edwards's abstract environments did).

Economists were influenced by Kahneman and Tversky's work. When David Grether (1980) conducted the first economics experiments on belief updating, he framed it as testing whether Kahneman and Tversky's representativeness heuristic describes people's beliefs when people are financially motivated and experienced, and he did not mention the earlier psychology literature at all.[16] Yet instead of posing hypothetical judgment scenarios via surveys as Kahneman and Tversky had done, Grether adopted the bookbag-and-poker-chip paradigm as his experimental methodology in order to make the random process transparent to participants and to better control the information that

[16] In personal correspondence, David Grether told me that early drafts of his paper had referenced the bookbag-and-poker-chip literature in psychology (as he had done in his review paper, Grether (1978)), but his recollection is that a referee asked him to remove those references.

participants might use to fill in unspecified scenario details. Subsequent economics experiments have continued to use the bookbag-and-poker-chip paradigm but have built on the findings of the precursor economics experiments rather than on the much earlier psychology experiments.

This section draws on both the earlier psychology literature and the more recent experiments in economics and psychology. To help organize this large body of evidence, I will supplement the literature review with a meta-analysis. To organize the findings, throughout the section I summarize a sequence of "stylized facts" that I will refer back to in subsequent sections of this chapter.

4.A. Conceptual Framework

To organize the evidence on belief-updating biases, I will use the following reduced-form model introduced by Grether (1980)[17]:

$$\pi(A \mid S) = \frac{p(S \mid A)^c \, p(A)^d}{p(S \mid A)^c \, p(A)^d + p(S \mid B)^c \, p(B)^d} \tag{4.4}$$

$$\pi(B \mid S) = \frac{p(S \mid B)^c \, p(B)^d}{p(S \mid A)^c \, p(A)^d + p(S \mid B)^c \, p(B)^d} \quad , \tag{4.5}$$

[17] To be more precise, equations (4.4)-(4.6) are the implicit model underlying Grether's specification. Grether introduced the empirical regression specification in equation (4.15) below (both with and without the indicator term), which can be derived by taking the logarithm of equation (4.6) below and adding a constant term and an error term. Many subsequent economics papers have followed Grether (1980) in estimating this equation or its sequential-sample analog, equation (4.21) below, introduced by Grether (1992). For an alternative organizing framework, see Epstein, Noor, and Sandroni (2008).

where $p(\cdot)$ refers to a true probability, $\pi(\cdot)$ refers to a person's (possibly biased) belief, and $c, d \geq 0$. The parameter c measures biased use of the likelihoods, and d measures biased use of the priors. Bayes' Theorem is the special case $c = d = 1$. I will not treat c and d as (fixed) structural parameters that *explain* people's updating. Instead, I use them merely to *describe* deviations from Bayesian updating. Much of this section focuses on establishing stylized facts about how c and d vary with features of the updating problem. In subsequent sections, I take these stylized facts as given and discuss theories of biased updating.

To interpret the magnitudes of c and d, it is helpful to write the model in the posterior-odds form that is analogous to equation (4.3). Dividing equation (4.4) by equation (4.5):

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \left[\frac{p(S \mid A)}{p(S \mid B)}\right]^{c} \left[\frac{p(A)}{p(B)}\right]^{d}. \tag{4.6}$$

From this equation, it is clear that $c < 1$ corresponds to updating as if the signals provided less information about the state than they actually do (*underinference*).[18] Symmetrically, $c > 1$ means updating as if the signals are more informative than they are (*overinference*). Similarly, $d < 1$ corresponds to treating the priors as less informative than they are and $d > 1$ to the opposite. Following the literature (which I review in Section 6), I call the former

[18] In the literature, what I refer to as underinference is often called "conservatism." To keep the distinction between theory and evidence clear, I reserve the term conservatism to refer to a particular theory of underinference discussed in Section 5.B.

base-rate neglect. (There is no accepted term for the latter because it is rare empirically, as we will see, but it could be called "base-rate over-use.")

This conceptual model has three important properties. First, when the priors are equal, $p(A) = p(B)$, the value of *d* does not matter for updating; the bias in posterior beliefs is entirely driven by *c*. Therefore, biases in inference can be isolated by studying settings with equal priors. For instance, in Edwards's (1968) example above, since the prior probabilities of the two urns are equal, we can describe people's biased posteriors as resulting from underinference. In this section I exploit this property to study biased inferences.

Second and symmetrically, when the likelihoods are equal, $p(S \mid A) = p(S \mid B)$, the bias in updating is entirely determined by *d*, and therefore, deviations from optimal use of prior information can be isolated by studying settings with equal likelihoods. Such settings are discussed in Section 6.

Third, and related to the first two properties, while researchers sometimes speak as if what matters for biased updating is whether likelihoods are underweighted or overweighted *relative* to priors, in fact the *absolute* values of *c* and *d* both matter. For example, suppose that $c = d < 1$, so that the relative weighting of likelihoods and priors is correct, but both are underweighted (as we will see is usually the case). Then in general, the agent's posterior odds will be biased—with *c* fully driving the bias if the priors are equal and with *d* fully driving the bias if the likelihoods are equal, as already noted. Therefore, contrary to what is sometimes said, the evidence for base-rate neglect (discussed in this section) is not in tension with the evidence (also discussed in this section) that people generally underinfer.

The *c* and *d* parameters can be estimated from updating from simultaneous samples, in which people update in response to a one-shot sample of signals, or from updating from sequential samples, in which people update dynamically as additional signals are observed. Because the latter is more complex, I begin with evidence from simultaneous samples in Section 4.B and then turn to evidence from sequential samples in Section 4.C.[19]

4.B. Evidence from Simultaneous Samples

Here I will review a set of stylized facts regarding biased inferences and biased use of priors that have emerged from simultaneous-sample experiments. I will both describe the results from specific experiments as well as report a meta-analysis intended to summarize the evidence from the literature as a whole. The meta-analysis extends the earlier meta-analysis reported by Benjamin, Rabin, and Raymond (2016, Appendix D) with additional data[20] and new analyses.

The vast majority of bookbag-and-poker-chip experiments focus on a particular class of updating problems: there are two states of the world, *A* and *B*; there are two signals, *a* and *b*; and the signals are drawn i.i.d., with probability θ_A of an *a* signal in state *A* and θ_B in state *B*. Participants are given the prior probabilities, and then they either observe a sequence of signals, such as *aabab*, or they are just told the total number of realized *a* and

[19] Recently, Augenblick and Rabin (2018) showed how a researcher can infer the directions of deviation of *c* and *d* from one based on observing how a person's probabilistic beliefs change in response to signals, even when the signals are not observed by the researcher. I am not aware of any empirical work yet that has estimated the biases using this approach.

[20] Specifically, here I add data from 6 new papers to the meta-analysis sample, bringing the total number of papers to 16. In addition, I conduct a new meta-analysis of 5 sequential-sample papers by combining sequential observations from 3 of the papers included in the earlier analysis with sequential-sample data from 2 new papers. The sequential-sample meta-analysis is discussed in Section 4.C below.

b signals, N_a and N_b. In simultaneous-sample experiments, participants' posterior beliefs are elicited only once, after the complete sample has been realized.

Most simultaneous-sample experiments further restrict attention to symmetric updating problems, in which (like in Edwards's example above) the probability of an *a* signal in state *A* is equal to the probability of a *b* signal in state *B*: $\theta \equiv \theta_A = 1 - \theta_B$. In the literature the parameter θ, which quantifies how diagnostic of the state any given signal is, is called the *diagnosticity* parameter. Without loss of generality, it is conventional to label the states as *A* or *B* such that $\theta > \frac{1}{2}$.

While the narrative literature review in this section is broader (for example, it includes non-binomial updating problems), the meta-analysis is restricted to two-state, binomial, symmetric updating problems. It uses the results from the 16 papers I could identify that (i) face experimental participants with updating problems from this class and (ii) report all the variables needed to calculate the correct answer—$p(A)$, $p(B)$, θ, N_a, and N_b—as well as the participants' mean or median posterior beliefs for at least one such problem. I have posted on my website all of the data and code underlying these analyses.[21]

I first ask: how commonly do people underinfer versus overinfer? To address this question, I focus on updating problems in which the prior probabilities of the two states are equal because, as noted above in Section 3.A, in these problems any error in people's posterior beliefs can be attributed to biased inference. I measure experimental participants'

[21] Although Grether (1992) does not report all the needed variables, David Grether provided this data to me and gave me permission to share it, so it is included in the meta-analysis and made available on my website. I have also posted data from asymmetric updating problems (where $\theta_A \neq 1\text{-}\theta_B$) on my website, even though these data are not included in the meta-analysis.

posterior beliefs using log posterior odds, $\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)$. This quantity is positive if participants believe that state *A* is more likely and negative if they believe that state *B* is more likely.

For each of the inference problems included in the meta-analysis, Figure 2 Panel A plots participants' log posterior odds on the y-axis against the correct log posterior odds, $\ln\left(\frac{p(A|S)}{p(B|S)}\right)$, on the x-axis. The identity line (the dashed line in the figure) corresponds to Bayesian inference. To interpret the regression slope (the solid line), note that taking the logarithm of equation (4.6), participants' log posterior odds can be written

$$\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right) = c\ln\left(\frac{p(S|A)}{p(S|B)}\right) + d\ln\left(\frac{p(A)}{p(B)}\right), \tag{4.7}$$

and taking the logarithm of equation (4.3), the correct log posterior odds are

$$\ln\left(\frac{p(A|S)}{p(B|S)}\right) = \ln\left(\frac{p(S|A)}{p(S|B)}\right) + \ln\left(\frac{p(A)}{p(B)}\right). \tag{4.8}$$

In both equations, the prior-odds term vanishes because the updating problems are restricted to those with equal priors:

$$\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right) = c\ln\left(\frac{p(S|A)}{p(S|B)}\right), \tag{4.9}$$

$$\ln\left(\frac{p(A\mid S)}{p(B\mid S)}\right)=\ln\left(\frac{p(S\mid A)}{p(S\mid B)}\right). \tag{4.10}$$

Substituting equation (4.10) into equation (4.9) yields

$$\ln\left(\frac{\pi(A\mid S)}{\pi(B\mid S)}\right)=c\ln\left(\frac{p(A\mid S)}{p(B\mid S)}\right). \tag{4.11}$$

Therefore, the regression slope in the figure is a measure of *c* that is averaged across the updating problems included in the analysis. At each point in the figure, the ratio $\ln\left(\frac{\pi(A\mid S)}{\pi(B\mid S)}\right)/\ln\left(\frac{p(A\mid S)}{p(B\mid S)}\right)$ is a measure of the biased-inference parameter *c* for that inference problem.[22] Points below the identity line in the first quadrant and above the identity line in fourth quadrant correspond to underinference ($c<1$).

From Figure 2 Panel A, it can be seen that in these experiments, participants underinfer more often than they overinfer. The slope of the regression line is $\hat{c}=0.20$, with a standard error of 0.063—far smaller than one. The figure also shows a locally linear regression curve, which suggests that the underinference tends to be more extreme when the correct inference is stronger.

[22] In the psychology literature on bookbag-and-poker-chip experiments, this quantity was referred to as the "accuracy ratio" (Peterson and Miller, 1965), and it was typically the main measure of biased updating relative to Bayes' Theorem. Sometimes, these experiments studied updating problems in which the priors are not equal, in which case the prior-odds terms in equations (4.7) and (4.8) do not vanish, so the accuracy ratio reflects a mixture of *c* and *d*: $\frac{\ln\left(\frac{\pi(A\mid S)}{\pi(B\mid S)}\right)}{\ln\left(\frac{p(A\mid S)}{p(B\mid S)}\right)}=\frac{c\ln\left(\frac{p(S\mid A)}{p(S\mid B)}\right)+d\ln\left(\frac{p(A)}{p(B)}\right)}{\ln\left(\frac{p(S\mid A)}{p(S\mid B)}\right)+\ln\left(\frac{p(A)}{p(B)}\right)}$. In order to identify biased inference separately from evidence on biased use of prior information, I focus throughout this section on estimators that distinguish between *c* and *d*.

The first column of Table 2 Panel A shows the linear regression results displayed in Figure 2 Panel A. In the second column, the analysis is restricted to updating problems from incentivized experiments. In those experiments, the estimate is $\hat{c} = 0.38$, with a standard error of 0.028, indicating somewhat less but still substantial underinference on average and less noisy behavior.

In experiments with binomial signals that did not meet all the criteria for inclusion in the meta-analysis, underinference has also been the general finding.[23] In addition, underinference has been the usual finding in experiments where, instead of the signals being binomial, the signals are multinomial.[24] When a signal is drawn from a normal distribution, underinference has occurred when the signal realization is far from its expected value in either state, and otherwise, nearly Bayesian inference or overinference has occurred.[25]

To summarize:

Stylized Fact 1. Underinference is by far the dominant direction of bias.

This conclusion may be surprising since, in our personal experiences, many of us observe people jumping to conclusions. After discussing the rest of the evidence and various theories, Section 10.A returns to this apparent tension and discusses potential reconciliations. Section 5 discusses the leading theories for explaining underinference.

[23] For example, Chinnis and Peterson (1968), Peterson and Swensson (1968), Sanders (1968), De Swart (1972a), De Swart (1972b), and Antoniou, Harrison, Lau, and Read (2015).
[24] For example, Beach (1968), Phillips, Hays, and Edwards (1966, Study 1), Dale (1968), Martin (1969), Martin and Gettys (1969), Shanteau (1972), and Chapman (1973).
[25] Nearly Bayesian inference was found by DuCharme and Peterson (1968, Studies 1 and 2) and DuCharme (1970, Studies 1 and 2), while overinference was found by Gustafson, Shukla, Delbecq, and Walster (1973).

I next ask: how is underinference related to sample size, $N = N_a + N_b$? As a measure of the bias in inference, I will use the updating-problem-specific estimate $\hat{c}$ discussed above, $\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right) / \ln\left(\frac{p(A|S)}{p(B|S)}\right)$.

A number of papers have manipulated sample size while holding constant other features of the inference problem and reported the results in such a way that the relationship between *N* and $\hat{c}$ can be seen. Every such paper has found that larger *N* is associated with more underinference as measured by smaller $\hat{c}$.[26]

Turning to the meta-analysis sample, which includes studies that do not manipulate *N*, Figure 3 Panel A plots the inference measure against *N*. The value of $\hat{c}$ is mostly smaller than one, as expected given that underinference is the predominant direction of bias. The slope of the regression line is negative, indicating that $\hat{c}$ is smaller at larger sample sizes. A locally linear regression suggests that the relationship between underinference and sample size is steeper at smaller sample sizes.

Stylized Fact 2. Underinference (as measured by $\hat{c}$) is more severe the larger the sample size.

Are inferences biased at a sample size of 1? While the regression line in Figure 3 Panel A suggests that there is underinference when *N* = 1, the value of the regression line here relies largely on extrapolation from larger sample sizes. Focusing only on the 16

[26] I have found nine such papers: Green, Halbert, and Robinson (1965), Pitz (1967), Peterson, DuCharme, and Edwards (1968, Study 2), Peterson and Swensson (1968), Sanders (1968), Kahneman and Tversky (1972a), Griffin and Tversky (1992, Study 1), Nelson, Bloomfield, Hales, and Libby (2001, Study 1), and Kraemer and Weber (2004).

updating problems with $N = 1$, the mean $\hat{c}$ is 0.70 with a standard error of 0.057; restricted to the 7 updating problems from incentivized experiments, the mean is 0.86 with a standard error of 0.078. Thus, the data from the meta-analysis sample points to underinference from a sample size of 1.[27]

Among experiments with binomial updating problems and a sample size of 1 that did not meet all the criteria for inclusion in the meta-analysis, nearly all found substantial underinference or slight underinference,[28] with one exception (Robalo and Sayag, 2014).[29] One experiment observed overinference in an experimental condition with asymmetric rates that are close to each other (Peterson and Miller, 1965, $\theta_A = 0.6$, $\theta_B = 0.4$). In an experiment with a sample size of 1 in which the signal was drawn from a multinomial distribution, Phillips, Hays, and Edwards (1966) found nearly Bayesian inference. As noted above, when a single signal is drawn from a normal distribution, underinference has occurred when the signal realization is far from its expected value in either state but not otherwise.[30]

Thus, while there are exceptions (which may or may not be systematic), the evidence from $N = 1$ samples can be summarized as generally finding underinference:

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[27] For sample sizes of 2, 3, 4, 5, and 6, the corresponding mean $\hat{c}$ is 0.73 (SE = 0.07), 0.98 (SE = 0.10), 0.52 (SE = 0.08), 1.06 (SE = 0.09), and 0.67 (SE = 0.10), respectively. Thus, the broad impression is underinference across these small sample sizes, but we cannot reject overinference for sample sizes of 3 and 5.

[28] Substantial underinference was found by Dave and Wolfe (2003) and Gettys and Manley (1968, Studies 1 and 2), whereas slight underinference was found by Chinnis and Peterson (1968), Peterson and Swensson (1968, Study 1), Kraemer and Weber (2004), Sasaki and Kawagoe (2007), and Ambuehl and Li (2018).

[29] Robalo and Sayag (2014) studied a symmetric binomial updating problem with 60-40 priors. Their experimental participants did not have posteriors that are systematically less extreme than Bayesian posteriors. Depending on the degree of base-rate neglect, their evidence could be consistent with either Bayesian inference or overinference.

[30] DuCharme and Peterson (1968, Studies 1 and 2), DuCharme (1970, Studies 1 and 2), and Gustafson, Shukla, Delbecq, and Walster (1973).

Stylized Fact 3. On average, people underinfer after observing only a single signal.

I next ask which features of the sample matter most for people's inferences. It turns out that for Bayesian inferences in (symmetric) inference problems, a sufficient statistic is the *difference* between the number of *a* and *b* signals: $N_a - N_b$. This fact can be seen by specializing equation (4.3) to the case of symmetric, binomial signals:

$$\frac{p(A \mid S)}{p(B \mid S)} = \left[\frac{\binom{N}{N_a} \theta^{N_a} (1-\theta)^{N_b}}{\binom{N}{N_a} (1-\theta)^{N_a} \theta^{N_b}} \right] \left[\frac{p(A)}{p(B)} \right]$$

$$= \left(\frac{\theta}{1-\theta} \right)^{(N_a - N_b)} \left[\frac{p(A)}{p(B)} \right]. \tag{4.12}$$

Kahneman and Tversky (1972a) pointed out that this feature of normatively correct inferences is counterintuitive. For example, to most of us, 2 *a*'s out of 2 feels like much stronger evidence in favor of state *A* than 51 *a*'s out of 100, but in fact they are equally strong evidence because $N_a - N_b = 2$ in both cases. Rather than relying on the sample *difference*, $N_a - N_b$, Kahneman and Tversky hypothesized that people intuitively draw inferences on the basis of the sample *proportion*, N_a / N.

Kahneman and Tversky tested this hypothesis in a set of ten hypothetical updating problems. One of these problems[31] was (p. 447):

> Consider two very large decks of cards, denoted *A* and *B*. In deck *A*, 2/3 of the cards are marked *a*, and 1/3 are marked *b*. In deck B, 1/3 of the cards are marked *a*, and 2/3 are marked *b*. One of the decks has been selected by chance, and 12 cards have been drawn at random from it, of which 8 are marked *a* and 4 are marked *b*. What do you think the probability is that the 12 cards were drawn from deck A, that is, from the deck in which most of the cards are marked *a*?

In this problem, which is similar to Edwards's problem quoted above, the proportion of *a* signals is 2/3, and the difference between the number of *a* and *b* signals is 4. Similarly, as in Edwards's problem, the median subject reported a belief of 70%, much weaker than the correct posterior of 94%. Two other problems, each asked to a different group of subjects, were the same except that the numbers of *a* and *b* signals were changed from 8 and 4 (in the quoted problem above) to 4 and 2 in one problem and to 40 and 20 in the other. These problems hold constant the proportion of *a* signals but, by manipulating the sample size, change the true probabilities to 80% and 99.9999%, respectively. Yet, consistent with Kahneman and Tversky's hypothesis, the median subject's reported belief was virtually unaffected: 68% and 70%, respectively.

[31] In the original statement of the problem, the cards were marked "X" and "O." I've changed them to "*a*" and "*b*" for consistency of notation with the rest of this chapter. Moreover, while Kahneman and Tversky quote directly from their problem with $\theta = 5/6$, I instead describe their other set of problems, with $\theta = 2/3$, for greater comparability with Edwards' illustrative problem above.

In other problems, Kahneman and Tversky varied the proportion but held constant the difference and found that people reported a higher belief in state *A* when the proportion of *a* signals was higher. Kahneman and Tversky's finding that beliefs depend *only* on the sample proportion is an extreme result[32]; other experiments in the literature (discussed next) also find support for the hypothesis that people's inferences are influenced by the sample proportion, but they generally find that the difference between the number of *a* and *b* signals also matters.

Evans and Dusoir (1977) also found that many people rely on sample proportion over sample size (in a more complex experiment, Evans and Pollard (1982) reach the same conclusion). They asked undergraduates to make pairwise judgments such as whether a sample of coin flips with 8 heads and 2 tails provides stronger or weaker evidence of a heads-biased coin than a sample of 70 heads and 30 tails. When sample proportion and sample size considerations conflicted, as in this example, more than two-thirds of participants endorsed the sample with the larger proportion as providing more evidence.

Griffin and Tversky (1992) quantified the relative roles of sample size and sample proportions in driving people's inferences. They posed twelve updating problems to each

[32] Kahneman and Tversky's results are also extreme in another way: they find that the median subject's posterior belief is completely insensitive to the diagnosticity parameter θ. For example, in three updating problems identical to the those mentioned above but with $\theta = 5/6$ instead of 2/3, the median subject's posterior belief was 70% in all three cases. As discussed below, such complete insensitivity to θ has not been observed in updating problems more generally. Why were Kahneman and Tversky's results so extreme? A possible explanation is that the median subject was following a simple heuristic of setting their posterior belief $\pi(A \mid N_a, N_b)$ roughly *equal* to the sample proportion N_a / N. As Kahneman and Tversky pointed out, for all three sample proportions they investigated, the median subject's posterior belief was very nearly equal to the sample proportion and was insensitive to both N and θ. Earlier, Beach, Wise, and Barclay (1970) proposed that people follow this heuristic (and cite Kriz (1967) as having proposed it even earlier). Beach, Wise, and Barclay found evidence consistent with this heuristic in simultaneous-sample updating problems but not sequential-sample updating problems. Marks and Clarkson (1972) found that roughly 2/5 of their experimental participants seemed to follow this heuristic. As discussed below, Griffin and Tversky (1992) found that their median participant's posterior was somewhat sensitive to N and θ, which is inconsistent with the median participant reporting beliefs according to this heuristic.

of their undergraduate participants, with equal priors for the two states and the diagnosticity parameter θ fixed at $\frac{3}{5}$. Across the twelve problems, the number of signals varied from 3 to 33, and the sample proportion varied from 0.53 (9 *a*'s out of 17) to 1 (3 *a*'s out of 3 and 5 out of 5). To assess how participants' use of sample size and sample proportion deviated from Bayesian inference, Griffin and Tversky estimated a regression equation that nests Bayesian inference as a special case. I will derive this regression in four steps.

The starting point is the formula for Bayesian inference in symmetric binomial problems, equation (4.12), when the priors are equal:

$$\frac{p(A\,|\,S)}{p(B\,|\,S)} = \left(\frac{\theta}{1-\theta}\right)^{(N_a - N_b)}.$$

The first step is to obtain a linear equation by taking the double logarithm[33]:

$$\ln\left(\ln\left(\frac{p(A\,|\,S)}{p(B\,|\,S)}\right)\right) = \ln(N_a - N_b) + \ln\left(\ln\left(\frac{\theta}{1-\theta}\right)\right).$$

[33] Griffin and Tversky ensured that all of the terms in this equation are well defined by setting $\theta = \frac{3}{5} > \frac{1}{2}$ and posing updating problems in which $N_a > N_b$. In the meta-analysis data I analyze below, I guarantee $\theta > \frac{1}{2}$ by labeling the states such that state *A* has the higher rate of *a* signals. However, $N_a > N_b$ does not hold for all of the observations. To include in the analysis the observations where $N_a < N_b$, I exploit the symmetry of the updating problem, switching N_a and N_b and replacing participants' posterior odds $\frac{\pi(A\,|\,S)}{\pi(B\,|\,S)}$ in equation (4.14) by $\frac{\pi(B\,|\,S)}{\pi(A\,|\,S)}$. For example, if participant's posterior odds were $\frac{1}{3}$ after observing a 4 *b*'s and 1 *a*, I enter it into the analysis as if the odds were 3 after having observed 4 *a*'s and 1 *b*. I drop the 25 observations for which $N_a = N_b$ in the simultaneous-sample experiments and 16 such observations in the sequential-sample experiments.

Second, to separate out the role of the sample proportion, the sample-difference term is decomposed into the sum of a sample-proportion term and a sample-size term:

$$\ln\left(\ln\left(\frac{p(A\,|\,S)}{p(B\,|\,S)}\right)\right)=\ln\left(\frac{N_a-N_b}{N}\right)+\ln(N)+\ln\left(\ln\left(\frac{\theta}{1-\theta}\right)\right).$$

Third, this rule for Bayesian inference is generalized by allowing the coefficients to differ from one, and a response-error term is added:

$$\ln\left(\ln\left(\frac{\pi(A\,|\,S)}{\pi(B\,|\,S)}\right)\right)=\alpha_0+\alpha_1\ln\left(\frac{N_a-N_b}{N}\right)+\alpha_2\ln(N)+\alpha_3\ln\left(\ln\left(\frac{\theta}{1-\theta}\right)\right)+\epsilon. \tag{4.13}$$

Finally, because θ did not vary across their updating problems, Griffin and Tversky absorbed the θ term into the constant:

$$\ln\left(\ln\left(\frac{\pi(A\,|\,S)}{\pi(B\,|\,S)}\right)\right)=\tilde{\alpha}_0+\alpha_1\ln\left(\frac{N_a-N_b}{N}\right)+\alpha_2\ln(N)+\epsilon. \tag{4.14}$$

The null hypothesis of Bayesian inference is $\alpha_1=\alpha_2=1$. By estimating regression equation (4.14), Griffin and Tversky tested whether the sample proportion and sample size

are weighted as much as they should be according to Bayesian inference as well as how they are weighted relative to each other.

Griffin and Tversky reported estimates of $\hat{\alpha}_1 = 0.81$ and $\hat{\alpha}_2 = 0.31$, respectively.[34] To interpret these results, note first that the hypothesis $\alpha_1 = \alpha_2$ is rejected; thus, experimental participants are not drawing inferences based on the difference between the number of *a* signals and the number of *b* signals. Next, the results indicate that both α_1 and α_2 are smaller than one, consistent with underinference on average (Stylized Fact 1), and $\alpha_2 < 1$ points to greater underinference from larger samples (Stylized Fact 2). Since the hypothesis $\alpha_1 = 1$ and $\alpha_2 = 0$ is rejected, Griffin and Tversky's results are less extreme than Kahneman and Tversky's (1972a): participants' inferences are not *entirely* driven by the sample proportion; they do take sample size into account to some extent. Finally, the results indicate that $\alpha_1 > \alpha_2$, meaning that relative to (the correct) equal weighting of sample proportion and sample size, sample proportion influences inferences by more.

