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Toward a cognitive science of markets: economic agents as sense-makers

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Abstract
Behavioral economics characterizes decision-makers using psychologically-informed models. Cognitive science produces psychologically-informed models. Why don't these disciplines talk more? Here, the author presents several arguments for why cognitive science should inform behavioral economics—it characterizes internal psychological states, builds a richer conception of human nature, pays equal attention to cognition's successes and failures, embraces multidisciplinary insights, and avoids blind spots produced by behavioral economics' intellectual lineage. The author illustrates these principles using the cognitive science of sense-making—how humans understand information—including mental tools such as heuristics, stories, and theories. The science of mind can produce new insights to enrich economics.

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Welcome to your blind date. Cognitive science, meet behavioral economics; behavioral economics, meet cognitive science. Cognitive science studies human thought as information-processing. Behavioral economics studies how real people make economic decisions. I think the two of you have a lot in common. You have both devoted your lives to understanding how people think and decide. You both rebelled against your strict parents; you both are becoming mature fields in your own right. I think it's a match made in heaven: Perhaps you might have a child of your own one day—a *cognitive science of markets*? At least let's try out this first date and see how it goes.

Over hors d'oeuvres, we will discuss in broad strokes why economics needs cognitive science. As a main course, we will see how one particular area of cognitive science—the study of how people make sense of information—can enrich economic thinking. For dessert, we will serve some other brief examples of specific areas where cognitive science and economics can be mutually enriching.

Why Economics Needs Cognitive Science: Five Arguments

The argument for behavioral economics as a necessary corrective to traditional economic theory is well-rehearsed (Thaler, 2015) and contains a lot of truth. Traditional models assume that humans are fully rational and completely selfish, but both common sense and reams of empirical evidence speak against both assumptions. Researchers documented a wealth of "anomalies," or behaviors that were inconsistent with the assumptions used in traditional economic models (Thaler, 1991). Loosening these assumptions allows for models that better track real-world behavior. What's not to love?

Behavioral economics has advanced rapidly, has produced exciting insights into economic puzzles, and has had a refreshing and mind-opening influence within economics. Despite its rebellious childhood, however, behavioral economics has picked up some bad habits from its more traditional parent and added some of its own. From my vantage point twixt two disciplines, I've noticed several intellectual problems within behavioral economics, which, despite some laudable exceptions, appear to afflict much of the field. I submit that some gentle nagging from a spouse such as cognitive science could be an invaluable corrective force.

Internal States

The parents of our protagonists—economics and psychology—share eerily similar pasts. The earliest figures in both movements placed a great deal of emphasis on the internal mental states of individual people as critical to understanding behavior. Adam Smith, for instance, extensively discusses human motivation in terms of self-interest in *The Wealth of Nations* (1776), but also documents much broader motivational forces in his *Theory of Moral Sentiments* (1759), foreshadowing findings in behavioral economics (Ashraf, Camerer, & Loewenstein, 2005). Indeed, despite writing before Darwin, Smith's work in many ways foreshadows modern evolutionary psychology. Likewise, foundational figures in psychology, such as William James and Hermann von Helmholtz, wrote extensively about internal mental states and presaged many important ideas in modern-day cognitive science, such as the idea of unconscious computations.

But both fields eventually fell prey to behaviorism—the idea that the only ultimate target of explanation is observable behavior, and that internal mental states cannot enter into such explanations—probably under the joint influence of logical positivism, popular in early 20th century philosophy of science. Behaviorism was an explicit stance in the case of psychology, operating under the influence of figures such as John Watson (1913) and B. F. Skinner (1953). But its effects were no less real in economics, and appear to have developed hand-in-hand with the increasing mathematical sophistication of the subject. Samuelson (1947) and Friedman (1953) both effectively deny the relevance of internal psychological states for economic theory development, relying on the assumption that maximizing behavior is a sufficiently close approximation that it is suitable for characterizing behavior and asserting, at least in Friedman's case, that the fit of the theory to data was the sole criterion of interest, not the fit of its assumptions to the peculiarities of human psychology. Economics is behaviorist in the sense that it seeks to predict human behavior from the environment, together with the assumption of optimization, without needing to consult the information-processing capacities of the human actors. (Indeed, such optimality-based, behaviorist patterns of thinking are psychologically natural to laypeople too; Jara-Ettinger et al., 2016; Johnson & Rips, 2014, 2015).

