

Washington University in St. Louis Washington University Open Scholarship

All Theses and Dissertations (ETDs)

Winter 1-1-2012

Epistemic Vigilance: The Error Management of Source Memory and Belief

Brian Bergstrom

Washington University in St. Louis

Follow this and additional works at: <https://openscholarship.wustl.edu/etd>

Recommended Citation

Bergstrom, Brian, "Epistemic Vigilance: The Error Management of Source Memory and Belief" (2012). *All Theses and Dissertations (ETDs)*. 995.

<https://openscholarship.wustl.edu/etd/995>

This Dissertation is brought to you for free and open access by Washington University Open Scholarship. It has been accepted for inclusion in All Theses and Dissertations (ETDs) by an authorized administrator of Washington University Open Scholarship. For more information, please contact digital@wumail.wustl.edu.

WASHINGTON UNIVERSITY IN ST. LOUIS

Department of Psychology

Dissertation Examination Committee:

Pascal Boyer, Chair

Richard Abrams

Carl Craver

Janet Duchek

Henry L. Roediger, III

James Wertsch

Epistemic Vigilance: The Error Management of Source Memory and Belief

by

Brian Bergstrom

A dissertation presented to the
Graduate School of Arts and Sciences
of Washington University in
partial fulfillment of the
requirements for the degree
of Doctor of Philosophy

December 2012

St. Louis, Missouri

TABLE OF CONTENTS

Acknowledgements	iii
Abstract	iv
Introduction	1
Why the Source Matters: Epistemics in Evolutionary Perspective	4
Coalition in Evolutionary Perspective	7
Source Processing in Social and Cognitive Psychology	10
The Sleeper Effect	10
Source Monitoring	16
Development of Trust	17
How Mental Systems “Believe”	19
Scope & Rationale of the Proposed Research	21
Pilot Experiment 1	24
Pilot Experiment 2	29
Pilot Experiment 3	33
Introduction to Dissertation Experiments	36
Experiment 1: Recalling the Source	37
Experiment 2: Source Valence	54
Experiment 3: Precaution 1	63
Experiment 4: Precaution 2	68
General Discussion	72
References	78
Appendices	81

ACKNOWLEDGEMENTS

The development of these studies emerged over many discussions with Pascal Boyer, who has been (and continues to be) a remarkable mentor. Not only a steadfast source of intellectual and academic guidance, he has been a gracious collaborator and wellspring of open-minded creativity. It is through his abiding patience and understanding that this dissertation reached completion, and I extend to him, with respect and admiration, my deepest gratitude.

I am grateful to Washington University for a dissertation fellowship that provided financial support during the production of this dissertation, and to Deanna Barch for helping me pave the path to completion.

Finally, I wish to thank my family for their unyielding moral support over the years. And to Monica (M), with whom the road now leads onward, and with whom my education never ends. *Amhrán duit.*

ABSTRACT OF THE DISSERTATION

Epistemic Vigilance: The Error Management of Source Memory and Belief

by

Brian Bergstrom

Doctor of Philosophy in Psychology

Washington University in St. Louis, 2012

Professor Pascal Boyer, Chairperson

The studies reported here explore some of the cognitive contours of *epistemic vigilance*—that is, how the mind adaptively tracks and monitors information. These studies ask how the mind considers the *source* of new information when making epistemic judgments. Four experiments investigate the role of source information in modulating the “truth bias” (an automatic tendency to presume new information is true). An evolutionary hypothesis is advanced: that a “support bias” exists for sources with special coalitional relevance (e.g., a best friend) that interacts with the truth bias to produce supportive patterns of recall that are unique to that source. Results partially support this hypothesis. Participants who read statements ostensibly from a real best friend (identified by participants) show patterns of recall that differ from recall for statements from other sources, and these patterns are consistent with a “support bias” interpretation. These results may be influenced by valence; using sources who are positive and negative (but not coalitional) also elicit modified patterns of recall—but *not* at the level of automatic belief. Finally, when the communicated content is precautionary information, evidence for the truth bias and the support bias disappears. The need for additional study of source-processing, and its relationship to statement structure and content, is discussed.

Introduction

Consider the following scenarios.

While driving to work, your car radio is tuned to NPR and you are learning about a new political development in the African nation of Sudan when, suddenly, the driver in front of you stops unexpectedly for a yellow light. You slam on your brakes and narrowly avoid impact. While waiting for your racing pulse to abate, the light turns green, NPR moves on, and Sudan is no longer on your mind.

Later that morning, you skim the news updates in *Science* and *Nature*. Your inbox announces the arrival of an important email, and your attention is diverted to an urgent matter for the next hour.

During lunch, a colleague relays to you a piece of historical trivia. Your cell phone rings with a call you have to take. When lunch resumes, conversation shifts and the trivia recedes into the past.

Now if at some future time you reencounter some of the information that was interrupted earlier (Sudan, *Science*, trivia), would you believe it? More specifically, would the interruption have affected your judgment of its truth? Would you remember where or from whom the information came? And if you did, would the source of the information affect your judgment of truth?

This last question represents the target inquiry of the present research, and the thinly-veiled hypothetical scenarios mark its pertinence to everyday affairs. Existing research on the cognitive dynamics of belief has suggested preliminary answers to how we ascertain truth under

conditions of compromised processing, but the question of how belief is affected by *source information*—from where or from whom our information derives—has not been fully addressed.

The line of research most relevant to the present inquiry, and which informs the empirical approach of the present studies, is the work of Dan Gilbert and colleagues on belief formation, or what might be called “default belief” (Gilbert, 1991). The basic question of his research has been how the mind treats new information during the very earliest stages of processing: that is, how the mind registers the “truth status” of new information as it is being comprehended, before any explicit judgment can be rendered. An interest in this early stage of belief formation is analogous to the real-world example of reading a news statement on the Internet and then having one’s attention eclipsed by an interrupting email. Before we have time to evaluate the new information, does the mind automatically assume it to be true (or false), or does the mind instead hold the information in a neutral state of agnostic uncertainty (until we can later assess it more scrupulously)?

Gilbert casts the origin of this question back several centuries to the philosophies of Rene Descartes and Baruch Spinoza, to their differing opinions on the question of whether it is possible to hold an idea or piece of information in a suspended state of nonbelief (i.e., a state where one ascribes neither truth nor falsity)—that is, whether we can “consider an idea” without “considering it so” (Gilbert et al., 1990). Descartes held the opinion that new information could be treated impartially, could be *comprehended* but not necessarily endorsed or rejected until one’s reasoned judgment was brought to bear. Spinoza, however, held the opinion that the very act of comprehending a statement included an initial assent, a tacit epistemic acceptance of the information as true which would persist unless deliberately revised (Gilbert, 1991).

Gilbert's research program aimed to assay these conjectures on original belief empirically, to know via experimental control whether or not the mind automatically assumes that new information is true when first encountered. Toward this end, the general protocol adopted (e.g., Gilbert et al., 1989; Gilbert et al., 1993) was a laboratory arrangement in which participants could (1) encounter new information (2) about which they had little or no background knowledge, and (3) occasionally be denied (via interruption) the time and attentional resources to think deeply about the content presented. The rationale for the third condition was that interruption, introduced at the right moment, could allow participants to read (comprehend) a new piece of information but then prevent them from thinking deeply about it (evaluate). By controlling a subject's exposure to the information and their processing of it, one can later measure subjects' impressions of these interrupted statements (i.e., whether they seem true or false) and gain insight into the cognitive presuppositions about which Descartes and Spinoza could only speculate. That is, one could assess the "default belief" of information that had been *comprehended* but not *evaluated*.

The results of these studies (discussed in greater detail below) give pride of place to Spinoza by supporting his suggestion that we "believe first," and only later explicitly affirm or reject our initial presuppositions (Gilbert et al., 1989; Gilbert et al., 1993). However, these studies have notably lacked any inclusion of *source information* (a communicator or other epistemic origin through which information is received), and this marks a substantial departure from how information is normally encountered. The new information we daily encounter almost always has a discernible source, and the goal of the present dissertation studies is to use the experimental protocol established by Gilbert to begin exploring the source memory and source monitoring questions with which we began. More specifically, the studies here explore the

possibility that the influence of source information on belief follows causal pathways of evolutionary relevance, reflecting selection pressures that acted on important epistemic differences among ancestral communicators.

Why the Source Matters: Epistemics in Evolutionary Perspective

Humans are by nature a social species, and (more importantly) a species where the vast majority of acquired knowledge is procured via innumerable communicative transactions between individuals, rather than wrought from direct individual experience (Tooby & Devore, 1987). Thus, for humans, information almost always comes *from* somewhere, typically *someone*. Even in contemporary environments dominated by electronic and print media such as journals, newspapers, television and the internet, the cultural transmission of information rarely omits information about authors or fails to provide traceable references, and media sources themselves can be construed as individual sources (e.g., MSNBC, FOX News, etc.), despite of course comprising the work of many distinct individuals.

However, for a species so unequivocally reliant upon others for information, the prospect of malicious deception is—and has been—a serious threat to evolutionary fitness (Cosmides & Tooby, 2000). Communicative interactions involve individuals with distinct identities, histories, motives, intentions, affiliations, and epistemic profiles (age, trustworthiness, background knowledge, reliability, and so on). On an evolutionary view, an individual who possessed complex language without also possessing some sensitivity to the epistemic differences among communicators would not have fared well by the measure of natural selection, because the potential dangers of misinformation are many and great, and the fitness consequences immediate: a rival who told you the green berries were safe to eat may be one rival less by morning.

As such, though many scholars have touted the enormous practical and adaptive utility of the language faculty (e.g., Deacon, 1997; Pinker, 1994), others have drawn attention to the cognitive difficulties and computational vulnerabilities that are inexorably linked to language (e.g., Bergstrom, Moehlmann, & Boyer, 2005; Cosmides & Tooby, 2000; Sperber, 2006). Without some form of *epistemic vigilance*—means of adaptively managing information to guard against error, false-believe, and deception—subjective knowledge stores would become quickly corrupted, and fitness quickly compromised. A language-wielding organism needs cognitive mechanisms that help guard against deceptive communication from peers or enemies by taking the reliability and knowledge of a speaker into account, updating old inferences in light of new information or special circumstance, filtering out useless information, and applying information in appropriately selective, conditional, or probabilistic ways. Psychological designs that failed to respond to these vulnerabilities would have exhibited poor biological fitness and been replaced by more efficient, reliable, consistent, and economical designs (Williams, 1966).

Sperber (2006) has suggested that the evolution of language occurred as an arms race between communicators and addressees, each with imperfect motives and knowledge, where the dynamics of truth versus untruth, and trust versus distrust, continually supplied selection pressure for cognitive mechanisms that enabled increasingly refined and savvy communication. Though “arms race” terminology fosters a sense of antagonism, linguistic communication eventually stabilized among humans, because contemporary humans (descendants of the arms race) do of course engage in give-and-take communicative practices. If the costs of language communication had outstripped its gains, the evolution of language would have stalled in its tracks. This does not mean that the dangers of exploitation are any less consequential simply because language managed to evolve. Instead, as Sperber (2006) notes, “if communication has

stabilized among humans, it must be that there are ways to calibrate one's confidence in communicated information so that the expected benefits are greater than the expected costs" (p. 182). Calibration must be the expected essence of the cognitive design, because no general rule to "trust" or "distrust" information would be feasible as a uniform epistemic tactic (that is, game theoretical models show that language could not have evolved if ancestral humans had simply adopted an "always trust" or "always distrust" approach to communication). The necessity of calibration—regulating trust according to communicator, circumstance, and experience—is a grounding principle of the present research.

One of several possible ways to calibrate trust for a given communicator is to rely on a heuristic of "known benevolence" (Sperber, 2006). For example, one can typically trust one's relatives more than strangers, and one's friends more than one's enemies. Such a heuristic is imperfect, of course, but alongside other tuning strategies can be useful. This is the strategy explicitly considered in the present research: exploring whether the epistemic processing of information is different when it comes from coalitionally-relevant sources ("friends" and "enemies") than when it comes from sources without coalitional relevance. All else being equal, over the long course of language evolution, it would be unlikely in the extreme that natural selection would prove indifferent to "known benevolence" as a cognitive parameter operating within a cognitive ensemble of error-management operators. The basic goal of this dissertation is to investigate the possibility that the cognitive operations governing the ascription of belief to new information also take into account the *source* of that information, even at the very earliest stages of information-processing previously researched by Gilbert and colleagues, and do so for reasons grounded in the evolutionary intersection of coalition and communication.

Coalition in Evolutionary Perspective

To appreciate the theoretical importance of evolved psychological mechanisms for epistemic vigilance, it is critical that one consider the Machiavellian picture of ancestral sociality painted by anthropological and archeological research. These studies reveal constant warfare *between* groups (e.g. combatant tribes), and also reveal constant cooperation, exchange, status and power struggles, alliance management, and coalitional fluctuations *within* groups. In short, language evolved in a context of incessant social chess where group opportunities and threats abounded from within and without, and where considering the source of acquired information would have paid substantial inferential dividends. Archaeological evidence points unambiguously to chronic warfare in ancestral environments (Keeley, 1996; LeBlanc, 2003), and studies of primate species closely related to humans also illustrate the importance of coalitional alliances in social life (e.g., de Waal, 1982; Byrne & Whitten, 1988). Trustworthiness, cooperativeness, and a good reputation were important factors for ancestral humans struggling with interdependent coordination (Kurzban & Neuberg, 2005; Neuberg & Cottrell, 2008), and a cognitive system designed to calibrate trust and belief among different sources would have proved decisive in successfully negotiating the social arena. As Kurzban & Neuberg (2005) note: “Any given [socio-linguistic] interaction ... carries opportunity costs, as limits on the size of an individual’s social network mean that a particular social interaction precludes some others. . . . [W]e should expect humans to exhibit *discriminate sociality* and to possess psychological mechanisms designed to preserve the benefits of sociality and simultaneously limit its costs” (p. 653).

Studies of social cognition reveal evidence of automatic and dynamic encoding of coalitional information (Kurzban, Tooby, & Cosmides, 2001; see also Cosmides, Tooby, &

Kurzban, 2003), selective fear-conditioning to facial images of out-group members (Navarrete, Olsson, Ho, Mendes, Thomsen, & Sidanius, 2009), domain-specific reasoning capacities geared for cheater-detection and social exchange (Cosmides & Tooby, 1992; Cosmides & Tooby, 2005), and selective trust, based on in-group/out-group affiliation (Foddy, Platow, & Yamagishi, 2009). These lines of evidence converge on a picture of ancestral sociality that was replete with social challenges (Buss, 2005; Daly & Wilson, 1988). With virtually all social contact among humans mediated by the use of language (Barkow, 1992), where people engage in “linguistic grooming” rather than the “physical grooming” that is more phylogenetically common among primates (Dunbar, 1990), emergent psychological designs for epistemic vigilance may have been crucial to managing linguistic interactions, and avidly favored by natural selection. On the grounds of such evolutionary reasoning, then, we expect the human mind to exhibit design features tailored to successfully navigating the kinds of socio-linguistic-epistemic challenges that were routinely faced under ancestral living conditions. Among these designs may be operations that automatically bias belief depending on the source, even prior to conscious or deliberate analysis, in a manner similar to Gilbert’s earlier findings. We suspect that “coalition” (operationalized in the present studies as robust patterns of friendship and antagonism) may mark a decisive source distinction that carries early processing consequences.

How might such designs evolve? Heuristics and biases can evolve according to “error management” logic. On this view, when the fitness costs of inferential errors in a particular domain are asymmetrical (i.e., Type I errors have more dire consequences than Type II errors, or vice versa), this can prompt natural selection to favor *adaptively biased* cognitive designs (Cosmides & Tooby, 1994; Haselton & Buss, 2000, 2003; Haselton, Nettle, & Andrews, 2005). For example, over the course of human evolution, the *over*perception of snake-like stimuli *as*

snakes would have had adaptive utility, because the evolutionary cost of mistaking a stick in the grass for a snake were fleeting and trivial, but the reverse error of mistaking a snake for a stick would, on average, have had much greater adaptive cost. This asymmetry in the fitness costs of different cognitive errors can lead natural selection to engineer bias into the cognitive apparatus (in this case, perceptual bias within the visual system). Importantly, even if that bias led to a greater number of total errors, if it led to lower adaptive cost, it could be favored. If the ideal neurological designs for optimal evolutionary fitness and perfect cognitive accuracy diverge even slightly, the biological bottom-line of reproductive fitness will nudge our experience of reality accordingly.

The research presented here explores the possibility that in the course of human evolution, natural selection may have favored the evolution of cognitive processes designed for *epistemic vigilance*, specifically designs that in their proper deployment serve “error management” coalitional functions in the realm of belief. We suspect that distinctive patterns of recall error in judgments of truth will be found for different sources of information, and that these differences will be functionally rooted in coalitional differences relevant to participants.

The outcome of these inquiries may prove relevant to any social affairs that tap coalitional dynamics: race relations, conflicts between religious affiliations, political factions, and much more. They may help determine whether ostensibly *epistemic* judgments may sometimes be a matter of *coalitional* judgment. If source differences are relevant to automatic belief, then some “errors” of recall may not be evidence of irrationality, nor evidence of limited processing capacity, but instead reflect evolved biases that served adaptive functions over evolutionary history. Furthermore, to the extent that belief processing *is* influenced by coalitional factors, other experimental manipulations which alter coalitional dynamics, such as

the influence of synchrony on cooperation (Wiltermuth & Heath, 2009), or the influence of affiliation cues on racial encoding (Kurzban, et al., 2001) may also serve as manipulations affecting belief formation.

Source Processing in Social and Cognitive Psychology

Queries regarding the general nature of *source processing* (a term of broad construal) have been carried out through a number of research programs—e.g. source monitoring, developmental studies of trust, and attitude/persuasion research (all discussed below)—and provisional paths of understanding are being charted. These related programs of research will be discussed to provide a larger empirical and historical context for the present research, and to distinguish the present experiments from this extant work.

The Sleeper Effect

Several decades of attitude and persuasion research under the rubric of the “sleeper effect” have examined the relevance of source information to how we calibrate opinions and attitudes. The sleeper effect has traditionally referred to a phenomenon whereby one’s attitude or opinion regarding a certain topic or argument (e.g., propaganda, politics, advertisements) changes over time (see Hovland & Weiss, 1951; Kelman & Hovland, 1953; for a review see Kumkale & Albarracín, 2004). More specifically, the effect denotes an attitude or opinion that becomes more congruent with an argument, even though one’s initial attitude or opinion was less congruent (or “discounted”) in virtue of its association with a dubious source (see Figures 1 and 2) (e.g., Cook, Gruder, Hennigan, & Flay, 1979; Gillig & Greenwald, 1974; Hovland & Weiss, 1951; Kelman & Hovland, 1953; Kumkale & Albarracín, 2004; Pratkanis, Greenwald, Leippe, & Baumgardner, 1988; Sitton & Griffin, 1980; Underwood & Pezdek, 1998; Weiss, 1953). For example, after reading about the benefits of a newly marketed product, your opinion of that

product might be skeptically low if the benefits of that product were promoted by the company that created it (i.e., your opinion would be discounted because of the source, and show low congruity with the advertised claim), but when queried weeks later, your opinion of that product would likely become more favorable (i.e., is no longer discounted, with a higher congruity between your opinion and the original advertisement). Ostensibly, this occurs because your opinion is no longer offset or discounted by a consideration of the source of the advertisement, either because the source has been forgotten, or is simply not retrieved at the time of judgment. The “sleeper” is thus your opinion, which with silent stealth gravitates toward greater agreement over time.

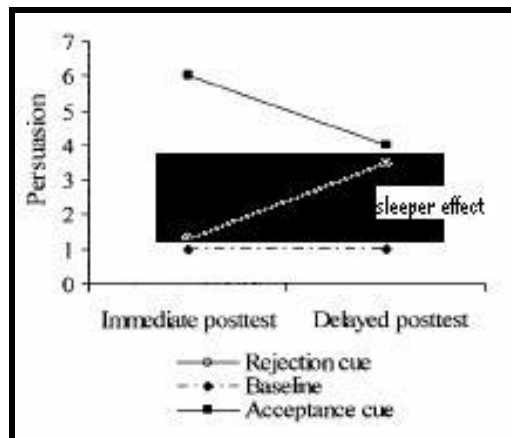


Figure 1. The sleeper effect (from Kumkale & Albarracín, 2004).

