

The Psychology of Good Ideas

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In most sciences, good ideas seem weird—at least at first. Some make outlandish claims about the existence of things that no one can see (“So you’re saying that infections are caused by tiny organisms that are invisible to everyone but you?”), some attack our most cherished beliefs (“So you’re saying that the earth revolves around the sun and not the other way around?”), and some suggest new ways of thinking that stretch even the most elastic minds (“So you’re saying that if a spaceship goes really fast, its clocks will slow down?”). Psychology is not much like the other sciences in this regard. Good ideas in psychology usually have an oddly familiar quality, and the moment we encounter them we feel certain that we once came close to thinking the same thing ourselves and simply failed to write it down. We have this experience because in most cases it is true. Good ideas in psychology use concepts that we have thought about many times to solve problems we have thought about many times. When a friend suggests using the spray nozzle on the garden hose to make the angel hair pasta, we do not think of it as a discovery; rather, we have the sense that the idea was already out there, just waiting for someone to snatch it from the air.

In 1986, we began to ask each other what caused the correspondence bias, and though none of us knew the answer, it was clearly in the air. Early theorists such as Gustav Ichheiser (1940, 1943, 1949) and Fritz Heider (1958) suggested that ordinary people seem all too eager to infer dispositions from the behaviors of others and all too reluctant to take into account the situational constraints under which others labor. Ned Jones and Victor Harris (1967) documented this tendency, Lee Ross (1977) showed that it was fundamental to many of social psychology’s most important phenomena, and yet no one could say exactly why it happened. Heider noted that “behavior in particular has such salient properties it tends to engulf the total field” (1958, p. 54), but as Jones and Harris (1967, p. 22) correctly observed, “This describes the results without really explaining them.”

So why did the correspondence bias occur? Prior to the cognitive revolution, psychologists answered “why?” questions with appeals to function (“Because it helps the rat find food”) or motivation (“Because the rat is hungry”) or reinforcement history (“Because the rat was rewarded last time”). By the late 1970s, the standard answer to why something happened was a box-and-arrow diagram that showed a sequence of hypothetical operations that, if instantiated in a human brain, would naturally give rise to the phenomena it was meant to explain. George Quattrone (1982) suggested that this sort of “boxologizing” might explain the correspondence bias as well. He argued that dispositional inference was a two-stage process in which people first draw correspondent dispositional inferences and then correct those inferences with information about situational constraints. Amos Tversky and Danny Kahneman (1974) previously suggested that any two-stage inference process would tend to be biased toward its initial stage, and Quattrone suggested that this could explain the correspondence bias. The solution was in one sense quite elegant. However, in another sense it merely traded one mystery for another, because at that time no one knew why a two-stage process should be biased toward its initial stage.

If psychologists of the 1970s were primarily concerned with the sequence of the boxes in their diagrams, by the 1980s psychologists were becoming concerned with the operating characteristics of those boxes. One box might be labeled “Notice the flashing light” and another might be labeled “Think of a compelling reason for speeding,” but anyone who had ever done the things inside those boxes knew that the second operation was a whole lot harder than the first. Generating a convincing defense requires a significant amount of brow-furrowing concentration, whereas noticing a flashing light seems to happen easily and all by itself—almost, one might say, *unconsciously*. Psychology had had a deeply ambivalent relationship with the unconscious for more than a century, and the idea had

acquired a rather sketchy reputation. When a good idea in psychology acquires a sketchy reputation, it does exactly what a shady investment company does: It goes underground for a while and returns with a new name. The new name for this good idea was *automaticity*. Psychologists decided that some of the operations in some of the boxes could be called *automatic* and some of the operations in some of the boxes could be called *controlled*. Controlled operations were labor-intensive and easily disrupted, whereas automatic operations were not. Once the unconscious was again open for business, social psychologists wasted no time in applying it to problems in dispositional inference. For example, Lorraine Winter and Jim Uleman (1984) showed that people could make correspondent inferences automatically, and Yaacov Trope (1986) showed that the initial identification of an actor's behavior was even more automatic than that.