Griffin and Tversky's regression can be replicated in the meta-analysis data. Because this data has variation in θ, I estimate equation (4.13) rather than equation (4.14). The first column of Table 3 Panel A shows the results. The estimates are consistent with Griffin and Tversky's reported estimates, not only qualitatively but even quantitatively: the

[34] Specifically, Griffin and Tversky (p. 416) wrote: "For the median data, the observed regression weight for strength (.81) was almost 3 times larger than that for weight (.31)." However, when I estimate equation (4.14) using the median data reported in Griffin and Tversky, I find $\widehat{\tilde{\alpha}_0}$ = 0.44 (SE = 0.115), $\hat{\alpha}_1$ = 1.02 (SE = 0.094), and $\hat{\alpha}_2$ = 0.17 (SE = 0.064). In personal communication with Dale Griffin, we were unable to recover how the regression in the paper differed from my regression. Regardless of which estimates are used, the main conclusions are the same.

estimated coefficient on sample proportion, $\hat{\alpha}_1$, is 0.85 with a standard error of 0.071, and the estimated coefficient on sample size, $\hat{\alpha}_2$, is 0.41 with a standard error of 0.049. (I discuss the coefficient on the θ term below.) The third column of the table repeats the analysis but restricted to incentivized experiments, and the results are similar. Thus, while sample size matters to some extent, the sample proportion has a much greater impact on participants' inferences on average.

Stylized Fact 4. Rather than depending on the sample difference, $N_a - N_b$, people's inferences are largely driven by the sample proportion, $\frac{N_a - N_b}{N}$.

Beginning with Grether (1980), several papers have investigated the hypothesis that the sample proportion has an especially large impact on inference when it equals the rate of *a* signals in one of the states. Grether's idea was that if the sample proportion equals (say) θ_A, then participants can rely on the representativeness heuristic (discussed in Section 7) in drawing an inference in favor of state *A*. Elaborating on this idea, Camerer (1987) referred to the hypothesis that people draw stronger inferences when the sample proportion exactly matches one of the rates as "exact representativeness."

In Grether's experiment, the prior probability of state *A* varied across conditions, equaling 1/3, 1/2, or 2/3. The probability of an *a* signal was θ_A = 2/3 in state *A* and θ_B = 1/2 in state *B*. Experimental participants observed a set of N = 6 signals and guessed whether the state was *A* or *B*. In some conditions, participants were paid a bonus for guessing accurately. To analyze his data, Grether ran a regression corresponding to

equation (4.7) but with indicator variables for the observed sample proportion matching the states' rates:

$$\ln\left(\frac{\pi(A\mid S)}{\pi(B\mid S)}\right)=\beta_0+\beta_1\ln\left(\frac{p(S\mid A)}{p(S\mid B)}\right)+\beta_2\ln\left(\frac{p(A)}{p(B)}\right)$$
$$+\beta_3 I\left\{\frac{N_a}{N}=\theta_A\right\}+\beta_4 I\left\{\frac{N_a}{N}=\theta_B\right\}+\eta\,. \tag{4.15}$$

Because participants reported a guess about the state rather than a posterior probability, Grether estimated a logistic regression version of this equation, and thus the absolute magnitudes of the coefficients are not straightforward to interpret. Across various specifications and subsamples, his results generally indicated $\hat{\beta}_3>0$ and $\hat{\beta}_4<0$, consistent with exact representativeness. However, in two similar experiments conducted subsequently, Grether (1992) found much more equivocal evidence.

Camerer (1987, 1990) aimed to test whether biased updating would survive in markets. He conducted an experimental asset market, in which participants traded a state-contingent asset. In each round of the experiment, participants observed a set of $N = 3$ signals before trading. The probability of an *a* signal was 2/3 in state *A* and 1/3 in state *B*. Consistent with exact representativeness, he found that when the observed sample contained 2 *a*'s and 1 *b*, the price of a state-contingent asset that pays off in state *A* was too high, and when the observed sample contained 1 *a* and 2 *b*'s, the price of a state-contingent asset that pays off in state *B* was too high.

To assess the evidence regarding "exact representativeness" more broadly in bookbag-and-poker-chip experiments, I analyze the meta-analysis sample. Because this sample has variation in *N* and θ and is restricted to updating problems with equal priors, I estimate a version of equation (4.13) rather than equation (4.15). Specifically, Column 2 of Table 3 Panel A shows the results when I have included an indicator for the sample proportion being equal to θ. (There is no indicator for the sample proportion being equal to 1-θ because, as per footnote 33, all observations are coded such that $\theta > \frac{1}{2}$ and $N_a > N_b$.)

The coefficient on this indicator is 0.02, with a standard error of 0.086. The sign is in accordance with what exact representativeness would predict, but the standard error is much larger than the point estimate. Thus, I find little evidence for exact representativeness in the meta-analysis sample, but the estimate is too noisy to draw strong conclusions.

Stylized Fact 5. While some experiments have found evidence of overinference, or less underinference, when the observed sample proportion equals the rate in one of the states, it has not been robustly seen across experiments.

As a final question about inference: how is underinference related to the diagnosticity parameter, θ? Almost every study[35] that varies θ while holding constant other features of the updating problem has found greater underinference (as measured by $\hat{c}$

[35] Green, Halbert, and Robinson (1965), Peterson and Miller (1965), Sanders (1968), Peterson and Swensson (1968, Studies 1 and 2), Peterson, DuCharme, and Edwards (1968, Study 2), Beach, Wise, and Barclay (1970), Kahneman and Tversky (1972a), Donnell and DuCharme (1975). Vlek (1965) and Vlek and van der Heiden (1967) are cited in Slovic and Lichtenstein (1971) as also finding this result, but I have not been able to track down those papers.

) for θ further from ½, with the exceptions of Gettys and Manley (1968), who found no relationship, and Shanteau (1972), who found the opposite in a multinomial-signal experiment.

Turning to the meta-analysis data, Figure 4 Panel A plots $\hat{c}$ against diagnosticity θ. The slope of the regression line is -0.97 with a standard error of 0.27. The negative slope indicates that as θ increases, there is more underinference on average, consistent with what the individual studies have found.

To control for other factors that affect inferences and to examine whether participants adequately account for θ (compared to Bayesian inference), I return to Table 3 Panel A and examine the coefficient on the diagnosticity term. As noted above, Bayesian inference implies that the coefficient on $\ln\left(\ln\left(\frac{\theta}{1-\theta}\right)\right)$ should equal one. Instead, as seen in Column 1, the coefficient estimate is 0.39 (standard error = 0.082). The estimate remains similar, 0.52 (standard error = 0.097), when the sample is restricted to incentivized studies (Column 3). The fact that this coefficient is in between zero and one indicates that subjects' inferences take the different rates of *a* signals across states into account but less strongly than they should.

In asymmetric updating problems (which are excluded from the meta-analysis), there is some evidence that people overinfer when the rate of *a* signals in state *A* is similar to the rate in state *B*. As mentioned above, Peterson and Miller (1965) found overinference in inference problems with a single signal when the rates were (θ_A,θ_B) = (0.6,0.43), but they found underinference when the rates were further apart: (θ_A,θ_B) = (0.83,0.17), (θ_A,θ_B) = (0.71,0.2), and (θ_A,θ_B) = (0.67,0.33). Griffin and Tversky (1992, Study 3)

posed a set of updating problems in which the number of *a* signals is 7, 8, 9, or 10. When the rates were far apart, (θ_A,θ_B) = (0.6,0.25), their experimental participants underinferred: the median posterior beliefs that the state is *A* were .60, .70, .80, and .90, respectively, whereas a Bayesian's posteriors would be .95, .98, .998, and .999. In contrast, when the rates were close together, (θ_A,θ_B) = (0.6,0.5), the participants overinferred, with median posterior beliefs .55, .66, .75, and .85, respectively, compared to a Bayesian's posteriors of .54, .64, .72, and .80.[36] Grether (1992, Study 2) also found overinference in asymmetric updating problems with (θ_A,θ_B) = (0.67,0.5). Recently, in simultaneous-sample updating problems with a single signal, Ambuehl and Li (2018) also found underinference when θ_A and θ_B are far apart and overinference when they are close together.

Stylized Fact 6. Underinference (as measured by $\hat{c}$) is more severe the larger is the diagnosticity parameter θ. In asymmetric inference problems, people may overinfer when the rates θ_A and θ_B are close together.

[36] Griffin and Tversky's (1992, Study 3) evidence may also be related to a hypothesis, proposed by Vlek (1965), that people underinfer by more when an event occurs that is unlikely in both states. In a test of this hypothesis, Beach (1968) ran a bookbag-and-poker-chip experiment with multinomial signals: the letters A-F written on the back of a card. Cards were drawn from one of two decks, a red deck and a green deck, which had different proportions of the letters. Different groups of participants faced decks with the same likelihood ratios for the letters but different probabilities. For example, for one group, the probability of an F card was 0.03 for the red deck and 0.06 for the green deck, and for another group, 0.16 and 0.32. Holding the likelihood ratio fixed, Beach found greater underinference when the probabilities were smaller, consistent with Vlek's hypothesis. Slovic and Lichtenstein (1971) reported that Vlek (1965) and Vlek and van der Heijden (1967) found similar results, but I have been unable to obtain those papers.

To conclude the summary of evidence from simultaneous-updating experiments, I turn to biased use of priors. Five bookbag-and-poker-chip experiments that manipulated the priors found that people under-use prior information relative to what is prescribed by Bayes' Theorem, and two found that people over-use prior information.[37]

To examine the evidence across studies, I now add into the meta-analysis sample the updating problems with unequal priors. Whereas we had focused on problems with equal priors in order to isolate biased inference, we cannot follow the analogous strategy here of focusing on problems with equal likelihoods because there is little evidence from such problems, and the results require more nuanced discussion; I defer discussion of that evidence to Section 6. Therefore, in order to identify biased use of priors, I will need to control for biased inferences.

To do so, I exploit the fact that we previously estimated biased inferences in regression equation (4.13). The fitted values from that regression tell us what people's posterior odds would be in an updating problem with equal priors. Here, I will treat the fitted values as telling us what people's (biased) subjective likelihood ratio would be, before it is combined with the prior odds. That is, we replace equation (4.6) by[38]

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \frac{\pi(S \mid A)}{\pi(S \mid B)}\left[\frac{p(A)}{p(B)}\right]^{d}, \tag{4.16}$$

[37] Those that found under-use are Green, Halbert, and Robinson (1965), Bar-Hillel (1980), Griffin and Tversky (1992, Study 2), Grether (1992, Study 3), and Holt and Smith (2009), while those that found over-use are Peterson and Miller (1965) and Grether (1992, Study 2).

[38] Alternatively, we could consider directly estimating equation (4.6), but I do not do so because it is misspecified if treated as a "structural" model: as discussed above, the results from estimating equation (4.13) tell us that the exponent c depends on sample size, sample proportion, and diagnosticity in the updating problem.

where $\frac{\pi(S|A)}{\pi(S|B)}$ is a person's subjective likelihood ratio, and for any given updating problem, we use equation (4.13) to obtain an estimate of the logarithm of this subjective likelihood ratio, $\widehat{\ln\left(\frac{\pi(S|A)}{\pi(S|B)}\right)}$.[39] Next, we take the logarithm of equation (4.16) and isolate the prior ratio on the right side:

$$\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right) - \widehat{\ln\left(\frac{\pi(S|A)}{\pi(S|B)}\right)} = d\ln\left(\frac{p(A)}{p(B)}\right). \tag{4.17}$$

Figure 5 plots the left-hand side of equation (4.17) against the right-hand side. If experimental participants correctly use prior odds, then $d = 1$, so the points should fall along the identity line (the dashed line). Instead, the slope of the regression line (the solid line) is less than one, indicating under-use of prior odds.

For more formal evidence, I estimate the regression equation:

$$\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right) - \widehat{\ln\left(\frac{\pi(S|A)}{\pi(S|B)}\right)} = \gamma_0 + \gamma_1 \ln\left(\frac{p(A)}{p(B)}\right) + \zeta. \tag{4.18}$$

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[39] There is a nuance: the predicted value from equation (4.13) is an estimator for $\ln\left(\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)\right)$, but for equation (4.17), what is needed is an estimate of $\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)$. Simply exponentiating the estimate $\widehat{\ln\left(\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)\right)}$ is not a consistent estimator for $\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)$ due to Jensen's inequality. I therefore generate an estimate of $\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)$ by calculating $e^{\hat{\mu}+\frac{1}{2}\hat{\sigma}^2}$, where $\hat{\mu} = \widehat{\ln\left(\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)\right)}$ and $\hat{\sigma}^2$ is the estimated variance of the residual from equation (4.13). This estimator is consistent under the assumption that $\ln\left(\frac{\pi(A|S)}{\pi(B|S)}\right)$ is normally distributed.

The results are shown in Table 4. The first column represents the regression illustrated in Figure 5, while the second column restricts the data to experiments with unequal priors, which is the subset of the data that identifies γ_1. As expected, the estimate of d, $\widehat{\gamma_1}$, is essentially the same in both columns: 0.60 with a standard error of 0.066. This estimate of d is substantially smaller than one, indicating that on average people under-use prior information.

The third column of Table 4 re-runs the analysis from column 1, this time restricted to incentivized experiments. In this case, the estimate of d is 0.43 with a standard error of 0.086, indicating even more extreme under-use of priors. Thus, both the evidence from individual papers and the evidence from the meta-analysis point rather strongly to under-use of prior information.

Stylized Fact 7. People exhibit base-rate neglect.

While the experiments discussed in Section 6 have been the focus of the literature on base-rate neglect, Stylized Fact 7 shows that the evidence for base-rate neglect extends to bookbag-and-poker-chip experiments.[40]

[40] Few papers have addressed the question of whether giving people feedback leads to more accurate updating, but what evidence there is suggests only limited impact. Specifically, two papers have studied the effect of telling experimental participants the correct posterior probabilities after each updating problem. Martin and Gettys (1969) compared the effect of doing so with the effect of merely telling them the true state. The group that received posterior-probability feedback underinferred by less than the group that received true-state feedback, but over 200 trials, there was no detectable learning in either group, except possibly very early on. Donnell and DuCharme (1975) found that telling experimental participants the correct posterior probabilities after each of 60 updating problems eliminated their underinference, with almost all of the learning occurring in the first 10 trials. However, when participants were then faced with a new updating problem for which naïve participants tend to infer correctly, they overinferred. Donnell and

4.C. Evidence from Sequential Samples

Up until now, I have focused on bookbag-and-poker-chip experiments in which the sample was presented simultaneously. A number of bookbag-and-poker-chip experiments, however, have been sequential-sample experiments: participants observe a sample sequentially and report updated beliefs after each new signal (or set of signals) is observed. I now turn to the evidence from these experiments.

An initial conceptual question—which matters for how the data should be analyzed—is how people "group" signals. In the terminology of Benjamin, Rabin, and Raymond (2016, Appendix A), who provide formal definitions, one hypothesis is that people are *acceptive*: they group together signals that are presented to them together, and they treat sets of signals presented separately as distinct samples. For example, suppose two independent signals are observed sequentially. If people are acceptive, then they would update their beliefs after each signal, with their updated beliefs after the first signal becoming their priors when updating in response to the next signal. Another leading hypothesis is that people are *pooling*: at any point in time, people pool all the signals they have received up until that point and update from their initial priors using the pooled sample.

For a Bayesian updater, the grouping of the signals is irrelevant. Continuing with the example of two independent signals, suppose that a Bayesian is acceptive and hence updates after each signal. Using equation (4.3), her posterior odds after the first signal are

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DuCharme concluded that the feedback had caused participants to report more extreme beliefs but not to become better at drawing inferences.

$$\frac{p(A \mid s_1)}{p(B \mid s_1)} = \frac{p(s_1 \mid A)}{p(s_1 \mid B)} \frac{p(A)}{p(B)},$$

and her posterior odds after the second signal are

$$\begin{aligned} \frac{p(A \mid s_1, s_2)}{p(B \mid s_1, s_2)} &= \frac{p(s_2 \mid A)}{p(s_2 \mid B)} \frac{p(A \mid s_1)}{p(B \mid s_1)} = \frac{p(s_2 \mid A)}{p(s_2 \mid B)} \left(\frac{p(s_1 \mid A)}{p(s_1 \mid B)} \frac{p(A)}{p(B)} \right) \\ &= \frac{p(s_1, s_2 \mid A)}{p(s_1, s_2 \mid B)} \frac{p(A)}{p(B)}. \end{aligned}$$

If instead, she updates after the second signal by pooling both signals and updating from her original priors, then her posterior odds after the second signal are

$$\frac{p(A \mid s_1, s_2)}{p(B \mid s_1, s_2)} = \frac{p(s_1, s_2 \mid A)}{p(s_1, s_2 \mid B)} \frac{p(A)}{p(B)},$$

which are the same as the posterior odds from updating sequentially.

For a biased updater, however, grouping matters (see Cripps, 2018, for related discussion). Using equation (4.6), if the agent is acceptive, then her posterior odds after the first signal are

$$\frac{\pi(A \mid s_1)}{\pi(B \mid s_1)} = \left[\frac{p(s_1 \mid A)}{p(s_1 \mid B)} \right]^{c(1)} \left[\frac{p(A)}{p(B)} \right]^{d},$$

where $c(N)$ denotes the bias in inference from a sample of size N (recall from Stylized Fact 2 that underinference is increasing in sample size). Her posterior odds after the second signal are

$$\frac{\pi(A \mid s_1, s_2)}{\pi(B \mid s_1, s_2)} = \left[\frac{p(s_2 \mid A)}{p(s_2 \mid B)}\right]^{c(1)} \left[\frac{p(A \mid s_1)}{p(B \mid s_1)}\right]^{d}$$

$$= \left[\frac{p(s_2 \mid A)}{p(s_2 \mid B)}\right]^{c(1)} \left[\left[\frac{p(s_1 \mid A)}{p(s_1 \mid B)}\right]^{c(1)} \left[\frac{p(A)}{p(B)}\right]^{d}\right]^{d}$$

$$= \left[\frac{p(s_2 \mid A)}{p(s_2 \mid B)}\right]^{c(1)} \left[\frac{p(s_1 \mid A)}{p(s_1 \mid B)}\right]^{c(1)d} \left[\frac{p(A)}{p(B)}\right]^{d^2}. \tag{4.19}$$

In contrast, if she pools the signals and then updates, then her posterior odds are

$$\frac{\pi(A \mid s_1, s_2)}{\pi(B \mid s_1, s_2)} = \left[\frac{p(s_1, s_2 \mid A)}{p(s_1, s_2 \mid B)}\right]^{c(2)} \left[\frac{p(A)}{p(B)}\right]^{d}$$

$$= \left[\left(\frac{p(s_2 \mid A)}{p(s_2 \mid B)}\right)\left(\frac{p(s_1 \mid A)}{p(s_1 \mid B)}\right)\right]^{c(2)} \left[\frac{p(A)}{p(B)}\right]^{d}$$

$$= \left[\frac{p(s_2 \mid A)}{p(s_2 \mid B)}\right]^{c(2)} \left[\frac{p(s_1 \mid A)}{p(s_1 \mid B)}\right]^{c(2)} \left[\frac{p(A)}{p(B)}\right]^{d}. \tag{4.20}$$

Equation (4.20) differs from equation (4.19) for two reasons: if the agent updates separately after each signal, then (i) the bias in inference is the bias corresponding to a sample size of

1 rather than 2, and (ii) the information from the first signal is incorporated into the agent's prior when the second signal is processed, and so her biased use of priors affects how the first signal enters into her final posterior. These differences can matter not only for the analysis of experimental data, but also for the implications of biased updating in real-world environments. For further discussion of the implications of (i) and (ii), see Sections 5.A and 6, respectively.

Only two papers have explicitly tested experimentally between different grouping hypotheses, and both find evidence against pooling. Pooling predicts that people's posterior beliefs should not depend on how signals are presented. In incentivized updating problems, Kraemer and Weber (2004) found that mean beliefs of experimental participants presented with a sample of 3 *a* signals and 2 *b* signals differed marginally from those of experimental participants who were instead shown the same signals as two separate samples, one with 3 *a*'s and 0 *b*'s and one with 0 *a*'s and 2 *b*'s. Kraemer and Weber similarly found a difference in posteriors when participants were presented with a single sample of 13 *a*'s and 12 *b*'s, as opposed to a sequence of two samples, 13 *a*'s and 0 *b*'s and then 0 *a*'s and 12 *b*'s. Shu and Wu (2003, Study 3) found that participants who observed 10 signals one at a time reported a different posterior belief than participants who observed the same 10 signals two at a time or five at a time.

Although less clean, comparisons between participants' posteriors in simultaneous-sample versus sequential-sample experiments also bear on the pooling hypothesis. Holding constant other features of the updating problems, pooling predicts no differences in participants' posteriors. Sanders (1968) found less extreme posterior odds in sequential-

sample updating problems, while Beach, Wise, and Barclay (1970) found more extreme posterior odds.

To obtain a more systematic comparison, I extended the meta-analysis sample to incorporate updating problems in which participants were asked to report posteriors after each signal in a sequence. As before, I restrict the sample to problems with equal (initial) priors. Figures 2-4 and Tables 2-3 each have a Panel B, which repeats exactly the analysis from Panel A but applied to these sequential-sample updating problems.

The figures and tables suggest that the same qualitative conclusions from the simultaneous-sample experiments carry over to the sequential-sample experiments: on average, participants' final posteriors are less extreme when updating from larger final sample sizes and more diagnostic rates. These qualitative conclusions also hold in every individual sequential-sample experiment that manipulated sample size N or diagnosticity θ.[41]

The pooling hypothesis, however, predicts that the results should be the same *quantitatively*, but differences between the sequential-sample and simultaneous-sample results are apparent in all the figures and tables. Table 3 Panel B provides another piece of evidence against the pooling hypothesis: the estimated constant term is statistically distinguishable from zero, which suggests that regression equation (4.13) is misspecified for the sequential-sample updating problems. Across sequential-sample and simultaneous-sample experiments with multinomial signals that held constant the final samples observed by participants, Labella and Koehler (2004) found that participants had final posteriors that

[41] For sample size, the experiments are Phillips, Hays, and Edwards (1966), Peterson and Swensson (1968), Sanders (1968), and Kraemer and Weber (2004); for diagnosticity, Phillips and Edwards (1966, Study 1 and 3), Pitz, Downing, and Reinhold (1967), Peterson and Swensson (1968), Sanders (1968), Chinnis and Peterson (1968), and Beach, Wise, and Barclay (1970).

differed in several ways, which is again inconsistent with the pooling hypothesis. While the evidence is not overwhelming, taken all together it casts substantial doubt on pooling.

Stylized Fact 8. In sequential-sample updating problems, people do not "pool" the signals (i.e., update as if they had observed a single, simultaneous sample).

Therefore, I tentatively conclude that people are acceptive, updating after each set of signals they observe—with the caveat that this conclusion has not been interrogated empirically.

Given that people underinfer (Stylized Fact 1) and under-use priors (Stylized Fact 7), one would expect to see that the final posterior odds in sequential-sample experiments are less extreme than Bayesian posterior odds. Indeed, essentially all sequential-sample experiments that I am aware of have found final posterior odds that are less extreme than Bayesian.[42] This is also true on average for the meta-analysis sample, as shown in Figure 2 Panel B and Table 2 Panel B.

The quantitative differences between Panels A and B of the figures and tables are difficult to interpret directly. If people are not pooling (Stylized Fact 8), then even if the *initial* prior odds put equal weight on the two states, people's subsequent prior odds in general will not. Consequently, their posterior odds at the end of a sequential sample reflect the effects of both biased inference and biased use of the priors.

[42] The papers for which this is true are Peterson, Ulehla, Miller, Bourne, and Stilson (1965), Peterson, Schneider, and Miller (1965), Phillips and Edwards (1966), Phillips, Hays, and Edwards (1966), Beach (1968), Chinnis and Peterson (1968), Dale (1968), Peterson and Swensson (1968), Sanders (1968), Beach, Wise, and Barclay (1970), Edenborough (1975), Dave and Wolfe (2003), Kraemer and Weber (2004), and Sasaki and Kawagoe (2007). The one, partial exception is Strub (1969), who finds that while it is true for naïve experimental participants, participants with extensive training update Bayesianly.

To disentangle the two, following Grether (1992), economists typically estimate a panel-data version of equation (4.15) (without the indicators for exact representativeness):

$$\begin{aligned}&\ln\left(\frac{\pi(A\mid s_1,s_2,\ldots,s_t)}{\pi(B\mid s_1,s_2,\ldots,s_t)}\right)\\&=\beta_0+\beta_1\ln\left(\frac{p(s_t\mid A)}{p(s_t\mid B)}\right)+\beta_2\ln\left(\frac{\pi(A\mid s_1,s_2,\ldots,s_{t-1})}{\pi(B\mid s_1,s_2,\ldots,s_{t-1})}\right)\\&+\eta_t,\end{aligned}\tag{4.21}$$

where the initial priors are assumed to be correct ($\frac{\pi(A)}{\pi(B)}=\frac{p(A)}{p(B)}$). This specification implicitly assumes that people are acceptive in grouping signals. As in equation (4.15), $\hat{\beta}_1$ is an estimate of *c* and $\hat{\beta}_2$ is an estimate of *d*.[43]

From the eight papers[44] that have estimated equation (4.21), the range of $\hat{\beta}_1$ is 0.25-1.23, with an inverse-variance-weighted mean of 0.53 (SE = 0.012). Taking this mean as an overall estimate of *c*, it indicates that participants underweight the likelihood ratio. From

[43] Möbius, Niederle, Niehaus, and Rosenblat (2014) pointed out that OLS is not a consistent estimator for equation (4.21) for two reasons: (a) $\ln\left(\frac{\pi(A\mid s_1,s_2,\ldots,s_{t-1})}{\pi(B\mid s_1,s_2,\ldots,s_{t-1})}\right)$ is correlated with *c* and *d* (if there is heterogeneity in *c* and *d* across participants) and (b) $\ln\left(\frac{\pi(A\mid s_1,s_2,\ldots,s_{t-1})}{\pi(B\mid s_1,s_2,\ldots,s_{t-1})}\right)$ has measurement error. In Möbius et al.'s experiment (discussed in more detail in Section 9), participants updated beliefs about their performance on an IQ quiz, and different participants faced different versions of the quiz. Möbius et al. estimated equation (4.21) using IV, using the quiz difficulty as an instrument for $\ln\left(\frac{\pi(A\mid s_1,s_2,\ldots,s_{t-1})}{\pi(B\mid s_1,s_2,\ldots,s_{t-1})}\right)$. Barron (2016) is the only other paper I am aware of that has addressed (a) and (b) and also did so using an IV estimation method. Both Möbius et al. and Barron found that their IV results are similar to their OLS results, but the estimates for equation (4.21) from other papers should interpreted with the caveat that they do not address (a) and (b).