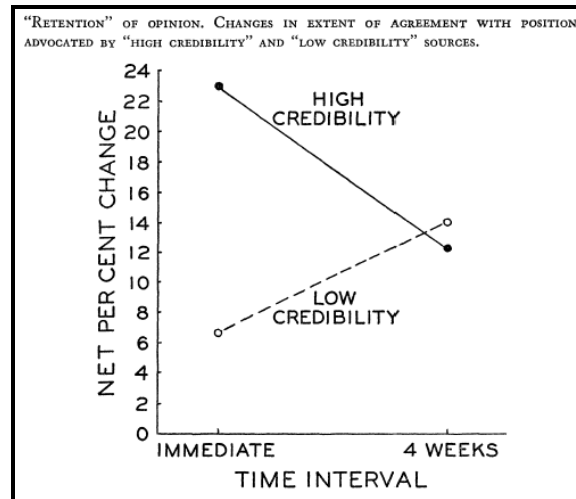


Figure 2. Original “sleeper effect” (from Hovland & Weiss, 1951).

Studies of the sleeper effect have typically placed a premium on ecological validity. Investigators have often eschewed artificial or fictional sources and stimuli in favor of information from real-world individuals, publications, and media. Indeed, the earliest studies used pilot surveys to assess participants’ source knowledge days or weeks prior to study, and then carefully matched sources to plausible content (e.g., Hovland & Weiss, 1951; Kelman & Hovland, 1953). These studies paired high-credible or low-credible sources with identical message content, and then documented the effect of these different sources (along with other variables) on subsequent attitudes or opinions.

While research on the sleeper effect reliably shows that people do consider the source of information (at least initially) and use that information to adjust their attitudes or opinions, there are a variety of important reasons to consider the sleeper effect an imprecise phenomenon and an inadequate gauge of how source information influences judgments of a statement’s veracity.

First, two different kinds of sleeper effect have been identified—“absolute” and “relative” sleeper effects (see Figures 3 and 4)—and these two types are associated with no less than five characteristic patterns of data (graphically illustrated in Figures 3 & 4), *all* of which are considered legitimate sleeper effects (for a review, see Kumkale & Albarracín, 2004). Any

effect whose purview has grown to encompass so many distinct signatures in the data should elicit concern about the unity of the phenomenon under investigation, and by extension, scrutiny is justified regarding the processes implicated in their production.

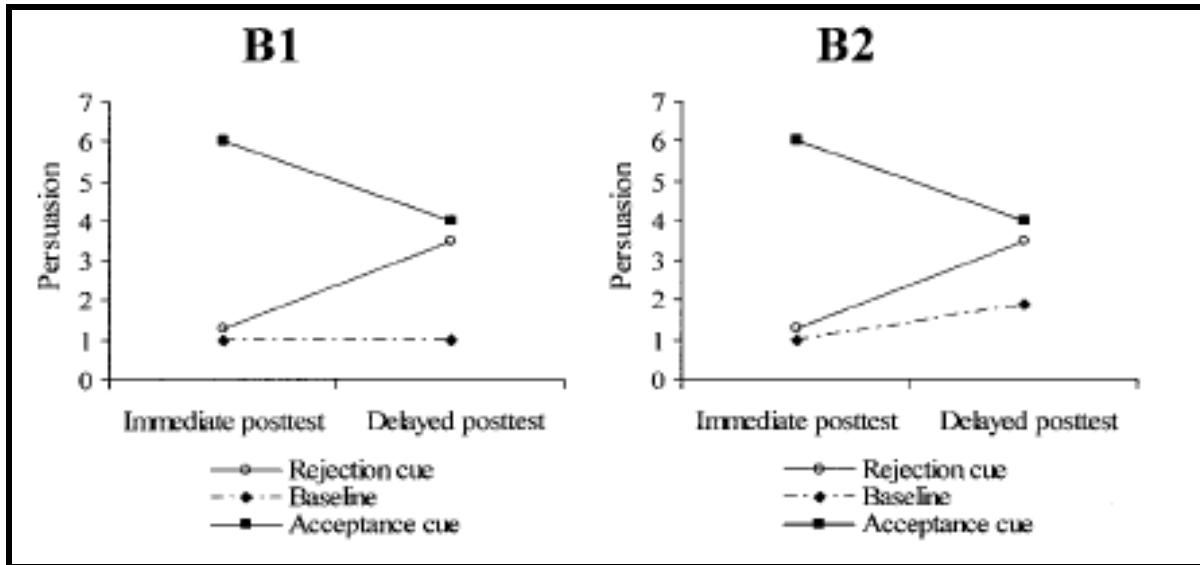


Figure 3. Examples of the “absolute” sleeper effect (from Kumkale & Albarracín, 2004).

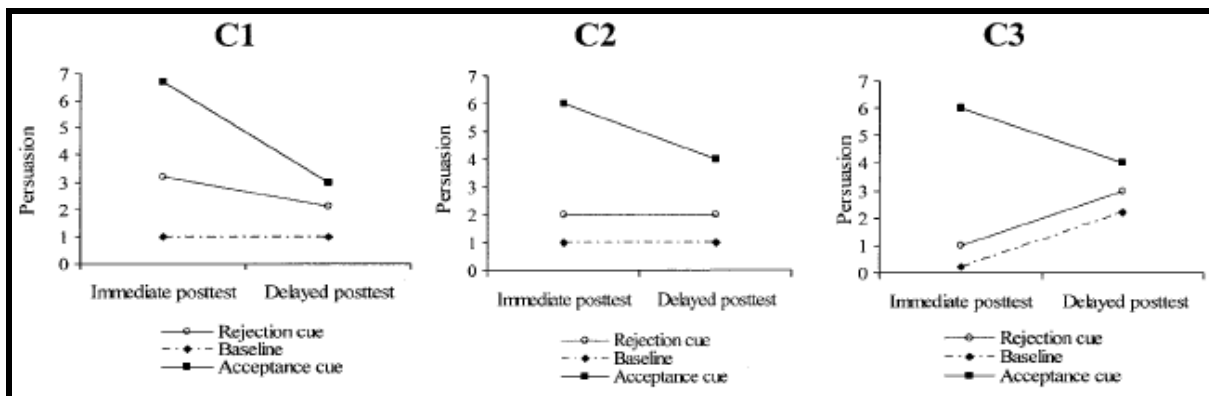


Figure 4. Examples of the “relative” sleeper effect (from Kumkale & Albarracín, 2004).

Second, efforts to delineate the conditions under which a sleeper effect can be reliably produced have shown that sufficient conditions are both very specific and highly contrived (e.g., Pratkanis et al., 1988). Evaluation of these conditions suggests that producing a sleeper effect requires: (a) a very interesting and persuasive message, (b) a highly non-credible source, (c) the presentation of source information *after* (as opposed to concurrently or prior to) the presentation

of the message, and (d) a considerable delay between measures (normally 1-8 weeks, though some experimental protocols have shortened this delay by increasing the number of items presented, thereby simulating memory degradation over time; see Gruder, Cook, Hennigan, Flay, Alessis, & Halamaj, 1978; Pratkanis et al., 1988). Examination of these enabling factors has shown that motivation, source credibility, timing of source presentation, and opportunity for elaborative encoding (of either content or source) critically modulate sleeper effects under laboratory conditions. Adjustments to any one of these variables can disrupt the sleeper effect. This makes the implications of the sleeper effect for our understanding of source processing difficult to interpret, and reduces the confidence with which we can generalize to real-world settings and other research paradigms.

Third, although three primary theoretical explanations have been advanced to account for the sleeper effect, relatively little empirical headway has been made in evaluating these accounts (Kumkale & Albarracín, 2004). Importantly, all three theoretical accounts pivot on source memory. The proposed theories venture that: (1) memory for the source of information simply *fades* over time, so that during a delayed post-test, the (now unavailable) source can no longer calibrate (i.e., discount) one's opinion as it did earlier; (2) memory for the source is retained but becomes *dissociated* from the message, so that when opinion is later re-assessed, the source is not spontaneously recalled and therefore cannot influence opinion; and (3) memory for the communicated content *and* the source fade over time, but at different rates (source decays faster), so that a *differential decay* between source and content drives the effect. To the extent that the sleeper effect hangs contingent on deficits of source memory, the question of what factors govern the encoding, storage, and retrieval of source information becomes increasingly important. But to date this has not been a primary focus of interest. Furthermore, although these

three accounts are related to other programs of research concerned with suggestibility and source memory (e.g., Johnson, Hashtroudi, & Lindsay, 1993), these other studies do not examine specifically the *change of attitude or opinion after a significant delay* that demarcates sleeper effect research from other memory studies (efforts to reduce the gap exist, however; see Sitton & Griffin, 1980; Underwood & Pezdek, 1998).

To summarize, these three critiques of the sleeper effect—its fragile and highly contingent production, its plurality of data signatures, and its under-specified cognitive origins—render it imperfectly suited to illuminating the questions of source-processing with which this dissertation is concerned. Nevertheless, the sleeper effect does set important precedents. It shows that people attend to the source of received information, and that the attributes of those sources (such as credibility) have consequences for attitude and judgment. It points up the need for a taxonomy of source characteristics (e.g., is credibility distinct from age and authority?), and thereby a consideration of parameter values that vary from one source to another and from one context to another. Where fluctuations in judgment or opinion are concerned, it points an implicating finger at source memory, and emphasizes the question of whether encoding, storage, and retrieval of source information is qualitatively or quantitatively different from the encoding, storage, and retrieval of other forms of information (such as propositional content). Finally, the long-standing ties between sleeper effect research and real-world domains such as politics and advertising cast a long, pragmatic shadow over the implications to be wrought from all of these considerations of source processing: findings from additional studies promise to be immediately relevant outside the lab.

Source Monitoring

Another domain of research that may seem to contain obvious connections to the present studies is the area of “source monitoring” (Johnson, Hashtroudi, & Lindsay, 1993; Johnson & Raye, 1981; Mitchell & Johnson, 2000). The source monitoring framework was established in the 1970s (originally under the rubric of “reality monitoring,” Johnson & Raye, 1981) with a general interest in how the mind keeps track of the when, where, and how of our experiences (and their mnemonic traces), without plunging into a well of catastrophic errors that might easily accompany a cognitive system faced with such a daunting computational challenge. Source memory within this framework is broadly construed to mean how the many elements of a person’s experience (i.e., “where and when [events] happen, the perceptual properties of the people and objects involved, what was said, the emotional state of participants, our interpretations of what happened,” etc.) are cobbled together, stored, and later reconstructed from memory (Johnson, Verfaellie, & Dunlosky, 2008).

Research within this domain often considers questions of *reality monitoring* (e.g., distinguishing something we only imagined from something we actually did) and *source monitoring* (e.g., distinguishing an action we merely observed from one we personally performed), along with related phenomena such as *imagination inflation* (where confidence in the reality of a counterfactual event grows or “inflates” after repeatedly imagining it; see Garry & Polaschek, 2000).

An emphasis on reconstruction abides this research, in that the focus falls upon on processes of attribution rather than specific memory for source information per se. The idea is that we use the details of our recollections (e.g., amount of perceptual detail) to make source inferences (e.g., whether a given memory is of an event we only imagined or one we actually

experienced). “People’s mental experiences do not have labels indicating where they came from. Rather, they make attributions about the sources on the basis of the characteristics of those mental experiences—qualities like perceptual, contextual, semantic, and emotional details—and records of the cognitive operations that created them” (Johnson, 2006, p. 761).

However, this program of research is one that has not generally assessed the veracity of information *depending* on whether it came from Person X or Person Y (Johnson, 2006). For example, studies of reality monitoring were concerned with “source” information such as context or semantic content, but this was construed to mean things such as spatial and temporal details of an occurrence (in the case of context) rather than details of a communicator, and identity of the information itself (in the case of semantic content) rather than the conceptual status of a communicator (see Johnson & Raye, 1981). As such, the issues addressed in this dissertation may be seen to fall under the large umbrella of source monitoring, and therefore a contribution thereto, though only indirectly informed by the extant research there. Still, the source monitoring framework captures many of the same interests with which the studies here are concerned. Johnson and Raye (1981) expressed: “in emphasizing the many transformations a stimulus might undergo [from encoding to retrieval], we run the danger of forgetting the dire functional implications of a memory system that is assumed to be so loosely tied to external events” (p. 69).

Developmental of Trust

While the influence of specific sources has been traditionally omitted from the source memory literature mentioned above, the inclusion of source information has been common in child development studies of trust in testimony. These studies take as their starting point the observation that most of the information acquired by children during their formative years is

procured not from first-hand experience but from the testimony of family and caregivers around them. This raises the question of how children decide whom to trust, when to trust, how much to trust, and under what circumstances to trust.

Studies in this area have revealed that preschool children (ages 3-4) are in fact reliably able to consider source information when making judgments about newly communicated information (for a review, see Harris, 2007; Heyman, 2008). For example, when faced with two different sources, one of which consistently labels common objects accurately and another which consistently labels them inaccurately, children will “trust” the more reliable informant when these same sources later offer novel labels for novel objects. That is, children distinguish credible from non-credible sources and use this discrimination as a basis for epistemic decision-making (Koenig, Clement, & Harris, 2004). They also retain these discriminations when tested a week later (Corriveau & Harris, 2009). Children attend to the age of a source as well and are able to make nuanced judgments based on age, preferring to trust adult sources over child sources, but switching that preference if a child proves a more reliable informant than the adult (Jaswal & Neely, 2006). Very young children also seem to understand and attend to the fact that some sources may have more knowledge than others (Robinson, Champion, & Mitchell, 1999).

However, other studies suggest that young children’s epistemic abilities, though impressive, leave room for growth. For example, children of kindergarten age or younger may not always take into account the biasing nature of source motives (Mills & Keil, 2005), even when those motives are made clear and distinct (Heyman & Legare, 2005). Despite these qualifications, it is nonetheless clear that children exhibit some sophistication when it comes to considering the source of encountered information, and to some extent these considerations are

able to influence judgment and decision-making. Paul Harris (2007) summarizes the prevailing view from these studies:

“We assume that children will use a variety of cues by which to appraise an informant and that each [cue] is likely to add to or subtract from some overall reservoir of trust. . . . We conjecture that young children, disposed as they are to rely on the testimony of others, also establish a cognitive profile of their informants – they form a global impression of each individual, regarding some as more epistemically trustworthy than others. We assume that this global impression regarding any given informant is based on some kind of aggregated metric. Information about the informant’s past inaccuracy, ignorance, uncertainty, or apparent idiosyncrasy is fed into that profile” (p. 138).

If such processing descriptions are true of children, they must apply to adults as well.

The overarching goal of this dissertation is to begin charting whether source profiles affect automatic belief.

How Mental Systems “Believe”

As noted earlier, the line of research that directly informs the present experiments is centered on “how we believe” (Gilbert, 1991). Previous research has suggested that when people initially encounter a new piece of information (e.g., by reading a sentence), there is no immediate distinction between *comprehending* and *evaluating*—that is, at the outset of an encounter with new information, we believe what we comprehend as an automatic epistemic default, and only later (if time and attention permit) submit that information to a second and more controlled evaluation where we confirm or reform our (pre)suppositions. This conclusion has emerged from studies showing that people more often recall *false* statements as *true* (not vice versa) if interrupted when learning whether a statement is true or false (Gilbert, et al., 1990; Gilbert, et al., 1993; though see Hasson, Simmons, & Todorov, 2005). Interruption ostensibly precludes effortful processing of a statement’s truth, thereby allowing us to tease apart comprehension and evaluation to reveal the dispositions of belief that precede explicit appraisal. So if we

automatically believe that new information is true, then interrupting subjects *as* they learn that a statement is false should reveal a later tendency to rate such interrupted statements as *true* (relative to statements that were not interrupted).

Consider one representative study (the “Hopi Language” experiment, Gilbert et al., 1990), in which participants saw a series of statements, one at a time, that nominally taught them Hopi Indian vocabulary words (e.g., “A ghoren is a jug” or “A monishna is a star”), followed by an indication of whether the statement was true or false (or else a blank screen that offered no information). On some of these trials, participants also heard an interrupting tone while reading TRUE or FALSE, and were required to press a button to register their recognition. This tone was intended to distract participants, producing a cognitive load that compromised their epistemic processing of the statement (i.e., whether it was true or false). Later, at test, they saw these statements again, and were asked to recall whether each had been true, false, had not been followed by any information, or was new.

Though our experience may intuitively suggest that we can comprehend a statement without necessarily judging its veracity, Gilbert’s results implied that the mind categorized information as *true* by default when interruption prevented controlled, explicit processing. When subjects were asked to recall the truth of statements seen earlier, they made the same number of errors on both true and false *uninterrupted* trials (recalling “true” statements as “false” as often as they recalled “false” statements as “true”). But for statements that *were* interrupted, subjects were more likely to mis-remember false statements as true, than true statements as false). It was this interaction, where the rate of true/false reversals depended on *interruption*, that gave force to the notion that the mind was bent toward an automatic presumption of truth (see Figure 5).

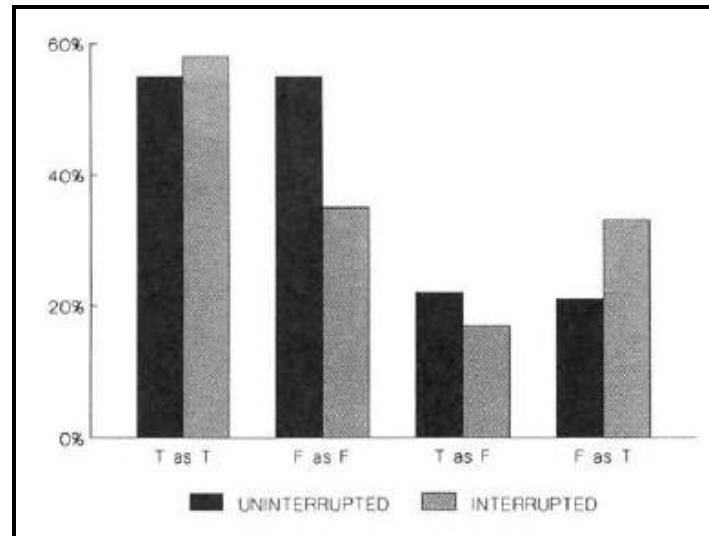


Figure 5. Accurate identification (left half) and reversal of statement truth (right half) from the Hopi Language experiment (Gilbert et al., 1990). “T as T” refers to true statements correctly recalled as “true” (vice versa for “F as F”), while “T as F” refers to originally true statements incorrectly recalled as “false” (vice versa for “F as T”).

But as previously noted, these original studies (and those that followed) did not consider the *source* of the information presented. Indeed, no information about the source was provided at all. Participants in these studies saw statements emerge on a computer screen *ex nihilo*. Although the absence of source information might be typical of experimental settings, such circumstances mark a conspicuous departure from the way people normally encounter information.

Scope & Rationale of the Proposed Research

The absence of source information in the foregoing experiments by Gilbert and colleagues ensured that an arguably unnatural form of processing would occur in those studies. If the human mind evolved in a context where language-communicated information almost always had a source, it is reasonable to imagine that the human mind would contain evolved processing systems that prepare it for content coming from different sources (Cosmides & Tooby, 2000). For this reason, the discontinuity between previous research protocols and naturalistic information-processing is an omission worthy of exploration.

The consideration of ancestral sociality and the fitness-loaded coalitional dynamics discussed earlier may make a default assumption of truth a dangerously vulnerable design for our cognitive apparatus. As noted by Kenrick and colleagues (Kenrick, Sadalla, & Keefe, 1998, p. 497), “From an evolutionary perspective, it makes no sense to consider cognitive processes independently of other processes such as motivation, emotion, or behavior; all are interrelated parts of adaptive systems designed to deal with functional problems.” We expect the inclusion of source information—specifically, coalitionally charged source information—to change the quality of one’s cognitive processing. Processing a statement in isolation from source may engage a number of mechanisms evaluating plausibility, relevance, etc., but the inclusion of source information may also engage processes concerned with *signal evaluation*. When a person chooses to verbalize, it almost always carries a communicative intent (people infrequently choose to speak for no purpose, or speak on accident). “[Language] behaviors are obviously signals because they serve little or no purpose outside of communication” (Andrews, 2001, p. 19). It is this intentional quality that requires humans to process not just the objective content of a statement, but also the latent communicative intent of the source (Sperber & Wilson, 1996). That is, the inclusion of source information may elicit signal processing that is distinct from content processing, and when the source is coalitionally significant we might expect such processing to reveal evidence of epistemic vigilance.