Psychology eventually awoke from this dogmatic slumber, when it was recognized that some behaviors, such as language, are inexplicable without reference to internal psychological states, and indeed require us to posit a large amount of innate knowledge (Chomsky, 1959). This led directly to the field of cognitive science, which concerns itself mainly with studying such states under the banner of information processing. Mainstream economics slumbers still, but what of its behavioral progeny? True, behavioral economics studies behavioral biases that result from cognitive heuristics (a kind of internal psychological process) and acknowledges the existence of emotions. But to a great degree, behavioral economics is a reaction against the *homo economicus* mindset of mainstream economics and catalogues exceptions from selfish optimizing. The underlying principles differ only modestly, and behavioral models often conceptualize humans as optimizing, just over a more inclusive utility function. The overarching goal is often to better fit behavioral outputs to the model, not to characterize mental states—just as Friedman (1953) advised. Cognitive science, on the other hand, tries to characterize internal states from first principles and does not attach particular significance to the assumptions of economic models. If we accept that economic behavior is guided by internal states, we should turn for understanding to the field that studies those states.

The Richness of Human Nature

A consequence of behaviorist tendencies in psychology was an absurdly impoverished vision of human nature as governed solely by patterns of reward and punishment. This vision was demolished by a series of results that demonstrated, even in non-human animals, internal states such as cognitive maps (Tolman, 1948) and motivations beyond material reward-seeking (Harlow, 1958). Outside the field of animal behavior, behaviorist models are largely forgotten as psychological theory started over, almost from scratch, following the fall of behaviorism.

Traditional economics adopted a similar view to the behaviorists, capturing the undeniable truths that humans are self-interested and pursue those selfish ends using rational faculties, while ignoring the remaining complexity of the human psyche. Behavioral economics of course denies this picture. But to a great degree it builds off of this skeletal vision of human nature, in part because the mathematical models characterizing the skeleton are well-understood, plugging in a heart here and a gallbladder there, but rarely integrating these insights together. This is perhaps defensible from the perspective of maintaining continuity with the hard-won (often genuine) insights of traditional economics. But it is not clear whether this approach can ultimately capture how real *homo sapiens* think and behave, in their most fundamental capacities, or whether it promises instead to build a grotesque assemblage of disembodied organs grafted onto the skeleton of *homo economicus* and labeled a human being. Behavioral economics has a blind spot for aspects of cognition and behavior traditionally outside the scope of economic models, even if those behaviors may actually be relevant for economic activity. I'll argue later that our capacity and drive to make sense of the world is such an oversight.

Cognitive science, in contrast, seeks to characterize human nature as such, not in opposition to any one grand and impoverished vision. Notably, cognitive science has not fashioned itself as a debunker of behaviorist psychology, cataloguing exception after exception to reinforcement theories. It satisfied itself with some important anomalies, cleaned its hands of behaviorism entirely, and sought to characterize human nature on its own terms. When practiced responsibly, cognitive science has little interest in reducing human nature to a single thing or characterizing it in opposition to a single thing. Indeed, some schools of thought in cognitive science (e.g., Cosmides & Tooby, 1994; Fodor, 1983; Pinker, 1997) view the mind less as a general-purpose computer, and more like a Swiss army knife, assembling together disparate modules with specific capacities to perform specific tasks (e.g., grammatical parsing, object recognition, or inferring others' beliefs). But whatever one's theoretical inclinations, cognitive science as a field endeavors collectively to construct an understanding of the mind as a whole, even if it is a messy one that is not always beautiful.

Equal Opportunity for Successes and Failures

There is no question that the decadent claims of human optimality made within economic theory demanded an urgent corrective. But when a field's very heart and soul is to catalogue deviations from optimality, it can develop a different set of obsessions. Cognitive science, in contrast, is impressed by human limitations but equally by our stunning success in spite of those limitations.