[44] These papers are Grether (1992), Möbius et al. (2007 / 2014), Holt and Smith (2009), Barron (2016), Charness and Dave (2017), Coutts (2017), Gotthard-Real (2017), and Buser, Gerhards, and van der Weele (2018). The analysis yielding the numbers reported in this paragraph are described in the Online Appendix to this chapter. Note that while Charness and Dave is included in the range of coefficients, it isn't included in the calculation of the inverse-variance-weighted mean since standard errors are not reported.

these same papers, the range of $\hat{\beta}_2$ is 0.51-1.88, with an inverse-variance-weighted mean of 0.88 (SE = 0.009). This estimate of *d* is consistent with base-rate neglect. Two other sequential-sample experiments also found evidence of base-rate neglect but did not estimate *d* (Phillips and Edwards, 1966, Experiment 1; Phillips, Hays, and Edwards, 1966).

Stylized Fact 9. In sequential updating problems, people both underinfer and exhibit base-rate neglect.

Several papers have examined updating at the individual-level and have found that, upon receiving a signal, one-third to one-half of participants do not update at all (e.g., Möbius, Niederle, Niehaus, and Rosenblat, 2014; Coutts, 2017; Henckel, Menzies, Moffatt, and Zizzo, 2017). While Coutts concluded that the underweighting of the likelihood ratio is driven by these observations, the other papers found that participants update by less than a Bayesian even when these observations are omitted.

From sequential-sample updating experiments, two other regularities are worth noting. First, several experiments have found a "primacy effect," meaning that signals observed early in the sequence have a greater impact on final beliefs than signals observed in the middle of the sequence (Peterson and DuCharme, 1967; Roby, 1967; Dale, 1968; De Swart and Tonkens, 1977), although DuCharme (1970) did not find a primacy effect.

Stylized Fact 10. In sequential updating problems, signals observed early in the sequence have a greater impact on final beliefs than signals observed in the middle of the sequence.

The primacy effect is predicted by prior-biased updating, as discussed further in Section 8.

Second, several studies have found a "recency effect," meaning that the most recently observed signals have a greater impact on final beliefs than signals observed in the middle of the sequence (e.g., Pitz and Reinhold, 1968; Shanteau, 1970, Study 2; Marks and Clarkson, 1972; Edenborough, 1975; Grether, 1992, Experiment 3).[45]

Stylized Fact 11. In sequential updating problems, the most recently observed signals have a greater impact on final beliefs than signals observed in the middle of the sequence.

The recency effect is predicted by base-rate neglect, as discussed further in Section 6. Both primary and recency effects provide further evidence against the "pooling" hypothesis, and hence constitute additional evidence for Stylized Fact 8.[46]

Section 5. Theories of Biased Inference

Most of the stylized facts outlined in the previous section had already been identified fifty years ago in the psychology literature on bookbag-and-poker-chip

[45] Unfortunately, it seems that there have been no experiments that aimed to identify both primacy and recency effects. Indeed, the typical experiment on "order effects" in this literature compares participants' posteriors after two sequences, one whose first half is mostly *a* signals and whose second half is mostly *b* signals, and another which is the reverse. Such a design can only identify which of the two effects dominates, and indeed, much of the literature has been framed in terms of whether there is a primacy *or* a recency effect.

[46] What is the effect of feedback and training on updating in sequential-sample experiments? Unfortunately, there is only a small amount of evidence, which I judge to be inconclusive. Phillips and Edwards (1966, Study 2) had their experimental participants report posteriors after each signal and told their experimental participants the true state after each of four 20-signal sequences. Posteriors were closer to Bayesian at the end of the fourth sequence than at the end of the first sequence but remained not extreme enough. Strub (1969) ran a sequential-sample updating experiment among a group of naïve participants and a group of trained participants, undergraduates who had received 114 hours of lecture sessions, demonstrations, problem-solving sessions, and other training in dealing with probabilities, including prior participation in bookbag-and-poker-chip experiments. Relative to the naïve participants, the trained participants had final posteriors that were much closer to Bayesian on average across updating problems, but the results are not reported in enough detail to evaluate whether the trained participants had biased beliefs in the updating problems considered separately.

experiments. Much of the work in that literature focused on testing three main theories to explain those regularities. These three theories remain the leading candidate explanations. This section reviews each of these in turn, in light of the current state of evidence and more recent and specific conceptualizations of the theories.[47]

5.A. Biased Sampling-Distribution Beliefs

Since people's sampling-distribution beliefs presumably influence their inferences, it is natural to look to sampling-distribution biases to provide an explanation of people's biased inferences. And indeed, biased sampling-distribution beliefs was a leading theory in the psychology literature on bookbag-and-poker-chip experiments (e.g., Peterson and Beach, 1967; Edwards, 1968; Slovic and Lichtenstein, 1971). Yet the theory was not explored much, and it received little attention in the subsequent literature (e.g., it is not mentioned at all by Fischhoff and Beyth-Marom, 1983).

To discuss the theory formally, suppose an agent updates according to Bayes' Theorem but using her biased sampling-distribution beliefs, $\pi(S \mid A)$ and $\pi(S \mid B)$, in place of the true likelihoods, $p(S \mid A)$ and $p(S \mid B)$.[48] If the agent has no additional biases, then her posterior beliefs will be:

[47] Reflecting these more specific conceptualizations, I will refer to the three theories by different names than were used in the older literature. That literature referred to the "misperception hypothesis," "misaggregation hypothesis," and "response-bias hypothesis." Instead, I call them biased sampling-distribution beliefs, conservatism bias, and extreme-belief aversion, respectively.

[48] As far as I am aware, none of the work on belief updating has taken partition-dependence into account (see Section 3.A). An implicit assumption in what follows is that the agent's posterior beliefs and sampling-distribution beliefs are elicited using the same partition of the state space. Otherwise, partition-dependence would distort these beliefs relative to each other.

$$\pi(A \mid S) = \frac{\pi(S \mid A)p(A)}{\pi(S \mid A)p(A) + \pi(S \mid B)p(B)} \tag{5.1}$$

$$\pi(B \mid S) = \frac{\pi(S \mid B)p(B)}{\pi(S \mid A)p(A) + \pi(S \mid B)p(B)}. \tag{5.2}$$

To see this theory's implications about inferences, it is helpful to rewrite the agent's posterior beliefs in odds form:

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \frac{\pi(S \mid A)}{\pi(S \mid B)} \frac{p(A)}{p(B)}. \tag{5.3}$$

Equation (5.3) predicts underinference whenever $\frac{\pi(S \mid A)}{\pi(S \mid B)}$ is less extreme than the correct likelihood ratio $\frac{p(S \mid A)}{p(S \mid B)}$.

For this theory to be qualitatively consistent with the evidence that people underinfer in general (Stylized Fact 1) and especially so for updating problems with larger sample sizes (Stylized Fact 2), people's sampling-distribution beliefs would have to be too flat relative to the true distributions and especially flat at larger sample sizes. As discussed in Section 3.E and 3.B, people's sampling-distribution beliefs appear to have these features.

Two experiments have directly tested equation (5.3) in the case of equal priors, when it simplifies to $\frac{\pi(A \mid S)}{\pi(B \mid S)} = \frac{\pi(S \mid A)}{\pi(S \mid B)}$. The first is Peterson, DuCharme, and Edwards (1968, Study 2). In stage one of their study, they elicited participants' posteriors beliefs in each of the 57 possible updating problems defined by the binomial parameter values θ = 0.6, 0.7, 0.8 and the three sample sizes N = 3, 5, 8. Stage one replicated the usual findings

of underinference on average and greater underinference with larger N and larger θ. In stage two, Peterson et al. elicited nine binomial sampling distributions, with each of the binomial parameter values $\theta = 0.6, 0.7, 0.8$ and each of the three sample sizes $N = 3, 5, 8$. As previously mentioned in Section 3.E, Peterson et al. found that participants' sampling distributions were nearly correct for $N = 3$ but were flatter than the correct distributions for $N = 5$ and especially for $N = 8$. Peterson et al. tested equation (5.3) by comparing the distributions produced in stage two to the inferences elicited in stage one.[49] When they plotted experimental participants' median log-posterior-odds calculated from stage two, $\ln\left(\frac{\pi(A \mid S)}{\pi(B \mid S)}\right)$, against participants' log-likelihood-odds calculated from stage one, $\ln\left(\frac{\pi(S \mid A)}{\pi(S \mid B)}\right)$, they found that "most points cluster extremely close to the identity line" (p. 242).[50]

The other paper is Wheeler and Beach (1968). Their study also had a sequence of stages. In the first stage, participants reported their beliefs about two binomial sampling distributions, with parameter values $\theta = 0.6$ and 0.8, both with a sample size of $N = 8$. As previously mentioned in Section 3.E, these sampling distributions were too flat. In the

[49] Peterson et al.'s study had two further stages. Stage three was designed to de-bias participants' sampling-distribution beliefs. Stage four repeated stage one, with no sampling distributions visible to the participants. The purpose of stage four was to test whether the de-biasing of participants' sampling-distribution beliefs from stage three also de-biased their inferences. Peterson et al. found that underinference was reduced in stage four relative to stage one, but they do not report participants' sampling-distribution beliefs in stage three. Thus it is not possible to assess the consistency between these beliefs and participants' posteriors in stage four.

[50] There were a few exceptions, which occurred in updating problems where the observed sample was in the far tail of the sampling distribution: 0, 1, 7, or 8 *a*'s out of 8, and 0 or 5 *a*'s out of 5. In these cases, participants inferred more strongly than would be expected given their sampling distributions. Peterson et al. suggested that these exceptions may be driven by participants assigning probabilities many times too high to these very unlikely samples, which have true probabilities smaller than 1%. While participants were allowed to estimate likelihoods smaller than 1%, Peterson et al. noted that doing so was inconvenient in their design. Peterson et al. also noted that the discrepancies could also be due to the fact that estimation errors in very small likelihoods can have a large effect on the likelihood odds.

second stage, participants bet on whether particular samples of size 8 (e.g., 6 *a*'s out of 8) came from an urn where the rate of *a* signals was 0.6 or an urn where the rate was 0.8. The prior probabilities of the two urns were equal. After each of 100 bets, which were incentivized, participants were told which urn was correct. To test equation (5.3), Wheeler and Beach compared the first 20 bets with participants' initial sampling distributions.[51] Their results were similar to Peterson et al.'s: there was a tight correspondence between inferences and sampling-distribution beliefs.

Both of these studies support the theory that people's inferences are consistent with Bayes' Theorem applied to their beliefs about sampling distributions. Both also suggest that the flatness of these distributions may account for the general finding of underinference. However, both had small numbers of participants: only 24 undergraduates in Peterson, DuCharme, and Edwards and 17 in Wheeler and Beach.

There is other, less direct evidence bearing on the biased-sampling-distribution theory of biased inferences. In particular, if the theory were true, then features of people's sampling-distribution beliefs (reviewed in Section 3) would be reflected in their inferences (reviewed in Section 4). I outline three of these possible links in turn, with the caveat that the evidence is thin regarding the latter two features of people's sampling-distribution beliefs.

[51] Like Peterson et al., Wheeler and Beach found that in inference problems with sample realizations in the tails, participants inferred more strongly than would be expected given their sampling distributions. Wheeler and Beach's study had further stages after the initial set of 100 bets: participants' sampling distributions were re-elicited, then they faced another 100 bets, their sampling distributions were elicited one last time, and then they faced a final 20 bets. The purpose of this procedure was to give participants feedback and experience about the sampling distributions. Participants' sampling distributions elicited at the beginning of the study were somewhat too peaked rather than too flat. In addition to testing equation (5.3) with data from the initial bets, Wheeler and Beach also tested it with data from the end of the study, comparing participants' 20 bets with their final sampling distributions, and they again found a tight correspondence.

First, in non-small samples—e.g., a sample size of at least 10—people's subjective sampling distributions are based on the *proportion* of *a* signals rather than the number of *a* signals (see Section 3.B). As Kahneman and Tversky (1972a) pointed out, if people's inferences are based on these distributions, then, for non-small sample sizes, people's inferences will depend on the sample proportion, as they indeed seem to (Stylized Fact 4).

Second, there is some evidence that people's sampling-distribution beliefs overweight the mean (see Section 3.D). If so, people put too much weight on sample proportions matching the population rate. This feature of sampling-distribution beliefs may explain "exact representativeness," the (not entirely robust) evidence that overinference occurs when the sample proportion matches the rate of one of the states (Stylized Fact 5).

Third, there is a bit of evidence that people's sampling-distribution beliefs have flat tails (see Section 3.C). This may be why people underinfer by more when the rates in the two states are further apart (Stylized Fact 6). If (say) the state is *A*, then the most likely samples will have sample proportions close to θ_A. The agent will overestimate the likelihood of these samples in state *B* because the agent's state-*B* sampling distribution has fat tails and will therefore underinfer on average—and this overestimation and consequent underinference will be more severe the further apart are θ_A and θ_B.

Thus, the biased-sampling-distribution theory may be consistent with nearly all of the stylized facts regarding biased inference reviewed in Section 4, with one important exception: the theory almost certainly *cannot* explain why underinference occurs on average in samples of size one (Stylized Facts 3 and 9). In order for the theory to do so, people would have to believe that the probability of an *a* signal in a single draw when the

rate is known to be θ is not equal to θ. As noted in Section 3.E, I am not aware of any direct evidence, but such a result seems implausible.

Several of the biases in people's sampling-distribution beliefs discussed above have not been captured in formal models. However, two features of people's subjective sampling distributions—generally being too flat and being based on the proportion of *a* signals in large samples—are reflected in Benjamin, Rabin, and Raymond's (2016) model of Non-Belief in the Law of Large Numbers (NBLLN). In addition to drawing out the implications of NBLLN for sampling-distribution beliefs, Benjamin et al. explored implications of NBLLN for biased inferences in economic settings, under the assumption of equations (5.1)-(5.2).[52]

[52] Edwards (1968, pp. 34-35) sketched a different model of biased sampling distribution beliefs: $\pi(N_a = n_a \mid A) = \frac{p(N_a = n_a \mid A)^{\varphi}}{\Sigma_{n=1}^{N} p(N_a = n \mid A)^{\varphi}}$, where $\varphi \in [0,1]$, and similarly for beliefs about state *B*. The agent has correct sampling-distribution beliefs if $\varphi = 1$ and uniform-distribution beliefs if $\varphi = 0$, while if $0 < \varphi < 1$, the agent's subjective sampling distribution is flatter than the true distribution, so the agent underinfers on average. Edwards also pointed out that in symmetric updating problems ($\theta_A = 1 - \theta_B$), the denominators in states *A* and *B* are equal, so $\frac{\pi(N_a = n_a \mid A)}{\pi(N_a = n_a \mid B)} = \left(\frac{p(N_a = n_a \mid A)}{p(N_a = n_a \mid B)} \right)^{\varphi}$. Therefore, in symmetric updating problems, the parameter φ is equal to the measure of biased inference *c* in equation (4.6). While Edwards's analysis stopped here, the model could be extended to capture sample-size neglect by replacing the constant φ with a decreasing function of sample size, $\varphi(N)$ with $\varphi(1) = 1$ and $\varphi(N) \to \frac{\breve{\varphi}}{N} > 0$ for *N* large, where $\breve{\varphi}$ is a constant. Because $\varphi(1) = 1$, the agent's sampling-distribution beliefs are correct when $N = 1$. In this model, for a large sample of binomial signals, it can be shown that the agent's subjective sampling distribution over sample proportions, $\pi\left(\frac{N_a}{N} = x \mid A \right)$, converges to a doubly-truncated normal distribution, $\frac{\phi(x)}{\Phi(1) - \Phi(0)}$, where ϕ is the pdf of a normal distribution with mean θ_A and variance $\frac{\theta_A(1-\theta_A)}{\breve{\varphi}}$, Φ is its cdf, and the distribution is truncated at 0 and 1. In this formula for the "universal distribution," the parameter $\breve{\varphi}$ enters the variance the way a sample size would, so it can be interpreted as the universal distribution's "effective sample size." Relative to Benjamin, Rabin, and Raymond's model of NBLLN, this model has several disadvantages: it is less tractable for some purposes because the mean of the large-sample distribution of proportions is not equal to θ_A (it is biased toward 0.5), and it is more difficult to combine with models of biased beliefs about random sequences.

In their model, signals are drawn i.i.d. from a binomial distribution whose rate of *a* signals is θ. The agent correctly understands that the probability of a single signal being *a* is θ, but her subjective sampling distribution is biased for sample sizes *N* larger than one. The Law of Large Numbers implies that, as $N \to \infty$, the true sampling distribution over the proportion of *a* signals, N_a / N, converges to a point mass at θ. As explained in Section 3.B, the agent instead believes that the sampling distribution of N_a / N converges to a "universal distribution" that has mean θ but full support on (0,1). Thus, the agent's subjective sampling distribution for large samples is very flat relative to the true sampling distribution.

When combined with equations (5.1)-(5.2), the model's basic implications for inference are straightforward. Let the agent's universal distributions for binomials with rates θ_A and θ_B be denoted $\pi_\infty\left(\frac{N_a}{N} \mid A\right)$ and $\pi_\infty\left(\frac{N_a}{N} \mid B\right)$, respectively. From equation (5.3), the agent's posterior odds after observing a large sample containing N_a *a*-signals will be

$$\frac{\pi(A \mid N_a \text{ out of } N)}{\pi(B \mid N_a \text{ out of } N)} = \frac{\pi_\infty\left(\frac{N_a}{N} \mid A\right)}{\pi_\infty\left(\frac{N_a}{N} \mid B\right)} \frac{p(A)}{p(B)} \tag{5.4}$$

Equation (5.4) formalizes two of the links between sampling-distribution beliefs and biased inferences that have already been noted above. First, since the universal distributions are based on sample proportions, so are the agent's inferences in a large

sample. Second, in large samples, because the agent's subjective sampling distribution is too flat, the agent underinfers. Furthermore, while the Bayesian will learn the true state with certainty in an infinite sample, the agent will remain uncertain even after observing an infinite sample. For example, if the true state is *A*, then (due to the Law of Large Numbers) the sample proportion will converge to the state-*A* rate θ_A with probability one. The agent's likelihood ratio in equation (5.4) will therefore converge to $\frac{\pi_\infty\left(\frac{N_a}{N}=\theta_A \mid A\right)}{\pi_\infty\left(\frac{N_a}{N}=\theta_A \mid B\right)}$, which is the ratio of the pdfs of the universal distributions, evaluated at θ_A. Since this likelihood ratio is a finite number, the agent's inference is limited.

Because the likelihood ratio is finite, it is clear from equation (5.4) that the agent's priors will continue to matter no matter how large a sample the agent observes. For this reason, Benjamin, Rabin, and Raymond argue that NBLLN can serve as an "enabling bias" for misbeliefs people have about themselves. In particular, if people have overoptimistic priors about their own abilities or preferences (for reasons unrelated to NBLLN), NBLLN may explain why they remain overoptimistic despite a lifetime of experience.

Benjamin, Rabin, and Raymond also explored the implications of NBLLN for people's demand for information. What is crucial for demand for information is what the agent *expects* to infer. While a Bayesian's expectations about his own inferences are correct, an agent with NBLLN has incorrect expectations because she has mistaken beliefs about the distribution of samples she will observe. Surprisingly, these mistaken beliefs can cause the agent to have *greater* willingness to pay for an intermediate-sized sample than a Bayesian would have. In particular, because the agent's subjective sampling distribution is

too flat, she thinks an extreme proportion of *a* signals that would be very informative about the state is more likely than it is. The agent may be willing to pay for the sample in the hope of such an extreme sample realization, even though a Bayesian would recognize that such an outcome is too unlikely to be worth paying for.

For a large sample, however, the agent anticipates drawing a weaker inference than a Bayesian would draw for any possible realization of the sample proportion (because a Bayesian will learn the truth in a large enough sample, while the agent's inference will be limited). Therefore, an agent with NBLLN always has lower willingness to pay for a large sample than a Bayesian would have. Benjamin, Rabin, and Raymond argue that this lack of demand for large samples is a central implication of NBLLN, which may contribute to explaining why statistical data is rarely provided by the market, as well as why people often rely on anecdotes rather than seeking larger samples.

For drawing out the implications of biased inferences when samples are observed sequentially, a crucial issue is how people group signals, as discussed in Section 4.C. For an agent with NBLLN, it makes all the difference whether 100 signals are grouped as a single sample, in which case she dramatically underinfers, or as 100 samples of size one, in which case she updates correctly after each signal and ends up with the same posteriors as a Bayesian!

As per Stylized Fact 8, there is evidence against the hypothesis that people "pool" all signals they have observed into a single large sample. It may therefore be reasonable to hypothesize that people are "acceptive" of the way signals are presented to them, processing signals as a sample when the signals are presented together. This evidence, however, relates only to how signals are grouped *retrospectively*, after they are observed.

The implications of NBLLN in many dynamic environments also depends on how the agent expects to group signals she hasn't yet observed. There is no necessary reason why people would *prospectively* group signals the same way they retrospectively group signals. Differences between retrospective and prospective grouping can generate dynamically inconsistent behavior. An important lesson that emerges from formally modeling NBLLN is the need for evidence on how people group signals both retrospectively and prospectively.

5.B. Conservatism Bias

The theory of biased inference that received by far the most attention in the literature on bookbag-and-poker-chip experiments is conservatism bias: when updating to posterior beliefs, people underweight their likelihood beliefs. Phillips and Edwards (1966) introduced conservatism bias and modeled it as

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \left[\frac{p(S \mid A)}{p(S \mid B)} \right]^{c} \frac{p(A)}{p(B)}. \tag{5.5}$$

Formally, this equation is the special case of equation (4.6) in which biased use of prior information is abstracted away ($d = 1$). Conceptually, however, there is a key difference: whereas equation (4.6) is intended as a reduced-form description used to summarize evidence from updating problems, conservatism bias is a structural model of the actual process of forming beliefs. Psychologically, conservatism bias is hypothesized to result from the difficulty of aggregating different sources of information (e.g., Slovic and Lichtenstein, 1971).

In a comparison with other theories of biased inference, Edwards (1968) cites three pieces of evidence in favor of conservatism bias. First, in several sequential-sample updating experiments conducted with symmetric binomial signals, estimates of the conservatism parameter *c* were found to be roughly independent of the numbers of *a* and *b* signals that occurred in a sample (Phillips and Edwards, 1966, Experiments 1 and 3; Peterson, Schneider, and Miller, 1965, as reported by Edwards, 1968; also in a multinomial-signal experiment: Shanteau, 1972).[53] This stability of estimates of *c* supported its interpretation as a structural parameter, and it is a challenging fact for alternative theories to explain.[54] It should be noted, however, that estimates of *c* were known to be smaller when the diagnosticity parameter θ was larger (Phillips and Edwards, 1966) and, in sequential-sample experiments, when the sample size *N* was larger (Peterson, Schneider, and Miller, 1965), as per Stylized Facts 6 and 2. While there was no clear explanation for the dependence on θ, greater conservatism for larger sample sizes had a ready interpretation: aggregating more information is more difficult.

Second, in settings where participants themselves provide the likelihood estimates, participants nonetheless update too little relative to Bayes' Theorem (e.g., Hammond, Kelly, Schneider, and Vancini, 1967; Grinnell, Keeley, and Doherty, 1971). For example, in some experiments, participants were asked to estimate the likelihood of different signals (e.g., reconnaissance reports) in different states of the world (e.g., impending war), and

[53] At first blush, this observation seems inconsistent with "exact representativeness"—stronger inferences when the observed sample proportion equals the rate in one of the states—as per Stylized Fact 5. However, the estimates of the conservatism parameter *c* presented in these papers were averaged across sample sizes, so it is not possible to assess whether or not there was evidence of exact representativeness.

[54] The model of sampling-distribution beliefs sketched in footnote 52 *does* imply that, in symmetric binomial updating problems, the measure of biased inference *c* is independent of the numbers of *a* and *b* signals in the sample. For Edwards (1968), explaining that observation was an important desideratum for evaluating a theory of underinference.

then they observed certain signals and were asked to update their beliefs (e.g., Edwards, Phillips, Hays, Goodman, 1968). In such an experiment, participants' biased posteriors cannot be attributed to biased sampling-distribution beliefs because the perceived likelihoods are elicited directly. This evidence, however, is not sufficient to conclude that participants update too little relative to Bayes' Theorem. One concern is that, while participants provide point estimates of the likelihoods, participants may in fact be uncertain about the likelihoods or report their point estimates with error. In either case, Bayes' Theorem applied to participants' point estimates is the wrong benchmark for comparing with participants' posteriors. Another concern is that Bayes' Theorem was calculated assuming that the signals are independent conditional on the states, but participants' beliefs about the likelihoods were typically not elicited in sufficient detail to test that assumption.

Third, people underinfer for sample sizes of one (Stylized Facts 3 and 9). This observation can be explained by conservatism bias, while as discussed above, it is a challenging observation to explain by biased sampling-distribution beliefs. Extreme-belief aversion, discussed next, is a competing explanation for this observation.

5.C. Extreme-Belief Aversion

Extreme-belief aversion is the term used by Benjamin, Rabin, and Raymond (2016, Appendix C) to refer to an aversion to holding or expressing beliefs close to certainty.[55] As a simple example, suppose there are two possible states, A and B, and the true probability of A is p. An agent with extreme-belief aversion would report that the probability of A is $\pi = f(p)$, where $f(p) > p$ for p sufficiently close to 0 and $f(p) < p$ for p sufficiently

[55] The more general term "extremeness aversion" is sometimes used to refer to a desire to avoid both extreme judgments and extreme choices (e.g., Lewis, Gaertig, and Simmons, 2018).

close to 1. Note that extreme-belief aversion is not specifically a theory of biased inference but rather a theory about bias in *any beliefs*.[56]

DuCharme (1970) argued that extreme-belief aversion is a major confound in belief-updating experiments that explains much of the evidence that had been interpreted as underinference. In support of this view, DuCharme reported two experiments. Both were sequential-sample bookbag-and-poker-chip experiments with two states and normally distributed signals. Using the results of each experiment, DuCharme produced a plot like Figure 2, graphing participants' log posterior odds ($\ln\left(\frac{\pi}{1-\pi}\right)$) against the Bayesian log posterior odds ($\ln\left(\frac{p}{1-p}\right)$). Both experiments resulted in similar plots: for Bayesian odds between -1 and +1, participants' odds virtually coincided with the Bayesian odds, but for Bayesian odds more extreme than -1 or +1, participants' odds were less extreme than the Bayesian odds. The plot was similar whether or not the data was restricted to the posteriors reported by participants after just a single signal had been observed. In an earlier paper that also reported two experiments with normally distributed signals, DuCharme and Peterson (1968) had found similar results. The results of these experiments are difficult to reconcile with conservatism bias but consistent with extreme-belief aversion.

[56] Extreme-belief aversion resembles probability weighting but is conceptually distinct. Probability weighting is a bias in how beliefs are *used* in decision making (rather than a bias in how they are formed or reported); it is discussed in Chapter XXX (by O'Donoghue and Sprenger) of this Handbook. Extreme-belief aversion is also distinct from an aversion to reporting a response at the extremes of the response scale, a bias that is sometimes called floor and ceiling effects. Such floor and ceiling effects have been documented in bookbag-and-poker-chip experiments. For example, experimental participants report less extreme beliefs when reporting their beliefs as probabilities, which are bounded between zero and one, than when reporting their beliefs as odds or log-odds, which have a response scale that is unbounded on at least one end (Phillips and Edwards, 1966, Experiment III). However, floor and ceiling effects seem unlikely to account for DuCharme and Peterson's (1968) evidence mentioned below because they elicited respondents' posterior odds, so the response scale did not have a floor or ceiling.