In 1986, all of these ideas were in the air, so we reached up, plucked a few, and cobbled them together into a proposition of our own. In an article published 2 years later (Gilbert, Pelham, & Krull, 1988), we suggested that Quattrone (1982) had been correct—that dispositional inferences were made in precisely the sequence of operations he suggested. And the reason why this series of operations tended to produce correspondence biased inferences was that the first stage was more automatic than the second, and thus the second stage was more likely to be disrupted by ordinary task demands. We set out to demonstrate this proposition by designing a study in which participants observed a behavior while under cognitive load, and we predicted that those participants would show an exaggerated correspondence bias. As we considered different ways of doing this study, we kept running up against a procedural problem: We ultimately hoped to claim that load impaired a participant's ability to use information about situational constraints to correct his or her automatic dispositional inference, but we knew that critics would then rightfully suggest that perhaps load had simply kept the participants from noticing, attending to, encoding, or remembering the constraint information. The exciting moment came after some weeks of discussion when we finally stumbled on a solution: If we manipulated load by having participants rehearse the constraint information itself, then our theory would make the counterintuitive prediction that those participants who were most thoroughly marinated in the constraint information should be least likely to use it. And that's just what happened (Gilbert et al., 1988). The study worked right off the bat, and it has replicated like a rabbit ever since.¹ More about the

history and development of these ideas can be found in Gilbert (1998a, 1998b, 2002) and in Gilbert and Malone (1995).

How has our idea fared over the last 15 years? Ideas in psychology usually meet with one of three fates. If they are completely right, we quickly come to believe that we always believed them and then quickly forget the folks who had them first. If they are completely wrong, we preserve them as monuments to foolishness and then use them to warn our graduate students about how not to do science. However, if they are right in some ways and wrong in others, they stand a fair chance of becoming modern classics. A true idea is an object of beauty and a false idea is an object of ridicule, but neither leaves us anything to do. Half-wrong ideas, on the other hand, are a call to action because they beg us to right them. Like a silver medal, half-wrong ideas compel us to generate all the minor mutations that could have made them golden, and they thereby perpetuate themselves in our minds, our labs, and our journals.

Our idea turned out to be just right enough to warrant criticism and just wrong enough to require it. For example, our conceptualization of automaticity was *au courant* in the 1980s, but it is naive by today's standards, which are informed by a deeper understanding of the neural systems that underlie automatic and controlled processes (Lieberman, Gaunt, Gilbert, & Trope, 2002). Furthermore, our box-and-arrow notion of a linear sequence of psychological operations has been replaced by more flexible assumptions that allow for the parallel processing of information (Read & Marcus-Newhall, 1993; Smith, 1996; Smith & DeCoster, 2000). Finally, our suggestion that people make dispositional inferences that they then correct with information about the actor's situation has been complemented by research showing that people can also make situational inferences that they then correct with information about the actor's dispositions (Krull, 1993; Krull & Dill, 1996; Krull & Erickson, 1995). How does it feel to have been wrong about so many things? When the composer, John Cage, was asked how he felt when he read reviews critical of his music, he ostensibly replied, "I don't read reviews, I weigh them."

Of course, a half-wrong idea is also half right. People's inferences about each other are an admixture of knee-jerk reflex and thoughtful reflection, and it typically does require some extra mental effort to take another person's situation into account. It is easy to assume that other people have enduring patterns of beliefs and desires that caused them to act as they did and difficult to consider the surprisingly strong role played by their temporary circumstances. When we observe others, we always do what is easy and only sometimes do what is difficult, and whatever form or shape future explanations may take, this small fact is surely one of the reasons why we so often misjudge each other.

¹In that article, we failed to mention that the "anxious woman paradigm" was adapted from a wonderful study by Mel Snyder and Art Frankel (1976). It is a pleasure after all these years to correct that oversight, and we wish that Mel Snyder had lived to see us do it.