The present studies bring these considerations into the realm of default belief. Following the protocol used by Gilbert and colleagues (Gilbert et al., 1990; Gilbert et al., 1993), we attach coalitional source information to presented statements, affording an opportunity for coalitional source processing to affect automatic belief. Such a design allows for full processing of both the source information and the statement content, while interruption truncates one’s processing of

statement veracity. Under such (interrupted) conditions, we hypothesize that the relative rate of true/false reversals will reflect coalition-contingent differences. The rationale, simply, is that in the absence of epistemic certainty (which interruption prevents), it would make adaptive sense for one's mental architecture to *affirm* coalitional affiliations by presuming truth for statements issued by one's best friends, but *not* for statements from someone least liked.

Two qualifications should be noted. First, this hypothesis does not speak to uninterrupted trials. It is possible that the full attention afforded by uninterrupted trials may elicit explicit, controlled coalitional processing for either source. It is also possible that full attention will allow subjects to discount source information due to the artificial setting. These possibilities are tangential, insofar as the project is geared to an examination of how *automatic* processes of belief might be spontaneously re-calibrated in light of the source.

Second, despite the important threats that attend an enemy, this hypothesis makes no specific prediction that reversal rates will change for enemies. One might imagine that *always disbelieving* an enemy might be a good default strategy as a guard against deception and a demonstration of non-affiliation. But such a strategy may leave one as vulnerable as *always believing*, since deceptive communication is rarely so careless or blunt as to exclude a truthful premise or context. The sophistication of deception would likely vary from enemy to enemy (along with age, intelligence, general knowledge, etc.), such that *no* alternative strategy would have been favored by natural selection for information encountered from a categorical enemy. However, where *positive* coalitional alliances are concerned (friends, partners, etc.), sheer accuracy is not always the concern of most importance. Coalitional alliance is concerned with the provisioning of social support. Under circumstances of epistemic uncertainty (as caused by

interruption), judgment of a statement's "truth" may not only engage processes of automatic belief, but also processes of social cognition supporting coalitional affirmation and endorsement.

A series of pilot studies were conducted to make sure that the experimental protocol used by Gilbert and colleagues would be amenable to modification, specifically that the manipulation of source as an additional factor would be feasible. What these pilot studies also demonstrate is that the inclusion of source information in these research paradigms can drive distinctive patterns of recall and error.

In the context of Gilbert's comprehension/evaluation paradigm (e.g., Gilbert et al., 1990), in which both *accuracy* (recalling true statements as true, and false statements as false) and reversals (recalling true statements as false, or false statements as true) are used as dependent measures, we found that attaching sources to statements (by presenting the names of participants' "best friend" and the person they "least like" alongside the statements) disrupted the pattern of errors found in the original studies. Notably, however, using a different set of sources in the same paradigm (e.g., a fictional professor at a nearby university, or a school-aged child) did *not* disrupt the pattern. These preliminary findings suggest the possibility that (a) different sources may have different epistemic profiles, (b) sources with different epistemic profiles may influence the way that communicated information is processed, and (c) qualitative or quantitative differences in observed processing across sources may reflect a form of *epistemic vigilance*—that is, cognition aimed at minimizing the cost of inferential errors.

Pilot Experiment 1

This first pilot experiment was designed to (a) verify that we could replicate the original findings by Gilbert and colleagues while (b) using a similar but not identical set of stimuli. Planning to add Source as a factor in forthcoming studies necessitated a stimulus set larger than

that used in Gilbert's original studies, and one amenable to maintaining power through an increased number of repeated-measures.

This experiment follows the protocol of Gilbert's (Gilbert et al., 1990) Experiment 1. From his model of how initial truth judgments are formed, one should expect an asymmetry in the proportion of statements recalled as true or false as a result of interruption. Specifically, interruption should cause participants to misremember false statements as true more often than true statements as false (with no asymmetry on interruption-free trials). From a "believe-first" perspective, this would occur because interruption had precluded the cognitive operations of "unbelieving" that are required for a person to change their default assumption of "true" to an updated belief that a statement is "false."

Method

Participants. Thirty-two WU undergraduates (21 female, 11 male), with a mean age 19.81 years, participated for course credit. All participants were native English speakers.

Materials. Fifty-six trivia statements served as the communication stimuli, along with indications of statement veracity ("TRUE," "FALSE," or no information) and a 500hz tone for interruption. Statements contained trivia/opinion content sufficiently arcane to be outside the normal range of common knowledge, which prevented participants from assessing the truth of statements prior to experimental suggestion (see Appendix A for full stimulus list). Representative examples include:

"The first postage stamp was introduced by Great Britain in 1840, and featured a portrait of Queen Victoria."

"'Syzygy' is the term for three celestial bodies arranged in a straight line, as during an eclipse where the Sun, Moon, and Earth are aligned."

“The requirement among American universities that printed dissertations be single-sided wastes 40 million pages of paper every year.”

“The first genetically engineered organism was a tobacco plant in 1983, grown in Wisconsin and designed to be resistant to key herbicides.”

Stimuli were presented on personal computers, centered horizontally and vertically. Stimulus length was controlled, so that each statement appeared in a single line (no instances of text-wrapping).

Design and procedure. Participants were individually seated at a private computer and fitted with headphones to ensure privacy of the interrupting tone. An instruction screen informed participants that they were involved in an experiment on basic aspects of communication, and that they were to pay attention to what was said and whether it was true or false. They were also told that because interruption is a regular feature of normal communication, they would occasionally hear an interrupting tone, at which point they should press the “P” key to register their awareness of the tone.

An initial study phase consisted of 48 trivia statements presented in random order. Following the “Hopi Language” experiment, each statement appeared on-screen for 8 seconds, followed by a 2-second inter-stimulus-interval (blank screen), followed by a 3-second presentation of statement truth (“TRUE” or “FALSE”) or no information (blank white screen). A final inter-stimulus-interval of 1 second provided a buffer between trials (14 second sum per trial). Among the 48 study statements, 20 were randomly labeled as “true” and 20 as “false,” and 8 received no truth information (blank screen).

Among the 40 trials specified as true or false, interruption occurred for 16 (8 true, 8 false). Following Gilbert, this was achieved using a 500hz tone, which sounded 750ms after the initial appearance of truth (“TRUE” or “FALSE”) and lasted 390ms. Subjects were given an

instruction to press the “P” key whenever the tone occurred. The function of the tone, as in the original study, was to disrupt participants’ processing of truth information.

The study phase was followed by a 15 second break, after which the test phase automatically began. During the test phase, participants were presented with 56 statements (the 48 previously viewed, plus 8 new statements) in random order, and asked to recall whether each statement was (a) true, (b) false, (c) had no information provided, or (d) was new (not presented during the study phase). The test was self-paced, and decisions were rendered by key-press (“true,” “false,” “new,” or “no information” were labeled as “T,” “F,” “N,” “I” on the 0-3 buttons of the keyboard number pad).

Results and Discussion

The proportion of statements that each subject accurately recalled as true or false was submitted to a 2 (Truth: true or false) x 2 (Interruption: yes or no) within-subjects ANOVA. This analysis did not reveal a main effect of Truth, $F(1,31) = 1.47, p = .234$, but did reveal a main effect of Interruption, $F(1,31) = 25.74, p < .001, \eta_p^2 = .45$, and a significant Truth x Interruption interaction, $F(1,31) = 5.98, p = .02, \eta_p^2 = .16$. The left half of Figure 6 illustrates that interruption reduced accuracy for false statements (61% vs. 39%), as expected, to drive an increase false statements erroneously recalled as true.

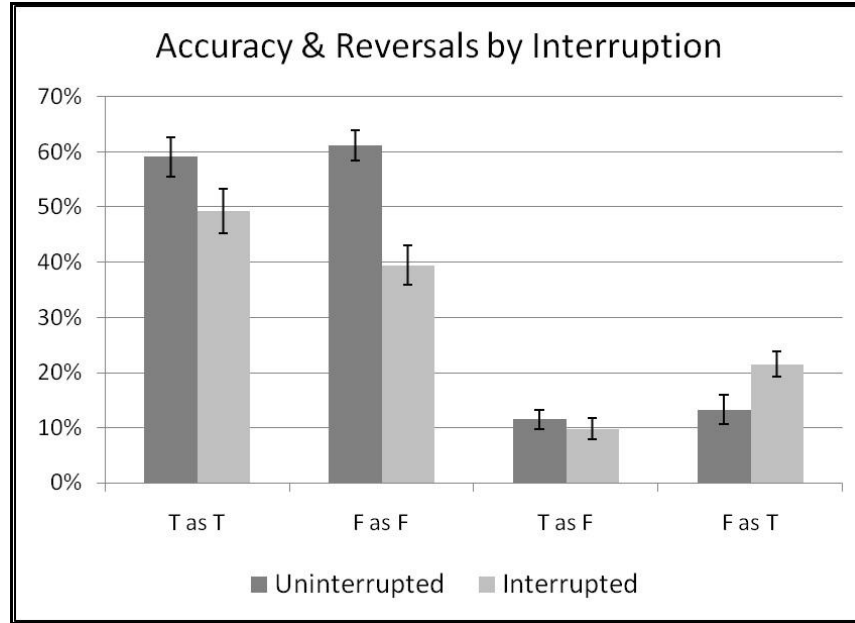


Figure 6. Accurate identification (left half) and reversal of statement truth (right half) in Pilot Study 1 (compare with the analogous data of Gilbert, et al., 1990, in Figure 5). “T as T” refers to true statements correctly recalled as “true” (vice versa for “F as F”), while “T as F” refers to originally true statements incorrectly recalled as “false” (vice versa for “F as T”).

A second ANOVA was performed using “reversals” as the dependent measure, defined as the percentage of true statements erroneously recalled as false, and false statements erroneously recalled as true. This analysis revealed a main effect of Truth, $F(1,31) = 7.51$, $p = .010$, $\eta_p^2 = .20$, and a main effect of Interruption, $F(1,31) = 4.28$, $p = .047$, $\eta_p^2 = .12$, as well as the expected Truth x Interruption interaction, $F(1,31) = 6.17$, $p = .019$, $\eta_p^2 = .17$. The right half of Figure 6 shows the percentage of reversals for true and false statements, and illustrates that false statements were affected by interruption much more than true statements. Interruption while reading that a statement was “false” was much more likely to lead to an epistemic reversal (21.5%) than interruption on a statement labeled as “true” (9.8%). Thus, interruption drove an increase in the error rates of false statements, $t(31) = 3.88$, $p = .001$, but this asymmetry did not exist for uninterrupted trials ($t < 1$), consistent with the interpretation that truth judgments are governed by belief dynamics that assume initial truth.

The results of this first experiment suggest that both of its aims were satisfied: the findings from Gilbert et al.'s (1990) "believe first" experiments were replicated, and this was accomplished using a novel set of stimuli. If the inclusion of source information proves to affect subsequent results, we therefore have reason to consider that neither the stimulus set nor the basic protocol are the root cause.

Pilot Experiment 2

Having replicated the original Gilbert findings using the alternative content of trivia statements, this second pilot experiment began to explore the question central to epistemic vigilance: To what extent does the source matter? Gilbert's "believe first, unbelieve later" hypothesis is a prediction about general information processing, so there is no reason (on his account) to expect source information to influence what are nominally automatic processes. But if mechanisms of epistemic vigilance exist, we might expect their influence to emerge when coalitional sources become involved. Here we extend the protocol of the previous experiment by introducing the variable of "source"—asking participants to conceive that the statements they confront originate from either their "best friend" or the person they "least like."

Method

Participants. Thirty-seven WU undergraduates (20 female, 17 male), all native English speakers, with a mean age 19.16 years, participated for course credit.

Materials. The materials were identical to those of Pilot Experiment 1, with the sole addition of source information: participants' "best friend" and a "least liked" individual. Stimuli were presented as before, with source information centered just above the trivia statements, for example:

John said that:

"The first postage stamp was introduced by Great Britain in 1840, featuring a portrait of Queen Victoria."

Design and procedure. The design and procedure were identical to that of Pilot Experiment 1, with the following modification. After each participant was seated at their computer, they were asked to type out the first name of the person they considered their “best friend,” and then the person that they “least liked.” In the few cases where participants asked for clarification, “best friend” was qualified to mean the friend they “most trust” while “least liked” was qualified to mean someone they “least trust.” They were also told that the sources had to be “real,” that is, people they actually knew (as opposed to, for example, President George W. Bush). In most cases, the names of appropriate individuals came effortlessly to the participants. The names of these real sources were typed into the experimental software (SuperLab), so that statement presentations during the study phase were paired with the names of these two sources. Each study trial thus began with one of the two sources (e.g., “John said that” or “Megan said that”) printed concurrently above the trivia statement. Half of the 48 statements were randomly paired with the “best friend” and the other half with the “least-liked” source.

Participants received the same study instructions as Pilot Experiment 1, amended only to say that they would see statements by people they knew, and that they should pay close attention to *what* was said, *who* said it, and whether it was *true* or *false*. The test phase was fully identical to the test of Experiment 1.

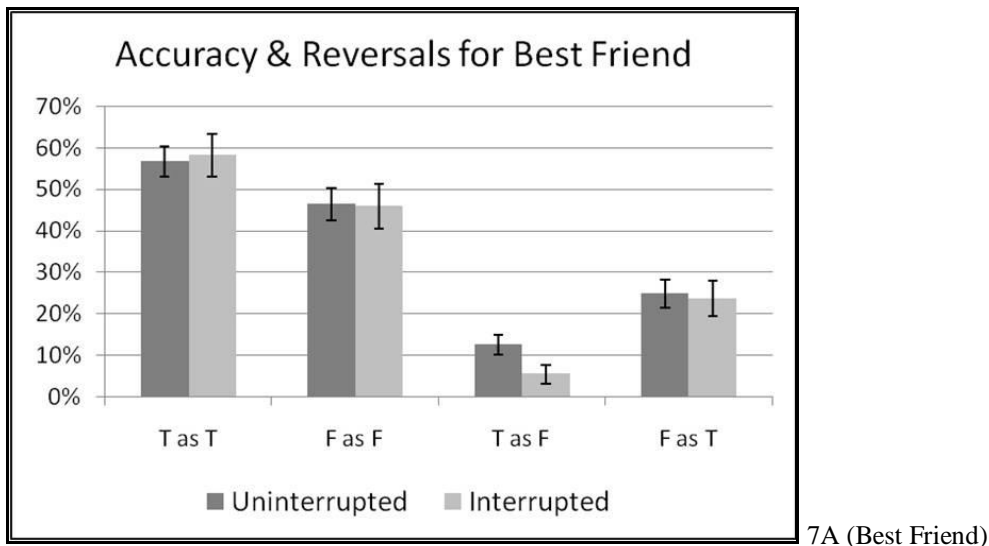
Results and Discussion

Does knowledge of source make a difference? We submitted the proportion of statements accurately recalled to a 2 (Truth) x 2 (Interruption) x 2 (Source) repeated-measures ANOVA. Significant main effects emerged for Truth, $F(1,36) = 10.92$, $p = .002$, $\eta_p^2 = .23$, Interruption, $F(1,36) = 5.06$, $p = .031$, $\eta_p^2 = .12$, and marginally for Source, $F(1,36) = 3.16$, $p = .084$, $\eta_p^2 =$

.08, as well as for the Interruption x Source interaction, $F(1,36) = 21.02$, $p < .001$, $\eta_p^2 = .37$ (all other effects n.s.).

This ANOVA was then repeated using reversals as the dependent measure, which revealed significant main effects for Source, $F(1,36) = 5.12$, $p = .030$, $\eta_p^2 = .13$, and Truth, $F(1,36) = 29.26$, $p < .001$, $\eta_p^2 = .45$, as well as two significant interactions, Interruption x Truth, $F(1,36) = 8.59$, $p = .006$, $\eta_p^2 = .19$, and Interruption x Source, $F(1,36) = 6.31$, $p < .017$, $\eta_p^2 = .15$.

While the analyses of Pilot Experiment 1 suggest that the “believe first” effect is robust, the source-inclusive analyses of Pilot Experiment 2 suggest that a more subtle interpretation is required. Figures 7A and 7B show the data for best friends and least-liked sources individually. When the source was the least-liked individual, the results show a preserved pattern of assuming initial truth (as in Pilot Experiment 1 and previous studies: compare with Figures 5 and 6), but when the source was a best friend, that pattern was disrupted.



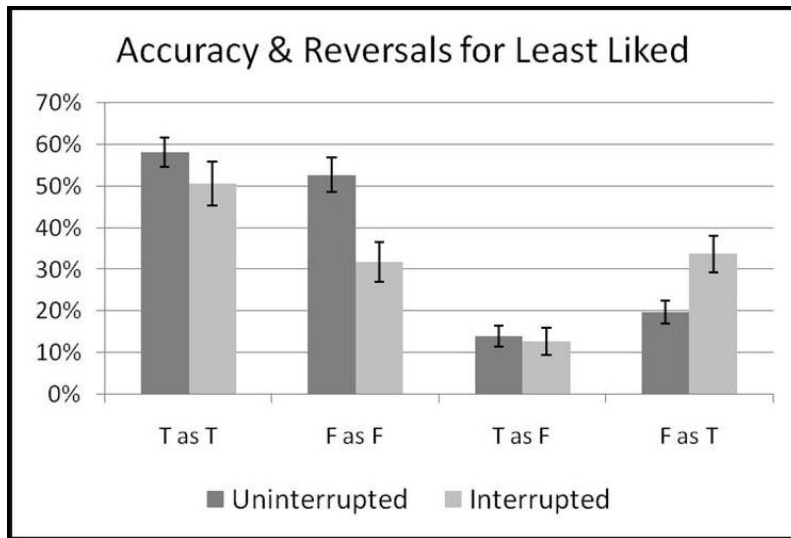


Figure 7B (Least-Liked)

Figures 7A / 7B. Accurate identification (left half) and reversal of statement truth (right half) in Pilot Experiment 2 for Best Friend (7A) and Least-Liked (7B). (Compare with the analogous data—without sources—from Gilbert et al., 1990).

For least-liked sources, interruption reduced accurate recall for false statements (32%) relative to true statements (51%), $t(36) = 2.67$, $p = .01$. This change is in-step with previous findings, the change thought to drive the rise in false-to-true reversals. But this change was only marginally significant for best friend sources (58% vs 46%), $t(36) = 1.84$, $p = .074$ (two-tailed), for whom the proportion of false-to-true reversals did not increase with interruption. While it is true that the data for both sources show that being interrupted provokes more F-as-T epistemic reversals—the expected asymmetry, $t(36) = 4.12$, $p < .001$ (least-liked), $t(36) = 3.46$, $p = .001$ (best friend)—the characteristics of the best friend data challenge this as a complete account.

Perhaps disconcertingly, these data seem to suggest that information coming from individuals we sharply dislike is processed no differently than when there is no source at all (in the present experimental context). Conversely, Figure 7A illustrates that information coming from best friends is comparatively impervious to the affects of interruption, with the singular exception of a *lowered* tendency to make T-as-F reversals. Do these data suggest an epistemic privilege when processing information from good friends? It may, and the dissertation

experiments to be conducted will pursue this possibility. But first, Experiment 3 was conducted to find out whether other important epistemic variables might disrupt the standard pattern of recall as well.

Pilot Experiment 3

How do we know that the observed recall differences were a function of coalitional status and not a function of differences in, for example, reliability or trustworthiness—traits that vary quantitatively among coalitional and non-coalitional sources alike. For evolutionary reasons, we would expect coalitional status to have cognitive import independent of source characteristics such as general knowledge, experience, likeability, and so on. To advance this hypothesis and qualify the previous results, tests must show that any results proffering coalitional interpretations cannot be accounted for by alternative, more mundane factors. Toward this end, a final pilot experiment replaced BF and LL sources with two non-coalitional sources—a college professor and a school-aged child. Though lacking coalitional affiliation, these sources could be expected to vary in terms of potentially relevant source dimensions such as age, general knowledge, authority, and reliability. If the results of our earlier source manipulations were rooted in non-coalitional causes, we should expect recall and reversal outcomes here to present similarly divergent patterns of recall for these new sources. On the other hand, if results for these new sources are indistinguishable, there would be less reason to think that the non-coalitional factors itemized above carry the explanatory power to account for the results of Pilot Experiment 2.

Method

Participants. 26 WU undergraduates (19 female, 7 male), all native English speakers, with a mean age 18.65 years, participated for course credit.

Materials. The materials of this study were fully identical to those of Pilot Experiment 2, with changes limited to source names and initial instructions.