Extreme-belief aversion is a distortion toward less extreme posteriors that does not depend on whether the correct posteriors are extreme due to an extreme likelihood or an extreme prior. Thus, extreme-belief aversion is a confound not only for findings that have been interpreted as underinference (Stylized Fact 1) but also those that have been interpreted as base-rate neglect (Stylized Fact 7). Moreover, the extreme-belief aversion explanation of these findings applies equally to sequential-sample and simultaneous-sample experiments (Stylized Fact 9) and to samples of any size. In particular, extreme-belief aversion provides an explanation for the apparent evidence of underinference after just a single signal (Stylized Fact 3), a fact that biased sampling-distribution beliefs cannot explain.

Based on the particular shape of extreme-belief aversion observed in his plots of the results mentioned above, DuCharme (1970) argued that the bias can also explain the evidence that has been interpreted as underinference being more severe on average when sample sizes are larger (Stylized Fact 2) and when the population rates θ_A and θ_B are further apart (Stylized Fact 6).[57] These stylized facts are based on measuring the amount of underinference by c, which is equal to $\ln\left(\frac{\pi}{1-\pi}\right) / \ln\left(\frac{p}{1-p}\right)$ in updating problems with equal priors (see Section 4.A and the discussion of Figure 2 in Section 4.B). DuCharme's plots imply that $c \approx 1$ when the Bayesian log posterior odds are within the interval [-1,+1], but $c < 1$ when the Bayesian odds are more extreme. Both larger sample sizes and

[57] These stylized facts are not implied by the general definition of extreme-belief aversion given above. For example, an extreme-belief averse agent could have posterior beliefs such that $\ln\left(\frac{\pi}{1-\pi}\right) = \chi \ln\left(\frac{p}{1-p}\right)$ for some constant $\chi \in (0,1)$. In that case, the measure of underinference c would be equal to χ regardless of how extreme the Bayesian posterior odds $\ln\left(\frac{p}{1-p}\right)$ are. If extreme-belief aversion took this form, then DuCharme's plot would have been a line through zero with slope χ.

population rates that are further apart make the expected Bayesian odds more extreme, leading in expectation to more severe underinference as measured by *c*.

The theory of extreme-belief aversion has not been developed in much detail. It is worth exploring whether extreme-belief aversion is actually the same phenomenon as compression of probabilities toward a uniform distribution, discussed in Section 3.A in the context of partition dependence. Such compression would indeed lead people to avoid reporting beliefs close to certainty. Whether or not the two biases are the same, they raise similar conceptual challenges.

Extreme-belief aversion probably contributes to biased updating, and it is a certainly a confound that should be taken into account when interpreting the evidence from updating experiments. However, Benjamin, Rabin, and Raymond (2016, Appendix C) argued that the stylized facts discussed in Section 4 cannot be entirely attributed to extreme-belief aversion. If extreme-belief aversion were the only bias at play, then experimental participants' reported posteriors (π) would be a fixed transformation of the correct posteriors ($\pi = f(p)$). That implies that in any two problems where the correct posteriors are the same, experimental participants' reported posteriors would also be the same. There are several clean test cases that contradict this prediction. For example, consider four of the updating problems in Griffin and Tversky's (1992, Study 1) belief-updating experiment (described in Section 4.B): 3 out of 3 *a* signals, 4 out of 5 *a* signals, 6 out of 9 *a* signals, and 10 out of 17 *a* signals. Because the difference between the number of *a* and *b* signals is always 3, the correct posterior is the same in all four problems. Experimental participants' median posteriors, however, were less extreme in the problems with larger sample sizes. Other, similar examples from Griffin and Tversky (1992)'s Study

1 and Kraemer and Weber (2004) also provide evidence of Stylized Fact 2 that is unconfounded by extreme-belief aversion.

Analogously, there are examples from Griffin and Tversky's (1992) Study 2 where the correct posteriors are the same across updating problems with different prior probabilities. Consistent with base-rate neglect (Stylized Fact 7), participants' posteriors were less extreme in the problems with more extreme priors. Similarly, Griffin and Tversky's (1992) Study 3 and Kahneman and Tversky (1972a) provide evidence, unconfounded by extreme-belief aversion, that participants underinfer when the rates θ_A and θ_B are further apart (Stylized Fact 6) and draw inferences based on sample proportions (Stylized Fact 4).

5.D. Summary

There is evidence for all three of the theories reviewed in this section: biased sampling-distribution beliefs, conservatism bias, and extreme-belief aversion. Biases in sampling-distribution beliefs are a natural starting point and may explain many of the stylized facts about biased inference from Section 4. To date, however, formal models of people's sampling-distribution beliefs capture only some of the relevant biases. In my judgment, this theory is particularly ripe for theoretical development and application in field settings. Evidence on how people group signals is needed in order to understand how the biases play out in dynamic settings.

Biased sampling-distribution beliefs seem unlikely to explain why people underinfer from single signals, whereas extreme-belief aversion and conservatism bias

could explain that evidence. In my view, experiments designed to disentangle the theories from each other and assess their relative magnitudes should be a priority.

Section 6. Base-Rate Neglect

The evidence reviewed in Section 4 indicates that in updating problems, with or without incentives for accuracy, people on average under-use prior information (Stylized Facts 7 and 9). This phenomenon was apparent from the psychology literature on bookbag-and-poker-chips—indeed, it was documented by Phillips and Edwards (1966, Experiment 1) in one of the first such experiments—but it was largely ignored in that literature. Kahneman and Tversky (1973) made this bias a focus of attention in the literature on errors in probabilistic reasoning and labeled it base-rate neglect.

Among various other surveys and experiments, Kahneman and Tversky (1973) presented an elegant demonstration of base-rate neglect and its properties. They asked experimental participants to assign a probability to the event that Jack is an engineer rather than a lawyer based on the following description:

> Jack is a 45 year old man. He is married and has four children. He is generally conservative, careful, and ambitious. He shows no interest in political and social issues and spends most of his free time on his many hobbies which include home carpentry, sailing, and mathematical puzzles.

There were two groups of participants. Both were provided the same description, but one group was told that it was randomly drawn from a set of 100 descriptions consisting of 30 engineers and 70 lawyers, while the other was told that the set included 70 engineers and 30 lawyers. Although we do not know participants' assessment of the likelihood ratio based on the description, $\frac{\pi(S \mid A)}{\pi(S \mid B)}$, Bayes' Theorem (equation (4.3)) implies that the first group of

subjects should have posterior odds $\frac{\pi(S \mid A)}{\pi(S \mid B)}\frac{0.70}{0.30}$, and the second group should have posterior odds $\frac{\pi(S \mid A)}{\pi(S \mid B)}\frac{0.30}{0.70}$. Bayes' Theorem therefore allows us to make an unambiguous prediction about the *ratio* of the posterior odds across the two groups: it should be $\frac{0.70/0.30}{0.30/0.70}$ ≈ 5.4. Contrary to this, the first group's mean probability that Jack is an engineer (averaged across this description and four similar others) was 55%, and the second group's was 50%, yielding a ratio of only $\frac{0.55/0.45}{0.50/0.50} \approx 1.2$. Thus, manipulation of the base rates had less of an effect on the posterior probabilities than would be prescribed by Bayes' Theorem. Such base-rate neglect in response to the description of Jack has been replicated many times, but whereas Kahneman and Tversky found complete neglect of base rates, in some other experiments, participants' posteriors reflected the base rates to some extent but less than they should according to Bayes' Rule (Koehler's (1996) Table 1).

To provide some insight when base-rate neglect occurs, Kahneman and Tversky then conducted two more versions of the same experiment. In one version, participants were given "no information whatsoever about a person chosen at random from the sample." In that case, participants reported probabilities that were equal to the base rates. Thus, in the absence of updating, participants understood the base rates correctly, and no base-rate neglect occurred.

In the other version of the experiment, participants were given the following description, which was intended to be completely uninformative regarding whether the person is a lawyer or engineer:

> Dick is a 30 year old man. He is married with no children. A man of high ability and high motivation, he promises to be quite successful in his field. He is well liked by his colleagues.

In this case, in both the 70%-30% and the 30%-70% groups, the median probability assigned to Dick being an engineer was 50%—implying complete base-rate neglect in this case. Participants relied on the description to make their judgment, even though the description was uninformative. Some subsequent experiments have also found base-rate neglect in response to uninformative descriptions (Wells and Harvey, 1978; Ginosar and Trope, 1987), but others instead found that participants' posteriors were equal to their priors (Swieringa et al., 1976; Ginosar and Trope, 1980; Fischhoff and Bar-Hillel, 1984; Hamilton, 1984). Manipulating the instructions, participant pool, and implementation of the experiment, Zukier and Pepitone (1984) and Gigerenzer, Hell, and Blank (1988) found that base-rate neglect sometimes occurs in response to uninformative descriptions and sometimes does not. Overall, the evidence for base-rate neglect in response to an uninformative description is much less robust than that for an informative description, but when there is an effect, it goes in the direction of base-rate neglect.

Taken together, the results of the no-description and the uninformative-description versions of the experiment have an important implication: base-rate neglect is triggered by *updating* the prior with information from a new signal. For a base-rate neglecter, there is a distinction between receiving no signal, in which case no updating occurs, and receiving an uninformative signal, which may cause updating and hence base-rate neglect to occur. For a Bayesian agent, in contrast, there would be no difference across these two cases.

To explain their results, Kahneman and Tversky (1973) argued that people judge probabilities based on the "representativeness" of the personality sketch to a lawyer or engineer, whereas the base rates are not relevant to judgments of representativeness (see Section 7.A for further discussion). Nisbett, Borgida, Crandall, and Reed (1976) suggested another psychological mechanism: the likelihood information is weighted more heavily because it is "vivid, salient, and concrete," whereas the base rates are "remote, pallid, and abstract." Bar-Hillel (1980) argued that base-rate neglect is more general than either of these explanations would predict. She documented base-rate neglect in a sequence of updating problems that specified both the base rates and the likelihoods as (abstract) statistics. For example, one of the most famous problems is the Cab Problem (originally due to Kahneman and Tversky, 1972b):

> Two cab companies operate in a given city, the Blue and the Green (according to the color of cab they run). Eighty-five percent of the cabs in the city are Blue, and the remaining 15% are Green.
>
> A cab was involved in a hit-and-run accident at night.
>
> A witness later identified the cab as a Green cab.
>
> The court tested the witness' ability to distinguish between Blue and Green cabs under nighttime visibility conditions. It found that the witness was able to identify each color correctly about 80% of the time, but confused it with the other color about 20% of the time.
>
> What do you think are the chances that the errant cab was indeed Green, as the witness claimed?

In this problem, the correct answer is $\frac{(0.8)(0.15)}{(0.8)(0.15)+(0.2)(0.85)} \approx 41\%$. Bar-Hillel (1980) found that only about 10% of her high school graduate respondents gave a response to this question close to the correct answer. The modal answer, which was given by 36% of respondents, was 80%. This answer reflects complete base-rate neglect. The same basic result has been replicated many times, including with much more extreme base rates (e.g., 99% Blue and 1% Green; Murray, Idling, Farris, and Revlin, 1987).

Bar-Hillel argued that the key variable underlying base-rate neglect is relevance: when the base rate is the only relevant information available, people use it; when other information is also relevant, people prioritize the information in order of relevance. Thus, people use the base rates more when they seem more relevant to the particular instance in the updating problem. Base-rate neglect in the Cab Problem could be explained by the observation that many participants believed the color distribution of cabs was irrelevant, as documented by informal interviews with respondents and experimental evidence from Lyon and Slovic (1976). Relevance, in turn, is influenced by specificity: when people have information about some population (e.g., 15% of cabs are Green) but also have information about a subset of that population (e.g., a witness identified that particular cab as Green), the latter seems more relevant for making a judgment about a member of the subset (e.g., the chances that the errant cab was Green). Specificity can also be achieved via a causal relationship. For example, in a variant of the Cab Problem, Tversky and Kahneman (1980) described the base rates by telling respondents "85% of the cab accidents in the city involve [blue] cabs," implying that blue cabs cause more accidents than green cabs. In this causal framing, they found far lower rates of base-rate neglect. In a sequence of updating

problems, Bar-Hillel manipulated the relevance of the base rates in different ways and showed that the degree of base-rate neglect varied accordingly. For example, base-rate neglect was largely eliminated in a variant of the Cab Problem where the specificity of the likelihood information was reduced to be comparable to that of the base rate (the witness did not see the cab but remembers hearing an intercom, which are installed in 80% of the Green cabs and 20% of the Blue cabs).

Much subsequent research on base-rate neglect has used updating problems like the Cab Problem, which specify both the prior probabilities and the likelihoods and are contextualized in hypothetical, realistic scenarios. Two examples from the economics literature are Dohmen et al. (2009), who documented widespread base-rate neglect in a representative sample of 988 Germans, and Ganguly, Kagel and Moser (2000), who found base-rate neglect in market experiments with financial incentives. While Bar-Hillel's (1980) and some other results show a high frequency of complete base-rate neglect, most of the evidence is less extreme and instead indicates that people's inferences usually do incorporate base rates to some extent, albeit less fully than prescribed by Bayes' Rule (Koehler, 1996). The evidence from bookbag-and-poker-chip experiments reviewed in Section 4 similarly points to underweighting of priors, rather than complete neglect (at least on average).

Troutman and Shanteau (1977) conducted two sequential-sample bookbag-and-poker-chip experiments whose results further suggest that it is the act of updating that triggers base-rate neglect. Beads were drawn with replacement from one of two boxes with equal prior probabilities. In one of the experiments, Box *A* contained 70/30/50 red/white/blue beads, and box *B* contained 30/70/50. To give a flavor of the results, after

an initial sample of two white beads, experimental participants' mean probability assigned to box *A* was 69.9%. The experimenter then drew a "null sample," consisting of no beads at all, and asked participants to update their beliefs. Participants' mean probability declined to 66.9% (SE of the change = 1.0%). To show that participants understood the lack of information contained in the null sample, Troutman and Shanteau presented another sequence in which the null sample occurred first. In that case, participants' mean probability was 50%.[58]

Much of the literature has focused on factors that increase or reduce the extent of base-rate neglect (for reviews, see Koehler, 1996, and Barbey and Sloman, 2007). For example, based on a literature review and two experiments, Goodie and Fantino (1999) concluded that base-rate neglect can be reduced but nonetheless persists even after extensive training with explicit feedback. Many papers have focused on the effect of framing the updating problem in terms of frequencies versus probabilities. After reviewing this literature, Barbey and Sloman (2007) conclude that frequency formats weaken base-rate neglect but do not eliminate it.

[58] Troutman and Shanteau also found in both experiments that when participants observed an "irrelevant sample" of all blue beads or a "mixed" sample of one red and one white bead—both are which are uninformative regarding *A* versus *B*—participants' posterior probability of Box *A* was similarly moderated toward 50%, and these effects were larger than that of the null sample. Across several sequential-sample experiments modeled on Troutman and Shanteau's, Labella and Koehler (2004) did not replicate this result, finding instead that participants' posteriors were unaffected by an irrelevant sample and became *more extreme* after a mixed sample. (Labella and Koehler did not study the effect of a "null sample.") However, in a simultaneous-sample version of their experiment, Labella and Koehler did find that participants' posteriors were weaker when an additional, mixed set of signals was included in the sample. The "null sample" result is a cleaner test of whether base-rate neglect is triggered by updating because observing an irrelevant or mixed sample could affect beliefs for two additional reasons discussed in this chapter. First, it may moderate beliefs if inferences are drawn based on the sample proportion (Stylized Fact 4), including in sequential samples if inferences are based on the pooled sample. Second, it could make beliefs more extreme due to prior-biased updating (Section 8) (which is indeed how Labella and Koehler interpreted their finding of more extreme beliefs after a mixed sample).

Researchers have discussed the pervasiveness of base-rate neglect in a variety of field settings, including psychologists' interpretations of diagnostic tests (Meehl and Rosen, 1955), courts' judgments in trials (Tribe, 1971), and doctors' diagnoses of patients (Eddy, 1982). In two experiments, Eide (2011) found that law students exhibit a similar degree of base-rate neglect in the Cab Problem as the usual undergraduate samples. In experiments with realistic hypothetical scenarios, school psychologists were found to be more confident but less accurate in assessing learning disability when base-rate information was supplemented with individuating information (Kennedy, Willis, and Faust, 1997).

Benjamin, Bodoh-Creed, and Rabin (2018) analyzed the implications of a formal model of base-rate neglect:

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \frac{p(S \mid A)}{p(S \mid B)} \left[\frac{p(A)}{p(B)} \right]^{d}. \tag{6.1}$$

with $0 < d < 1$. Equation (6.1) is the special case of equation (4.6) in which biased inferences are abstracted away ($c = 1$). However, unlike equation (4.6), equation (6.1) is treated as a structural model of the belief-updating process. In this model, neglect of base rates (i.e., population frequencies), as in the evidence discussed above, is treated as a special case of underweighting priors in general. As per the evidence from Kahneman and Tversky (1973) discussed above, it is assumed that the agent updates whenever a signal is observed, even if the signal is uninformative.

A number of implications follow directly from equation (6.1). First, whereas a Bayesian treats all signals symmetrically, a base-rate neglecter is affected more by recent

than less recent signals. To see this, note that the base-rate neglecter's posterior odds after one signal are

$$\frac{\pi(A \mid s_1)}{\pi(B \mid s_1)} = \frac{p(s_1 \mid A)}{p(s_1 \mid B)}\left[\frac{p(A)}{p(B)}\right]^d;$$

and after two signals,

$$\frac{\pi(A \mid s_1, s_2)}{\pi(B \mid s_1, s_2)} = \frac{p(s_2 \mid A)}{p(s_2 \mid B)}\left[\frac{p(s_1 \mid A)}{p(s_1 \mid B)}\right]^d\left[\frac{p(A)}{p(B)}\right]^{d^2}. \tag{6.2}$$

Because the older signal becomes part of the prior when the new signal arrives, the older signal is down-weighted twice, whereas the new signal is down-weighted only once. Thus, base-rate neglect provides an explanation of the "recency effects" observed in the bookbag-and-poker-chip experiments (Stylized Fact 11). As discussed below, in economic settings, these recency effects can generate adaptive expectations and extrapolative beliefs.

Second, the base-rate neglecter's long-run beliefs fluctuate in accordance with an ergodic (stationary long-run) distribution. Iterating the derivation of equation (6.2) and taking the logarithm, the agent's log posterior odds after observing t signals are $\sum_{\tau=0}^{t} d^{t-\tau} l_\tau$, where $l_\tau \equiv \frac{p(s_\tau \mid A)}{p(s_\tau \mid B)}$ denotes the log likelihood of the τ^{th} signal for $\tau > 0$ and $l_0 \equiv \frac{\pi(A)}{\pi(B)}$ denotes the log prior odds. Since this sum is an AR(1) process, it converges in the limit $t \to \infty$ to an ergodic distribution, as long as the l_τ's are bounded. Thus, while a Bayesian

will eventually identify the true state with certainty, a base-rate neglecter will never become fully confident, and her beliefs will forever fluctuate even if the environment is fundamentally stationary.

This in turn implies that in settings where the agent observes many signals, base-rate neglect will cause her to ultimately become underconfident about the state. Such underconfidence contrasts with the impression one might get from examples like the Cab Problem, where base-rate neglect causes people to be overly swayed by a signal indicative of an event that is unlikely given the base rates. While base-rate neglect can cause people to "jump to conclusions" after a single signal that goes in the opposite direction of the base rates—as in almost all of the updating problems used to study base-rate neglect—in the long run it is a force for persistent uncertainty (see Section 10.A for related discussion).

Third and finally, equation (6.1) has a counterintuitive implication that Benjamin, Bodoh-Creed, and Rabin call the "moderation effect": when the agent's prior in favor of a state is sufficiently strong, a supportive signal can *dampen* the agent's belief about the state! The moderation effect occurs because the new signal has less of an impact on the agent's posterior than down-weighting the prior. Although surprising, there is evidence of the moderation effect in existing data. For example, consider the updating problems in Griffin and Tversky's (1992) Study 2, where the rate of *a* signals was 0.6 in state *A* and 0.4 in state *B*, participants were informed about a sample of size 10, and the prior probability of state *A* was 90%. When the sample contained 5 *a*'s, participants reported a median posterior of 60%; when 6 *a*'s, 70%; and when 7 *a*'s, 85%. In all of these cases, the participants' posterior belief was *lower* than the prior of 90%, consistent with a moderation effect. (Their posterior belief exceeded 90% only when the sample had at least 8 *a's*.)

Benjamin, Bodoh-Creed, and Rabin drew out the implications of base-rate neglect in settings of persuasion, reputation-building, and expectations formation. The results are particularly straightforward in a simple expectations formation setting. Suppose the agent is forming expectations about some parameter θ, say, the expected return of some asset. The agent's current prior is normally distributed, $N\left(\theta_0, \frac{1}{\nu_0}\right)$, with some mean θ_0 and precision ν_0. The agent then updates her beliefs after observing a noisy signal of θ drawn from a normal distribution, $x \sim N\left(\theta, \frac{1}{\nu_x}\right)$, with precision ν_x. As is well known, a Bayesian's posterior would be normally distributed and centered around a precision-weighted mean of the prior mean θ_0 and the signal *x*:

$$E\left[\theta \mid x\right] = \frac{\nu_0}{\nu_0 + \nu_x}\theta_0 + \frac{\nu_x}{\nu_0 + \nu_x}x = \theta_0 + \left(\frac{\nu_x}{\nu_0 + \nu_x}\right)(x - \theta_0).$$

The base-rate neglecter's posterior also turns out to be normally distributed, but centered around a different mean:

$$\begin{aligned} E_{\text{BRN}}\left[\theta \mid x\right] &= \frac{\nu_0 / d^2}{\nu_0 / d^2 + \nu_x}\theta_0 + \frac{\nu_x}{\nu_0 / d^2 + \nu_x}x \\ &= \theta_0 + \left(\frac{\nu_x}{\nu_0 / d^2 + \nu_x}\right)(x - \theta_0). \end{aligned} \tag{6.3}$$

Because base-rate neglect causes the agent to treat the prior as less informative than it is, the base-rate neglecter updates as if the precision of the prior distribution is shrunken by a factor of d^2. Consequently, as shown in equation (6.3), the agent's expectations are overly influenced by the recently observed signal. Such expectations can generate extrapolative beliefs, in which the agent over-extrapolates from recent returns when predicting future returns. As discussed in Chapter XXX (by Barberis) of this Handbook, extrapolative beliefs are an important ingredient in explaining a variety of puzzles in finance.

When studying the implications of base-rate neglect in field settings, a crucial issue is how people group signals (as previously discussed in Sections 4.C and 5.A). Benjamin, Bodoh-Creed, and Rabin make the plausible assumption that beliefs are updated after each new signal is observed, but there are other possibilities. For example, all previously observed signals could be pooled together into a single sample. If the agent then updates using her original priors and the pooled sample, then earlier signals would *not* be down-weighted more than recent signals. While Stylized Fact 8 summarizes evidence against such pooling, the evidence is relatively thin. In settings where the agent's future beliefs are relevant, it also matters whether or not the agent believes she will exhibit base-rate neglect and how she anticipates grouping signals she may receive in the future. There is no evidence on these issues.

Section 7. The Representativeness Heuristic

In his Nobel lecture, Daniel Kahneman (2002) recollected how his collaboration with Amos Tversky began when he invited Tversky to give a guest lecture in his graduate psychology course at Hebrew University in 1968-1969. Tversky, whom as a Ph.D. student had been mentored by Ward Edwards, lectured about conservatism bias. Kahneman was deeply skeptical for a number of reasons, including the everyday experience that—contrary to conservatism bias—people commonly jump to conclusions on the basis of little data. Kahenman's reaction shook Tversky's faith in thinking about people as merely a biased version of Bayesian, and they met for lunch to discuss their experiences and hunches about how people *really* judge probabilities.

Their collaboration blossomed into the enormously influential "heuristics and biases" research program (see, e.g., Gilovich, Griffin, and Kahneman, 2002). To explain this research program, Tversky and Kahneman (1974) drew an analogy with visual perception. People perceive objects as physically closer when they can be seen more sharply. This perceptual heuristic has some validity but leads to systematic errors when visibility is unusually good or poor. Similarly, Tversky and Kahneman argued, a small number of simple heuristics are useful for a wide range of complex probabilistic judgments but also generate systematic biases.

7.A. Representativeness

The first heuristic Kahneman and Tversky (1972a) proposed—and the central one for probabilistic reasoning—is the representativeness heuristic. They defined it as "evaluat[ing] the probability of an uncertain event, or a sample, by the degree to which it

is: (i) similar in essential properties to its parent population; and (ii) reflects the salient features of the process by which it is generated" (p. 431).[59] Across several papers (Kahneman and Tversky, 1972a, 1973; Tversky and Kahneman, 1983), Kahneman and Tversky argued that the representativeness heuristic is the psychological process that generates the LSN (Section 2.A), sample-size neglect (Section 3.A), and base-rate neglect (Section 6), as well as several other biases such as the conjunction fallacy (described below).[60] For each of these biases, Kahneman and Tversky reported evidence from many different surveys and experiments.

Kahneman and Tversky (1972a) focused on people's beliefs about random samples. They argued that in order for a sample to be representative of the population from which it is drawn, it must satisfy both parts of the definition of representativeness: (i) the sample proportions must match the population rate, and (ii) systematic patterns must be absent. They called part (i) the LSN, and some of the evidence is described in Section 2.A. As an example of part (ii), they pointed to prior findings that people judged fair-coin-flip sequences with a pattern, such as HTHTHTHT, to be less likely than sequences that have the same number of heads and tails but no obvious pattern (e.g., Tune, 1964).

Kahneman and Tversky (1973) argued that sample-size neglect and base-rate neglect are consequences of the representativeness heuristic because sample sizes and base

[59] Kahneman and Frederick (2002) further developed the theory of the representativeness heuristic. In their formulation, when people are asked to make judgments about probability, they instead give the answer to the much simpler question about representativeness. They argue that such "attribute substitution" is a general characteristic of intuitive judgment: when asked a question that would be difficult and effortful to answer (requiring "System 2" thinking), people answer a much simpler question (that has an effortless and quick "System 1" answer) whenever they can.

[60] Later, Kahneman and Tversky (1982) drew a distinction between judgments *of* representativeness, relating to judgments about whether a random sample is representative (including the biases discussed in Sections 2 and 3), and judgments *by* representativeness, relating to use of the representativeness heuristic to make predictions and judge probabilities (including biased inference and base-rate neglect). Kahneman and Tversky argued that the evidence supported both hypotheses.

rates do not enter into judgments of representativeness. Similarly, the representativeness heuristic explains why regression to the mean is not intuitive to people, since it is also unrelated to representativeness.