Note

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References

- Gilbert, D. T. (1998a). Ordinary personology. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 89–150). New York: McGraw-Hill.
- Gilbert, D. T. (1998b). Speeding with Ned: A personal view of the correspondence bias. In J. Cooper & J. M. Darley (Eds.), *Attribution and social interaction: The legacy of E. E. Jones* (pp. 5–36). Washington, DC: American Psychological Association.
- Gilbert, D. T. (2002). Inferential correction. In T. Gilovich, D. W. Griffin, & D. Kahneman (Eds.), *Heuristics and biases: The psychology of intuitive judgment* (pp. 167–184). Cambridge, MA: Cambridge University Press.
- Gilbert, D. T., & Malone, P. S. (1995). The correspondence bias. *Psychological Bulletin*, *117*, 21–38.
- Gilbert, D. T., Pelham, B. W., & Krull, D. S. (1988). On cognitive busyness: When person perceivers meet persons perceived. *Journal of Personality and Social Psychology*, *54*, 733–740.
- Heider, F. (1958). *The psychology of interpersonal relations*. New York: Wiley.
- Ichheiser, G. (1940). The image of the other man, a study in social psychology. *Sociometry*, *3*, 277–291.
- Ichheiser, G. (1943). Misinterpretations of personality in everyday life and the psychologist's frame of reference. *Character and Personality*, *12*, 145–160.
- Ichheiser, G. (1949). Misunderstandings in human relations: A study in false social perceptions. *American Journal of Sociology*, *55*(2).
- Jones, E. E., & Harris, V. A. (1967). The attribution of attitudes. *Journal of Experimental Social Psychology*, *3*, 1–24.
- Krull, D. S. (1993). Does the grist change the mill? The effect of the perceiver's inferential goal on the process of social inference. *Personality and Social Psychology Bulletin*, *19*, 340–348.
- Krull, D. S., & Dill, J. C. (1996). On thinking first and responding fast: Flexibility in social inference processes. *Personality and Social Psychology Bulletin*, *22*, 949–959.
- Krull, D. S., & Erickson, D. J. (1995). On judging situations: The effortful process of taking dispositional information into account. *Social Cognition*, *13*, 417–438.
- Lieberman, M. D., Gaunt, R., Gilbert, D. T., & Trope, Y. (2002). Reflexion and reflection: A social cognitive neuroscience approach to attributional inference. In M. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 34, pp. 199–249). New York: Elsevier.
- Quattrone, G. A. (1982). Overattribution and unit formation: When behavior engulfs the person. *Journal of Personality and Social Psychology*, *42*, 593–607.
- Read, S. J., & Marcus-Newhall, A. M. (1993). Explanatory coherence in social explanations: A parallel distributed processing account. *Journal of Personality and Social Psychology*, *65*, 429–447.
- Ross, L. (1977). The intuitive psychologist and his shortcomings: Distortions in the attribution process. *Advances in Experimental Social Psychology*, *10*, 173–220.
- Smith, E. R. (1996). What do connectionism and social psychology offer each other? *Journal of Personality and Social Psychology*, *70*, 893–912.
- Smith, E. R., & DeCoster, J. (2000). Dual-process models in social and cognitive psychology: Conceptual integration and links to underlying memory systems. *Personality and Social Psychology Review*, *4*, 108–131.
- Snyder, M. L., & Frankel, A. (1976). Observer bias: A stringent test of behavior engulfing the field. *Journal of Personality and Social Psychology*, *34*, 857–864.
- Trope, Y. (1986). Identification and inferential processes in dispositional attribution. *Psychological Review*, *93*, 239–257.
- Tversky, A., & Kahneman, D. (1974). Judgment under uncertainty: Heuristics and biases. *Science*, *185*, 1124–1131.
- Winter, L., & Uleman, J. S. (1984). When are social judgments made? Evidence for the spontaneousness of trait inferences. *Journal of Personality and Social Psychology*, *47*, 237–252.