Design and procedure. The design and procedure of this experiment did not deviate from that of Pilot Experiment 2, with two exceptions. First, participants were no longer asked to type in the names of sources; instead, the fictional names of a college professor (“Dr. Johnson”) and an eight-year old child (“Thomas”) were programmed into the experimental script and not allowed to vary. Second, the instructions were modified to accommodate the new source information. These instructions read:

This experiment is concerned with the dynamics of basic communication. You will see a number of trivia statements made by either Dr. Johnson (a memory psychologist at a respected university) or Thomas (an eight-year-old child from a nearby school). Some of these statements will be followed by an indication of whether the statement is true or false. Please pay close attention to WHAT is said, WHO said it, and whether it is TRUE or FALSE.

The rest of the instructions (regarding interruption, etc.) were not changed, and the test instructions and procedure were the same as before.

Results and Discussion

Data from two participants were removed from analysis because of a failure to press the “P” key in response to the interrupting tone, thereby making it impossible to properly gauge the effect of interruption for these subjects. Using data from the remaining 24 subjects, we once again submitted both accuracy and reversal measures to 2 (Truth) x 2 (Interruption) x 2 (Source) ANOVAs. Looking first at accuracy, main effects emerged for Truth, $F(1,23) = 15.94$, $p = .001$, $\eta_p^2 = .41$, and Interruption, $F(1,23) = 7.05$, $p = .014$, $\eta_p^2 = .24$, but no other effects or interactions were significant. Turning to reversals, the same picture emerged, showing main effects of Interruption, $F(1,23) = 5.61$, $p = .027$, $\eta_p^2 = .20$, and Truth, $F(1,23) = 29.97$, $p < .001$,

$\eta_p^2 = .57$, and a Truth x Interruption interaction, $F(1,23) = 8.39$, $p = .008$, $\eta_p^2 = .27$, but no main effect or contingent interactions involving Source.

Figures 8A and 8B show the data for Professor (8A) and Child (8B). These results show that the “truth bias” can be replicated using two novel sources, and the failure of any main effect or interaction involving Source suggests a total absence of meaningful differences between the two sources used (in terms of the measures of this study). Equally important, a comparison of these results with those of the previous experiment warrants the tentative conclusion that there is a meaningful difference between sources that carry coalitional relevance and those that do not. Although the findings of this experiment do not confirm a coalitional interpretation of source effects, they are in line with one, and they suggest that factors such as authority and general knowledge were not the ones driving previous recall differences among coalitional sources.

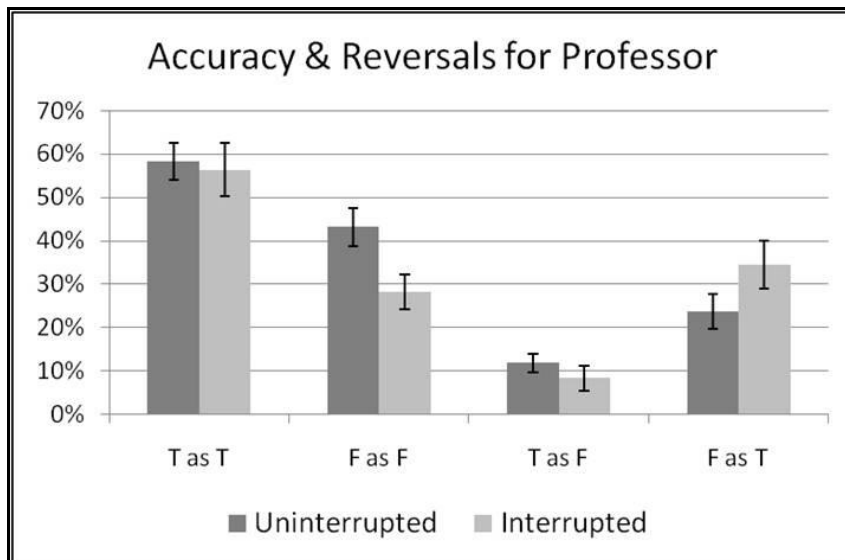


Figure 8A (Professor)

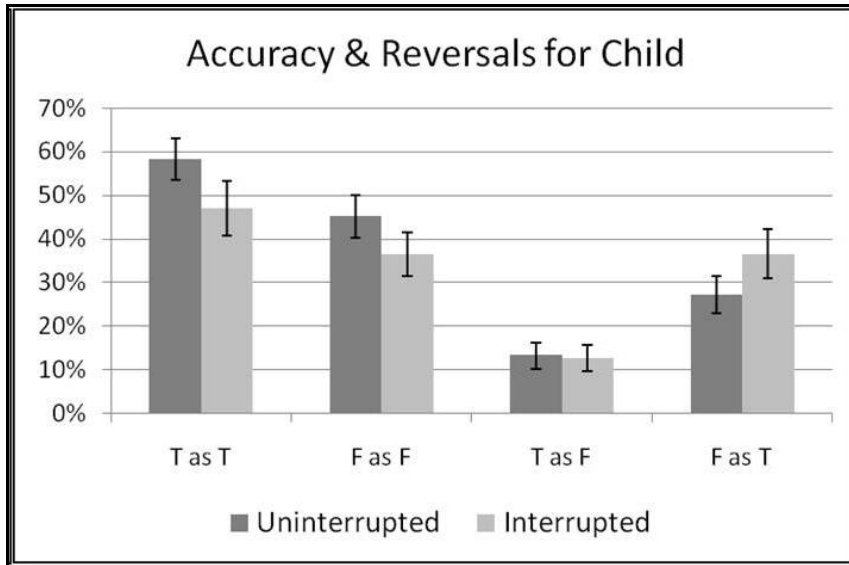


Figure 8B (Child)

Figures 8a / 8b. Percentage of statements accurately recalled (or incorrectly reversed) as a function of statement truth and interruption, for Professor (8A) and Child (8B).

Introduction to Dissertation Experiments

The set of hypotheses explored below consider the possibility that the human mind contains prophylactic processes which evolved to accomplish epistemic vigilance in light of the adaptive social challenges that humans faced under ancestral conditions, specifically those concerned with the management of coalitional affiliations.

The preceding pilot studies provided initial evidence consistent with a coalitional interpretation. When trivia statements appeared to come from coalitional proxies (one's best friend or the person one least likes), patterns of recall and error differed for the two sources. Specifically, when the source was one's best friend, interrupted subjects were *more* likely to mis-remember false statements as true, and *less* likely to mis-remember true statements as false—a result that did not occur with sources other than best friends.

To attribute this difference to coalitional factors, however, requires additional support and theoretical refinement. Other plausible explanations must be ruled out, and three

possibilities will be explored here. First, it is important to know whether subjects actually recall the sources for each statement. By its nature, a coalitional interpretation presumes that a mnemonic link has been formed between source and truth. A study that requires judgments of both source and truth should reveal links between the two. (A related but secondary interest lies in knowing whether a recalled source—accurate or not—influences judgments of truth, and conversely whether recalled validity—accurate or not—influences judgments of source.)

Second, although the last pilot experiment spoke against the import of authority and knowledgability (insofar as default belief is concerned), it is possible that the coalitional interpretation can be better accounted for through the simple difference in *valence* attached to best friend and least-liked sources. That is, perhaps any two sources distinguished by a sharp positive/negative contrast will produce a difference in recall comparable to those found for best friends and disliked individuals.

Third, it would be useful to know whether the effect of the BF/LL manipulation generalizes to statements beyond trivia, statements that carry more real-world relevance. For example, the use of precautionary statements (e.g., regarding health or physical danger) would not only improve ecological validity but also assess whether a meaningful relationship exists between coalitional agents and statements nominally affecting one's welfare. These three proposals are explored in turn.

Experiment 1: Recalling the Source

Asking participants to judge the truth *and* the source of a statement introduces unique opportunities. Asking for judgments of truth and source about a single statement is a natural juxtaposition that allows us to see to what extent these judgments hang together. If memory for truth is independent of memory for source, then the functional argument of the present studies

can be reasonably called into question. Although it is possible that source/truth effects exist more at an implicit level than an explicit level, it is unlikely that implicit effects would not also produce some discernible patterns within explicit recall. As such, the details of source memory may provide meaningful qualifications to the patterns of recall demonstrated for truth alone.

However, new challenges of interpretation accompany this revised protocol. The order in which judgments are requested becomes important, since responses in the first half of a test trial may affect decisions in the second half. If a statement's truth is queried first, then the test trial is ostensibly similar to the test trials of previous studies (which asked exclusively for statement veracity), and thus represents an opportunity to replicate the original results. Conversely, if a statement's source is queried first, this should provide a relatively pure measure of source memory.

But predictions for the second half of each test trial are more difficult to articulate, because responses provided for the first half may elicit implicit effects, explicit recollection, and any idiosyncratic processing uniquely provoked by a given statement. A number of interesting predictions may be articulated regarding the potential *interaction* of implicit and controlled influences, but these are not strictly relevant to the issue of automatic belief.¹

For those subjects who are first queried about statement truth, results are predicted to show a replication of the results from Pilot Experiment 2, where least-liked individuals show the

¹ For example, a reasonable prediction might be that initial source judgments would bias subsequent truth responses irrespective of whether those initial source judgments were accurate—e.g., statements that are first judged to have come from a best friend would be more often recalled as true than statements first judged to have come from an enemy, even if those initial source judgments were wrong. Such results would be akin to findings in the study of the “truth effect,” where respondents judge statements as having greater validity when they are *perceived* to have been repeated, even when they weren't (Bacon, 1979). But again, such predictions would not speak to the issue of default belief with which these studies are concerned, because each secondary judgment is always the product of both signal (implicit associations between source, statement, and validity) and noise (ancillary and potentially controlled processes engendered by making the first judgment).

same pattern of automatic belief as when no source is present, but best friends show a pattern that is biased towards truth—interruption produces an *increased* rate of reversals from false-to-true, and a *decreased* rate of reversals from true-to-false.

For those subjects who are first queried about source memory, it is predicted that results will show approximately equal rates of source accuracy for those trials that were not interrupted. But for those statements that *were* interrupted, the prediction is that results will show a slight bias towards recalling one's best friend as the source instead of one's LL person. This might seem like a counter-intuitive prediction in light of the fact that source-statement pairings themselves are never interrupted (only the processing of truth information is interrupted), and if a truth bias is the only process operating, then interruption should *not* influence recall rates of source (both should be approximately equal, or 50%, whether interrupted or not). But if a bias towards coalitional support is operating as well, we might expect (under interrupted conditions) that the default presumption of truth will facilitate or bias source memory toward one's best friend (adaptive congruency), but not towards enemies.

In addition, one might reasonably predict asymmetries among the reaction times for source and truth judgments. If a "support bias" exists for best friends, then reaction times (for both truth and source judgments) should be quickest when a statement pairs a best friend with truth (adaptive congruency), and slower reaction times when the best friend is associated with falsity. Furthermore, interruption should amplify these differences to the extent that it successfully minimizes the influence of explicit recollection. No such RT differences are anticipated for statements from least-liked individuals, because no clear adaptive benefits would have accrued to a default presumption of truth or falsity for enemies, as it would for best friends.

Method

Participants. Seventy-three Washington University undergraduates (39 female, 34 male), all native English speakers, with a mean age of 19.5 years, participated for course credit. Participants were randomly assigned to one of two between-group conditions: one group receiving test questions in the order of *source judgment* then *validity judgment* for each individual statement (no blocking) at test (Group A, N = 40), the other group receiving test questions in the opposite order (Group B, N = 33).

Materials. The same 56 trivia statements used in the pilot experiments served as communication stimuli (see Appendix A), along with the same indications of validity (TRUE, FALSE, or no information), the 500hz tone for interruption, and source names determined by participants (friends and enemies). Stimuli were presented on personal computers, centered horizontally and vertically, with source information appearing just above the statements. Stimulus length was controlled, so that each statement appeared in a single line, with no instances of text-wrapping.

Design and procedure. The design and procedure of the study phase was identical to that of Pilot Experiment 2, with the exception that the presentation of truth information (TRUE or FALSE) was flashed for only 500ms instead of 2000ms. Because the original Gilbert procedure left the truth stimulus onscreen for a full two seconds, this may have allowed the quickest responders (those pressing the “P” key immediately upon hearing the tone) an opportunity to return their attention to the validity stimulus before it left the screen. (Informal monitoring of pilot participants suggested that this occurred at least some of the time, with participants occasionally placing an expectant finger on the P key, whereupon the interrupting tone would only have diverted attention for a fraction of a second.) Such circumstances render

the tone somewhat less suited to its task of interruption. Reducing the on-screen time of the validity stimulus is therefore intended to guard against the return of attention, making sustained awareness of validity more difficult, and thereby, it is hoped, augmenting the critical division between comprehension and evaluation upon which these studies are based.

The test phase also diverged slightly from the previous studies by including judgments of both truth and source. Once again the test phase included previously presented statements (along with eight new filler statements), but in addition to judging whether a statement had been presented as true or false, participants were now asked to also recall which source had “made” the statement (their best friend or their least-liked acquaintance). The order of these two questions (truth and source) was counter-balanced as a between-subjects manipulation. The rationale for the less economical between-subjects approach is that the number of critical interrupted trials was already relatively small (sixteen, with eight per source), so that a within-subjects manipulation (e.g. two blocks of test trials, with order of questions counter-balanced across blocks) would require sacrificing resolution in the data (fewer repeated-measures per condition) or else a considerable increase in the number of study statements and test trials. Neither compromise was deemed more important than maintaining continuity with previous experiments, such that a between-subjects approach was adopted.

Results

Due to inattention and/or a failure to follow directions, data for six participants were removed prior to analysis—two from Group A where source memory was queried first, and four from Group B where statement validity was queried first—leaving data from 67 participants available for analysis (38 from Group A, 29 from Group B). Across these 67 participants, overall source accuracy was good ($M = .69$, $SE = .02$, range = .43 to .93) and significantly above

chance (.50), $t(66) = 12.85$, $p < .001$. This held at the individual level as well: given a .50 probability of success on each of the 48 test trials that were originally paired with a source (BF or LL), recall rates that equaled or exceeded 63% (via binomial calculation) would have qualified as above-chance performance. Using this criterion, 50 of the 67 participants (75%) performed at above-chance levels, and only one subject scored lower than .50 (.43). This indicates that most participants were able to recall source information reasonably well within the constraints of the modified experimental protocol. Recall for statement truth was also good ($M = .56$, $SE = .02$, range = .30 to .78) and above chance, $t(66) = 21.09$, $p < .001$. In this case, given a .25 probability of success on each of the 40 test trials that had originally been tagged with truth information (the four response choices at test being *true*, *false*, *no information*, or *new*), 38% accuracy or higher would have qualified as above-chance performance; 63 of the 67 participants (94%) met this criterion. However, given that recognition memory alone would have been fairly sufficient to eliminate “new” responses as a competitive option for these 40 test trials, a more stringent level of chance probability (.33) may be more appropriate. At this more conservative level (requiring 47% accuracy), 51 of the 67 participants (76%) still met or exceeded this success rate.² Taken together, these recall data indicate that the protocol employed in the present experiment—modified from the foregoing studies by adding source judgment alongside truth judgment—did not place deleterious demands on participants’ mnemonic competence. Although this does not yet establish any causal links between source and truth at the implicit level of default belief, these recall rates are compatible with such possibilities and render the present hypotheses plausible by demonstrating participants’ mnemonic capacity to retain both pieces of information.

² The performance of those scoring below chance cannot simply be attributed to inattention: of the 17 participants scoring below chance on source memory, only four of them were also among the 16 scoring below chance on truth memory.

Recall for validity. To look more closely at memory for statement validity, recall accuracy was submitted to a 2 (source: BF, LL) x 2 (truth: true, false) x 2 (interruption: yes, no) repeated-measures ANOVA (a between-groups ANOVA revealed no statistical difference due to the order in which source/truth queries were administered, so data was collapsed across the two groups for analysis). Main effects emerged for all three variables: truth, showing that true statements were better recalled than false statements (.63 and .47, respectively), $F(1,66) = 46.38$, $p < .001$, $\eta_p^2 = .36$; source, showing that validity was better recalled for statements from a BF than an LL (.59 and .52, respectively), $F(1,66) = 13.92$, $p < .001$, $\eta_p^2 = .17$; and interruption, showing that uninterrupted statements were better recalled than interrupted statements (.59 and .52, respectively), $F(1,66) = 13.50$, $p < .001$, $\eta_p^2 = .17$.

More importantly, all two-way interactions were significant. First, as predicted by the original Gilbert hypothesis, an interaction between interruption and truth showed that interruption caused recall to suffer, but only for false statements, $F(1,66) = 8.28$, $p = .005$, $\eta_p^2 = .11$. According to the original Gilbert hypothesis, this occurs because statements that are originally comprehended as false—but not evaluated, due to interruption—are erroneously recalled as true by default.

However, an interaction between source and interruption further revealed that the diminution of accuracy triggered by interruption was borne primarily by LL sources. When the source was a best friend, accuracy for statement validity was virtually unaffected by interruption (.58 when interrupted, .60 when not), a notable exception to the signature drop in accuracy expected among interrupted false statements. Conversely, when the source was an LL, the interrupting tone caused recall to drop from .58 to .45, $F(1,66) = 16.22$, $p < .001$, $\eta_p^2 = .20$ (see Figure 9).

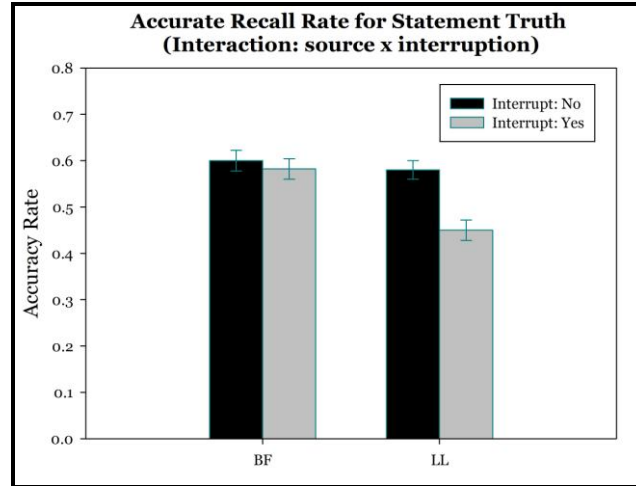


Figure 9: Interruption decreased accurate recall for truth information, but only for LL sources.

Finally, a truth x source interaction showed that recall for validity was best when true statements were paired with a best friend. Recall for statements presented as false was the same across sources (both .47), but for statements presented as true, a performance gap emerged between them, with a visible advantage for BFs ($M_{BF} = .71$, $M_{LL} = .56$), $F(1,66) = 21.08$, $p < .001$, $\eta_p^2 = .24$ (see Figure 10).

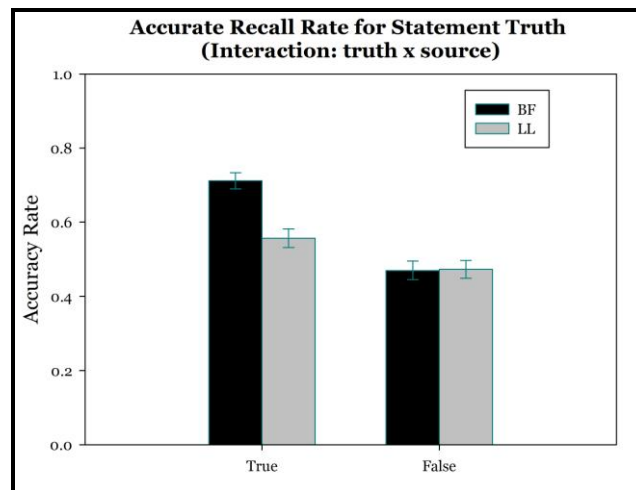


Figure 10: Recall for T statements better than F statements, but only for BF sources.

Where does this leave us with regard to default belief? To answer that question requires an inspection of reversal data. The same $2 \times 2 \times 2$ ANOVA was repeated using reversals of statement truth as the unit of analysis (i.e., proportion of true statements recalled as false, and

proportion of false statements recalled as true).³ This revealed the anticipated main effect of truth, indicating that false statements were more likely to be erroneously recalled as true (.28) than vice versa (.14), $F(1,66) = 38.95$, $p < .001$, $\eta_p^2 = .37$, and importantly, the anticipated interaction between interruption and truth, showing that interruption specifically *increased* reversals from false-to-true, $F(1,66) = 7.04$, $p = .01$, $\eta_p^2 = .10$ (see Figure 11).