Tversky and Kahneman (1983) introduced a new bias, the conjunction fallacy, which they argued could be caused by each of several mechanisms, including the representativeness heuristic. The conjunction fallacy is when people believe that the conjunction of two events, A *and* B, has higher probability than one of its constituents, say, A. Such a belief violates a basic law of probability. In one of several examples, Tversky and Kahneman reported results from a series of variants of the now-famous "Linda problem." In this problem, respondents were first given a brief description of Linda:

> Linda is 31 years old, single, outspoken and very bright. She majored in philosophy. As a student, she was deeply concerned with issues of discrimination and social justice, and also participated in anti-nuclear demonstrations.

In one of the variants, 142 undergraduates were asked which of two statements (presented in a random order) is more probable:

> Linda is a bank teller.
>
> Linda is a bank teller and is active in the feminist movement.

Tversky and Kahneman predicted that people would commit the conjunction fallacy because the description of Linda was constructed to be representative of a feminist and not representative of a bank teller. Consistent with the conjunction fallacy, 85% of respondents indicated that the second statement was more likely. A natural alternative explanation is that interpret "Linda is a bank teller" as implying that she is not active in the feminist movement, but a majority of respondents still committed the conjunction fallacy when the first statement was replaced by "Linda is a bank teller whether or not she is active in the feminist movement."

Tentori, Bonini, and Osherson (2004) reviewed evidence that the conjunction fallacy is robust to many potential confounds and persists when participants make incentivized bets and when the problem is framed in terms of frequencies. While Zizzo, Stolarz-Fantino, Wen, and Fantino (2000) found that making the error more obvious to participants reduced the frequency of the fallacy, Zizzo et al. and Stolarz-Fantino, Fantino, Zizzo, and Wen (2003, Experiment 5) found that for participants given feedback or monetary rewards, the effect occurred at rates similar to those for control participants. On the other hand, Charness, Karni, and Levin (2010) found that it is much less common when experimental participants are incentivized or work in teams. For an overview of non-representativeness-based explanations of the conjunction fallacy, see Fisk (2016).

While a wide array of biases can be accounted for by the representativeness heuristic, critics allege that representativeness is too vague and flexible a concept to be useful (e.g., Evans and Pollard, 1982, p. 101; Gigerenzer, 1996).[61] The theory potentially

[61] Gerd Gigerenzer's (1996) critiques were aimed broadly at the heuristics-and-biases research program and were empirical as well as theoretical. The central empirical claim was that many of the biases are weaker when problems are framed in terms of frequencies rather than probabilities. Kahneman and Tversky (1996) agreed with this claim (and indeed, Tversky and Kahneman (1983) anticipated it) but emphasized that the

has many degrees of freedom if "similar in essential properties" and "salient features of the process" can be defined differently in different settings. A related critique is that it merely creates the appearance of parsimony by giving a single name to distinct phenomena. The most pointed version of the critique is that representativeness is merely a label for, or redescription of, intuitive judgments of probability (Gigerenzer, 1996, p. 594), rather than an explanation of them.

In their original presentation of representativeness, Kahneman and Tversky (1972a, p. 431) anticipated this concern but argued that the agreement in people's judgments adequately pinned down its meaning:

> Representativeness, like perceptual similarity, is easier to assess than to characterize. In both cases, no general definition is available, yet there are many situations where people agree which of two stimuli is more similar to a standard, or which of two events is more representative of a given process. In this paper…we consider cases where the ordering of events according to

biases nonetheless largely persist in a frequency framing. Theoretically, Gigerenzer disputed the normative status of the Bayesian model and, more relevantly for economics, he argued that the proposed heuristics were too vague. For example, Gigerenzer (1996, p. 592) wrote: "Explanatory notions such as representativeness remain vague, undefined, and unspecified with respect both to the antecedent conditions that elicit (or suppress) them and also to the cognitive processes that underlie them…The problem with these heuristics is that they at once explain too little and too much. Too little, because we do not know when these heuristics work and how; too much, because, post hoc, one of them can be fitted to almost any experimental result." Gigerenzer's own research program differed in both research strategy and emphasis. The research strategy pursued by him and his colleagues focused on specifying precise algorithms to fit experimental data on people's judgments (e.g., Gigerenzer, Hertwig, and Pachur, 2011). Their emphasis was on the high quality of the resulting judgments, rather than on deviations from Bayesian reasoning. This work is less relevant for economics than Kahneman and Tversky's both because the kinds of judgments studied are less central and because the Bayesian model already provides a good "as if" model of unbiased judgments. A recent, related line of work in cognitive science aims to explain biases as resulting from optimal cognitive strategies given limited cognitive resources (e.g., Lieder, Griffiths, and Hsu, 2018). Such work holds promise of answering Gigerenzer's directives to be specific about cognitive processes and to make precise predictions, while keeping the focus on biases that result from relying on heuristics.

> representativeness appears obvious, and show that people consistently judge the more representative event to be the more likely, whether it is or not.

In subsequent work, Tversky and Kahneman (1983) identified some regularities in judgments of representativeness. First, it is directional: it is natural to describe an outcome (e.g., a sample) as representative of a causally prior entity (e.g., a population), but usually not vice-versa. Second, when both can be described in the same terms, such as the mean or other salient statistics, then representativeness partly reduces to similarity of these statistics. However, as noted above, sharing features of the random process is also relevant. Third, common instances are usually more representative than rare events. However, there are notable exceptions; for example, a narrow interval around the mode of a distribution is often more representative than a wider interval near the tail that has greater probability mass (an observation related to the evidence discussed in Section 3.D that people's sampling-distribution beliefs overweight the mean). Fourth, an attribute is more representative of a class if it is more diagnostic, i.e., if its relative frequency in that class is higher than in a reference class. For example, 65% of undergraduates surveyed by Tversky and Kahneman stated that it is more representative of Hollywood actresses "to be divorced more than 4 times" than "to be Democratic," even though 83% of a different sample of undergraduates stated that, among Hollywood actresses, more are Democratic than divorced more than 4 times. The reason, Tversky and Kahneman argued, is that the odds of Hollywood actresses *relative to other women* being four-times divorced is much greater than the odds of Hollywood actresses *relative to other women* being Democratic. Fifth, an

unrepresentative instance of a category can nonetheless be representative of a superordinate category (e.g., a chicken is not a representative bird, but it is a fairly representative animal).

Formal modeling of representativeness provides the most persuasive response to the vagueness critique. Tenenbaum and Griffiths (2001) formalized the notion of representativeness as diagnosticity (the fourth regularity in the list above). To do so, they need to specify the relevant reference classes. For example, suppose the reference class for a fair coin is a usually alternating coin. Then the sequence HHTHTTTH is more representative of a fair coin than HTHTHTHT because the likelihood ratio $\frac{p(\text{HHTHTTTH} \mid \text{fair})}{p(\text{HTHTHTHT} \mid \text{fair})}$, which equals 1, is greater than the likelihood ratio $\frac{p(\text{HHTHTTTH} \mid \text{alternating})}{p(\text{HTHTHTHT} \mid \text{alternating})}$, which is less than 1. Tenenbaum and Griffiths did not propose an ex ante theory of the reference class, so its specification remains a degree of freedom in operationalizing this notion of representativeness.

7.B. The Strength-Versus-Weight Theory of Biased Updating

In an influential paper, Griffin and Tversky (1992) proposed a theory that aims to unify many updating biases within a common framework. According to their theory, the psychological process of belief updating has two stages: people form an initial impression based on the "strength" of the evidence, and then they adjust this impression based on the "weight" of the evidence. The strength, or extremeness, of the evidence is determined by its representativeness. The weight, or credence, of the evidence reflects other factors that matter for normatively correct updating. The adjustment for weight is insufficient, causing people's updating to be excessively influenced by the representativeness-related features of the evidence. The predictions of the theory then come from specifying what is strength

and what is weight. Griffin and Tversky applied their theory to seven belief biases, three of which are relevant for this chapter and discussed here.

First, following Kahneman and Tversky (1972a), they identified the proportion of *a* signals in a sample with the representativeness of the sample (see Section 3.B). Thus, they theorized that in drawing inferences from a sample of binary signals, the sample proportion ($\frac{N_a - N_b}{N}$) is strength and the sample size (N) is weight. The theory then explains why inferences are too sensitive to sample proportion (Stylized Fact 4) and insufficiently sensitive to sample size (Stylized Fact 2). In their Study 1, Griffin and Tversky posed twelve simultaneous-sample bookbag-and-poker-chip updating problems that vary in sample proportion and sample size; this study is discussed in Section 4.B, and its results are included in Section 4's meta-analysis. Consistent with the theory, when estimating equation (4.14), Griffin and Tversky found that the coefficient on sample proportion is greater than the coefficient on sample size.

Griffin and Tversky also found (consistent with the relatively small coefficient on sample size) that their experimental participants overinferred from sample sizes of 3 and 5 and underinferred from sample sizes of 9, 17, and 33. Based on this finding, they suggested that their theory might reconcile the general finding that experimental participants in bookbag-and-poker-chip experiments underinfer (Stylized Fact 1) with the evidence from Tversky and Kahneman (1971) that scientific researchers conclude too much from evidence obtained in small samples. However, this suggestion is not compelling. The sample sizes for the research studies examined by Tversky and Kahneman (1971) were 15, 20, 40, and 100, which are in the range of sample sizes where Griffin and Tversky find *under*inference. Moreover, as shown in Figure 3A and discussed in Section 4, Griffin and

Tversky's finding of overinference is unusual; overinference is *not* the predominant pattern across bookbag-and-poker-chip experiments for sample sizes of 3 and 5. With more complete financial incentives than Griffin and Tversky, Antoniou, Harrison, Lau, and Read (2015) replicated their Study 1 results but found underinference for all sample sizes when they controlled for risk preferences over the incentives (see Figure 3 from their 2013 working paper).

Second, Griffin and Tversky argued that their theory could explain base-rate neglect (Stylized Fact 7) if the likelihood information is the strength of the evidence and the prior probabilities of the states are the weight. This supposition follows from the argument that prior probabilities do not enter into judgments of representativeness, as discussed above. In their Study 2, Griffin and Tversky posed twenty-five updating problems that vary the prior probabilities of the two states and the number of *a*'s in a sample of 10 signals; this study is included in Section 4's meta-analysis on the use of prior probabilities in updating. Their results provide particularly clean evidence that experimental participants' posteriors are not sensitive enough to the prior probabilities.

Third, as discussed in Section 8.B in the context of Fischoff and Beyth-Marom's (1983) explanation of prior-biased updating, Griffin and Tversky argued that people focus on how well the evidence fits a "given" hypothesis but not how well it fits an "alternative" hypothesis. To apply this idea to a bookbag-and-poker-chip updating problem, suppose the likelihood of a sample under state A, $p(S \mid A)$, is higher than the likelihood under state B, $p(S \mid B)$. The higher likelihood is identified with the strength of the evidence and the lower likelihood with the weight. For example (as also described in Section 3.B), in Griffin and Tversky's Study 3, they posed updating problems in which the number of *a* signals is 7, 8,

9, or 10. When the rates were close together, (θ_A,θ_B) = (0.6,0.5), the experimental participants overinferred, reporting posteriors too favorable to state *A*. However, when the rates were further apart, (θ_A,θ_B) = (0.6,0.25), the participants' posteriors were only slightly less favorable to state *A*, and thus they dramatically underinferred. Griffin and Tversky argued that this is because when evaluating the likelihood ratio, $\frac{p(S \mid A)}{p(S \mid B)}$, participants' overweighted the numerator and underweighted the denominator. They argued that this application of their theory explains why people underinfer by more in bookbag-and-poker-chip experiments when the rates are further apart (Stylized Fact 6).

Griffin and Tversky's strength-versus-weight theory is appealing because it explains so many biases, but it is not clear how useful the theory is for economists. It amounts to saying that people primarily judge posterior probabilities according to representativeness but also incorporate Bayesian reasoning to some extent. Economic models of biased updating generally nest pure bias and Bayesian updating as polar cases and assume that people are in between (for discussion, see Section 10.B). For economists, the challenge in capturing the strength-versus-weight theory, then, is the same as the challenge in capturing other representativeness-based theories: formalizing what representativeness means.

7.C. Economic Models of Representativeness

All of the models discussed in previous sections of this chapter are designed to capture biases that have been attributed to the representativeness heuristic. Most directly, Rabin's (2002) and Rabin and Vanayos's (2010) models of the Law of Small Numbers are

aimed directly at formalizing judgments of how representative a sample is of the population from which it is drawn.

Zhao (2018) proposed a model that formalizes the sense of representativeness based on similarity. He assumed that people judge the likelihood of A given S by assessing the similarity of A to S, and he proposed an axiomatic characterization of an ordinal similarity index. Under some assumptions, the judged similarity of A to S is the geometric mean of the two conditional probabilities: $p(A \mid S)^{\varsigma} p(S \mid A)^{1-\varsigma}$, where $0 < \varsigma < 1$. Zhao showed that his model can accommodate the conjunction fallacy. For example, consider the Linda problem. According to the model, an agent's belief that Linda is a bank teller (BT) *and* a feminist (F), conditional on the description of Linda as a social-justice activist (the signal S), depends on the similarity of $\mathrm{BT} \cap \mathrm{F}$ to S, which equals $p(\mathrm{BT} \cap \mathrm{F} \mid S)^{\varsigma} p(S \mid \mathrm{BT} \cap \mathrm{F})^{1-\varsigma}$. By comparison, the agent's conditional belief that Linda is a bank teller depends on the similarity of BT to S, which equals $\pi(\mathrm{BT} \mid S) = p(\mathrm{BT} \mid S)^{\varsigma} p(S \mid \mathrm{BT})^{1-\varsigma}$. The former can be larger than the latter if $p(S \mid \mathrm{BT} \cap \mathrm{F})$ is sufficiently larger than $p(S \mid \mathrm{BT})$. Zhao also showed that his model generates base-rate neglect: dividing the similarity of state A to signal S by the similarity of state B to signal S gives

$$\left(\frac{p(A \mid S)^{\varsigma} p(S \mid A)^{1-\varsigma}}{p(B \mid S)^{\varsigma} p(S \mid B)^{1-\varsigma}} \right) = \frac{p(S \mid A)}{p(S \mid B)} \left(\frac{p(A \mid S) p(S)}{p(S \mid A)} \frac{p(S \mid B)}{p(B \mid S) p(S)} \right)^{\varsigma} = \frac{p(S \mid A)}{p(S \mid B)} \left(\frac{p(A)}{p(B)} \right)^{\varsigma}.$$

This is the same as the formula for base-rate neglect in equation (6.1), with the base-rate neglect parameter d equal to the similarity parameter ς. For economic applications, it is a

limitation of Zhao's framework that the similarity judgment is an ordinal measure, i.e., defined up to a monotonic transformation. Because of that, the resulting similarity judgments cannot directly be treated as probabilistic beliefs for the purposes of decision making.

Gennaioli and Shleifer (2010) proposed a model that, like Tenenbaum and Griffiths (2001), formalizes the sense of representativeness based on diagnosticity. The key idea underlying Gennaioli and Shleifer's model is that, when people are judging the probability of some event, the states of the world that are most representative of the event are most likely to come to an agent's mind, i.e., to be remembered or attended to. People then overestimate the probabilities of these states. Gennaioli and Shleifer refer to the bias in what comes to mind as "local thinking." Implementations of this idea in different environments have been developed not only in Gennaioli and Shleifer (2010) but also in Bordalo, Coffman, Gennaioli, and Shleifer (2016) and Bordalo, Gennaioli, and Shleifer (2018), each of which is described below.

Gennaioli and Shleifer (2010) applied their model to explain several biases, including the conjunction fallacy. To illustrate, consider the Linda problem. There are two dimensions of the state space: bank teller (BT) versus social worker (SW) and feminist (F) versus non-feminist (NF). Suppose that the true probabilities of each of four states are:

	Feminist	Non-Feminist
Bank Teller	20%	10%
Social Worker	60%	10%

When assessing the probability of an event that fully pins down the state, the agent's belief is correct because there is no scope for biased attention or recall to play a role; e.g., the agent's belief about the probability that Linda is a bank teller and a feminist, $\pi(\text{BT} \cap \text{F})$, is the true probability, $p(\text{BT} \cap \text{F})$= 20%. However, when assessing the probability of an event that leaves uncertainty about the state, then the agent differentially attends to (or remembers) the states that are more representative of the event. This assessment can be broken down into two steps. First, given the focal event, the representativeness of each possible "scenario" (some event along a different dimension) is judged according to its diagnosticity. For the focal event {Linda is a bank teller}, the representativeness of the scenario that she is a feminist is $\frac{p(\text{F}\,|\,\text{BT})}{p(\text{F}\,|\,\text{SW})} = \frac{20\%/(20\%+10\%)}{60\%/(60\%+10\%)} = 0.78$, and the representativeness of the scenario that she is a non-feminist is $\frac{p(\text{NF}\,|\,\text{BT})}{p(\text{NF}\,|\,\text{SW})} = \frac{10\%/(20\%+10\%)}{10\%/(60\%+10\%)} = 2.33$. Second, the agent judges the probability of the focal event by aggregating across all scenarios, weighted by their representativeness. In the starkest and simplest case, the agent puts full weight on the most representative scenario. In that case, when judging the probability of the event {Linda is a bank teller}, the agent thinks only about the scenario in which Linda is a non-feminist, and thus $\pi(\text{BT}) = p(\text{BT} \cap \text{NF}) = 10\%$. Since $\pi(\text{BT})$ is smaller than $\pi(\text{BT} \cup \text{F})$, the agent has committed the conjunction fallacy.

The model also generates a form of base-rate neglect. In the Linda-problem example, when told that Linda is a bank teller, the agent becomes certain that Linda is a non-feminist despite the fact that, unconditional on bank teller versus social worker, former

activists like Linda are much more likely to be feminists (80% probability) than non-feminists (20% probability). This base-rate neglect occurs because the agent's judgments of the representativeness of Linda depend only on the conditional probabilities $p(\mathrm{F}\,|\,\mathrm{BT})$, $p(\mathrm{F}\,|\,\mathrm{SW})$, $p(\mathrm{NF}\,|\,\mathrm{BT})$, and $p(\mathrm{NF}\,|\,\mathrm{SW})$ and not on the base rates $p(\mathrm{F})$ and $p(\mathrm{NF})$.

Gennaioli and Shleifer also developed an extension of their model to capture some of the evidence of partition dependence reviewed in Section 3.B. Consider an example similar to theirs (based on Fischhoff, Slovic, and Lichtenstein, 1978). There are three possible causes of car failure: the state space is {battery problems, fuel problems, and ignition problems}. People are asked the probability that a car's failure to start is *not* due to battery problems. The model aims to explain why, when asked to assign probabilities to three bins {battery, fuel, ignition}, people report a higher total probability for non-battery causes than when asked to assign probabilities to the two bins {battery, non-battery}. Suppose the true probabilities are p(battery) = 60%, p(fuel) = 30%, and p(ignition) = 10%. When asked to assign probabilities to all three states, the agent is not biased and judges the probability of non-battery as π(non-battery | {battery, fuel, ignition}) = p(fuel) + p(ignition) = 40%. However, when asked to assign probabilities to {battery, non-battery}, the agent's assessment of the probability of the event {non-battery} is distorted by overweighting the likelihood of its constituent states according to their representativeness. Analogous to the Linda-problem example, this distortion can be broken down into two steps. In the first step, the representativeness of each constituent state is judged. The representativeness of {fuel} is $\frac{p(\text{fuel}\,|\,\text{non-battery})}{p(\text{fuel}\,|\,\text{battery})}$, while the representativeness of {ignition} is $\frac{p(\text{ignition}\,|\,\text{non-battery})}{p(\text{ignition}\,|\,\text{battery})}$. Unfortunately, in this environment, these measures of representativeness are not well-

defined because the denominators are zero. Gennaioli and Shleifer therefore extended their model by proposing that when these likelihood ratios are not well-defined, people instead measure representativeness by just the numerators. Thus, the representativeness of {fuel} is $p(\text{fuel}|\text{non-battery}) = \frac{30\%}{30\%+10\%} = 0.75$, and the representativeness of {ignition} is $p(\text{ignition}|\text{non-battery}) = \frac{10\%}{30\%+10\%} = 0.25$. In the second step, the agent judges the probability of the event {non-battery} by aggregating across its constituent states, weighted by their representativeness. In the stark case where the most representative state is given full weight, the agent judges the probability of {non-battery} to be equal to the probability of {fuel}: $\pi(\text{non-battery} \mid \{\text{battery, non-battery}\}) = p(\text{fuel}) = 30\%$. This perceived probability is smaller than $\pi(\text{non-battery} \mid \{\text{battery, fuel, ignition}\}) = 40\%$. The psychology of the model is that when the agent assesses the probability of the event {non-battery}, the possibility of ignition problems (the less representative state) does not come to mind.

Bordalo, Coffman, Gennaioli, and Shleifer (2016) applied the representativeness-as-diagnosticity idea to stereotyping. This model develops the logic underlying Tversky and Kahneman's (1983) example, mentioned above, of why "being divorced more than 4 times" is a stereotype of Hollywood actresses. Adapting an example from Bordalo et al., consider the stereotype of Florida residents being elderly. There are two groups, Florida residents and U.S. residents overall. According to the 2010 Census, the percentage of residents 65 and over is 17% in Florida and 13% in the US overall. The model assumes that the agent knows these percentages but does not remember them. When assessing the age distribution of Florida residents, the more representative scenarios (i.e., age intervals) are differentially recalled or attended to. The 65+ age group is more representative of

Florida residents than the <65 age group because $\frac{p(\text{age } 65+ \mid \text{FL})}{p(\text{age } 65+ \mid \text{US})} > \frac{p(\text{age } <65 \mid \text{FL})}{p(\text{age } <65 \mid \text{US})}$. Consequently, the agent's assessment $\pi(\text{age } 65+ \mid \text{FL})$ is an overestimate relative to $p(\text{age } 65+ \mid \text{FL})$, while $\pi(\text{age } <65 \mid \text{FL})$ is an underestimate. This example illustrates the two central implications of the model: stereotypes have a "kernel of truth," but they can nonetheless be extremely inaccurate. In addition to providing a number of other illustrative examples (such as Asians are good at math, Republicans are rich, Tel Aviv is dangerous), Bordalo et al. reports laboratory experiments with abstract groups and exogenous frequencies, as well as an empirical application to survey data on actual and perceived ethical views of liberals and conservatives across many political issues. The results overall are consistent with the model. Arnold, Dobbie, and Yang (forthcoming) and Alesina, Miano, and Stantcheva (2018) find that the kernel-of-truth hypothesis provides a good explanation of judges' bias against blacks in bail decisions and residents' beliefs about immigrants, respectively. A parameter of the model governing how strongly representativeness influences beliefs is also estimated to have similar values across papers that estimate it (Bordalo et al., 2016; Arnold, Dobbie, and Yang, forthcoming).

Bordalo, Gennaioli, and Shleifer (2018) explored how representativeness-influenced beliefs may generate extrapolative expectations in asset markets (discussed in detail in Chapter XXX (by Barberis) of this Handbook). The state of the economy at time *t* is denoted ω_t. The rational expectation of ω_t at time *t*-1 is $E[\omega_t \mid \omega_{t-1}] \equiv f(\omega_{t-1})$. The key assumption in this setting is that at time *t*, when forecasting next period's state ω_{t+1}, the agent assigns higher probability to states that are more representative of ω_t relative to

$f(\omega_{t-1})$, i.e., states with larger $\frac{p(\omega_{t+1} \mid \omega_t)}{p(\omega_{t+1} \mid f(\omega_{t-1}))}$. Intuitively, the most representative state is the one that has experienced the largest increase in its likelihood based on recent news. Thus, the agent's forecast of next period's state is given by the probability density function:

$$\pi(\omega_{t+1} \mid \omega_t) = p(\omega_{t+1} \mid \omega_t)\left(\frac{p(\omega_{t+1} \mid \omega_t)}{p(\omega_{t+1} \mid f(\omega_{t-1}))} \right)^{\rho} \frac{1}{Z}, \tag{7.1}$$

where $\rho > 0$ is the parameter governing how strongly representativeness influences beliefs and $Z \equiv \int_{+\infty}^{x=-\infty} p(\omega_{t+1} = x \mid \omega_t)\left(\frac{p(\omega_{t+1} = x \mid \omega_t)}{p(\omega_{t+1} = x \mid f(\omega_{t-1}))} \right)^{\rho} dx$ is a normalizing constant. Bordalo, Gennaioli, and Shleifer refer to the mean of the beliefs in equation (7.1) as "diagnostic expectations" because the beliefs overweight states that are most diagnostic of ω_t relative to $f(\omega_{t-1})$.

These beliefs turn out to have a particularly convenient form when ω_t follows an AR(1) process whose shocks are distributed normally with mean zero and variance σ^2. In that case, $\pi(\omega_{t+1} \mid \omega_t)$ is a normal distribution with variance σ^2 and mean

$$E_t[\omega_{t+1}] + \rho(E_t[\omega_{t+1}] - E_{t-1}[\omega_{t+1}]). \tag{7.2}$$

It is clear from equation (7.2) that diagnostic expectations for period $t + 1$ overreact to the new information received at time t. It is this property of diagnostic expectations that generates extrapolative expectations. Bordalo, Gennaioli, and Shleifer embed diagnostic

expectations in a dynamic macroeconomic model and show that it can explain several facts about credit cycles that are difficult to reconcile with a rational-expectations model.

The local-thinking model of representativeness reviewed in this subsection has two main limitations. First, additional assumptions may be needed to apply it in new settings. For example, a key ingredient for diagnostic expectations is the assumption that representativeness for ω_{t+1} is assessed by its diagnosticity for ω_t relative to $f(\omega_{t-1})$. Although it may be plausible, this assumption does not follow from the local-thinking model. As discussed above, applying the model to explain partition dependence requires an assumption about how representativeness is judged when the likelihood ratio is not well-defined. More generally, it is not clear how to apply the model in settings that do not fit the basic setup of existing applications. For example, does the model make predictions about people's beliefs about the distribution of 100 flips of a fair coin, and if so, how should the model be specified? An important challenge going forward is to specify a set of assumptions or guidelines that eliminate the degrees of freedom in applying the model.

Second, the model does not explain the representativeness-related biases that motivate it across the range of settings in which those biases are observed. For example, the model's explanation of partition dependence is that people do not fully remember or attend to all of an event's constituent states; in the example above, when the event is described as "non-battery problems," the agent thinks only of fuel but not ignition problems. Yet partition dependence is observed even when an event is described as the union of its constituent states—e.g., "either fuel or ignition problems" instead of "non-battery problems" (as in many of Tversky and Kohler's (1994) examples)—a case when there is little scope for differential memory or attention to play a role in generating the bias.

The evidence discussed in Section 3.B on people's sampling-distribution beliefs about coin flips pertains to partition dependence in which an event is described explicitly as the union of its constituent states (e.g., "0, 1, 2, or 3 heads").[62] Similarly, the model has no mechanism for explaining base-rate neglect in simple updating problems where attention and memory are unlikely to play large roles, as in much of the evidence reviewed in Sections 4 and 6.