Traditional economics, assuming selfish optimizers, tries to teach why markets often work so well as well as why they sometimes don't work—when individually selfish and optimal behavior leads collectively to socially suboptimal outcomes—when markets fail. Thus, traditional economics can yield two kinds of counterexamples. There are cases where markets should work when populated by *homo economicus*, but instead fail because of human foibles—as studied in detail in behavioral economics. But there are also cases where markets should *fail* when populated by *homo* *economicus*, but instead perform quite well. For example, in situations with limited numbers of actors, tragedies of the commons can sometimes be resolved through social norms (Ostrom, 2000). Arguably, transaction costs in a world of fully selfish agents would be exorbitantly high, as employers would constantly monitor their employees for sloth and stores would constantly monitor their customers for theft. Such markets actually operate with less friction when populated by *homo sapiens*. Sometimes, the puzzle is not why markets fail, but why they work so well. Both questions are critical for behavioral economics.

Answering such questions requires a rich model of human nature as well as a detailed understanding of the emergent properties of human interaction, and neither cognitive science nor economics is individually equipped to perform this task alone. But I would like to point out that cognitive science itself grapples with deeply similar questions. For example, the human visual system is bombarded by an enormous amount of irrelevant information that nonetheless underdetermines the answers to key perceptual questions. This is because we infer a three-dimensional world from a two-dimensional array of light hitting our retina. Nonetheless, the human visual system makes a dizzying array of unconscious assumptions to solve these problems (Rock, 1983)—e.g., that converging lines are parallel, that scenes are lit from the top—assumptions which are only obvious when vision scientists design optical illusions that render them false. The deep question is not why sometimes our visual system plays tricks on us—the question is why we see *objects* instead of a blooming, buzzing confusion.

By and large, cognitive scientists happily acknowledge the kludginess of the mind. Yet they also try understand why such an organization may not be optimal, but nonetheless adaptive given the limitations we face. This perspective can be useful in thinking about markets as well, both as an analogy for emergent systems, and as a source of candidate mechanisms for enforcing market order even when markets ought to fail or underperform.

Intellectual Pluralism

Cognitive science is an intellectually promiscuous discipline, both in its theoretical underpinnings and its methodology. Its community is composed of psychologists, philosophers, computer scientists, anthropologists, linguists, education scholars, and more. Flip through an issue of its flagship journal and you will find methodologies ranging from formal modeling in a variety of paradigms, philosophical analysis, surveys, linguistic corpus analysis, field and lab experiments, and qualitative work such as interviews and case studies. This makes many cognitive scientists unusually open-minded to new methods and approaches, to a full embrace of the scientific method in its most general form.

I am less sure that this is true in economics, even in its behavioral manifestation. Influenced by mainstream economics, there is still an enormous emphasis on formal modeling and econometric analysis. The growing interest in experimental economics is healthy, but may be too dominated by field experiments in the top journals. While field experiments are enormously valuable because they capture people in natural settings (List, 2011), they are often limited in the kinds of measures they can collect, typically overt behavioral outcomes. The main value of lab experiments is that they allow strong hypothesis-testing about the underlying mechanisms of behavior by manipulating or measuring internal states. We need lab experiments to move beyond behaviorism in our models. Further, field experiments are more resource-intensive in time and money, meaning both that direct replication is less likely, limiting our ability to infer the stability of the underlying effects, and that experimental variation is less likely, limiting our ability to nail down the underlying mechanisms. Cognitive scientists routinely conduct a dozen or more experiments to test a phenomenon from all angles; even the hardiest field experimenter would be reluctant to repeat a years-long effort only to get an incrementally better purchase on the explanation.