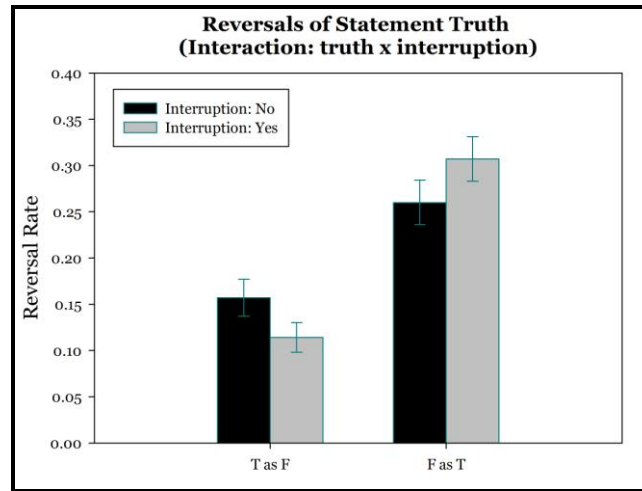


Figure 11: The “truth bias,” showing that interruption caused FasT reversals to increase, but not TasF.

Equally important, however, was a truth x source interaction indicating that although the expected Gilbert reversal from false-to-true occurred for both sources, reversals from true-to-false were *less* common when the source was a BF (.11, versus .17 for LLs), $F(1,66) = 6.23$, $p = .015$, $\eta_p^2 = .09$ (see Figure 12). As predicted by the coalitional model of an adaptive congruent “support bias,” a planned comparison of true-to-false reversals confirmed that these observed source differences were significant at the level of implicit, automatic processing exposed by interruption ($M_{BF} = .082$, $M_{LL} = .146$), $t(66) = 2.47$, $p = .016$.

³ Unless otherwise stated, there were no significant between-group differences (due to question order), and data was collapsed for all subsequent analyses.

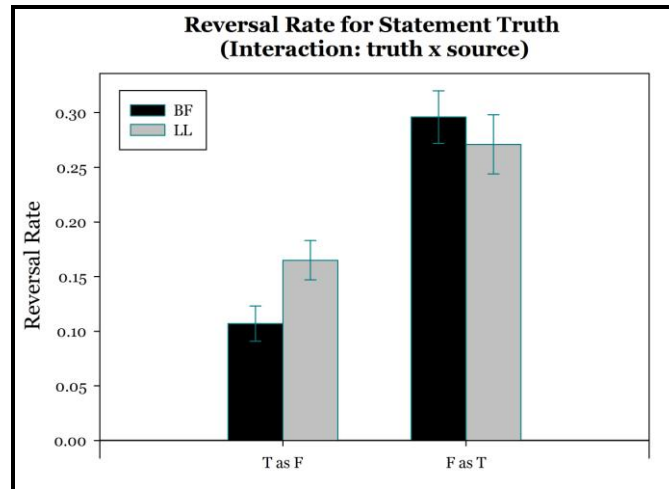


Figure 12: Adaptively congruent recall errors: TasF errors low for BFs.

An examination of reaction time data for these validity judgments revealed a large difference between Groups A and B ($M_s = 1390\text{ms}$ and 5592ms , respectively), $F(1,1,65) = 187.7$, $p < .001$, $\eta_p^2 = .74$, but this was merely a result of the different order in which questions were asked. For both groups, whichever question came second (validity or source), a much shorter response time was required because there was no need to re-read the statement upon which judgment was being made. Because of this large (though benign) overall difference, reaction times for validity judgments were analyzed separately by group.⁴

Looking first at Group B, for whom truth judgments came before source judgments, a main effect of source was observed, showing that the validity of statements coming from best friends were evaluated more quickly than statements from least-liked individuals ($M_s = 5384\text{ms}$ and 5800ms , respectively), $F(1,28) = 7.46$, $p = .01$, $\eta_p^2 = .21$. However, this main effect was qualified by the predicted source x interruption interaction, showing that interruption prompted

⁴ RT data for both validity judgments and source memory judgments (discussed below) was conservatively trimmed. M_s and SD_s were calculated within each group (A and B), and also within each type of judgment (source and validity). RTs greater than 3 SD_s above the mean were removed, as well as RTs $< 200\text{ms}$ (if it was the first query of a test trial) and $< 100\text{ms}$ (if it was the second query of a test trial). The same selection criteria were applied to the RT data in Experiments 2 and 3. In this experiment, these selection criteria resulted in the removal of 1.6% of the total RT data (130 of 8,042 RT data points).

participants to make their source judgments a full second quicker for statements from BFs than from LLs ($M_s = 5161\text{ms}$ and 6177ms , respectively), $F(1,28) = 9.58$, $p < .01$, $\eta_p^2 = .26$ (see Figure 13).⁵ Importantly, interruption also resulted in an increased frequency of truth attributions. That is, interruption not only led participants to be *quicker* in their source memory for BF statements, it also led them to judge more of those statements as being *true*. On average, an interrupted statement sourced to a BF was almost twice as likely to be rated “true” as an uninterrupted statement sourced to a BF (a 95% higher rate).⁶

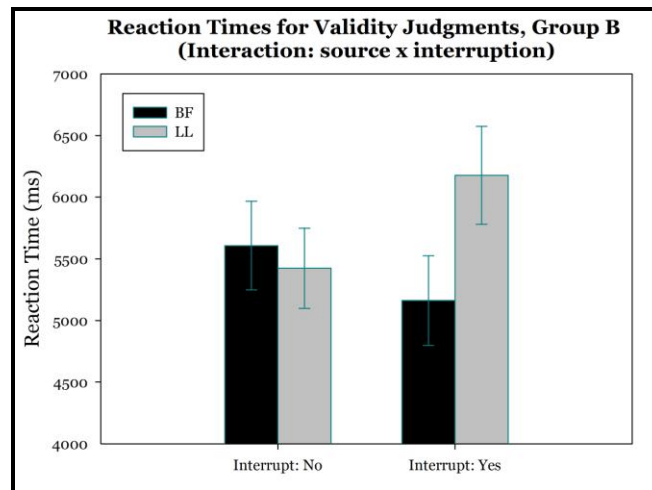


Figure 13: Interruption led to quicker RTs for BFs (on truth judgments).

In sharp contrast, an analysis of the validity reaction time data for Group A, for whom validity judgments came after source judgments, revealed no main effects or interactions.

Recall for source. Turning to source memory performance, did recall differ between the two sources? Yes, but indirectly. Overall source accuracy was in fact identical for BFs and LLs (.69 for both). But a 2 (source: BF, LL) x 2 (truth: true, false) x 2 (interruption: yes, no) within-subjects ANOVA, taking source memory performance as the dependent measure, revealed

⁵ All other comparisons, n.s.

⁶ An interrupted statement sourced to an LL was 59.9% more likely to be rated “true” compared to an uninterrupted statement sourced to an LL, so interruption increased the likelihood of truth attributions in general. But it increased the likelihood at a much higher rate among BFs than LLs (59% higher).

several suggestive interactions. First, an interaction between truth and interruption showed that although source memory was the same for true and false statements that were not interrupted (.70), interruption led to a decline in source memory for false statements (.65), $F(1,66) = 4.11$, $p = .047$, $\eta_p^2 = .06$ (see Figure 14).

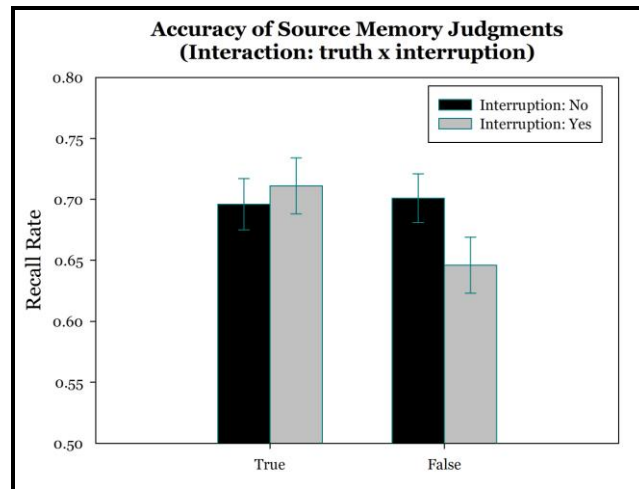


Figure 14: Interruption produced a decrease in source memory accuracy, but only for statements presented as false. This change, however, is explained by a source x interruption interaction (described below), showing that interruption affected the two sources differently—*improving* source accuracy for BFs but *decreasing* source accuracy for LLs.

The nature of this change becomes somewhat clearer when we consider an additional interaction observed between source and interruption. This shows that the decline produced by interruption was specific to LL sources (from .72 down to .66), whereas a small degree of *improvement* was observed for BF sources (from .68 up to .70), $F(1,66) = 3.72$, $p = .058$, $\eta_p^2 = .04$. As can be seen in Figure 15, although source memory was in fact better for LLs when interruption was absent, this relationship inverted when interruption was present.

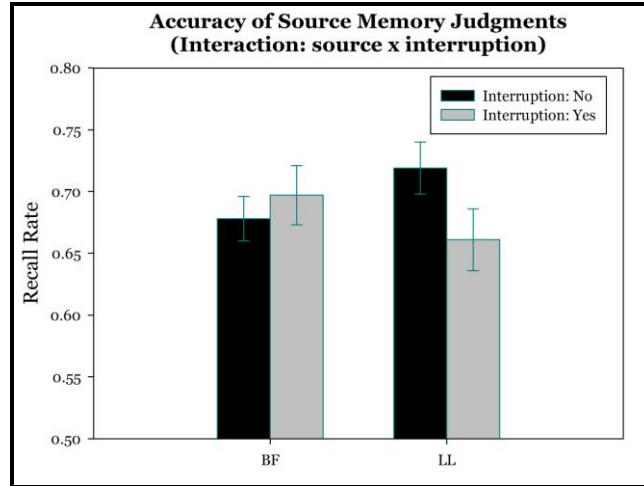


Figure 15: Interruption caused source accuracy to decrease for LLs, but not BFs.

Finally, a trending source x truth interaction indicated that although source memory for LLs was the same whether statements had been tagged as true or not (.69), source memory for BFs was slightly better for true statements (.72), and slightly worse for false statements (.66), $F(1,66) = 2.92, p = .09, \eta_p^2 = .04$ (see Figure 16).

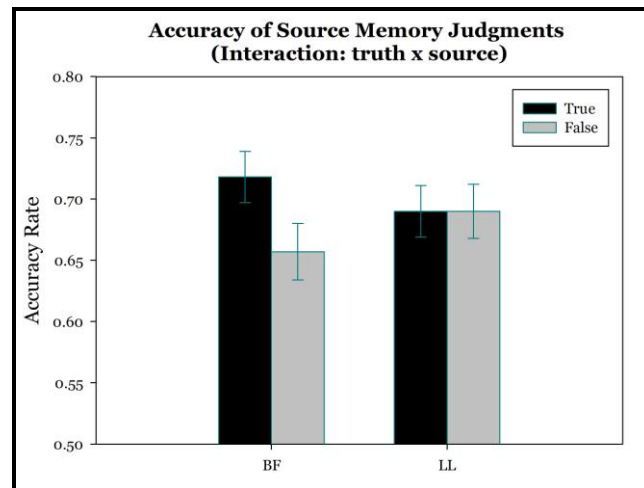


Figure 16: Rates of source memory recall for LLs were the same whether the statement had been true or false, but source memory for BFs was significantly better for true statements than false statements.

Strictly speaking, none of these source analyses bear directly on the matter of default belief, but they do suggest a complex dynamic between the two sources. The fact that interruption produced the interactions reviewed here should call our attention to the possibility

that some portion of these source dynamics are operating at the same implicit level of cognition as Gilbert's truth bias.

What about the predicted difference in source memory for individuals in Group A (where source memory was queried prior to validity), as a function of interruption? The prediction was that interruption would have its typical effect of facilitating a truth bias for those statements, and in Group A, the condition where source memory was queried *prior* to validity, a support bias would lead participants to displace their sense of *truth* onto their judgment of *source*, leading to an increased rate of BFs being recalled as the source.

In line with this prediction, participants in Group A showed no tendency to recall one source more than the other on uninterrupted trials (source recall rates for uninterrupted trials were $M_{BF} = .498$, $M_{LL} = .503$), but a tendency *was* observed to recall BFs more frequently than LLs on trials where interruption *had* been present (source recall rates for interrupted trials: $M_{BF} = .531$, $M_{LL} = .469$). Although this difference was not statistically significant, $t(37) = 1.43$, $p = .16$, it was in the predicted direction. If interruption leads to a default presumption of truth, it would be adaptively congruent for that presumption to cast a shadow upon source judgments—in this case nudging source memory away from a balanced proportion of choices toward a rate favoring best friends over enemies.

Notably, this trend was also connected to a change in truth judgments. Among those statements that had been interrupted, subjects who first selected their BF as the recalled source *also* recalled those statements being *true* at a higher rate than when LL was selected as the source (recall rates were $M_{BF} = .522$, $SE = .03$; $M_{LL} = .425$, $SE = .03$), $t(37) = 2.51$, $p = .017$. But this only held true for interrupted statements. When statements had not been interrupted, subjects who first selected their BF as the recalled source were *not* more likely to judge

statements as true than when LLs were selected (recall rates were $M_{BF} = .429$, $SE = .03$; $M_{LL} = .394$, $SE = .03$), $t < 1$. Of course, since truth judgments were made after source judgments, it is possible that controlled processing partially accounts for this finding. But it would not help explain why interruption made a *selective* difference for the two sources, since any deliberate, controlled processing would presumably affect validity judgments regardless of whether the statements had been interrupted. Seeing that interruption leaves this mark in the data is suggestive of automatic epistemic processes that facilitate judgment in directions that are adaptively congruent with coalitional support.⁷

Turning to reaction time data for source memory, as with the RT data for validity judgments a large difference between Groups A and B was observed ($M_s = 4858\text{ms}$ and 1082ms respectively), $F(1,1,65) = 345.2$, $p < .001$, $\eta_p^2 = .84$, but as already indicated this was merely a result of the different order in which questions were asked. Each group took more time to answer the first question they faced (whether validity or source judgment). Once again, RTs for source judgments were analyzed separately by group.

Reaction times for Group A, for whom source judgments came before validity judgments (and for whom *no* RT differences of any kind were observed with regard to validity RTs), showed a main effect of truth, indicating that source memory decisions were rendered more quickly for true statements than for false statements ($M_T = 4732\text{ms}$, $SE = 185$; $M_F = 4984\text{ms}$, $SE = 170$), $F(1,37) = 4.61$, $p = .038$, $\eta_p^2 = .11$. Two important interactions qualify this finding,

⁷ Although no predictions were made for Group B, where validity judgments were made first and then source judgments, we see a somewhat similar picture. Looking at the conditional probability of choosing either source depending on whether the validity judgment was true or false, we found a difference between source recall (even for uninterrupted trials), with a strong tendency to choose a BF over an LL if the initial truth judgment had been *true*, (recall rates for BFs and LLs on uninterrupted statements first judged as true were $M_{BF} = .429$, $SE = .03$; $M_{LL} = .271$, $SE = .02$), $t(37) = 3.26$, $p = .002$. However, this difference was magnified when statements had been interrupted; that is, the tendency to choose a BF over an LL when the initial validity judgment was true grew *larger* (recall rates for interrupted statements first judged as true were $M_{BF} = .522$, $SE = .03$; $M_{LL} = .253$, $SE = .03$), $t(37) = 4.80$, $p < .001$.

however. First, an interaction between truth and interruption showed that source memory decisions for true and false statements were made at identical rates when interruption was absent ($M_s = 4817$ and 4815 , respectively), but interruption elicited faster RTs for true statements and longer RTs for false statements ($M_s = 4647$ and 5154 , respectively), $F(1,37) = 7.35$, $p = .01$, $\eta_p^2 = .17$ (see Figure 17).

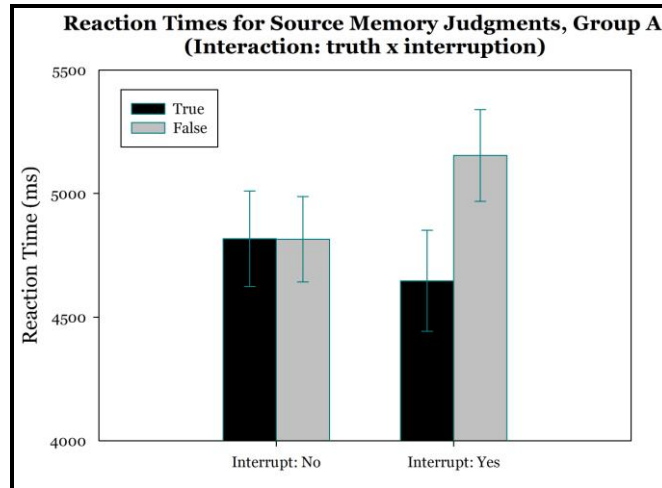


Figure 17: Interruption caused source memory RTs to decrease for true statements, and increase for false statements.

Second, and critically, a source x truth interaction revealed that it was BF reaction times that were affected by interaction. While reaction times for LL judgments were approximately equal across epistemic states ($M_T = 4906$, $M_F = 4950$), BF source judgments were faster when true ($M_T = 4558$) and slower when false ($M_F = 5019$), $F(1,37) = 4.07$, $p = .051$, $\eta_p^2 = .10$ (see Figure 18).

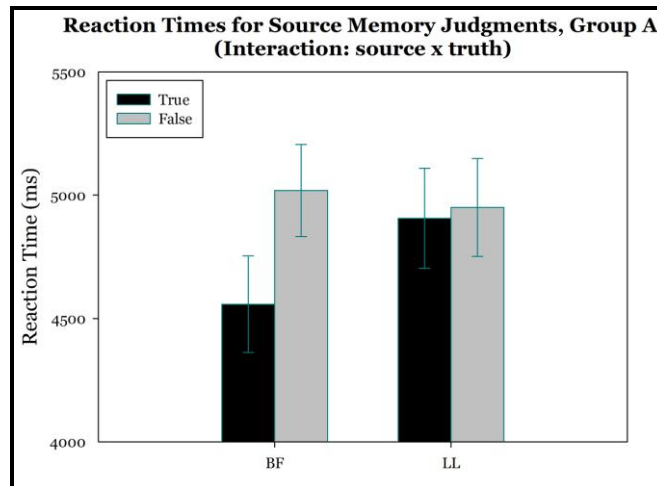


Figure 18: The effect of interruption on RTs was borne entirely by BF sources (adaptively congruent): RTs for best friends became faster for true statements, but slower for false statements.

In stark contrast, an analysis of the Group B RTs for source memory, where source judgments came after validity judgments, revealed no significant statistical effects or interactions (all $ps > .14$).

Discussion

The results of Experiment 1 encourage the continued use of the present protocol. Accuracy for both source and truth information was above chance, indicating that the Gilbert protocol was robust to the insertion of additional variables and test measures, without inflicting detrimental costs on memory performance. With regard to measuring both truth and source at test, the order in which those questions are posed did not seem to have a significant impact on memory performance, although reaction times were considerably affected. RTs for the second half of each test trial were significantly faster than RTs for the first half, but more interestingly, the source-truth-interruption relationships observed among the RTs for the first half of each test trial (whether source or truth) were wholly absent among the RTs for the second question (whether source or truth).

Recall and reaction time data for statement truth provided provisional supporting evidence for the coalitional hypotheses advanced here. Although the signature components of the Gilbert effect were preserved (interruption fostered an overall increase in false-to-true reversals), they were modulated by source information. The best recall accuracy was found among statements that had paired a best friend with truth, and the impact of interruption was to produce *fewer* true-to-false reversals for best friends than for least-liked acquaintances. Additionally, the reaction times for these judgments proved adaptively congruent: interruption led to faster RTs for statements originally paired with a best friend, *and* increased truth attributions for those statements.

Source memory data also revealed adaptively congruent results. The best source memory performance was found for statements pairing a best friend with truth, while the worst performance was found among statements pairing a best friend with falsity. Moreover, the impact of interruption on reaction times was markedly selective: times became faster for statements that had paired a best friend with truth. And analyses of conditional probability indicated that, among Group A participants who judged source memory prior to statement validity, interruption prompted participants to select BFs as the source slightly more often than LLs, and prompted them to recall truth for those BFs at a higher rate than when interruption was absent—a set of findings consistent with the hypothesis of coalitional support operating at the level of implicit epistemic bias.