Advocates of the local-thinking model would argue that it represents a different approach to behavioral-economic theory than the models discussed in earlier sections. While those models aim to capture the psychology and experimental evidence regarding a particular bias, the local-thinking model aims to capture a central intuition about representativeness that cuts across biases. The model is also motivated as much by empirical examples as by the psychology evidence. Moreover, the attention and memory mechanisms underlying the local-thinking model are consistent with its orientation toward empirical applications, since field settings often do have scope for such mechanisms to play a role. Because of this orientation, advocates would argue, the model may hold promise of organizing a wider array of evidence from field settings.

7.D. Modeling Representativeness Versus Specific Biases

Kahneman and Tversky's work on representativeness had a far more profound influence on economics than Edwards's earlier work on conservatism bias. Indeed, the early research in economics on errors in probabilistic reasoning—despite relying on bookbag-and-poker-chip experiments like Edwards's—was framed as testing whether the

[62] In the partition-dependence literature, the cases where the event is described explicitly as unions of its constituent states are call "explicit disjunctions," and other cases are called "implicit disjunctions." Using that terminology, the local-thinking model provides an explanation for the latter but not the former.

representativeness heuristic would persist in shaping beliefs under more stringent conditions, such as when people face incentives and have experience (e.g., Grether, 1980, 1992; Harrison, 1994) or face market discipline (e.g., Duh and Sunder, 1986; Camerer, 1989).

Much of the subsequent economic modeling, however, has focused on biases (the LSN, NBLLN, etc.), rather than on the representativeness heuristic per se. An advocate of modeling biases could argue that when heuristics generate nearly optimal probabilistic reasoning, the Bayesian model is an adequate "as if" representation. It is the precisely the biases—the deviations from the Bayesian model—that are needed to improve the accuracy of economic analysis. Analogously, models of *deviations* from exponential discounting and expected utility have proven useful for economics, even in the absence of more detailed models of the psychological processes underlying intertemporal and risky decision making.

Yet modeling the representativeness heuristic is appealing. Doing so holds the promise of capturing many biases at once and of explaining why particular biases may be more or less powerful under certain circumstances. On the other hand, because judgments of representativeness are so psychologically rich, it may be that no simple economic model can capture more than a narrow slice of the wide range of phenomena that representativeness encompasses.

In my opinion, both approaches have merit. Any model, whether of a bias or a heuristic, should be evaluated by the usual criteria of good economic models: broad applicability, predictive sharpness, and empirical accuracy. I further discuss these and other modeling issues in Section 10.B.

Section 8. Prior-Biased Inference

In this section and the next, I return to the topic of inference. In this section, I review evidence and theory related to drawing inferences in a manner that is biased in favor of current beliefs. Informal observations of such a bias date back at least to Francis Bacon (1620). In the psychology literature, the term *confirmation bias* is commonly used to refer to a variety of different psychological processes related to seeking out, interpreting, and preferentially recalling information or generating arguments supportive of one's current beliefs (e.g., Nickerson, 1998). The work I review falls under the umbrella of confirmation bias but is narrowly focused on updating from signals that have been observed. To reflect my relatively narrow focus, I adopt the new term *prior-biased inference*.

8.A. Conceptual Framework

To be more precise about what I mean by prior-biased inference, I build on the reduced-form empirical model from Section 4.A, equation (4.6), rewritten here for convenience:

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \left[\frac{p(S \mid A)}{p(S \mid B)}\right]^{c} \left[\frac{p(A)}{p(B)}\right]^{d}.$$

Recall from Section 4 that in general it has been found that $c < 1$ (Stylized Facts 1 and 9), and in symmetric binomial updating problems, $c = c(N, \theta)$ is decreasing in the sample size N (Stylized Fact 3) and in the diagnosticity parameter θ (Stylized Fact 6). Prior-biased

inference is the possibility that c may depend on whether a newly observed signal reinforces or weakens current priors.

Specifically, as in Charness and Dave (2017)[63], I describe the bias as a discrete difference in the amount by which beliefs are updated depending on whether the signal is confirming or disconfirming[64]:

$$\frac{\pi(A\,|\,S)}{\pi(B\,|\,S)} = \left[\frac{p(S\,|\,A)}{p(S\,|\,B)}\right]^{c_0 + I\{S \text{ is confirming}\}\cdot c_{\text{conf}} + I\{S \text{ is disconfirming}\}\cdot c_{\text{disconf}}} \left[\frac{p(A)}{p(B)}\right]^{d}, \qquad (8.1)$$

where $I\{S \text{ is confirming}\}$ equals 1 if $\frac{p(A)}{p(B)}$ and $\frac{p(S\,|\,A)}{p(S\,|\,B)}$ are both greater than 1 or both less than 1, and $I\{S \text{ is disconfirming}\}$ equals 1 if one of them is greater than 1 and the other is less than 1. As before, d is a measure of base-rate neglect. Now, however, there are three reduced-form parameters describing biased inference: c_0 when the priors are equal, $c_0 + c_{\text{conf}}$ when the signal is confirming of current beliefs, and $c_0 + c_{\text{disconf}}$ when the signal is disconfirming of current beliefs. The prior-biased-inference hypothesis is $c_{\text{conf}} \geq 0 \geq$

[63] To be more precise, equation (8.1) is the implicit model underlying Charness and Dave's (2017) empirical specification, which is equation (8.2) below.

[64] There are other reasonable specifications that have not been explored. For example, a continuous and symmetric version of prior-biased inference would be:

$$\ln\left(\frac{\pi(A\,|\,S)}{\pi(B\,|\,S)}\right) = c_0 \ln\left(\frac{p(S\,|\,A)}{p(S\,|\,B)}\right) + d\ln\left(\frac{p(A)}{p(B)}\right) + c_{00}\left[\ln\left(\frac{p(S\,|\,A)}{p(S\,|\,B)}\right)\cdot\ln\left(\frac{p(A)}{p(B)}\right)\right].$$

In this specification, prior-biased inference amounts to adding an interaction term to equation (4.7). The measure of biased inference is then a continuous function of the priors, $c_0 + c_{00}\ln\left(\frac{p(A)}{p(B)}\right)$, and consistent with this specification, Pitz, Downing, and Reinhold (1967, Figures 2-4) found that the difference between confirming and disconfirming signals in the amount of inference is increasing in the difference between the priors. Interestingly, in this specification, the bias could alternatively be described as having the constant c_0 as the measure of biased inference but having the measure of base-rate neglect be a continuous function of the likelihoods: $d + c_{00}\ln\left(\frac{p(S\,|\,A)}{p(S\,|\,B)}\right)$.

c_{disconf}, with at least one inequality strict.

In the literature, the term "confirmation bias" is sometimes used to mean the opposite of base-rate neglect: $d > 1$. However, the evidence reviewed in Sections 4 and 6 indicate that base-rate neglect ($d < 1$) is the general direction of bias (Stylized Fact 1). With prior-biased inference defined as in equation (8.1), it is separately identifiable from base-rate neglect, and the two biases can coexist. What I call prior-biased inference is identified by the *asymmetric* response to signals that confirm versus disconfirm current priors.

Although conceptually distinct in my formulation, prior-biased inference and base-rate neglect will often push in opposite directions in a particular updating problem because prior-biased inference tends to reinforce an agent's current beliefs, while base-rate neglect will often move an agent's beliefs away from certainty (for further discussion, see Section 10.A). Moreover, despite the general tendency for people to underinfer (Stylized Fact 7), if $c_0 + c_{\text{conf}} > 1$, then prior-biased inference would cause people to overinfer when they receive confirming signals.

8.B. Evidence and Models

The evidence usually adduced for confirmation bias comes from *belief polarization* experiments, in which the beliefs of people with different priors who observe the same mixed signals are typically found to move *further apart*. In a classic experiment, Lord, Ross, and Lepper (1979) recruited 24 proponents and 24 opponents of capital punishment to be experimental participants (selected based on how they had filled out an in-class political questionnaire). The participants read a brief summary of a study that either found

evidence in favor of capital punishment as a deterrent or found opposite evidence. The participants were then asked to report the change in their attitudes. Next, the participants read a detailed account of the study. The change in their attitudes was again elicited, and they were also asked to judge the quality and convincingness of the study. After reading the brief summary, which did little more than provide an unambiguous statement of the study's conclusion, proponents and opponents both reported that their attitudes moved in the direction of the study's conclusion. In contrast, after participants read the detailed account, which included information about the study's procedures, criticisms of the study, and rebuttals to the criticisms, participants whose prior beliefs disagreed with the conclusion reverted to their prior beliefs. Moreover, participants whose prior attitudes agreed with the study's conclusion judged the study to be valid and convincing, while those whose prior beliefs disagreed with the conclusion highlighted flaws and alternative explanations. Finally, after participants read the detailed accounts of both the pro- and anti-capital punishment studies, belief polarization occurred, with both proponents and opponents reporting that their attitudes had become more extreme but in opposite directions.

This belief-polarization effect has been replicated across a range of contexts, including political beliefs such as the causes of climate change (Fryer, Harms, and Jackson, 2017), interpersonal beliefs such as a person's level of academic skills (Darley and Gross, 1983), and consumer beliefs about brand quality (Russo, Meloy, and Medvec, 1998). Reviews of the literature that are critical (e.g., Miller et al., 1993; Gerber and Green, 1999) have highlighted that the effect is not always found, and when it is, it shows up when

participants' *changes* in beliefs are elicited but not when the before and after *levels* of their beliefs are elicited and compared.

Belief polarization is often interpreted as evidence of a bias relative to Bayesian updating. In particular, as Lord, Ross, and Lepper (1979) argued informally, while it is *not* an error for people to infer that a study that aligns with their priors is higher quality, it *is* an error when people go on to use their prior-influenced assessment of the study to update their prior in opposite directions.[65] Baliga, Hanany, and Klibanoff (2013) formally proved that agents cannot update in opposite directions in a simple Bayesian model, but they showed that polarization can occur if agents are ambiguity averse. Moreover, a number of researchers have shown that belief polarization can be consistent with Bayesian reasoning in richer models (e.g., Dixit and Weibull, 2007; Andreoni and Mylovanov, 2012; Jern, Chang, and Kemp, 2014; Benoît and Dubra, 2018). For instance, Benoît and Dubra (2018) showed how belief polarization can occur when people have private information about an "ancillary matter" that does not have direct bearing on the issue of interest but matters for the interpretation of evidence. To give a concrete example, in the Lord, Ross, and Lepper experiment, this ancillary matter might be the proposition that studies reaching right-wing conclusions tend to be politically motivated and less intellectually honest. People who believe that proposition are more likely to have discounted evidence in favor of capital punishment as a deterrent *in the past* and are therefore more likely to enter the experiment as an opponent of capital punishment. They are also more likely to discount the evidence in favor of capital punishment as a deterrent *during the experiment*. If both proponents and

[65] Fryer, Harms, and Jackson (2017) formalize this error of "two-step updating" described by Lord, Ross, and Lepper.

opponents of capital punishment update their priors when reading the study that confirms their views but discount the evidence from the other study, then their beliefs will polarize.[66]

At the cost of being more abstract than the belief-polarization experiments, sequential bookbag-and-poker-chip experiments provide cleaner evidence for prior-biased inference. These experiments rule out many alternative explanations by studying fully specified updating problems; for example, they leave little room for unobserved "ancillary matters." However, confirmation bias is generally thought to be stronger when people observe ambiguous data that could be interpreted as either consistent or inconsistent with the currently favored hypothesis (Nickerson, 1998). To the extent that the data in bookbag-and-poker-chip experiments is unambiguous, such experiments may understate the magnitude of prior-biased inference that may occur when the information content of signals is more subject to interpretation.

In the earliest bookbag-and-poker-chip experiment that directly investigated prior-biased inference, Pitz, Downing, and Reinhold (1967) posed updating problems like those described in Section 4.C. The prior probabilities of the two states, *A* and *B*, were equal. The probability of a signal matching the state, θ, was known to participants and equal to 0.6, 0.7, or 0.8. Ten participants saw chunks of $N = 5$ signals at a time, ten saw chunks of $N = 10$ signals, and ten saw chunks of $N = 20$ signals. Consistent with the evidence reviewed in Section 4.B, underinference was greater when the sample size of signals was larger (larger *N*) and when the signals were more discriminable (larger θ). Moreover—

[66] Some of the subsequent experiments are cleaner than the Lord, Ross, and Lepper experiment because the prior is randomly assigned. For example, in Darley and Gross's (1983) experiment, before watching a video of a nine-year-old girl and rating her academic skills, participants were either told that her family was of high or low socioeconomic status. As Rabin and Schrag (1999) noted, such a design rules out non-common priors as a possible explanation for belief polarization.

consistent with prior-biased inference—Pitz, Downing, and Reinhold found less underinference when the signals confirmed the currently favored hypothesis. When they examined individual-level updating, Pitz, Downing, and Reinhold found that, following a single disconfirming signal, many participants revised their beliefs as if they had observed a confirming signal or did not revise their beliefs at all. In sequential-updating experiments in which participants updated after a single signal at a time, Geller and Pitz (1968) and Pitz (1969) replicated these findings, but in two experiments with normally distributed signals, DuCharme and Peterson (1968) found the opposite (i.e., stronger inference in response to a disconfirming signal).

In sequential updating experiments that begin with equal priors on the two states, prior-biased inference predicts that signals observed early on will have a greater impact on final beliefs than signals observed later: the early signals will move the priors to assign higher probability to one of the states, and then subsequent updating will be biased in favor of that state. As mentioned in Section 4.C, such "primacy effects" have indeed been found in most sequential-sample experiments that tested for them (Stylized Fact 10).

Three sequential updating experiments in the economics literature have reported tests for prior-biased inference. One of these experiments found evidence of it (Charness and Dave, 2017) and two did not (Eil and Rao, 2011; Möbius, Niederle, Niehaus, and Rosenblat, 2014), but none found the opposite.[67]

[67] Across the experiments in this literature, there are several regularities that may be related to prior-biased updating but which I do not discuss because I do not know how to interpret these regularities. For example, Pitz, Downing, and Reinhold (1967), Shanteau (1972), and Buser, Gerhards, and van der Weele (2018) found that, fixing the diagnosticity of the signal θ, the absolute change in beliefs (in units of probability) when updating does not depend on the priors. As another example, Coutts (2017) found a kind of primacy effect in which signals observed more frequently in the past were weighted more heavily when observed subsequently.

In Charness and Dave's (2017) experiment, the prior probabilities of the two states were equal, and the probability of a signal matching the state was $\theta = 0.7$. Each participant observed six signals sequentially and, after each signal, recorded his subjective probability of the states. Participants were incentivized for accuracy. Charness and Dave's regression equation is based on the logarithm of equation (8.1)[68]:

$$\ln\left(\frac{\pi(A \mid s_1, s_2, \ldots, s_t)}{\pi(B \mid s_1, s_2, \ldots, s_t)}\right) = \beta_0 + \beta_1 \ln\left(\frac{p(s_t \mid A)}{p(s_t \mid B)}\right) + \beta_2 \ln\left(\frac{\pi(A \mid s_1, s_2, \ldots, s_{t-1})}{\pi(B \mid s_1, s_2, \ldots, s_{t-1})}\right) \quad (8.2)$$

$$+\beta_3 I\{s_t \text{ is confirming}\} + \beta_4 I\{s_t \text{ is disconfirming}\} + \eta_t,$$

where $I\{s_t \text{ is confirming}\}$ equals 1 if $\frac{\pi(A \mid s_1, s_2, \ldots, s_{t-1})}{\pi(B \mid s_1, s_2, \ldots, s_{t-1})}$ and $\frac{p(s_t \mid A)}{p(s_t \mid B)}$ are both greater than 1 or both less than 1, and $I\{s_t \text{ is disconfirming}\}$ equals 1 if one of them is greater than 1 and the other is less than 1.

Charness and Dave estimated both $\hat{\beta}_1$ and $\hat{\beta}_2$ to be less than one, consistent with underinference and base-rate neglect in updating problems that start from equal priors (as per Stylized Fact 9). Moreover, they estimated $\hat{\beta}_3 > 0$ and $\hat{\beta}_4 < 0$, consistent with prior-biased inference. Charness and Dave also found that $\hat{\beta}_1 + \hat{\beta}_3 > 1$, meaning that their

[68] Charness and Dave parameterized the regression slightly differently, replacing $\ln\left(\frac{p(s_t \mid A)}{p(s_t \mid B)}\right)$ with a dummy taking the value 1 if the t^{th} signal is *a* and -1 if the t^{th} signal is *b*. This specification is equivalent to equation (8.2), but the coefficients β_1, β_3, and β_4 in equation (8.2) need to be multiplied by 0.847 in order to equal the corresponding coefficients in Charness and Dave's specification.

experimental participants overinferred when a confirming signal was observed. Although Pitz, Downing, and Reinhold (1967) did not report estimates from regression equation (8.2), their experimental participants often overinferred after a confirming signal in updating problems with low diagnosticity ($\theta = 0.6$) and extreme priors, but underinferred after a confirming signal in updating problems with high diagnosticity ($\theta = 0.7$ or 0.8) or nearly equal priors.

What explains the prior-biased inference that has been observed in bookbag-and-poker-chip experiments? Fischhoff and Beyth-Marom (1983, pp. 247-248) proposed that, rather than correctly using the likelihood ratio to draw inferences, people assess how consistent the signal is with the hypothesis they are testing—which is generally the currently favored hypothesis—and do not take into account its consistency with other hypotheses. That proposal dovetails nicely with Pitz, Downing, and Reinhold's (1967, p. 391) suggestion that participants may "not perceive isolated disconfirming events as being, in fact, contradictory to their favored hypothesis. For example, if they are fairly certain that the 80 per cent red bag is being used, a single occurrence of a blue chip will not be unexpected, and consequently may not lead to a decrement in subjective certainty." Fischhoff and Beyth-Marom argued that this bias of ignoring alternative hypotheses helps explain a variety of other observations. For example, when psychics offer universally valid personality descriptions, people are often impressed by how well it fits them without regard to the fact that it would fit others equally well. As discussed in Section 7.B, Griffin and Tversky (1992) subsequently argued that this same bias explains why people underinfer by more in bookbag-and-poker-chip experiments when the signal rates are further apart (Stylized Fact 6).

Second, Pitz, Downing, and Reinhold (p. 391) speculated that prior-biased inference may arise because participants are unwilling to report a decrease in confidence once they have "committed" to supporting one state as more likely. As a test of this hypothesis, Pitz (1969) conducted a sequential bookbag-and-poker-chip experiment in which he manipulated the salience of the participants' posterior after the last signal when reporting their next posterior. He found prior-biased updating when participants reported posteriors after each signal and their posterior after the previous signal was visually displayed, but prior-biased updating was almost completely eliminated when the previous posterior was not displayed or when participants reported posteriors only at the end of a sequence of signals. However, a contrary result was found in another bookbag-and-poker-chip experiment (Beach and Wise, 1969): participants who reported beliefs after each signal ended up with virtually the same posteriors as participants who reported beliefs only after a sequence of signals. In a formal model related to the commitment hypothesis, Yariv (2005) assumed that an agent has a preference for consistency and can choose her beliefs. She showed that when the observed signal confirms the agent's prior, the agent may choose posteriors that are overconfident.

Eil and Rao (2011) proposed another hypothesis to explain prior-biased updating: people want their guesses to be correct, so they view confirming evidence as "good news" and update more strongly in response to good news than bad news. However, this hypothesis presupposes that *preference*-biased inference occurs, but as discussed in the next section, the evidence on preference-biased inference taken as a whole is not straightforward to interpret.

Rabin and Schrag (1999) proposed a formal model of what they call "confirmatory bias" in order to study the implications of prior-biased updating. The central assumption is that the agent sometimes misperceives disconfirming signals as confirming. This assumption is meant to capture many of the psychological mechanisms that may underlie confirmation bias. While misperception seems implausible as a literal description of the psychology underlying prior-biased inference in bookbag-and-poker-chips experiments, it actually fits nicely with the evidence that experimental participants sometimes update in the wrong direction in response to disconfirming signals, although it cannot explain the evidence of overinference from confirming signals mentioned above.

Formally, the agent begins with equal priors on the two states *A* and *B* and observes a sequence of i.i.d. signals, $s_t \in \{a, b\}$, where the signal matches the state with probability $\theta > \frac{1}{2}$ and does not match with probability $1-\theta$. If the agent's priors are equal, or if she observes a signal that matches the state that she currently thinks is more likely, then her *perceived signal* is equal to the true signal s_t. However, if she observes a signal that does not match the state favored by her current priors, then with probability $q > 0$ she misperceives the disconfirming signal to be a confirming signal. The agent is unaware that she misperceives signals. She updates using Bayes' Rule but using the perceived signals instead of the true signals.

Rabin and Schrag drew out several main implications of the model. First, relative to a Bayesian who observed the same number of *a* and *b* signals, the agent on average will have overconfident beliefs. That is because the agent is likely to have misperceived some disconfirming signals as confirming her current beliefs, causing her to believe more strongly than she should in her currently favored hypothesis.

Second and surprisingly, if a Bayesian observer sees that a sufficiently biased agent believes that one state, say *A*, is more likely despite having perceived a sufficiently mixed set of signals, then the Bayesian observer may conclude that the *other* state is in fact more likely. The reason is that some of the signals that the agent perceived as *a* signals were likely to have been *b* signals, which in turn means that *b* signals were likely the majority. This implication, while striking, inherently applies to a scenario that is very unlikely because it requires that the signals are highly informative (θ close to 1), in which case the sample perceived by the agent is unlikely to be sufficiently mixed.

Third, if the bias is sufficiently severe or the signals are sufficiently uninformative (θ close to ½), then when observing an infinite sequence of signals, there is positive probability that the agent will converge to certainty on belief in the wrong state. That is because once the agent starts believing in the wrong state, confirmatory bias is likely to cause her to perceive subsequent signals as continually building support for that hypothesis.

Pouget, Sauvagnat, and Villeneuve (2017) examined the implications of Rabin and Schrag's model in financial markets, assuming that some fraction of traders are rational and some fraction have confirmatory bias. They showed that the model can explain three well-known observations. First, excess volume arises simply because rational and biased traders disagree and are therefore willing to trade. Second, excess volatility occurs because the biased traders are too optimistic following an initial positive signal and too pessimistic following an initial negative signal. Third, momentum arises and bubbles occur because once biased traders are optimistic, they underreact to negative signals, so future prices are expected to be higher than current prices. Pouget, Sauvagnat, and Villeneuve also derived some novel predictions of the model: differences of opinion among traders are larger

following a sequence of mixed signals; traders are less likely to update their beliefs in the same direction as the current signal when previous signals have pointed in the opposite direction; and traders are less likely to update their beliefs in the same direction as the current signal when previous belief changes have been in the opposite direction. They found evidence consistent with these predictions using quarterly earnings surprises as a proxy for signals, dispersion in analysts' earnings forecasts as a proxy for differences of opinion, and analysts' revisions of annual earnings as a measure of beliefs updating.

Section 9. Preference-Biased Inference

This section discusses another potential inference bias, which I call *preference-biased inference*: when people receive "good news" (i.e., information that increases expected utility), they update more than when they receive "bad news." In the literature, this bias has been referred to as *asymmetric updating* (Möbius, Niederle, Niehaus, and Rosenblat, 2014) or the *good news-bad news effect* (Eil and Rao, 2011). Almost all of the research on this bias has been relatively recent. Preference-biased inference is a possible mechanism underlying a bias toward optimistic beliefs.

My focus on biased inference from signals that have been observed is (again) narrow relative to a broader literature in psychology and behavioral economics related to a range of psychological processes that can cause beliefs to become optimistic, such as strategic ignorance (avoiding information sources that may reveal bad news; see Golman, Hagmann, and Loewenstein, 2017, for a review) and self-signaling (taking actions that one later interprets as impartially revealing good news; e.g., Quattrone and Tversky, 1984; Bodner and Prelec, 2003; Bénabou and Tirole, 2011). There is evidence for several such processes. For example, strongly pointing to strategic ignorance, many people at risk for Huntington's disease refuse to be tested even though the test is inexpensive and accurate (Oster, Shoulson, and Dorsey 2013), and similarly for HSV (Ganguly and Tasoff, 2016).

9.A. Conceptual Framework

To be precise about preference-biased inference, I once again elaborate on the reduced-form empirical model from Section 4.A, equation (4.6):

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \left[\frac{p(S \mid A)}{p(S \mid B)}\right]^{c} \left[\frac{p(A)}{p(B)}\right]^{d}.$$

In preference-biased inference, people draw stronger inferences—i.e., c is larger—in response to a signal that favors the state that they prefer. Without loss of generality, suppose expected utility in state A, denoted U_A, is at least as large as expected utility in state B, denoted U_B. Following Möbius, Niederle, Niehaus, and Rosenblat (2014), I describe the bias as a discrete difference in the amount by which beliefs are updated depending on whether the signal is good news or bad news:

$$\frac{\pi(A \mid S)}{\pi(B \mid S)} = \left[\frac{p(S \mid A)}{p(S \mid B)}\right]^{c_0 + I\{S \text{ is good news}\} \cdot c_{\text{good}} + I\{S \text{ is bad news}\} \cdot c_{\text{bad}}} \left[\frac{p(A)}{p(B)}\right]^{d}, \qquad (9.1)$$

where $I\{S \text{ is good news}\}$ equals 1 if $S = a$ and $U_A > U_B$, $I\{S \text{ is bad news}\}$ equals 1 if $S = b$ and $U_A > U_B$, and both indicators equal 0 if $U_A = U_B$. As always, d is a measure of base-rate neglect, but now there are three reduced-form parameters describing biased inference: c is the same biased-inference measure discussed in Section 4, which alone governs the bias if the agent has no preference between states; $c + c_{\text{good}}$ is the measure of biased inference in response to good news; and $c + c_{\text{bad}}$ is the measure of biased inference in response to bad news. The preference-biased-inference hypothesis is $c_{\text{good}} > c_{\text{bad}}$.

Note that this specification of the preference-biased-inference hypothesis does not require that $c_{\text{good}} \geq 0$ or $c_{\text{bad}} \leq 0$; it is conceivable that having "valenced" signals (i.e., that

are good or bad news) could affect the overall amount of underinference or overinference relative to having unvalenced signals.

9.B. Evidence and Models

In one of the pioneering papers on preference-biased inference[69], Möbius, Niederle, Niehaus, and Rosenblat (2014) argued that it may arise as an "optimal bias" for agents who get utility directly from holding optimistic beliefs. In Möbius et al.'s model, which builds on the theoretical framework from Brunnermeier and Parker (2005), agents can choose ex ante (i.e., before observing any signals) the weight they put on the likelihood ratio—the value of c in equation (4.6)—for each possible signal they might observe. The benefit of deviating from Bayesian updating is that beliefs can end up being more optimistic, but the cost is that biased beliefs can lead to suboptimal behavior. In the model, the agent optimally chooses to weight bad news less than good news. Moreover, to offset the increased risk of suboptimal behavior, the agent optimally chooses to underweight the likelihood ratio for all signals. Thus, the agent has conservatism bias (as in Section 5.B) but is more conservative in response to bad news than good news. Bénabou (2013) proposed a model in which an agent can choose whether or not to process a signal that has been observed (i.e., to not pay attention to it, explain it away, or not think about it); if the agent gets anticipatory utility from putting high probability on the good state, then the agent may selectively ignore bad news.