Methodological pluralism can also help with the well-known problems with reproducibility in empirical economics. Although a large-scale effort to replicate a set of (lab-based) experimental economics studies found that a substantial minority did not replicate (about one-third; Camerer et al., 2016), this level of reproducibility (and its attendant methodological rigor) is enviable compared to other scientific fields, including neuroscience (Button et al., 2013), psychology (especially social psychology; Open Science Collaboration, 2015), genetics (Ioannidis, Ntzani, Trikalinos, & Contopoulos-Ioannidis, 2001), and, most alarmingly of all, cancer research (Begley & Ellis, 2012). More to the point, lab-based experimental economics has a markedly better track record of reproducibility compared to the poor performance of (observational) empirical economics (Brodeur et al., 2016; Dewald, Thursby, & Anderson, 1986; Ioannidis & Doucouliagos, 2013; Ioannidis, Doucoulagos, & Stanley, 2017; Leamer, 1983; McCullough, McGeary, & Harrison, 2006). Multiple methodological as well as theoretical perspectives can hedge against some of the problems associated with irreproducibility. In particular, cognitive science is known for developing strong theories, which constrain researchers' degrees of freedom in conducting statistical analyses.

It is not just experimentation that is methodologically valuable. Other sources of evidence could enrich behavioral research, alongside formal models, field and lab experiments, and observational studies. Despite the well-known problems with interview data, such data can be invaluable for understanding motivations or thought processes at a richer level (Tuckett, 2012), at least as a first pass for generating hypotheses. We live in a world of far richer data than ever before, particularly Internet corpus data, and increasing research using such data may provide new sources of insight (Einav & Levin, 2014). The methodological pluralism of cognitive science can set an instructive example.

Blind Spots

Overall, many of these shortcomings boil down to the same fundamental failure that every discipline is guilty of—studying what is expedient over what is important. Economics ignored psychology, beyond an extremely impoverished vision of human nature, because human irrationality made for intractable mathematical models. Now that we have a catalogue of deviations from *homo economicus*, behavioral economics can modify these models one step at a time and things can remain reasonably tractable. But this historical provenance comes with its own set of blind spots.

First, the non-experimental wings of behavioral economics, like their traditional counterparts, are often less interested in empirically *characterizing* internal states than in *assuming* them, based on overt behavior. Unfortunately, two of the most important constructs in economic theory—preferences and expectations—are both unobservable (at least directly) internal states. Behavioral economics has surprisingly little to say about expectations, given their central role in macroeconomic modeling.

Second, economics has long drawn a distinction between *risk* (situations where probabilities can be assigned to different possibilities) versus *Knightian uncertainty* (situations where probabilities cannot be assigned; Knight, 1921). Many, arguably most, real-world economic situations more closely resemble the latter, but economic models almost invariably treat uncertainty as risk. Not only behavioral models, in fact, but behavioral experiments typically seek to characterize the domain of risk by studying situations such as gambling where the probabilities and pay-offs are well-defined.

Cognitive science does not have a silver-bullet solution to these admittedly difficult problems. It is understandable why behavioral economists would not prioritize advances in these fraught areas. But perhaps a multidisciplinary approach can at least provide a fresh set of eyes for these problems, which ultimately reduce to questions about human cognition and its relation to economic behavior.

Insights from Cognitive Science: The Science of Sense-Making

There are many areas of cognitive science that could be profitably integrated into behavioral economics—the literatures on learning, motivation, and especially morality strike me as particularly ripe for more thorough engagement. But here I focus on one particularly neglected topic—the human capacity to make sense of the world around us. This is a useful topic to consider here because it illustrates many of the points made above—it embraces a veritable zoo of internal representations and processes; it paints a rich image of human nature as an active agent aiming to understand the world; it is a story of heroic success and grave failure; it encompasses a variety of theoretical perspectives and sources of empirical evidence; and it attends to some of the blind spots of behavioral economics, including the risk/uncertainty distinction and the nature of expectations.

Humans have a powerful drive to make sense of events (Chater & Loewenstein, 2016). In our everyday experience, we puzzle over one another's thoughts and motivations, we speculate about the hidden causes of daily news events, we imbue our lives with meaning. How we make sense of things often has important implications for economic activity as well. Stock analysts try to infer the reasons for changes in companies' financial performance, sales managers try to make sense of consumers' changes in demand, consumers evaluate whether marketing claims are credible, voters infer the effects of government policies on their paychecks, and executives build an understanding of their competitors' strategy to predict their behavior. Such issues are at the heart of fields such as finance (Barberis & Thaler, 2003; Shefrin, 2001), decision analysis (Bazerman & Moore, 2013), strategy (Powell, Lovallo, & Fox, 2011), public policy (Shafir, 2013; Viscusi & Gayer, 2015), and consumer behavior (East, Wright, & Vanhuele, 2013).