Experiment 2: Source Valence

An important difference between friends and enemies is their contrasting *valence*: friends are reliably associated with positive affect, whereas enemies are reliably associated with negative affect. But these associations present a potential confound for the coalitional interpretations

explored here. Any discrepancy that arises in the pattern of default belief might be due *not* to the influence of coalitional dynamics, but to the clear affective differences that distinguish the sources. This possibility can be tested. Although the attribute of valence is intrinsic to matters of coalitional affiliation, the reverse is not true: valence applies to a vast set of psychological phenomena having nothing to do with coalition. Therefore, by changing the *type* of sources chosen by participants—namely, to sources with positive or negative valence but without coalitional affiliation—we may assess the influence of valence on default belief, independent of coalitional considerations. If the results observed with non-coalitional sources overlap with those produced with best friends and enemies, then the postulated import of coalitional cognition on default belief needs to be scrutinized closely. Alternatively, if the results obtained differ from those obtained with coalitional sources, then we are justified in our continued exploration of coalitional constructs, which would include, but not be reducible to, differences of valence. The predictions ventured here are straightforward: truth judgments (rendered in the context of the present protocol) will not differ between the two non-coalitional sources, despite their manifest differences in valence. To implement this manipulation, participants were asked to type in the names of their most-liked and least-liked high school teachers.⁸

Method

Participants. Thirty-three undergraduates from Washington University (12 female, 21 male) with a mean age 19.9 years, participated for course credit. All participants were native English speakers. No between-groups design was used to vary the order in which participants

⁸ High school teachers were chosen instead of college professors because (1) some of our participants (e.g. freshmen) had limited experience to draw from, truncating their options (i.e. seniors have been exposed to more instructors), (2) pilot studies using preferred and disliked professors as sources revealed that even among older students, subjects' choices were not sufficiently valenced (some students had no professors they especially disliked), making the designation of "least-liked" a forced and non-representative label, and (3) pilot studies indicated that participants were easily able to identify high school teachers about whom they retained strong affective sentiments.

answered source and validity questions at test. For each statement, all participants were asked to recall the source first, and then validity.

Materials, design, and procedure. All aspects of the experiment were identical to Pilot Experiment 2 except for the following. First, participants were asked to enter the names of the *high school teacher* they had most liked (positive valence without coalitional affiliation), and the one they had least-liked (negative valence without coalitional affiliation).

The test phase was identical to Experiment 1 in its random presentation of 56 trivia statements (48 old, 8 new), and participants were once again be asked to (1) recall the source of each statement, and (2) recall whether each statement had been true, false, followed by no information, or was new.

Results

Recall for validity. Data from one participant was removed prior to analysis due to inattention. Does the valence of the source, independent of coalitional affiliation, influence default belief? Addressing recall for statement validity first, the proportion of statements accurately recalled as true or false was submitted to a 2 (truth) x 2 (interruption) x 2 (source) repeated-measures ANOVA. A main effect emerged for interruption, showing that the truth of uninterrupted statements was remembered better ($M = .53$, $SE = .03$) than interrupted statements ($M = .47$, $SE = .03$), $F(1,31) = 6.68$, $p = .015$, $\eta_p^2 = .18$. But this was qualified by two interactions. First, the expected interaction between truth and interruption was found, indicating that interruption had reduced accuracy for false statements but not true statements (presumably because the false statements were being erroneously recalled as true), $F(1,31) = 4.85$, $p = .035$, $\eta_p^2 = .14$.

However, an additional interaction between source and interruption colors this interpretation by revealing that the impact of interruption was borne entirely by LL teachers rather than ML teachers, $F(1,31) = 5.08$, $p = .031$, $\eta_p^2 = .14$. For most-liked teachers, recall was not affected at all by interruption (.50 in both conditions), but interruption produced a decrease in accuracy for least-liked teachers (.44 when interrupted, versus .55 when not) (see Figure 19).

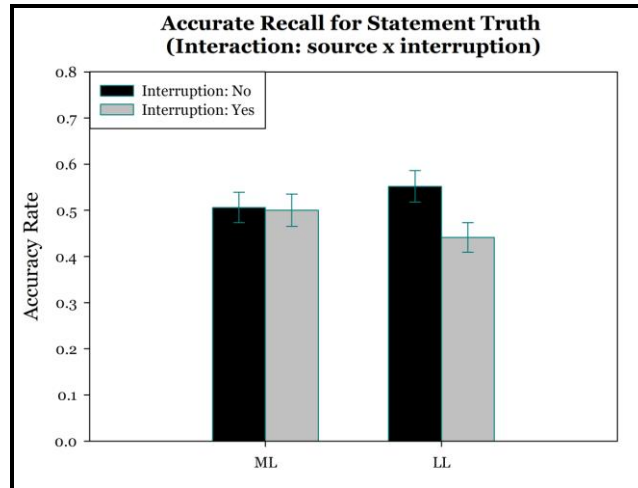


Figure 19: Interruption decreased accurate recall for truth information, but only for LL teachers.

An ANOVA was then performed upon rates of truth-reversal, resulting in a main effect of truth (as expected, reversals from false-to-true were more common than reversals from true-to-false: $M_s = .22$ and $.14$ respectively; $SE_s = .03$ and $.02$ respectively), $F(1,31) = 6.30$, $p = .018$, $\eta_p^2 = .17$. But no other significant findings emerged (all $p_s > .13$), including no truth x interruption interaction, anticipated to show that false-to-true reversals were more common following interruption.

When reaction times for validity judgments were examined, a single significant interaction was observed between source and interruption, showing that RTs for truth judgments were somewhat slower for ML teachers when statements were not interrupted, but reversed (truth

judgments somewhat faster for ML teachers) when statements were interrupted, $F(1,31) = 7.11$, $p = .012$, $\eta_p^2 = .19$ (all other F s < 1) (see Figure 20).

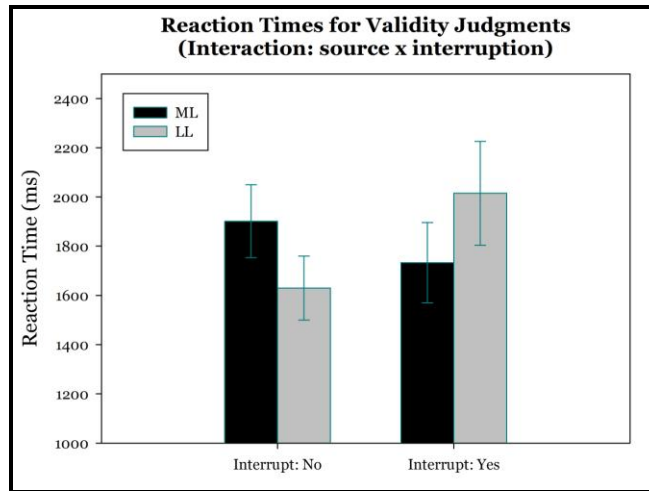


Figure 20: Interruption made validity RTs faster for ML teachers, and slower for LL teachers.

Recall for source. Turning to source memory performance, overall accuracy rates were comparable whether the target was the most-liked high school teacher ($M = .73$, $SE = .02$) or the least-liked teacher ($M = .72$, $SE = .03$). A repeated-measures ANOVA revealed no main effects, but one interaction between truth and source, $F(1,31) = 5.89$, $p = .021$, $\eta_p^2 = .16$ (all other p s $> .20$). This finding indicated that recall of one's most-liked teacher was better for statements that had been true (.78 versus .68 for false statements), whereas the successful recall of one's least-liked teacher did not depend on whether the statement had been true or false (.70 in both cases) (see Figure 21).

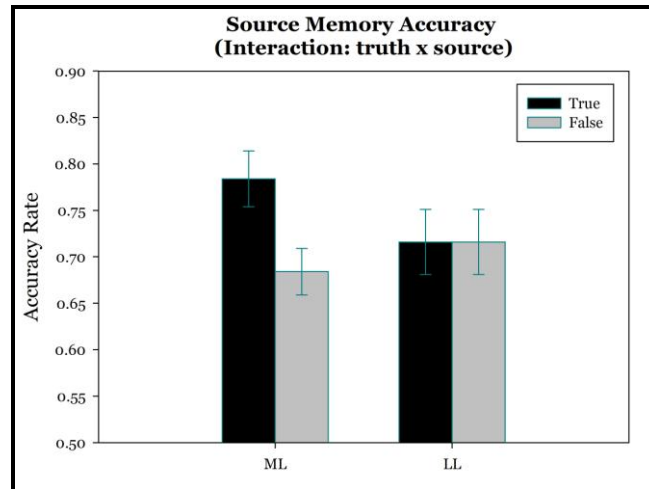


Figure 21: For LL teachers, successful recall of source was the same for both true and false statements, but for ML teachers, recall was significantly better for true statements.

A closer examination of the source memory data revealed no systematic tendency to recall one source over the other, even under conditions of interruption. For the 32 uninterrupted statements which had been presented during the study phase, participants recalled their most-liked teacher as the source 52% of the time, a proportion which did not differ from chance: $t(31) = 1.25$, $p > .20$, and for the 16 *interrupted* statements, participants were still no more likely to recall their ML teacher as the source (51%), $t(31) < 1$, $p > .60$. Nonetheless, for the purpose of comparing these results to those of Experiment 1, an examination of conditional probabilities was made, but revealed no significant differences between sources: whether subjects first decided that the source was their ML or LL teacher had no impact on whether they subsequently decided the statement's validity was true or false, and this held regardless of interruption ($t_s < 1$).

An inspection of RTs for source memory revealed that the overall decision times required to recall the two types of sources did not significantly differ from each other: ($M_s = 4892$ ms and 5188ms, for ML and LL respectively), $F(1,31) = 2.01$, $p > .15$. However, two interactions were observed, one between source and truth (participants were quickest to assess the source of statements that paired their favorite teacher with truth, and slowest on statements that paired their

least-favorite teacher with truth), $F(1,31) = 9.45, p = .004, \eta_p^2 = .23$, and also between source and interruption (in the presence of interruption, RTs became quicker for statements paired with ML teachers, whereas in the absence of interruption, RTs for ML and LL teachers were approximately the same), $F(1,31) = 21.16, p < .001, \eta_p^2 = .41$ (see Figures 22 and 23).

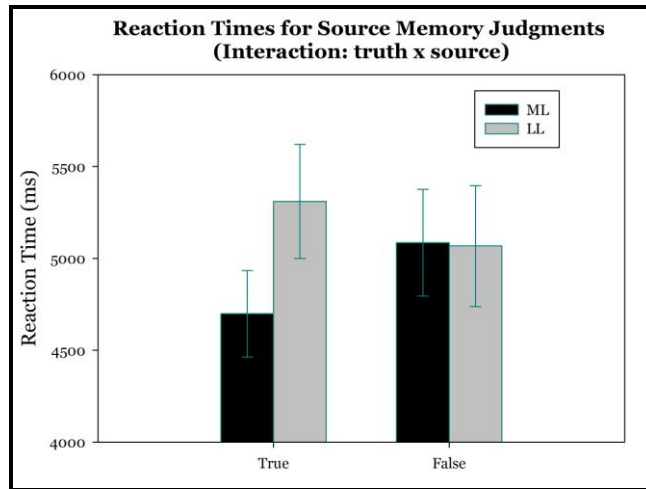


Figure 22: For statements that had been presented as true, recall of source information was significantly faster for ML teachers than for LL teachers.

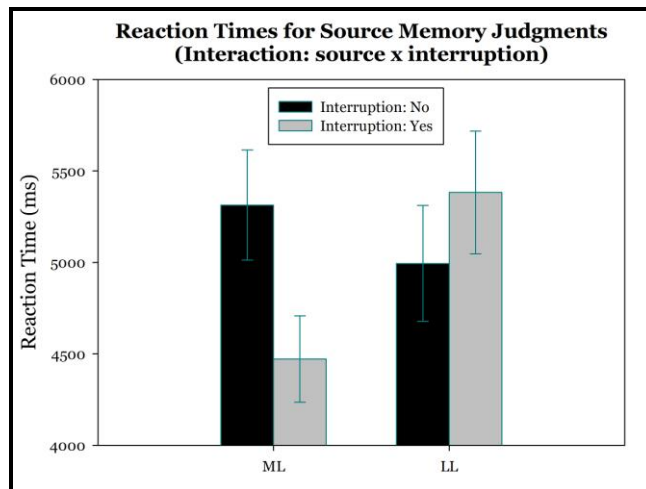


Figure 23: Interruption led source memory RTs to decrease for ML teachers and increase for LL teachers.

Discussion

The results of this experiment will require a more nuanced treatment in the general discussion, but we can say here that (1) although valence does seem to influence recall of source and truth information, (2) in ways similar to coalitional sources in Experiment 1, (3) these differences were less visible at the level of default belief: among the truth-reversal data (recalling true statements as false, or false statements as true), there were no observed interactions between source and truth, nor between source and interruption.

In terms of recall for truth information, interruption decreased overall accuracy for false statements (as expected), but it also selectively affected LL teachers rather than ML teachers (in much the same way that enemies were affected in Experiment 1), which was not anticipated. In terms of the reversal data, there were no interactions, but the *pattern* of the data still bears much resemblance to the reversal data obtained in Experiment 1, and some of the nonsignificant interactions (e.g., source x interruption, $p = .13$) may have reached levels of significance if the statistical power of this study had matched that of Experiment 1 (where sample size was double that of Experiment 2).

In terms of RTs for validity statements, no findings were anticipated here. Indeed, in Group A of Experiment 1 (which comparably assessed source memory first and then truth) no significant findings related to validity RTs were found. But here a significant interaction was observed between source and interruption (similar to Group B of Experiment 1), showing that interruption prompted faster RTs for statements associated with ML teachers, and slower RTs for statements associated with LL teachers.

Where source memory is concerned, there were additional parallels with Experiment 1. Successful recall of LL teachers occurred at a similar rate for both true and false statements, but

not for ML teachers: when the source was an ML teacher, source was recalled significantly better if the statement had also been true. Reaction time data for source memory also revealed source differences: source was recalled significantly faster if a statement had paired an ML teacher with truth, but significantly slower if it had paired an LL teacher with truth. Finally, as for best friends in Experiment 1, interruption led to selectively quicker RTs for ML teachers.

In short, when our future understanding of coalitional cognition finally allows us to delineate the parameters that fully demarcate it as a cognitive construct, we can be reasonably confident that valence will play a significant role in its constitution (or covary strongly with an equally important third variable).

However, it would be unreasonable to dismiss the relevance of coalition, because several key source distinctions emerged between the two studies. Unlike Experiment 1, where true information was better recalled when paired with a BF versus an LL, and true statements were more likely to be reversed (T-as-F) for LLs than BFs, no such relations were found among ML and LL teachers. And though as already noted it is quite possible that some marginal trends would reach significance with increased statistical power, it is also true that some of the observed interactions between ML and LL sources here moved in different directions than the interactions between best friends and enemies. For instance, in Experiment 1 the reaction times for source memory showed that the fastest judgments were for statements that joined BFs with truth, and the slowest judgments were for statements that joined BFs with falsity—an adaptively congruent effect, consistent with the idea of default epistemic support of coalitional ties. However, in Experiment 2, RTs for the source memory of false statements did not differ between ML and LL teachers. Additionally, interruption did not prompt greater recall of MLs as the source, nor a greater likelihood of veracity judgments in favor of truth when sourced to an ML (as occurred

with Group A in Experiment 1, for whom statistical power was comparable). Finally, in Experiment 1 the effect of interruption on source memory RTs was to increase speed for statements that paired BFs with truth (adaptive congruency), whereas the effect of interruption on ML teachers in Experiment 2 was not specific to either true or false statements.

Experiment 3: Precaution

The rationale for using remote trivia statements has been, as with the original Gilbert studies, to ensure that participants' background knowledge would not influence their assessment of statement truth; in this way, the computer-provided truth tag should be the participants' only subjective basis for recalling truth. However, this approach may reduce ecological validity to the extent that it ruptures the natural link between a speech act and its intentional purpose(s) (e.g. Grice, 1957; Sperber & Wilson, 1996). Outside of artificial environments (e.g. formal schooling), communicative acts are more likely to be immediately relevant (e.g., social news, gossip, precautionary warnings, health advice) and less in the service of transmitting comparatively esoteric semantic information (e.g. trivia). As such, a more compelling protocol would seek to include statements that are epistemically ambiguous but also potentially relevant. Such is the aim of this final experiment.

Although studies in areas other than epistemic belief have indicated the likelihood of domain-specific cognition devoted to precautionary concerns (e.g., contagion, contamination, physical danger; see, e.g., Boyer & Lienard, 2006), there are no theoretical reasons advanced here as to why the domain of precaution would interact with coalitional sources at the level of default belief. As such, the hypotheses ventured here are the same as those for Experiment 1: a coalitional "support bias" that manifests itself in (1) the reversal data (when the source is an LL, the data will remain no different than when no source is present, but when the source is a best

friend, interruption will increase the perception of truth, resulting in an *increase* of false statements recalled as true, and a *decrease* of true statements recalled as false), (2) in source memory (a bias, for interrupted statements, towards recalling one's best friend as the source when source is queried before validity), and (3) in reaction times (for source and truth judgments, where RTs are quickest when a statement pairs a best friend with truth, and amplified by interruption).

Method

Participants. Thirty-five Washington University undergraduates (24 female, 11 male), with a mean age 19.21 years, participated for course credit or \$5.00.

Materials, design, and procedure. All aspects of the experiment were identical to Experiment 1, with the key exception of a change to the statement stimuli. In place of arcane trivia, the set of statements were changed to ambiguous precautionary statements: admonitory pronouncements of uncertain validity (see Appendix B for full stimulus set). Examples include:

“Bruised fruit is more likely to be contaminated by bacterial pathogens.”

“Drinking water from a plastic bottle that was left in a heated car may contain toxins.”

“There are more germs on a bathroom door handle than on the bathroom floor.”

Pilot data were gathered for these statements on their general plausibility (using a Likert scale of 1-7). Only fully ambiguous statements (with average validity ratings between 3.2 and 4.8) were selected for inclusion, to prevent the interference of background knowledge and to preserve the function of the computer-provided truth tag. These precaution statements were randomly assigned to one source of the other, with the constraint that the aggregated plausibility means for statements attributed to each source equal a fully neutral 4.0. No other aspects of the protocol were changed.

Results

Data from three participants were removed prior to analysis due to implementation of the wrong SuperLab script (in one case) and inattention (two cases). The accuracy of an additional three participants was nearly two *SDs* below the mean (and within chance levels), but statistical analyses were not qualitatively changed by their omission from the data, so the reports that follow include them.

Recall for validity. Did the domain of information make a difference? Was precautionary information treated differently than more mundane types of information? Overall accuracy for the recall of statement validity was .59 ($SE = .03$), comparable to the previous two experiments. The proportion of precautionary statements accurately recalled as true and false was once again submitted to a 2 (truth) x 2 (interruption) x 2 (source) repeated-measures ANOVA. Main effects were observed for source, where better accuracy was observed for statements associated with a best friend ($M_{BF} = .62$, $SE = .04$; $M_{LL} = .56$, $SE = .03$), $F(1,31) = 4.28$, $p = .047$, $\eta_p^2 = .12$, as well as for interruption, indicating that truth was better recalled on uninterrupted trials ($M = .64$ versus $.55$ for interrupted trials), $F(1,31) = 8.79$, $p = .006$, $\eta_p^2 = .22$. However, these main effects were qualified by a single two-way interaction between source and interruption, showing that the modulating effect of interruption was restricted to least-liked sources (accuracy .49, versus .64 when uninterrupted), and did not affect accuracy when the source was a best friend (accuracy .61, versus .63 when uninterrupted), $F(1,31) = 5.14$, $p = .031$, $\eta_p^2 = .14$ (see Figure 24). However, unlike previous studies, the effect of interruption on LL accuracy was not restricted to false statements; accuracy for both true and false statements plummeted (hence the absence of a source x truth x interruption interaction).

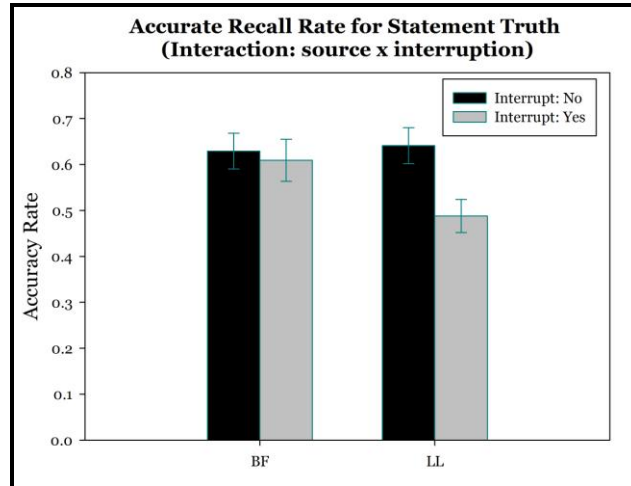


Figure 24: Interruption decreased accurate recall for truth information, but only for LL sources.