In the economics literature, the evidence regarding preference-biased inference comes from sequential-updating experiments, in which participants are updating about a

[69] All results from Möbius et al. are from the most recent, 2014 working paper, but the original working paper is from 2007.

preference-relevant event. Möbius at al. conducted one of the earliest such experiments. Each participant took an IQ test. The two states of the world are A = {scored in top half of the IQ test} and B = {scored in bottom half}. Participants' beliefs were measured both before and after the IQ test and then again after each of four, independent binary signals. Each signal matched the true state with probability θ = 0.75. The belief elicitation was incentive compatible.

Möbius et al. estimated a regression equation corresponding to the logarithm of equation (9.1) above:

$$\begin{aligned}\ln\left(\frac{\pi(A\,|\,s_1,s_2,\ldots,s_t)}{\pi(B\,|\,s_1,s_2,\ldots,s_t)}\right) &\\ = \delta_1 I\{s_t = a\}\ln\left(\frac{p(s_t\,|\,A)}{p(s_t\,|\,B)}\right) + \delta_2 I\{s_t = b\}\ln\left(\frac{p(s_t\,|\,A)}{p(s_t\,|\,B)}\right) &\\ +\delta_3 \ln\left(\frac{\pi(A\,|\,s_1,s_2,\ldots,s_{t-1})}{\pi(B\,|\,s_1,s_2,\ldots,s_{t-1})}\right) + \zeta_t, &\end{aligned} \tag{9.2}$$

In terms of equation (9.1), δ_1 gives an estimate of $c + c_{\text{good}}$, δ_2 gives an estimate of $c + c_{\text{bad}}$, and δ_3 gives an estimate of the base-rate neglect parameter d. Möbius et al. found $\hat{\delta}_1 = 0.27$ (SE = 0.01) and $\hat{\delta}_2 = 0.17$ (SE = 0.03).[70] Both are less than one, indicating underinference in response to both good and bad news. Moreover, the estimates imply $c_{\text{good}} > c_{\text{bad}}$, consistent with preference-biased inference.

[70] For the coefficient on the prior ratio, Möbius et al. estimate $\hat{\delta}_3$ = 0.98 (SE = 0.06). Since this coefficient is essentially one, it indicates that there is no base-rate neglect in Möbius et al.'s data. As discussed in Section 4.C, most sequential-sample experiments find stronger evidence of base-rate neglect.

Experiments on preference-biased inference typically include a control condition in which participants are updating about an event that is *not* preference-relevant. In Möbius et al.'s control condition, participants repeated the updating task, except with reference to the performance of a robot rather than their own performance. The robot's initial probability of being a high type was set equal to the multiple of 0.05 closest to the participant's post-IQ-test belief about herself. That way, the state of the world about which the participant was updating had essentially the same prior probability and differed only in not being preference-relevant. In this control condition, Möbius et al. found less underinference overall and no asymmetry.[71]

While Möbius et al. found that bad news was underweighted by more than good news, the evidence from similar experiments taken as a whole is mixed. Three papers have found stronger inference from good news: Möbius et al. (2014), Eil and Rao (2011), and Charness and Dave (2017). The opposite result—stronger inference from *bad* news—was found in three papers: Ertac (2011) and Coutts (2017), as well as by Kuhnen (2015) for outcomes that take place in the loss domain (but not those that take place in the gain domain). Five papers have tested and found no evidence for asymmetry: Grossman and Owens (2012), Buser, Gerhards, and Van der Weele (2016), Schwardmann and Van der Weele (2016), Barron (2016), and Gotthard-Real (2017). Note also that while Eil and Rao (2011) found stronger inference from good news for participants' beliefs about their own beauty, they found no evidence for asymmetry for participants' beliefs about their own IQ.

[71] This is the result with their preferred sample restrictions, including only participants who updated at least once in the correct direction and never in the wrong direction (their Table 4 Column I). In the full sample, the amount of underinference is stronger overall and asymmetric, with greater updating in response to *a* signals (their Table 4 Column III).

There does not appear to be a neat explanation for the puzzling differences in results across experiments. Coutts (2017) suggested that since the experiments differ in the prior probability of state *A*, what appears to be preference-biased inference might actually be driven by prior-biased inference. However, Möbius et al. (2014) and Eil and Rao (2011) found evidence for preference-biased inference despite not finding prior-biased inference. Moreover, three papers tested for preference-biased inference with controls for priors or for prior-biased inference (Schwardmann and Van der Weele, 2016; Charness and Dave, 2017; Coutts, 2017) and reached different conclusions about the presence and direction of preference-biased inference.[72]

Another hypothesis is that different results across experiments may arise from differences in signal structure, which varies a great deal across the experiments. For example, different from Möbius et al. (2014), Ertac (2011) elicited participants' probabilities of scoring in the top, middle, or bottom tercile on a math quiz, and then provided a perfectly informative signal that performance is top/not-top or bottom/not-bottom. However, there are opposite results even across experiments with similar signal structures. For instance, Coutts's (2017) design is similar to Möbius et al.'s (2014), except with $\theta = 0.67$ instead of 0.75.

In parallel with the economics literature on preference-biased inference, there is a literature in psychology and neuroscience based on a different experimental design. In the pioneering experiment, Sharot, Korn, and Dolan (2011) presented participants with 80

[72] As noted at the end of Section 8.B, Eil and Rao (2011) hypothesized the opposite: that what appears to be evidence for prior-biased inference may actually be due to preference-biased inference, if people consider prior-supporting signals to be good news. Consistent with this hypothesis, Eil and Rao found little evidence of prior-biased inference when separately examining updating in response to signals that are good versus bad news, but the data are quite noisy. Their intriguing hypothesis does not appear to have been tested in other papers. However, the mixed overall evidence regarding preference-biased inference, combined with the relatively stronger evidence overall regarding prior-biased inference, leans against this hypothesis.

randomly ordered short descriptions of negative life events, such as having one's car stolen or having Parkinson's disease. Participants were asked the likelihood of the event happening to them (without incentives for accuracy). Participants were then shown the population base rate of the event, and their belief was re-elicited. "Good news" is defined as learning that the base rate is lower than the participant's initial probability. Almost all of the experiments in this literature find that the absolute change in participants' probabilities is larger in response to good news than bad news.[73]

Wiswall and Zafar (2015) reported a related study as part of a broader field experiment on the effects of providing information about earnings on students' beliefs and choices of undergraduate major. They provided 240 students with mean earnings of age-30 individuals and 255 students with the same information broken down by college major. Pooling across the two groups, they found that the information caused students who learned that they had overestimated population earnings to revise their own expected earnings downward by $159 per $1,000, while those who had underestimated earnings revised upward by $347 per $1,000. As Wiswall and Zafar highlight, however, the difference is far from statistically distinguishable, with a *p*-value of 0.327.

These experiments, however, have a design limitation: because receipt of good news versus bad news is not randomly assigned—whether the news is good or bad depends on one's prior belief—those who receive good news about a particular event may differ on unobservables from those who receive bad news. For example, those who are more

[73] The experiments finding such asymmetric updating include Sharot, Guitart-Masip, et al. (2012), Sharot, Kanai, et al. (2012), Moutsiana et al. (2013), Chowdhury et al. (2014), Garrett and Sharot (2017), Garrett et al. (2014), Korn et al. (2014), Kuzmanovic, Jefferson, and Vogeley (2015, 2016), and Krieger, Murray, Roberts, and Green (2016). An exception is Shah et al. (2016), who argued that the findings of asymmetry are due to a variety of methodological limitations with this kind of study design. Garrett and Sharot (2017), however, argued that the original findings are robust to addressing these limitations.

optimistic about an event may also be more confident about it and therefore update less in response to news. (The bookbag-and-poker-chip experiments discussed above eliminate such confounds by randomly assigning good and bad news.) Wiswall and Zafar partially addressed such a potential confound by testing whether any asymmetric response to good news versus bad news is associated with demographics they measured, and they found no evidence for such correlation.

Beginning with Kuzmanovic, Jefferson, and Vogeley (2015), some recent work in psychology and neuroscience overcomes this limitation by randomly assigning bogus base-rate information (e.g., Marks and Baines, 2017).[74] For example, Kuzmanovic et al. followed the same basic design as Sharot et al.—eliciting the participant's belief about likelihood of an event, providing the population base rate, and then re-eliciting the participant's belief—but told the participant that the population base rate is equal to the participant's belief plus or minus a random number. These experiments confirm the finding from the earlier studies that participants update more in response to good news than bad news.[75]

Taken all together, the evidence on preference-biased inference is confusing. In the economics literature, there are many bookbag-and-poker-chip experiments that reach

[74] Within experimental economics, providing bogus information is viewed as deceptive, and deceiving experimental participants is generally considered unacceptable (or at least unethical), especially if non-deceptive methods could be used instead. A non-deceptive method of randomizing the numbers provided to participants would be to show them actual numbers obtained from different sources (as was done in a different context by Cavallo, Cruces, and Perez-Truglia, 2016).

[75] A related strand of work in psychology and neuroscience conducts two-armed bandit experiments. In each round, participants can receive a payoff from either of two bandits, which give rewards at different, unknown rates. The rate of reinforcement learning is estimated separately in response to better-than-expected outcomes and worse-than-expected outcomes (i.e., positive and negative reward prediction errors). Lefebvre et al. (2017) found that experimental participants learn at a higher rate from better-than-expected outcomes. Palminteri et al. (2017) replicated this finding but also found that when the counterfactual payoffs from the unchosen bandit is also revealed in each round, then for these counterfactual payoffs, participants learn at a higher rate from *worse*-than-expected outcomes. Palminteri et al. interpreted their result as consistent with prior-biased inference: people update more in response to information that confirms their current choice.

opposite conclusions, and there the obvious candidate explanations for the differences in findings do not seem to be right. In the psychology and neuroscience literature, the experiments are based on a different design, and the results are nearly unanimous in finding evidence in favor of preference-biased inference. Sorting out the reasons why different experiments reach different conclusions should be a priority.

Section 10. Discussion

This chapter has reviewed a range of belief biases. In this final section, I comment on some interrelated, overarching issues that relate to many of the biases and to the literature as a whole.

10.A. When Do People Update Too Much or Too Little?

Do people update too much or too little, relative to Bayesian updating? The predominant view in the literature has shifted over time. The early literature focused exclusively on conservatism bias and characterized people as generally underinferring. As mentioned in Section 7, upon first learning about this literature from Amos Tversky in 1968, Daniel Kahneman (2002) recalled thinking "The idea that people were conservative Bayesian did not seem to fit with the everyday observation of people commonly jumping to conclusions." Much of Kahneman and Tversky's work, especially on the LSN (Tversky and Kahneman, 1971) and base-rate neglect (Kahneman and Tversky, 1982), focused on examples of people updating too much. Enamored with the new methods and findings from research on representativeness, psychologists lost interest in the conservatism literature and started doubting its methods and conclusions. As Fischhoff and Beyth-Marom (1983) summarized the general view at the time:

> In the end, this line of research [on bookbag-and-poker-chip experiments] was quietly abandoned…This cessation of activity seems to be partly due to the discovery of the base-rate fallacy, which represents the antithesis of conservatism and other phenomena that led researchers to conclusions such

> as the following: "It may not be unreasonable to assume that…the probability estimation task is too unfamiliar and complex to be meaningful" (Pitz, Downing, and Reinhold, 1967, p. 392). "Evidence to date seems to indicate that subjects are processing information in ways fundamentally different from Bayesian…models" (Slovic and Lichtenstein, 1971, p. 728). "In his evaluation of evidence, man is apparently not a conservative Bayesian; he is not Bayesian at all" (Kahneman and Tversky, 1972a, p. 450).

My view—hopefully communicated throughout this chapter—is that *whether* people update too much or too little is the wrong question. A better question is *when* we may expect one versus the other.

Here is a broad-brush summary, focusing on several of the main biases reviewed in this chapter and on the usual case of updating about state A versus B from independent binomial signals, with a signals having probability $\theta_A > \frac{1}{2}$ in state A and probability $\theta_B < \frac{1}{2}$ in state B. By and large, people update too little, with three exceptions. First, when θ_A and θ_B are close together, people overinfer from signals and hence update too much (Section 4.A). Second, people may overinfer and thus update too much due to prior-biased updating, when the signal goes in the *same* direction of the priors (Section 8). Third, people may update too much due to base-rate neglect, when the priors are extreme and the signal goes in the *opposite* direction of the priors (Section 6). As noted in Section 8.A, these latter two biases—prior-biased updating and base-rate neglect—push in opposite directions. A plausible conjecture is that prior-biased updating dominates when the priors are close to

50-50 whereas base-rate neglect dominates when the priors are extreme, but I am not aware of any work that has directly examined how these two biases interact.

10.B. Modeling Challenges

Following Barberis, Shleifer, and Vishny (1998), many models of belief biases have been what Rabin (2013) calls "quasi-Bayesian," meaning that the agent has the wrong model of the world but is fully Bayesian with respect to that wrong model. Of those discussed in this chapter, only the models of the LSN (Rabin, 2002; Rabin and Vanayos, 2010) are quasi-Bayesian. The model of prior-biased updating (Rabin and Schrag, 1999) is closely related; the agent is Bayesian but misreads some of the signals she observes. Quasi-Bayesian and misread-signal models are attractive analytically because the standard machinery for studying Bayesian models can be brought to bear. They are also attractive theoretically because the agent's beliefs are logically consistent (despite being incorrect); as discussed below, logical inconsistencies raise thorny issues that have barely begun to be studied.

The quasi-Bayesian and misread-signal models that have been proposed to date are also examples of what Rabin (2013) calls "portable extensions of existing models (PEEMs)." PEEMs are defined by two properties: (i) they embed the Bayesian model as a special case for particular values of one or more bias parameters, and (ii) they are portable across environments in the sense that the independent variables are the same as for existing models. PEEMs are attractive for a number of reasons. Most relevantly for the discussion here, once the parameters of a PEEM are pinned down by empirical estimates, the model has no degrees of freedom beyond those that are already available in the Bayesian model.

The models of NBLLN (Benjamin, Rabin, and Raymond, 2016), partition dependence (Benjamin, Moore, and Rabin, 2018), base-rate neglect (Benjamin, Bodoh-Creed, and Rabin, 2018), and local thinking (Gennaioli and Shleifer, 2010) are neither quasi-Bayesian models nor PEEMs. The models are not PEEMs because they fail criterion (ii): there is an independent variable that is irrelevant for a Bayesian agent but relevant in the model. In particular, as discussed in Sections 4.C, 5.A, and 6, for the models of NBLLN and base-rate neglect, the grouping of signals needs to be specified in order to pin down the model's predictions (more generally, He and Xiao (2017) show that grouping will matter for *any* non-Bayesian updating rule). For partition dependence, it is the set of bins that is a crucial new independent variable. For the model of local thinking, as discussed in Section 7.C, additional assumptions may be needed to apply it outside the context where it has been formulated.

Because the models have new independent variables that must be specified in applications, the models have degrees of freedom that the Bayesian model does not have. In some cases, these degrees of freedom may not be a problem for studying the model in an experiment because, by framing the judgment problem in a particular way, the experimenter can plausibly control the new independent variables. In applied settings, however, a researcher will often not have such control and may not observe, say, how an agent groups the signals she observes or how she partitions the state space into bins when formulating her beliefs.

When the degrees of freedom are left unspecified, the models are less powerful than the Bayesian model because they rule out fewer possible observations (i.e., assumptions can be made ex post to rationalize what was observed). To turn a non-PEEM into a PEEM,

additional modeling is needed to pin down the values of the free parameters as a function of observable characteristics of the judgment problem.[76] In the cases of NBLLN, partition dependence, base-rate neglect, and local thinking, there is currently little evidence available to guide such modeling. New experiments will be needed to provide that evidence.

Because these models are not quasi-Bayesian, the agent's beliefs are not internally consistent across different framings of the same judgment problem. This is not necessarily a problem in individual decision-making environments as long as the agent always views the problem in the same frame, but it raises the question of what an agent would believe if she views the same problem with different frames over time. Would the agent always use the current frame, despite knowing that she herself had previously thought about the problem differently?

Additional complications arise in environments with strategic interaction between agents. Such environments often require assumptions about higher-order beliefs about agents' biases and framing of the judgment problem, not only what Agent 1 believes about Agent 2 but also what Agent 1 believes Agent 2 believes about Agent 1, etc. A natural assumption is naïveté: Agent 1 believes that other agents make the same predictions and draw the same inferences as she does. But what if Agent 1 knows that other agents frame the information differently (say, Agent 1 observes 20 samples of individual signals but knows that Agent 2 observes the entire sample of 20 signals at once), or if the other agent's behavior is inconsistent with holding the same beliefs as Agent 1? Addressing these and

[76] The same issue arises with other models in behavioral economics, for example, with the reference point in models of reference-dependent preferences. As discussed in Chapter XXX (by O'Donoghue and Sprenger), recent work on loss aversion has devoted substantial attention to understanding how the reference point for gains and losses is endogenously determined.

other questions requires an equilibrium concept that can accommodate belief biases. Chapter XXX (by Eyster) of this Handbook addresses these and related issues in the context of several errors in reasoning, but it does not study the same biases that are the focus of this chapter. There is much fertile ground for new evidence and theory to begin to understand how errors in probabilistic reasoning play out dynamically and in environments with strategic interaction.

10.C. Generalizability from the Lab to the Field

Much of the evidence reviewed in this chapter has been from bookbag-and-poker-chip experiments or similarly abstract laboratory studies. Such studies typically provide the cleanest evidence on errors in probabilistic reasoning because the properties of the random processes and the information provided to participants can be tightly controlled. This control enables researchers to rule out alternative interpretations of apparent belief biases. Yet laboratory evidence is often prone to concerns about generalizability: laboratory behavior may give a misleading impression of how people behave in the field settings that are of primary interest to economists. I will briefly highlight five potentially relevant differences between the typical laboratory environment and the typical field setting that could limit generalizability: incentives, experience, markets, populations, problem structure, and framing.

Grether's (1980) seminal economic experiments on errors in probabilistic reasoning were motivated by questions of whether the biases found in psychology experiments would also be found in settings where participants were incentivized and experienced and where the random processes were made transparent and credible. To

achieve transparency and credibility, Grether (1980) adopted the bookbag-and-poker-chip experimental design and drew balls from urns in front of participants. In addition to testing for deviations from Bayesian updating, he also studied the robustness of these deviations to incentives for correct answers and experience with the same updating problem. In earlier work from psychology, there was also some attention to the effect of incentives (e.g., Phillips and Edwards, 1966) and experience (e.g., Martin and Gettys, 1969; Strub, 1969). Aggregating over findings from many papers, the meta-analysis results from Section 4 suggest that, overall, the presence of incentives in bookbag-and-poker-chip experiments does not eliminate deviations from Bayesian updating. Among papers that examine the effect of experience, a typical finding is that it reduces but does not eliminate bias (e.g., Camerer, 1987). I am not aware of any systematic overview of the effects of experience.

Other groundbreaking, early economics papers in this literature addressed whether deviations from Bayesian updating would persist in experimental asset markets and influence market outcomes (Duh and Sunder, 1986; Camerer, 1987; Anderson and Sunder, 1995; Camerer, 1990). In general, these papers found that base-rate neglect and exact representativeness do influence market prices, although the effects are weak and reduced when experimental participants gain experience (for a brief review, see Camerer, 1995, pp. 605-608). The work has addressed only a few of the many relevant questions that might be asked about markets. For example, one might conjecture that in life insurance markets, where supply-side competition may drive prices to marginal cost (as determined by actuarial tables), in equilibrium belief biases influence quantities rather than prices (who buys insurance).

While much of the laboratory evidence on belief biases to date is from student samples, a number of papers have examined generalizability to other populations. For example, Dohmen, Falk, Huffman, Marklein, and Sunde (2009) found that the GF is widespread in a representative sample from the German population. There is relatively little evidence, however, on how the *magnitude* of biases compares across populations. Since students are often found to be less biased than other, less educated, demographic groups, it seems likely that evidence from student samples understates the prevalence and magnitude of biases. Relevant to the question of how much bias can be expected in particular field settings, some research has studied how individual characteristics are correlated with biases (e.g., Stanovich and West, 1998; Stanovich, 1999). Relatedly, for making predictions about how biases interact, it may be valuable to know how biases are correlated with each other in the population (for some work along these lines, see, e.g., Stango, Yoong, and Zinman, 2017; Falk, Becker, Dohmen, Enke, Huffman, and Sunde, 2018; Chapman, Dean, Ortoleva, Snowberg, and Camerer, 2018).

A longstanding generalizability concern is related to differences in problem structure between the lab and the field. Specifically, people's beliefs may result from heuristics or mental models that are well adapted to real-world problems—i.e., they do not lead to systematic biases in naturalistic environments—but that cause biased responses in the problems posed in the lab. For example, Winkler and Murphy (1973) argued that real-world random processes are typically different from the i.i.d. processes in bookbag-and-poker-chip experiments, e.g., featuring positive autocorrelation and non-stationarity. They argued that in these real-world settings, people update correctly, but when faced with the unfamiliar, artificial i.i.d. settings created in the lab, people behave as they would when

facing real-world random processes. This behavior generates underinference in i.i.d. settings, but researchers would be mistaken to generalize that people underinfer in the field. The force of the problem-structure critique is weakened by a lack of evidence or clear intuition on what the relevant real-world random processes actually looks like (indeed, while Winkler and Murphy posited positive autocorrelation of real-world random processes in order to explain underinference, the GF is sometimes rationalized by arguing that real-world random processes are *negatively* correlated.) Moreover, while experimental participants surely do bring some expectations from their everyday experiences into the lab, the problem-structure critique does not provide a plausible explanation for all of the lab evidence. For example, everyone has enough experience with coin flips to understand what the random process is when told that a fair coin is being flipped, and much of the evidence for the GF and other biases can be (and has been) generated using coin flips. Furthermore, if a particular version of the critique predicts that people form beliefs as if outcomes were generated by a specific, non-i.i.d. (but internally consistent) random process, then it cannot explain why people's beliefs are internally inconsistent, with beliefs depending on the question they are asked (see Section 3.F).

Another generalizability concern is that whether and how people are biased depends on how problems are framed. Most famously, some biases are smaller in magnitude when problems are posed in terms of frequencies rather than probabilities, and frequencies have been argued to be more common in field settings (e.g., Tversky and Kahneman, 1983; Gigerenzer and Hoffrage, 1995).

More generally, the cognitive processes underlying belief formation and revision, such as perception, attention, and memory, plausibly operate differently in natural

environments than they do in abstract settings. For example, people may pay more attention or process information more effectively when they are more familiar with or more interested in the context. Some versions of this concern can be and have been studied in the lab. For example, in a bookbag-and-poker-chip experiment with accounting students as participants, Eger and Dickhaut (1982) found less underinference when the experiment was framed in terms of an accounting problem rather than as an abstract problem. Yet it is not necessarily the case that biases are smaller in more naturalistic settings; for example, Ganguly, Kagel and Moser (2000) found that base-rate neglect was *stronger* in an experimental market when it was framed in terms of buying and selling stocks than in terms of abstract balls and urns.

All of the above evidence notwithstanding, the most compelling response to concerns about generalizability to the field is field studies. Of the topics discussed in this chapter, the GF, the hot-hand bias, and base-rate neglect are relatively well documented in field settings. Most of the other biases in this chapter are in need of more field evidence. For example, could it be that preference-biased updating powerfully influences our political and social beliefs, even if it is difficult to reliably observe in bookbag-and-poker experiments? For biases lacking much field evidence, I urge caution in generalizing from abstract laboratory settings, and as discussed further in 10.E below, I advocate field studies as a high priority.

10.D. Connecting With Other Areas of Economics

In behavioral finance, research on forecasting errors has drawn on the biases reviewed in this chapter. In particular, the LSN, base-rate neglect, and local thinking have

been argued to be leading contributors to extrapolative expectations; see Chapter XXX (by Barberis) of this Handbook.

The relevance of errors in probabilistic reasoning to economics, however, should be far broader. Indeed, as noted at the very beginning of this chapter, belief biases could matter for any context of decision making under risk, including portfolio choice, insurance purchasing, and search and experimentation. Belief biases should also be crucial for research on stereotyping and statistical discrimination, since these can be based on erroneous beliefs (e.g., Bordalo, Coffman, Gennaioli, and Shleifer, 2016; Bohren, Imas, and Rosenberg, 2018). Belief biases should similarly be central to the study of persuasion, since persuaders will aim to exploit the biases of persuadees. Yet these and other areas of economics remain virtually untouched by insights from the literature on belief biases and are thus fertile ground for enterprising researchers.

There are at least two other literatures within economics which, to date, have proceeded almost completely independently from the work reviewed in this chapter despite being closely related. The first is the line of work on sticky expectations (e.g., Gabaix and Laibson, 2002; Mankiw and Reis, 2002) and learning in macroeconomics (see, e.g., Evans and Honkapohja, 2001). Research in macroeconomics may benefit from the accumulated evidence and theorizing about belief biases, and behavioral economists should take on the challenge of explaining key features of macroeconomic beliefs.

The second is the literature on survey measurement of expectations (e.g., Viscusi, 1990; Manski, 2018; Coibion, Gorodnichenko, and Kamdar, forthcoming). Sampling-distribution biases would be especially relevant to that literature, and in particular, the survey literature should be aware of and correct for partition dependence (Section 3.A) and

extreme-belief aversion (Section 5.C). Conversely, experimental research that elicits sampling distributions would benefit from methodological advances in the survey literature, such as modeling and adjusting for measurement error and for rounding of numerical answers (e.g., Giustinelli, Manski, and Molinari, 2018).

10.E. Some Possible Directions For Future Research

To end this chapter, I highlight three directions for future research that seem to me to be especially important. First, although the tradition in behavioral economics has been to focus on one bias at a time, studying several biases at once will often be essential in research on belief biases. Doing so may be necessary to separately identify the biases. For example, partition dependence can be a confound for assessing other sampling-distribution biases, biased inference is often confounded with biased use of prior information, and prior-biased updating and preference-biased updating are often confounded with each other. Studying biases jointly will also be important to assess the robustness of the predictions that arise from one bias to the presence of another bias. For example, as discussed above in Section 10.A, prior-biased updating and base-rate neglect make opposite predictions about whether people will update too much or too little; studying the interaction between the biases will be necessary to understand when one or the other dominates.

Second, the efforts to model belief biases have taught us that some additional evidence is needed as an input to further modeling, and new experiments should collect that evidence. For example, as discussed in Sections 4.C, 5.A, and 6, when modeling how people update after observing a sequence of signals, predictions may hinge on an assumption about how people group the signals. Few experiments to date have addressed

that question (with the exceptions of Shu and Wu, 2003, and Kraemer and Weber, 2004). In many dynamic settings, another important modeling assumption is what people expect about how their own beliefs will evolve if they observe additional signals. Similarly, in strategic interactions, a key assumption is what people believe about how others' beliefs will evolve. Evidence is needed to inform those assumptions, as well.