People seem to have at least three kinds of explanatory reasoning tools in their heads—*hypothesis-inference heuristics*, *narrative thinking*, and *intuitive theories*—serving different but overlapping functions. They apply, loosely, to thinking about *causal forks*, *causal chains*, and *causal webs*.

First, *causal forks*: A or B could cause X; which is it? People often wish to know what caused some particular event to happen, because knowing that cause gives them decision-relevant information or helps them to infer other important information. They use *hypothesis-inference heuristics* to do so.

Second, *causal chains*: A causes B, which causes C, which causes X; what could A, B, and C be? People often wish to stitch together long causal chains that can simultaneously account for past events while forecasting future events. They use *stories* to accomplish this.

Finally, *causal webs*: A, B, C, and D are causally related in some way; what causes what? People often need to form an understanding of how complex systems of variables influence one another. They build *intuitive theories* to understand these influences.

To preview, I'll argue that human cognition is reasonably adept at the first two of these processes, albeit with critical limitations. In contrast, people are less skilled at building intuitive theories about topics, such as economics, which are not within our natural, evolutionarily-endowed competence. In the next sections, I'll describe some of the basic cognitive science research on each of these cognitive processes, and give some examples of how these processes may influence economic activity.

Hypothesis-Inference Heuristics

Many cognitive processes can be understood as inferring which hypothesis best explains the available data (a process dubbed *abductive inference* by the philosopher Charles Peirce [1997/1903]). Vision allows us to infer which configuration of objects in the world best explains the two-dimensional light patterns hitting our retina; language understanding allows us to infer which meaning of a sentence best explains a sequence of sounds; memory allows us to infer which sequence of past events best explains the disparate traces of recollection rattling around our brains. Typically these processes are automatically executed by the brain without our even realizing we are assessing hypotheses at all. But other sense-making processes sometimes require more conscious effort, such as inferring what caused some event that we observed, which social category a person belongs to based on their traits, or what your friend is thinking based on their actions. I've developed a theoretical framework called *explanatory logic* to try to understand what these seemingly very different processes of hypothetical inference have in common, and what underlying cognitive processes they share (Johnson, 2018a). Any such processes would likely be important for understanding how we think and behave in economic contexts.

For instance, suppose Barbara, the CEO of a firm, observes that competing firm Acme Labs is buying up large quantities of tin. She wants to know why this is, given that only one of Acme's current products – the Model T widget – includes major tin components. It could be because Acme has expanding its production of T-widgets, which are also produced by Barbara's firm, or it could be because Acme is introducing a new tin-based product, which may or may not compete directly with Barbara's products. These two inferences have very different implications for Barbara as CEO. In the former case, she may need to take aggressive steps to head off Acme's widget play, perhaps through aggressive advertising, price promotions, or quality improvements. In the latter case, Barbara may want to know what new product Acme is introducing, perhaps looking to introduce her own version of this product or to re-position one of her existing products as a substitute for it. Explanations matter.

One approach, tailor-made for thinking through such problems, is Bayesian inference (Pearl, 1988). We assign initial degrees of belief to each hypothesis (*prior probabilities*) and update these initial beliefs in light of how well each hypothesis explains the evidence (*likelihoods*). If Barbara is a strict Bayesian, she would first consider, in the absence of knowing about the tin purchase, the relative odds that Acme would expand T-widget production versus introduce a new product. Let's say the former is twice as likely as the latter. Next, Barbara would evaluate the fit of each hypothesis to the evidence, that is, how likely the observed tin purchase would be given expanded production of T-widgets versus a new product. Let's say the tin purchase is enough to make a heck of a lot of T-widgets, so that it is somewhat implausible and the tin purchase is four times more plausible under the new-product hypothesis. Reverend Bayes gave Barbara a neat trick for computing which hypothesis is therefore likelier given the observed evidence—simply multiplying these two ratios ($2/1 * 1/4 = 1/2$). With this final step, Barbara concludes it is twice as likely the company is introducing a new product rather than expanding T-widget production and can plan accordingly.