This ANOVA was then repeated using reversals as the dependent measure, but no significant findings were observed. Likewise, an analysis of RTs for truth judgments also revealed no significant main effects or interactions, aside from a marginal main effect of truth (statements presented as true were recalled more quickly: $M_T = 1491\text{ms}$, $M_F = 1648\text{ms}$), $F(1,31) = 4.11$, $p = .051$, $\eta_p^2 = .12$.

Recall for source. Turning to source memory (which was queried before memory for truth), best friends were recalled better on average ($M = .79$, $SE = .02$) than least-liked individuals ($M = .72$, $SE = .02$), $F(1,31) = 5.39$, $p = .027$, $\eta_p^2 = .15$. However, a strong interaction between truth and source revealed that BFs were better remembered for true statements ($M_T = .82$, versus $M_F = .76$), while LLs were better remembered for false statements ($M_F = .79$, versus $M_T = .66$), $F(1,31) = 16.91$, $p < .001$, $\eta_p^2 = .35$ (see Figure 25). Additionally, a marginal 3-way interaction was observed between source, truth, and interruption, indicating that interruption affected the two sources differently: it improved BF source accuracy among *false* statements (but not true), while improving LL source accuracy among *true* statements (but not false), $F(1,31) = 2.94$, $p = .097$, $\eta_p^2 = .09$ (see Figures 26a and 26b).

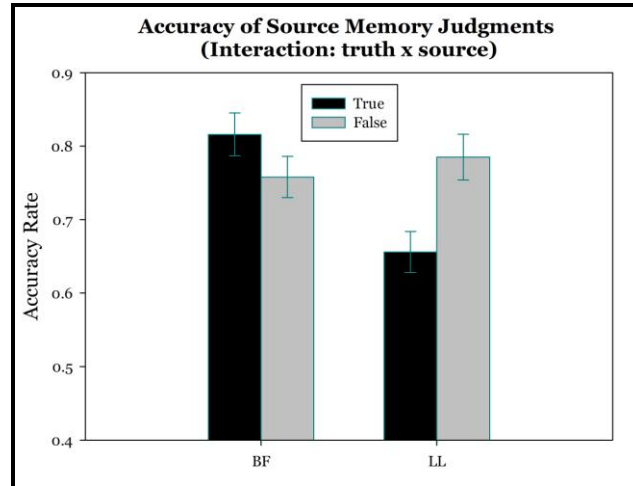
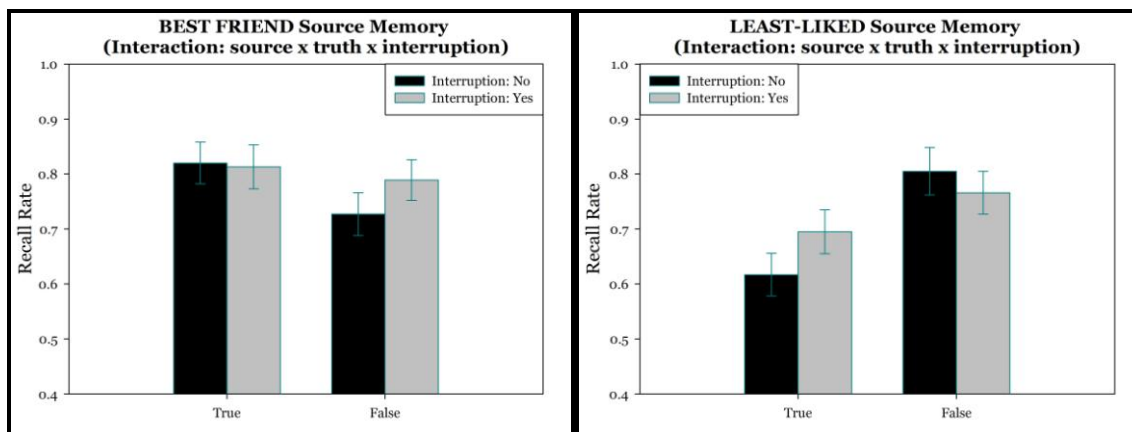


Figure 25: Recall of validity for false statements was the same for both BF and LL sources, but successful recall of *true* statements was much better for BF sources than LL sources.



Figures 26a and 26b: A 3-way source x truth x interruption interaction of source memory: Interruption caused source accuracy for BFs to increase for *false* statements (but not true), while interruption caused LL source accuracy to increase for *true* statements (but not false).

As with Experiment 1, there was no reliable tendency for participants to recall one source more than the other on *uninterrupted* trials (source recall rates for uninterrupted trials were $M_{BF} = .531$, $M_{LL} = .469$), $t(31) = 1.70$, $p = .10$, but a reliable tendency to do so on *interrupted* trials (source recall rates for interrupted trials: $M_{BF} = .535$, $M_{LL} = .465$), $t(31) = 2.12$, $p < .05$. Although this difference was small, it was statistically significant and in the predicted direction. Moreover, this trend was connected to a change in truth judgments. For interrupted statements,

subjects who first selected their BF as the recalled source also recalled those statements being *true* at a higher rate than when their LL was the selected source (recall rates for truth were $M_{BF} = .430$, $SE = .03$; $M_{LL} = .309$, $SE = .03$), $t(31) = 2.69$, $p < .01$. For *uninterrupted* trials, however, this pattern was no longer statistically reliable (recall rates were $M_{BF} = .467$, $SE = .04$; $M_{LL} = .378$, $SE = .04$), $t(31) = 1.93$, $p = .062$. This pattern of findings, which was also found in Experiment 1 (among BFs and LLs), but not in Experiment 2 (among ML teachers and LL teachers) adds some measure of plausibility to the notion of a support bias operating specifically among coalitional allies (best friends).

An examination of reaction times for source judgments revealed only a main effect of interruption, showing that source memory decisions were quicker for statements that had not been interrupted ($M = 4081$ ms, compared to $M = 4348$ ms for interrupted statements), $F(1,31) = 8.46$, $p = .007$, $\eta_p^2 = .21$.

The results from the present experiment deviate substantially from the foregoing two. In order to clarify the results, an additional point of comparison was sought.

Experiment 4: Precaution 2

In an effort to get a clearer picture of the relationship between source and recall for the truth of precautionary information, a second group of participants were run using the same protocol as those in 3-A but omitting the source memory assessment at test.⁹ This was an attempt to implement the original Gilbert study using precautionary statements with as little modification as possible (i.e. without any noise potentially associated with providing source memory judgments, especially since both Experiment 2 and Experiment 3A had participants making source judgments *prior* to making validity judgments).

⁹ Forty-eight participants were recruited from the WU human subjects pool (28 female, 20 male; mean age 19.28). Data for three participants was removed prior to analysis due to evidence of inattention and/or failure to follow directions.

Recall for validity. Under these slightly modified conditions, overall accuracy for the recall of statement validity was .58 ($SE = .02$). Upon examination of recall for truth information, main effects were observed for truth, indicating that true statements were better recalled than false statements ($M_T = .62, SE = .03; M_F = .54, SE = .03$), $F(1,44) = 7.65, p = .008, \eta_p^2 = .15$, and for interruption, indicating that uninterrupted statements were better recalled ($M = .62, SE = .02$) than interrupted ones ($M = .53, SE = .03$), $F(1,44) = 8.40, p = .006, \eta_p^2 = .16$. However, these were qualified by a marginal source x interruption interaction, showing interruption to produce a decrement in recall, but selectively for statements from an LL source, $F(1,44) = 3.01, p = .09, \eta_p^2 = .06$ (see Figure 27).

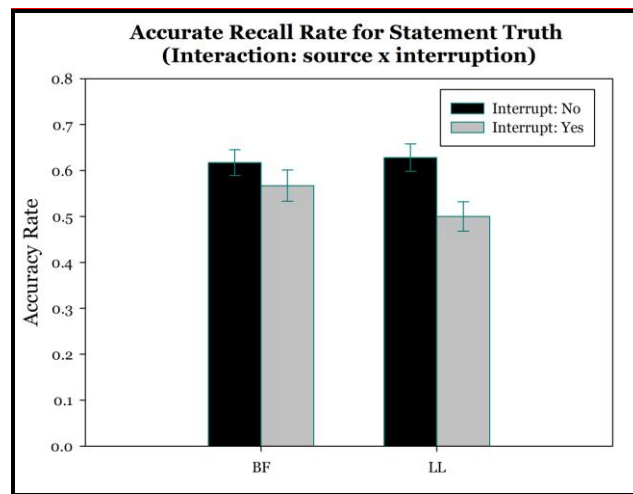
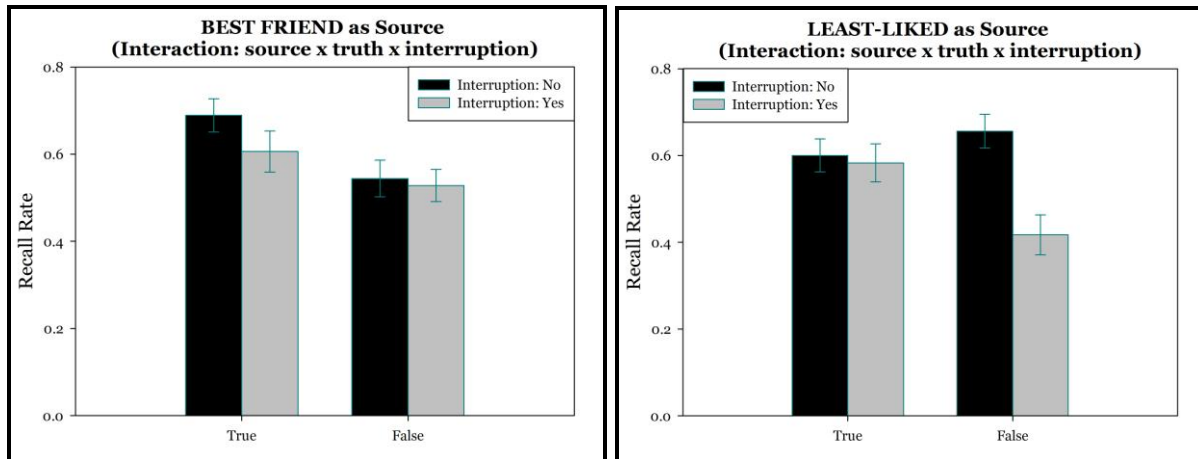


Figure 27: Interruption significantly reduced recall of statement validity for LL sources but not BF sources.

A three-way (source x truth x interruption) interaction was also observed, showing that *interruption* made a selective impact on the accurate recall of *false* statements associated with *LL* sources, $F(1,44) = 7.60, p = .008, \eta_p^2 = .15$ (see Figures 28a and 28b).



Figures 28a and 28b: A 3-way interaction within memory for statement validity: *interruption* selectively reduced the accurate recall of *false* statements associated with *LL* sources.

Turning to reversals as the dependent measure, a single main effect was observed for truth, indicating that false statements were more likely to be reversed to “true” than vice versa ($M_s = .20$ [$SE = .02$] and $.14$ [$SE = .02$], respectively), $F(1,44) = 5.02$, $p = .03$, $\eta_p^2 = .10$. No other analyses were statistically significant.

Finally, analysis of reaction times for truth judgments revealed only a main effect of truth, such that decision times for true statements were slightly shorter ($M = 3297$ ms, $SE = 127$) than those for false statements ($M = 3542$ ms, $SE = 142$), $F(1,44) = 7.28$, $p = .01$, $\eta_p^2 = .14$.

Discussion

When precautionary statements are the communicated information, the source of that information continues to matter, but not in the same way as previous studies. In these precaution studies, (1) information labeled as true was better remembered by best friends than least-liked individuals, (2) interruption exerted a detrimental influence on recall for information from LLs, but not BFs, and (3) source memory was best when statements paired truth with BFs.

But in the domain of reversal data, where the truth bias typically makes its signature mark, we see an absence of significant results (in Experiment 3) and no source differences of any

kind (in both Experiments 3 and 4). Reaction time data in both studies also failed to reveal any distinctions between the two sources, neither for source nor validity judgments.

These are unexpected results, but it would be premature to discount the importance of further study in this area. It is possible that a more detailed set of theoretical predictions needs to be specified with regard to precautionary statements, given that, in areas of study other than epistemic belief, research has indicated the presence of domain-specific cognition devoted to precautionary concerns (e.g., contagion, contamination, physical danger) (see, e.g., Boyer & Lienard, 2006).

But another important possibility is a subtle difference among the precautionary statements that was absent among the trivia statements; namely, that *the precaution statements provided useful information when false*. Hasson and colleagues (Hasson et al., 2005) were able to replicate the Gilbert truth bias for statements that did not permit informative inferences when false, but were *not* able to replicate the truth bias when false statements *did* permit informative inferences. To use one of their examples, learning that *George owns a television* is false implies several possible things about George. It could mean that he is an avid reader with no interest in television, or it could mean that he streams everything he watches to a computer, or that he is so financially impoverished he can't afford a television (or an apartment in which to put it). Of course, none of these possibilities need be true. But the point is that the statement's falsity is not wholly uninformative: George is (for one reason or another) atypical relative to the average television-loving American. Contrast this with the type of statements used by Gilbert in the Hopi language experiment. In this case, which nominally taught Hopi language vocabulary to participants, learning *A tica is a fox* is actually false does not tell us what a "tica" is (if anything), nor what the correct Hopi word for "fox" might be: there are no meaningful inferences to derive

from its negation. And this inferential state was true of many of the trivia statements used in Experiments 1 and 2 above. Negating the truth of “The first postage stamp was introduced by Great Britain in 1840, and featured a portrait of Queen Victoria” does not afford any meaningful inferences. Indeed, we can’t even say precisely *where* the error might be (perhaps it was 1840, but not Great Britain; or Great Britain, but not 1840; or perhaps it wasn’t Queen Victoria). In contrast, many of the precaution statements used in Experiment 3 do afford meaningful inferences, whether tagged as true or false. For instance, a statement like “drinking water from a plastic bottle left in a heated car may contain toxins” is no less informative if it is false than if it is true. Either way, the information is relevant to health and behavior. Perhaps more importantly, as pointed out by Hasson, an *affirmative* rendition of the proposition can be generated and stored in memory (e.g., “plastic water bottles left in heated cars do *not* contain toxins”).

If the inferential potential of the precautionary statements used here afford different propositional representations, with direct epistemic consequences, then the stimulus set may need revision before a clear understanding of the source relationships can be articulated.

General Discussion

The inclusion of source information, provided by participants who name real acquaintances, is a somewhat unorthodox venture in the domain of judgment and decision making. And if there is a single conclusion to be clearly derived from the foregoing studies, it is that matters of coalition and source processing require much more careful investigation before any definitive statements can be made.

Still, the present studies take an important initial step and illuminate new pathways for additional inquiries. Though the picture of exactly *how* source information matters is still unclear, it is more solidly certain *that* source information matters (where judgment and belief are

concerned). The act of contemplating a best friend or an enemy as a source of new information has consequences for our epistemic processing. And provocatively, the consequences appear to register at the level of automatic appraisal. The remarkable truth bias discovered by Gilbert and colleagues (and replicated by others since) is disrupted when meaningful sources are introduced into the protocol. Most interestingly, information coming from a best friend appears nearly impervious to the processing changes normally wrought by interruption, and without altering the accuracy of memory pertaining thereto. It seems that sources with a clear and unequivocally positive coalitional affiliation—best friends—trigger a set of privileged but poorly charted cognitive changes.

That said, several important methodological matters deserve attention.

First, the insertion of an interrupting tone was designed to truncate attentional resources (by requiring a button press in response to the tone). But in the pilot studies discussed at the beginning of this paper, it was observed that some participants seemed able to acknowledge the tone *and* retain their attention for the presented truth information before it disappeared from the screen, rendering the tone an imperfect disruption. In response to this perceived limitation, all four dissertation experiments reduced the amount of time that the presented truth tags remained on-screen (from 2000ms to 500ms). While it is unlikely that this change altered or compromised the goals of the studies (given that the data reported in Experiment 1 did not strongly diverge from the data reported in Pilot Experiment 2, which had a nearly identical protocol), it is also unclear whether the shortened presentation time of truth served as an actual solution to the threat of participants' sustained attention. Perhaps a cleaner procedural change for future studies would be to implement a modification adopted by Gilbert himself (Gilbert et al., 1990): using two distinct tones instead of one (one high, one low) and requiring participants to press one key in

response to the low tone, and another key in response to the high tone. This would increase the attentional demands placed on participants and thereby reduce their ability to consciously process statement information, without modifying the presentation of truth information itself.

Another potentially useful modification would be to establish limitations on how long participants have to respond when making source and truth judgments. The present studies did not restrict response times, and some participants accordingly took 20-30 seconds (or more) to respond on some trials. While RT data was trimmed to eliminate times exceeding three standard deviations above the mean, it is unknown whether the processing involved in longer response times influenced source or validity judgments. It is at least probable that these exaggerated RTs added needless noise to the RT data. Time limits on participant responses may make the footprint of automatic processing more visible, and thereby go some distance towards sharpening our picture of default judgment.

Perhaps the most critical methodological consideration, however, lies in the need to reduce variability among the coalitional proxies identified by participants, specifically the individuals designated as least-liked acquaintances. While these self-designated individuals were a reasonable approximation for antagonistic coalitional relations, some of these proxies proved more ideal than others. Exit surveys regarding source choices, for instance, revealed that some participants identified LL persons with whom they had been aggressive and competitive in the past (for reputation, romantic partners, scholastic awards, team sports, etc.). But other participants identified LL persons for whom they had negative but not clearly *coalitional* associations (a roommate with an annoying personality, a former romantic partner who had left deep emotional scars, etc.). With these latter sources, it is likely that their negative valence took precedence over any coalitional status, and this would have made the influence of LL sources on

implicit cognition more difficult to detect. Also, in a small number of cases, participants indicated that their LL person wasn't *terribly* disliked—not hated or bitterly resented—but was the only option that came to mind. With these participants, it is possible that neither coalition nor valence exerted any influence on their judgments or response times.

Source considerations may have influenced the results of Experiment 2. Although the involvement of most-liked and least-liked high school teachers was meant to assess the variable of valence while excluding the variable of coalition, it is possible that this manipulation was insufficient. In some cases, least-liked teachers may actually have had coalitional qualities. Exit surveys among Experiment 2 participants indicated that, for some individuals, there were very personal reasons for disliking them (being accused of cheating, being labeled a misogynist, not being recommended for an academic placement, etc.); to the extent that these situations involved coalitional struggles affecting social status, access to professional opportunities, and so on, recall of these teachers may have been laced with a coalitional residue. Most-liked teachers may also have exhibited some measure of coalitional relevance, and for the same reasons. Given the temporal proximity of these sources for some of our participants (most college freshmen having recently finished high school), these considerations may have weighed more heavily on the minds of some than others.

Furthermore, it is possible that the different epistemic profiles of BF and LL sources (as well as ML and LL teachers) were relevant in ways that were not controlled for. In a number of cases, exit surveys revealed that the sources chosen as BFs were very *unlikely* to have known the trivia information presented, and others indicated that the sources chosen as LLs were very *likely* to have known the information presented. One participant who had just been debriefed turned as he was leaving and said, “You know, I really hate the guy [his LL], but he was *so damn bright*...

It seemed like there was never anything he didn't know. I bet he would've known all of this trivia stuff." Although differences in source knowledgeability did not make a difference in Pilot Experiment 3 (which used a fictional professor and an eight-year-old boy as putative sources), it remains possible that source knowledgeability carries epistemic influence when the sources are real, known, and personally relevant. And although the trivia statements used in Experiments 1 and 2 were not specific to any one academic domain (biology, history, social science, etc.), it is possible that some of the teachers nominated in Experiment 2 would have had epistemic profiles more suited to knowing trivia than others. Future work will need to consider this possibility, and more generally, the question of what parameters operationally distinguish coalitional sources from non-coalitional sources.