Finally, the vast majority of evidence on belief biases comes from laboratory studies; more field evidence is needed to probe generalizability (as discussed in Section 10.C) and to assess the economic importance of the biases. Most existing field evidence is from gambling (e.g., Metzger, 1985), lotteries (e.g., Clotfelter and Cook, 1993), and sports (e.g., Gilovich, Vallone, and Tversky, 1985), environments where the true probabilities are known or can be reliably estimated and where data have long been publicly available. However, recent work has begun to examine other settings. For example, Chen, Moskowitz, and Shue (2016) studied reviews of loan applications and judges' decisions in refugee asylum court (in addition to umpires' calls on baseball pitches), and Augenblick and Rabin (2018) studied how beliefs evolve over time in prediction markets. As has occurred with other areas of behavioral economics, once it becomes clear that errors in probabilistic reasoning matter in a range of economically relevant field settings, this area of research will become part of mainstream economics.

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Table 1. Experimental participants' mean beliefs for each bin (from Benjamin, Moore, and Rabin, 2018)

Experiment 1 (convenience sample of 104 adults)

	Number of heads out of 10 flips											
Partition	0	1	2	3	4	5	6	7	8	9	10	Sum
(A)	6.1%	6.4%	8.0%	9.0%	12.3%	20.0%	12.7%	8.9%	7.3%	6.5%	2.7%	100%
(B)	18.3%				21.5%	28.1%	18.3%	13.8%				100%
(C)	33.9%					36.2%	29.9%					100%
(D)	18.0%	36.0%	35.9%	36.7%	38.2%	39.4%	37.7%	34.2%	29.7%	27.9%	11.1%	345%

Experiment 2 (308 undergraduates)

	Number of heads out of 10 flips											
Partition	0	1	2	3	4	5	6	7	8	9	10	Sum
(A)	2.2%	3.8%	5.5%	9.3%	15.1%	28.3%	14.9%	9.2%	5.5%	3.9%	2.4%	100%
(B)	15.9%				18.3%	32.1%	18.1%	15.6%				100%
(C)	34.0%					32.9%	33.2%					100%
(D)	4.3%	6.7%	11.8%	16.6%	26.4%	34.3%	24.7%	17.4%	11.9%	6.6%	3.9%	164.7%

Table 2. Regression of Participants' Log-Posterior-Odds on Bayesian Log-Posterior-Odds

	(A) Simultaneous		(B) Sequential	
	(1) All data	(2) Only incentivized	(1) All data	(2) Only incentivized
$\ln(p(S \mid A) / p(S \mid B))$	0.201 (0.063)	0.383 (0.028)	0.349 (0.025)	0.528 (0.018)
Constant	0.029 (0.087)	-0.064 (0.089)	0.153 (0.055)	0.062 (0.037)
R^2	0.462	0.764	0.808	0.965
#obs	147	76	111	43
#papers	14	6	5	2

Notes: Panel A: restricted to updating problems with equal priors. Panel B: restricted to updating problems with equal initial priors, and log-posterior-odds are calculated from final posteriors. Heteroskedasticity-robust standard errors in parentheses.

Table 3. Regression of Participants' Log-Log-Posterior-Odds on Features of the Observed Sample

	(A) Simultaneous			(B) Sequential		
	(1) All data	(2) All data	(3) Only Incentivized	(1) All data	(2) All data	(3) Only Incentivized
$\ln N$	0.411	0.412	0.562	0.773	0.771	1.024
	(0.049)	(0.050)	(0.082)	(0.056)	(0.055)	(0.071)
$\ln\left(\frac{2N_a - N}{N}\right)$	0.848	0.850	0.870	0.805	0.804	0.829
	(0.071)	(0.075)	(0.117)	(0.073)	(0.073)	(0.070)
$\ln\ln\left(\frac{\theta}{1-\theta}\right)$	0.394	0.395	0.515	0.640	0.643	1.275
	(0.082)	(0.082)	(0.097)	(0.151)	(0.149)	(0.480)
$I\left\{\frac{N_a}{N} = \theta\right\}$		0.022			0.269	
		(0.086)			(0.149)	
Constant	-0.052	-0.054	-0.120	-0.610	-0.620	-0.726
	(0.080)	(0.082)	(0.104)	(0.096)	(0.095)	(0.151)
R^2	0.631	0.631	0.648	0.713	0.720	0.895
#obs	147	147	76	111	111	43
#papers	14	14	6	5	5	2

Notes: Panel A: restricted to updating problems with equal priors. Panel B: restricted to updating problems with equal initial priors, and log-log-posterior-odds are calculated from final posteriors. States *A* and *B* are labeled so as to maximize the number of observations included in the regression; see footnote 33. Heteroskedasticity-robust standard errors in parentheses.

Table 4. Regression of Participants' Log-Posterior-Odds Adjusted for Inference Biases on Log-Prior-Odds

	(1) All data	(2) Only unequal priors	(3) Only incentivized
$\ln\left(p(A)/p(B)\right)$	0.601 (0.066)	0.601 (0.066)	0.434 (0.086)
Constant	0.064 (0.039)	0.120 (0.066)	0.149 (0.053)
R^2	0.321	0.398	0.145
#obs	296	149	167
#papers	15	7	6

Notes: Simultaneous-sample updating problems only. Heteroskedasticity-robust standard errors in parentheses.

Figure 1a. Sample-size neglect for binomial with rate $\theta = 0.5$ (from Kahneman and Tversky, 1972)

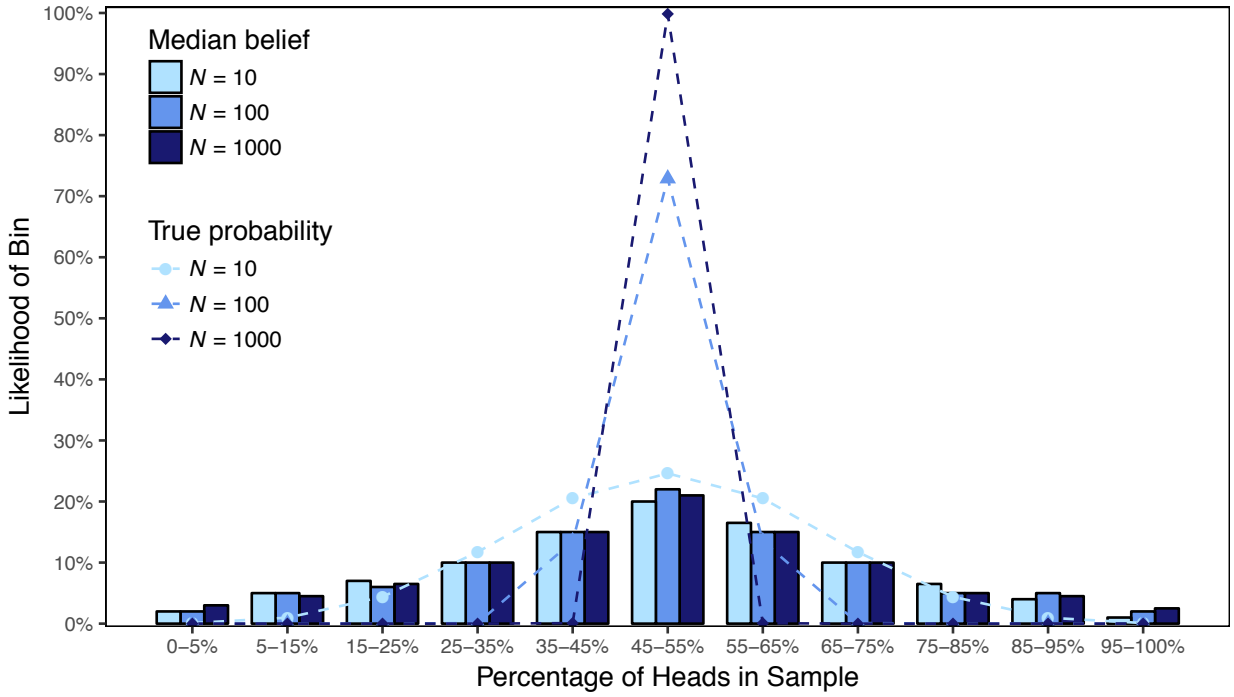


Figure 1b. Sample-size neglect for binomial with rate $\theta = 0.8$ (from Kahneman and Tversky, 1972)

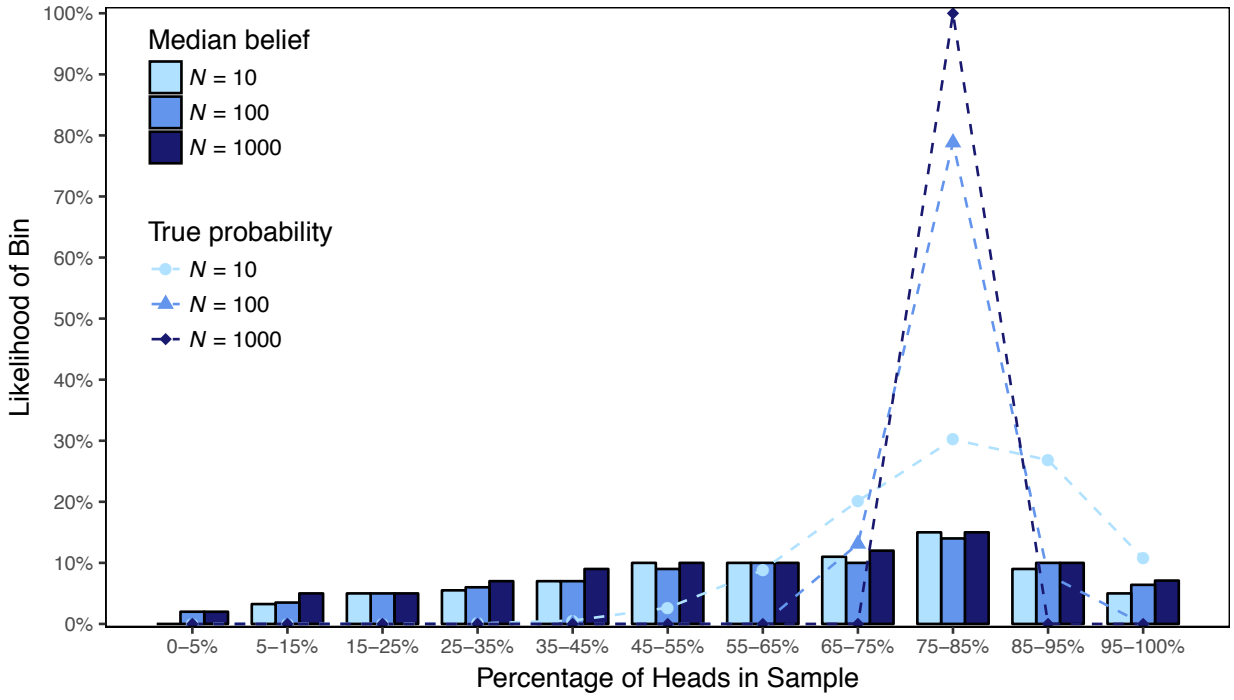
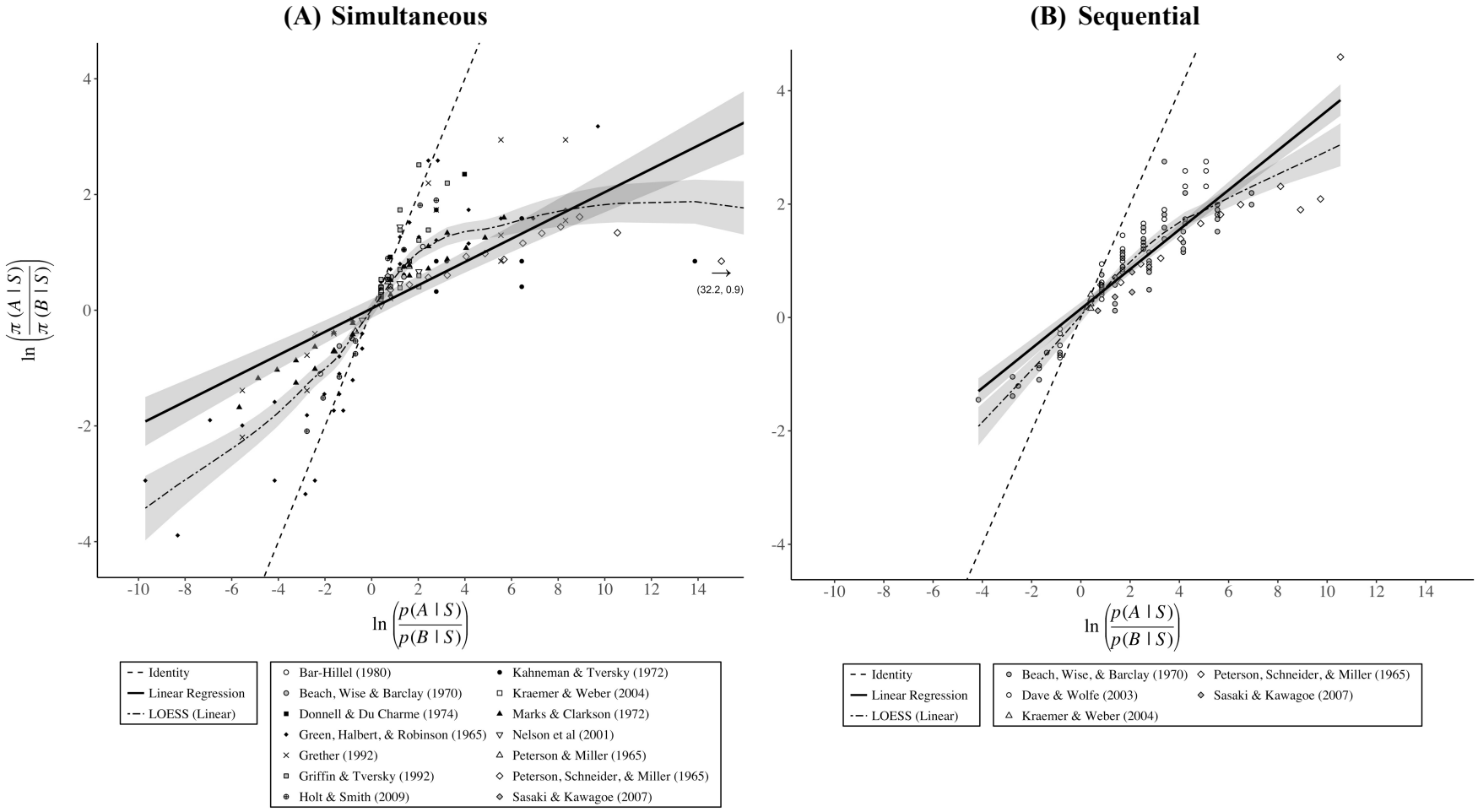
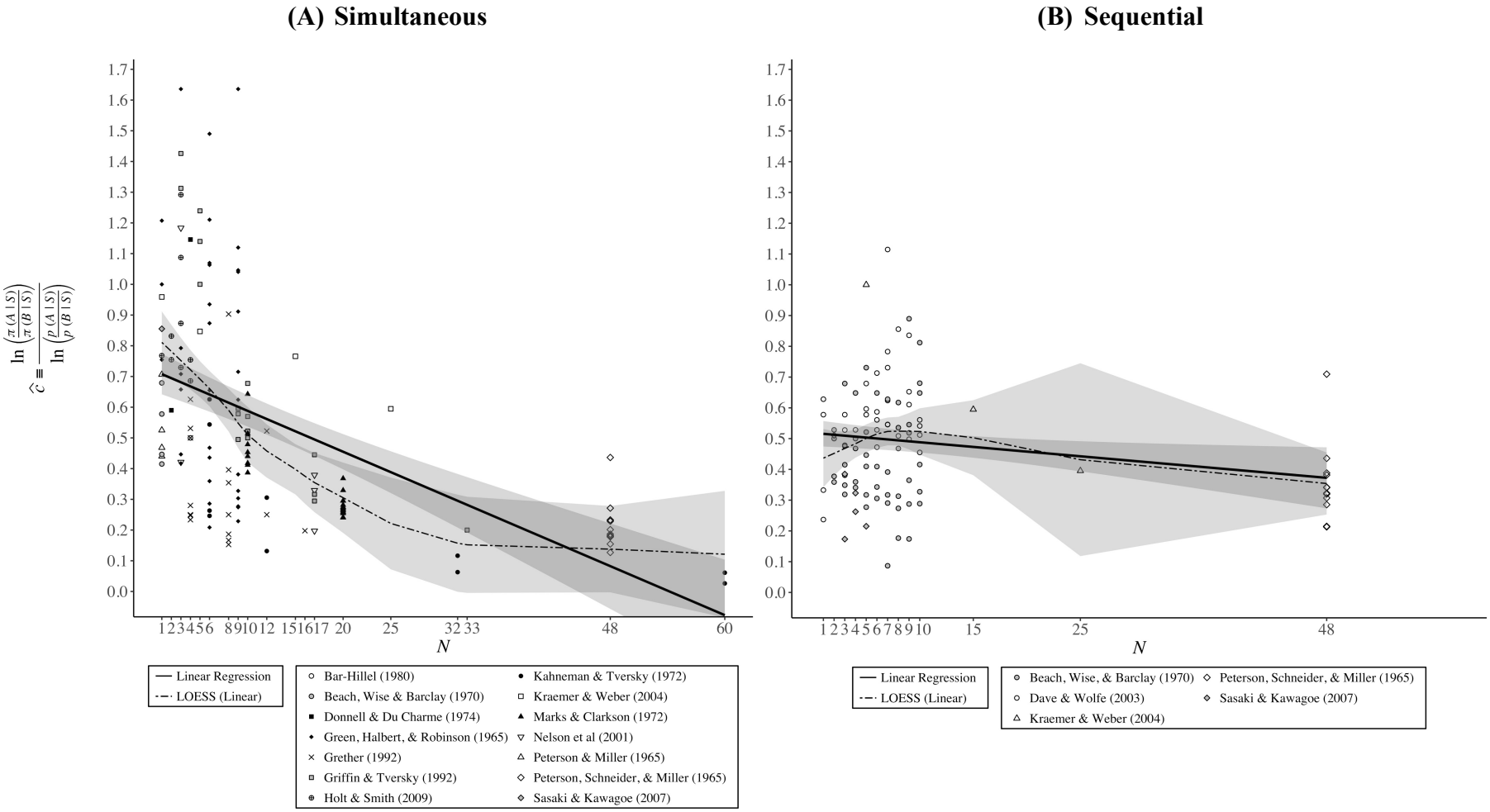


Figure 2. Participants' Log-Posterior-Odds versus Bayesian Log-Posterior-Odds



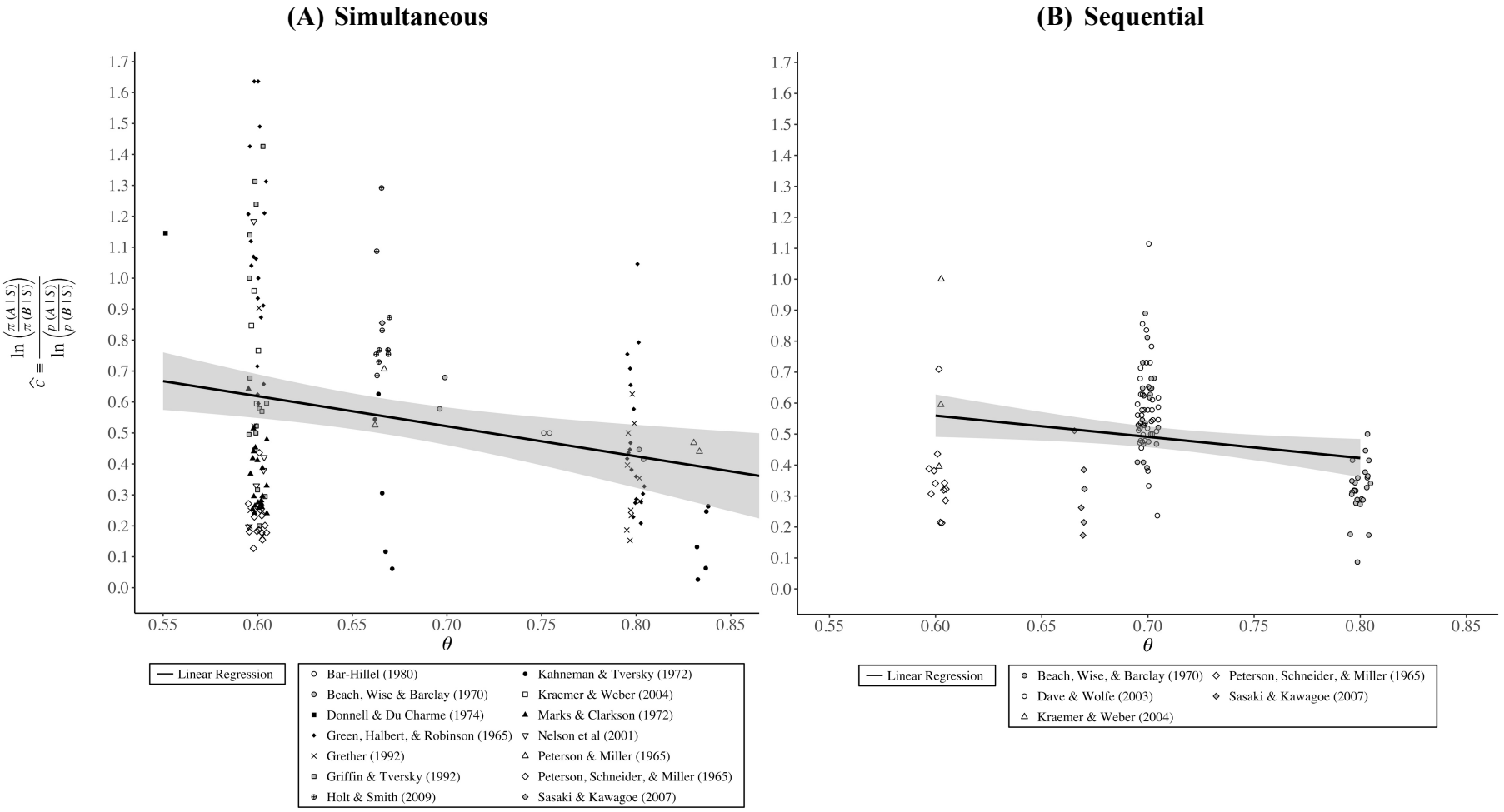
Notes: Panel A: restricted to updating problems with equal priors. Panel B: restricted to updating problems with equal initial priors, and log-posterior-odds are calculated from final posteriors. LOESS is implemented in R with a span of 0.75. Shaded regions are 95% confidence intervals.

Figure 3. Inference Measure $\hat{c}$ versus Sample Size N



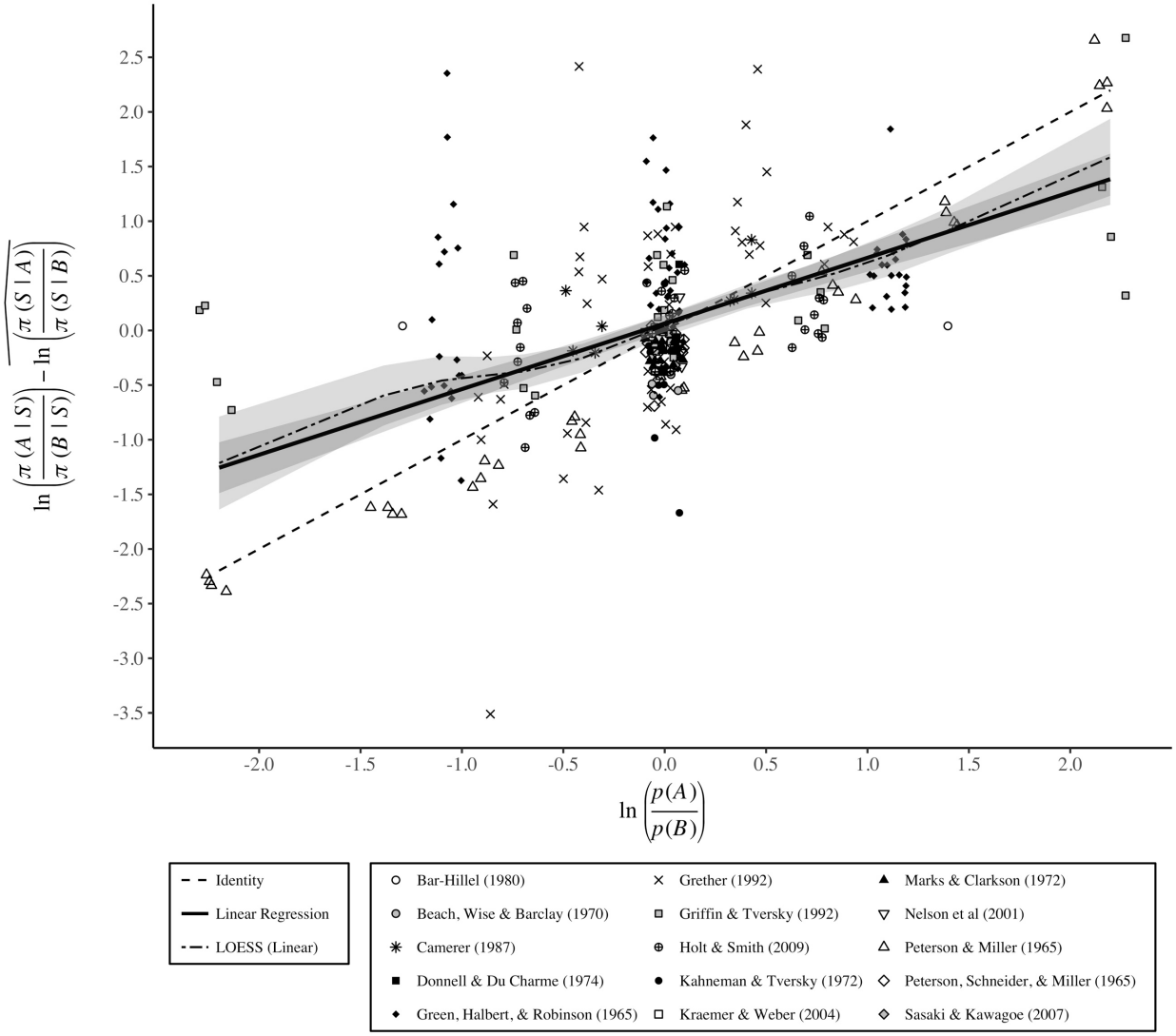
Notes: Panel A: restricted to updating problems with equal priors. Panel B: restricted to updating problems with equal initial priors, N refers to final sample size, and log-posterior-odds are calculated from final posteriors. LOESS is implemented in R with a span of 0.75. Shaded regions are 95% confidence intervals.

Figure 4. Inference Measure $\hat{c}$ versus Diagnosticity θ



Notes: Panel A: restricted to updating problems with equal priors. Panel B: restricted to updating problems with equal initial priors, and log-posterior-odds are calculated from final posteriors. Shaded regions are 95% confidence intervals.

Figure 5. Participants' Log-Posterior-Odds Adjusted for Inference Biases versus Log-Prior-Odds



Notes: Simultaneous-sample updating problems only. LOESS is implemented in R with a span of 0.75. Shaded regions are 95% confidence intervals.