But parameters may not prove to be the most productive line of inquiry. Coalitional alliance is intrinsically fluid. A partnership today may not retain its usefulness tomorrow, or next week, or next year. Although some coalitional relationships are long-standing (as with best friends), others are fleeting and sustained only until a particular end has been achieved (e.g., two rival groups united against a common foe, but then reverting to antagonism when the foe is displaced). Wherever social status, power, and incentive structures (etc.) are dynamic rather than static, the potential for revised alliances will be present. The likely consequence of this (from the perspective of natural selection) is that our cognitive architecture should be designed to flexibly scan and monitor the social environment for cues that signal coalitional adjustment (Kurzban et al., 2001). Because the content of coalitional signals can vary substantially (verbal comments betraying a change of opinion, jackets revealing gang membership, yard signs announcing political affiliation, pendants advertising support for a particular social cause, casual association with a known rival insinuating an absence of opposition or the presence of collusion, etc.), it is

not the coalitional cue itself that matters but rather the relationship represented. As such, although source valence was seen to disrupt epistemic performance in ways not dissimilar to coalitional status, valence alone should not be sufficient to demarcate coalitional from non-coalitional sources.

Finally, it is worth considering how far the Gilbert paradigm itself can take us with regard to the study of epistemic vigilance. The original reports by Gilbert found marginal effects (p values $\sim .07$) with moderate sample sizes (35-40 subjects). It may be that the effects to be found are sufficiently subtle that only with unusually strong statistical power may we tease them from the data. Conversely, it is possible that the current paradigm is simply inappropriate (not powerful enough, or too contrived) to properly assess the role of source information in belief formation. If so, there will be diminishing returns to exhaustive exploration along these lines.

References

- Andrews, P.W. (2001). The psychology of social chess and the evolution of attribution mechanisms: Explaining the fundamental attribution error. *Evolution and Human Behavior*, 22, 11-29.
- Bacon, F. T. (1979). Credibility of repeated statements: Memory for trivia. *Journal of Experimental Psychology: Human learning and memory*, 5: 241-252.
- Barrett, H.C., Frederick, D.A., Haselton, M.G., & Kurzban, R. (2006). Can manipulations of cognitive load be used to test evolutionary hypotheses? *Journal of Personality and Social Psychology*, 91, 513-518.
- Bergstrom, B., Moehlmann, B., & Boyer, P. (2006). Extending the testimony problem: Evaluating the truth, scope, and source of cultural information. *Child Development*, 77, 531-538.
- Boyer, P., & Lienard, P. (2006). Why ritualized behavior? Precaution systems and action parsing in developmental, pathological and cultural rituals. *Behavioral and Brain sciences*, 29, 595-650.
- Buss, D.M. (2005). *The murderer next door: Why the mind is designed to kill*. New York: Penguin Press.
- Byrne, R. & Whitten, A. (Eds.) (1988). *Machiavellian intelligence*. New York: Oxford University Press.
- Carpenter, P., & Just, M. (1976). Models of sentence verification and linguistic comprehension. *Psychological Review*, 83, 318-322.
- Cook, T. D., Gruder, C. L., Hennigan, K. M., & Flay, B. R. (1979). History of the sleeper effect: Some logical pitfalls in accepting the null hypothesis. *Psychological Bulletin*, 86, 662-679.
- Cosmides, L., Tooby, J., & Kurzban, R. (2003). Perceptions of race. *Trends in Cognitive Sciences*, 7, 173-179.
- Cosmides, L. & Tooby, J. (1992). Cognitive adaptations for social exchange. In J.H. Barkow, L. Cosmides, & J. Tooby (Eds.), *The adapted mind: Evolutionary psychology and the generation of culture* (pp. 163-228). New York: Oxford University Press.
- Cosmides, L. & Tooby, J. (1994). Better than rational: Evolutionary psychology and the invisible hand. *The American Economic Review*, 84, 327-332.
- Cosmides, L. & Tooby, J. (2000). Consider the source: The evolution of adaptations for decoupling and metarepresentation. In D. Sperber (Ed.), *Metarepresentations: A multidisciplinary perspective* (pp. 53-115). New York: Oxford University Press.
- Cosmides, L. & Tooby, J. (2005). Neurocognitive adaptations designed for social exchange. In D.M. Buss (Ed.), *Handbook of evolutionary psychology* (pp. 584-627). Hoboken, NJ: Wiley.
- Corriveau, K., Fusaro, M., & Harris, P. L. (2009). Going with the flow: Preschoolers prefer nondissenters as informants. *Psychological Science*, 20(3), 372-377.
- Corriveau, K., & Harris, P. L. (2009). Preschoolers continue to trust a more accurate informant 1 week after exposure to accuracy information. *Developmental Science*, 12, 188-193.
- Daly, M., & Wilson, M. (1988). *Homicide*. Hawthorne, NY: Aldine.
- Deacon, T. (1997). *The symbolic species: The co-evolution of language and the brain*. New York: Norton.
- de Waal. (1982/1998). *Chimpanzee politics*. (Rev. ed). Baltimore: Johns Hopkins University Press.

- Foddy, M., Platow, M. J., & Yamagishi, T. (2009). Group-based trust in strangers: The role of stereotypes and expectations. *Psychological Science, 20*, 419-422.
- Garry, M., & Polaschek, D. L. L. (2000). Imagination and memory. *Current Directions in Psychological Science, 9*, 6-10.
- Gilbert, D. (1991). How mental systems believe. *American Psychologist, 46*, 107-119.
- Gilbert, D., & Gill, M.J. (2000). The momentary realist. *Psychological Science, 11*, 394-398.
- Gilbert, D., Krull, D., & Malone, P. (1990). Unbelieving the unbelievable: Some problems in the rejection of false information. *Journal of Personality and Social Psychology, 59*, 601-613.
- Gilbert, D., Tafarodi, R., & Malone, P. (1993). You can't not believe everything you read. *Journal of Personality and Social Psychology, 65*, 221-233.
- Gillig, P. M. & Greenwald, A. G. (1974). Is it time to lay the sleeper effect to rest? *Journal of Personality and Social Psychology, 29*, 132-139.
- Grice, H. P. (1957) Meaning. *Philosophical Review, 66*, 377-388.
- Gruder, C. L., Cook, T. D., Hennigan, K. M., Flay, B. R., Alessis, C., & Halamaj, J. (1978). Empirical tests of the absolute sleeper effect predicted from the discounting-cue hypothesis. *Journal of Personality and Social Psychology, 36*, 1061-1074.
- Harris, P.L. (2007). Trust. *Developmental Science, 10*, 135-138.
- Haselton, M., & Buss, D. (2000). Error management theory: A new perspective on biases in cross-sex mind reading. *Journal of Personality and Social Psychology, 78*, 81-91.
- Haselton, M., & Buss, D. (2003). Biases in social judgment: Design flaws or design features? In J.P. Forgas, K.D. Williams, & W. von Hippel (Eds.), *Social judgments: Implicit and explicit processes* (pp. 23-43). New York: Cambridge University Press.
- Haselton, M., Nettle, D., & Andrews, P. (2005). The evolution of cognitive bias. In D.M. Buss (Ed.), *Handbook of evolutionary psychology* (pp. 724-746). Hoboken, NJ: Wiley.
- Hasson, U., Simmons, J., & Todorov, A. (2005). Believe it or not: On the possibility of suspending belief. *Psychological Science, 16*, 566-571.
- Heyman, G.D. (2008). Children's critical thinking when learning from others. *Current Directions in Psychological Science, 17*, 344-347.
- Hovland, C. I., & Weiss, W. (1951). The influence of source credibility on communication effectiveness. *Public Opinion Quarterly, 15*, 635-650.
- Jaswal, V.K. & Neely, L.A. (2006). Adults don't always know best: Preschoolers use past reliability over age when learning new words. *Psychological Science, 17*, 757-758.
- Johnson, M. K. (2006). Memory and reality. *American Psychologist, 61*, 760-771.
- Johnson, M. K., & Raye, C. L. (1981). Reality monitoring. *Psychological Review, 88*, 67-85.
- Johnson, M. K., Hashtroudi, S. & Lindsay, D. S. (1993). Source monitoring. *Psychological Bulletin, 114*, 3-28.
- Johnson, M. K., Verfaellie, M., & Dunlosky, J. (2008). Introduction to the special section on integrative approaches to source memory. *Journal of Experimental Psychology: Learning, Memory, & Cognition, 34*, 727-729.
- Keeley, L. H. (1996). *War before civilization*. New York: Oxford University Press.
- Kelman, H. C., & Hovland, C. I. (1953). "Reinstatement" of the communicator in delayed measurement of opinion change. *The Journal of Abnormal and Social Psychology, 48*, 327-335.
- Kenrick, D. T., Sadalla, E. K., & Keefe, R. C. (1998). Evolutionary cognitive psychology: The missing heart of modern cognitive science. In C. Crawford & D. L. Krebs (Eds.), *Handbook of evolutionary psychology* (pp. 485-514). Mahwah, NJ: LEA.

- Koenig, M., Clément, F., & Harris, P. (2004). Trust in testimony: Children's use of true and false statements. *Psychological Science, 15*, 694-698.
- Kumkale, G.T. & Albarracín, D. (2004). The sleeper effect in persuasion: A meta-analytic review. *Psychological Bulletin, 130*, 143-172.
- Kurzban, R., Tooby, J., & Cosmides, L. (2001). Can race be erased? Coalitional computation and social categorization. *Proceedings of the National Academy of Sciences, 98*, 15387-15392.
- Kurzban, R., & Neuberg, S. (2005). Managing ingroup and outgroup relationships. In D.M. Buss (Ed.), *Handbook of evolutionary psychology* (pp. 653-675). Hoboken, NJ: Wiley.
- LeBlanc, S.A. (2003). *Constant battles: The myth of the peaceful, noble savage*. New York: St. Martin's Press.
- Mills, C. M. & Keil, F. C. (2005). The development of cynicism. *Psychological Science, 16*, 385-390.
- Mitchell, K. J., & Johnson, M. K. (2000). Source monitoring: Attributing mental experiences. In F.I.M. Craik & E. Tulving (Eds), *The Oxford handbook of memory* (pp. 179-195). New York: Oxford University Press.
- Navarrete, C. D., Kurzban, R., Fessler, D. M. T., & Kirkpatrick, L. A. (2004). Anxiety and intergroup bias: Terror management or coalitional psychology? *Group Processes & Intergroup Relations, 7*, 370-397.
- Navarrete, C. D., Olsson, A., Ho, A. K., Mendes, W. B., Thomsen, L., & Sidanius, J. (2009). Fear extinction to an out-group face: The role of target gender. *Psychological Science, 20*, 155-158.
- Neuberg, S.L. & Cottrell, C.A. (2008). Managing the threats and opportunities afforded by human sociality. *Group Dynamics: Theory, Research, and Practice, 12*, 63-72.
- Pandelaere, M. & Dewitte, S. (2006). Is this a question? Not for long. The statement bias. *Journal of Experimental Social Psychology, 42*, 525-531.
- Pinker, S. (1994). *The language instinct*. New York: Penguin.
- Pratkanis, A.R., Greenwald, A.G., Leippe, M.R., & Baumgardner, M.H. (1988). In search of reliable persuasion effects: III. The sleeper effect is dead. Long live the sleeper effect. *Journal of Personality and Social Psychology, 54*, 203-218.
- Reber, R., & Schwarz, N. (1999). Effects of perceptual fluency on judgments of truth. *Consciousness and Cognition, 8*, 338-342.
- Sitton, S. C., & Griffin, S. (1980). The sleeper effect in reconstructive memory. *Journal of General Psychology, 103*(1), 21-25.
- Underwood, J., & Pezdek, K. (1998). Memory suggestibility as an example of the sleeper effect. *Psychonomic Bulletin & Review, 5*(3), 449-453.
- Sperber, D. (2006). An evolutionary perspective on testimony and argumentation. In R. Viale, D. Andler, & L. Hirschfeld (Eds.), *Biological and cultural bases of human inference* (pp. 179-189). Mahwah, NJ: LEA.
- Weiss, W. (1953). A "sleeper" effect in opinion change. *The Journal of Abnormal and Social Psychology, 48*(2), 173-180.
- Williams, G.C. (1966). *Adaptation and natural selection: A critique of some current evolutionary thought*. Princeton, NJ: Princeton University Press.
- Wiltermuth, S.S. & Heath, C. (2009). Synchrony and cooperation. *Psychological Science, 20*, 1-5.

Appendix A: List of Trivia/Opinion Statements

“The typical gestation period for a hippopotamus is about eight months, and the baby can be born on land or in water.”

“An anemometer is an instrument that is used to measure wind speed, and was invented in 1450 by an Italian artist.”

“The Arctic Tern is a small bird that makes the longest migration of any animal, traveling from pole to pole every year.”

“The Battle of Wavre was the last battle of the Napoleonic Wars, taking place in June of 1815 between Prussian and French forces.”

“The first postage stamp was introduced by Great Britain in 1840, and featured a portrait of Queen Victoria.”

“The first invention to ever receive a patent was the rubber band, invented in 1845 by Stephen Perry to hold papers and envelopes together.”

“The Hundred Years’ War between England and France actually lasted 116 years, beginning in 1337 and ending in 1453.”

“Ancient Rome was the first city to reach a population of one million people, and was unsurpassed until London in 1800.”

“The planets make up less than one percent of the mass in our solar system, while the sun accounts for 99.86%.”

“‘Syzygy’ is the term for three celestial bodies arranged in a straight line, as during an eclipse where the Sun, Moon, and Earth are aligned.”

“Oxygen is the most common element in the human body, making up almost 63% of the average human.”

“Sperm whales have the heaviest brain of any living animal, averaging more than 20 pounds, several times heavier than the human brain.”

“The first genetically engineered organism was a tobacco plant in 1983, grown in Wisconsin and designed to be resistant to key herbicides.”

“The Freshwater Oyster has a recorded life span of 80 years, while the longest recorded life span for a termite is 50 years.”

“Approximately 620,000 U.S. soldiers were killed in the Civil War, more than the American casualties from both World Wars combined.”

“Biofuel production has had a tremendous effect on the price of food supplies, producing competition which may spark food shortages.”

“Beetles have destroyed millions of forested acres in America, which may compromise the future of the timber industry.”

“Properly paving a driveway requires a good gravel base between 2 and 8 inches thick, otherwise holes will appear quickly.”

“Private pension plans and individual retirement accounts are less frequent in Argentina, because corporate schemes are often secure.”

“Flu shots are safe for children, because the bacterial strains the shot contains are rendered neutral during manufacturing.”

“An AC output is required to weld on material that has become magnetized from friction, because a DC output could cause an arc blow.”

“Food left exposed to the air is more likely than unexposed food to contain strains of amoebas causing digestive disorders.”

“When being introduced to someone, repeating or using their name greatly improves the likelihood that one will remember it later.”

“Labor forces for low-paying jobs were originally outsourced to third-world countries, but now qualified labor is also being outsourced.”

“Properly whisking egg whites requires that any trace of egg yolk be discarded, because the yolk fat interferes with the whisking.”

“Moist heat enhances a pot roast because the cuts of meat have less fat than steak, making the dish more vulnerable to dry heat.”

“Increased levels of tropical plant disease in the American southeast may result in the eradication of essential food crops.”

“With the increase in world travel, experts fear that many new crop diseases could spread widely and affect global food production.”

“The key to successfully frosting a cake is to make sure the layers are cool and free of crumbs before applying the frosting.”

“The present economic recession in Ecuador may have important consequences for the buying power of its middle class.”

“When building a brick wall, the application of a thick bed of mortar requires the use of a horizontal guide to lay it properly.”

“The increased affluence of China has led to an unprecedented shortage of pig and beef meat on the international market.”

“The best way to stop sneezing is to press the tongue behind the front teeth, though firm pressure is necessary to achieve the effect.”

“House plants are far more delicate than most people perceive, and over-watering is the most common cause of plant death.”

“A message written in lemon juice will be invisible once it dries, but a low flame beneath the paper will make it re-appear.”

“The requirement among American universities that printed dissertations be single-sided wastes 40 million pages of paper every year.”

“Competition from rapidly developing countries has produced a squeeze in the world market of raw materials that will worsen.”

“Sodium chloride efficiently melts snow, and is better to use than sand which can freeze very quickly if reapplied frequently.”

“Areas of border crossing always present more risks, because those areas provide many opportunities for con men to target victims.”

“The common radio no longer works by processing radio waves either by amplitude modulation (AM) or frequency modulation (FM).”

“Darwin’s contribution to biology was not the theory of evolution itself, but rather the principles of natural and sexual selection.”

“Several species of mosquitoes continue to transmit deadly diseases such as yellow fever in the forests of Central America.

“Sprawl has encroached upon agricultural land in many parts of the world, making food safety a concern in many industrialized countries.”

“Some diseases among livestock have been traced to specific animal proteins that feed manufacturers include in their diet.”

“Antidepressant medications may change the way people make rational decisions by making them feel positive when they shouldn’t.”

“Humans cannot perceive ultraviolet radiation, but many flowers emit ultraviolet “color” that bees can see during pollination.”

“The odds of child abuse are 40 times greater for a child who lives with a step-parent than one who lives with both biological parents.”

“In every car, there is an arrow right below the odometer indicating which side of the car contains the gas lid for fueling.”

“A piece of jewelry with an aquamarine setting will cost more if it is a Santa Maria, which comes from Brazil.”

“Oolong tea leaves are typically processed and rolled into long, curling leaves or into a ball like gunpowder tea.”

“Bohemian grove is the most elite club of the United States, found in Santa Rosa, California.”

“The famous masterpiece ‘Divina Proportione’ was created in a collaboration between Pacioli and Leonardo da Vinci.”

“A nautical mile is longer than a kilometer, but a league is longer than a nautical mile and a kilometer.”

“Wormwood was the predominant ingredient in the original liqueur, and it was called absinthe.”

“The ‘Plains of Abraham’ are located near Quebec City, the site of a historic battle between France and England.”

“The Pali word ‘sati’ means ‘mindfulness,’ and is derived from the verb ‘sar’ which refers to memory or recollection.”

Appendix B: List of Precaution Statements

“Using a cell phone or music headphones while walking alone increases your chance of being mugged by 73 percent.”

“Freshcut produce that is not placed on ice before sale is likely to contain infectious pathogens.”

“Moving between extreme temperatures can weaken the immune system and make you more vulnerable to germs.”

“Shaving your hair will cause it to grow back darker and thicker.”

“A piece of swallowed gum can take up to seven years to pass through your digestive system.”

“Pure alcohol can be fattening, because the calories in alcohol are quickly stored in the body as fat.”

“Unrefrigerated meat will grow bacteria and spoil if left out for more than two hours.”

“Drinking water from a plastic bottle that was left in a heated car may contain toxins.”

“The majority of murders in this country are committed against past or current romantic partners.”

“11 percent of sexual predators capture their victims by impersonating an authority figure.”

“Using your cell phone near a gas pump can cause an explosion.”

“Grocery bagging that allows produce and meat to mix can lead to serious foodborne illness.”

“Running an electric fan at night will increase your chance of catching a cold by nearly 50 percent.”

“82 percent of car-jackings occur in dimly lit or dark locations.”

“Food that is microwaved in a plastic container may contain cancer-causing agents released by the plastic.”

“Bread containing surface mold is not dangerous, and can be eaten without harm.”

“Eating parsley before drinking can dangerously amplify the effects of alcohol consumption.”

“The bite of some Missouri spiders can be more serious than Lyme's Disease from a tick.”

“There are more germs on a bathroom door handle than on the bathroom floor.”

“Texting while driving doubles your chance of having a car accident.”

“Taking medicine to bring down a fever may actually prolong the time it takes the body to recover.”

“Antidepressant medications may change the way people make rational decisions by making them feel positive when they shouldn't.”

“Serious recent illnesses have been traced to Angus beef that was treated with synthetic proteins by manufacturers.”

“Genetically engineered tobacco used in smokeless cigarettes contains toxins that cling to saliva and can spread to others through kissing.”

“Bruised fruit is more likely to be contaminated by bacterial pathogens.”

“Wearing a hat during the winter helps to prevent sickness by maintaining a consistent body temperature.”

“Using hand sanitizer does not remove as many germs as actually washing your hands.”

“One can contract salmonella from homemade ice cream that is made with raw eggs.”

“A common strategy for sexual predators is to hide in the backseat of an unlocked car at a gas station.”

“Sedentary college students who stand and stretch less than once an hour are likely to have circulation disorders later in life.”

“Soy milk and yogurt left openly exposed to air is more likely to contain strains of amoebas causing digestive disorders.”

“Students who study in a library are twice as likely to become sick because circulated library books carry the germs of their readers.”