In fact, some cognitive scientists believe that the great unifying process underlying the diverse cognitive processes I mentioned above—perception, language understanding, causal reasoning, and so on—is precisely Bayesian inference (Lake et al., 2017; Tenenbaum et al., 2011). This may be surprising to many behavioral economists, used to siding with psychologists over mainstream economists in the rationality wars, because this community of computational cognitive scientists essentially believe that human beings are rational, perhaps nearly optimal, information processors. Indeed, mathematical models of human behavior that assume Bayesian inference fit behavior well in a wide range of tasks, including how people learn the meaning of words (Xu & Tenenbaum, 2007), other people's goals (Baker, Saxe, & Tenenbaum, 2009), the masses of objects (Hamrick et al., 2016), the properties associated with categories (Tenenbaum, Griffiths, & Kemp, 2006), whether two variables are causally related (Griffiths & Tenenbaum, 2005), and how responsibility should be assigned for outcomes (Gerstenberg et al., 2018), among many other kinds of inferences.

I am not, however, a Bayesian—at least, I do not believe that people have a generalized Bayesian engine in their brains that optimally solves hypothesis-inference problems. I accept the evidence mentioned above, and acknowledge that people are remarkably close to optimal Bayesians for some kinds of tasks. People are amazingly good at tasks such as perception that are "encapsulated" from conscious thought, and surprisingly skilled at many highly constrained tasks that require more explicit reasoning, such as those studied in the Bayesian papers cited above. But these tasks are not typical of many real-world problems that humans solve. Realistic hypothesis-inference problems pose at least four seemingly insuperable challenges, so daunting that no known algorithm can solve them with anywhere near human adeptness. These are the challenge of searching through a potentially infinite set of possible hypotheses (*hypothesis space limits*), the challenge of making inferences in situations where potentially critical information is unknown (*information limits*), the challenge of the imprecision of most of our knowledge (*specification limits*), and the challenge of exponential explosions in the computational complexity of apparently optimal reasoning strategies (*capacity limits*). Simply put, optimal Bayesian inference in all but the simplest cases is not merely difficult—it is impossible.

Parallel to the question of why markets are not perennially failing, then, the question we must ask ourselves here is why humans are not bumbling around the planet with no clue what is going on. We are shockingly good at hypothesis-inference problems, despite these limitations. How is this possible?

The answer is that humans use a suite of heuristics and strategies to circumvent these limits. Within cognitive science, there are two clashing notions of heuristics. The glass-half-empty "heuristics and biases" approach (Kahneman, 2011; Tversky & Kahneman, 1974) familiar to behavioral economists says that humans fall back on these short-cuts to reduce effort, largely out of cognitive laziness, and emphasizes the systematic biases associated with heuristic thinking. The glass-half-full "adaptive heuristics" approach (Gigerenzer & Goldstein, 1996) emphasizes the adaptiveness of heuristics relative to optimizing procedures such as linear regression, and argues that heuristics exploit the most relevant information while ignoring less critical information that can introduce overfitting.

I actually agree with both of these approaches—the glass is both half-full *and* half-empty! Heuristics *do* sometimes lead to systematic biases. And they usually *are* adaptive. This is because cognition without short-cuts is impossible (Chomsy, 1965; Keil, 1981). Most of the time, biased-but-reasonable inferences are better than no inferences at all, and we have a set of heuristics that work reasonably well for hypothesis-inference problems despite a distinct lack of optimality. This view is probably closest to the "resource rationality" view common among some proponents of Bayesian inference (Shenhav et al., 2017; see also Simon, 1955), which says that people do use biased heuristics to solve problems, but deploy these heuristics in a way that ultimately optimizes over our sharply limited cognitive resources. Replace the still-too-idealistic "optimal" with the more-lukewarm "reasonable" and this view seems to usefully reconcile Kahneman, Tversky, and Gigerenzer.

Now, let's consider how heuristics circumvent each of the limits I mentioned above.

First, *hypothesis space* limits reflect the fact that the world usually does not supply its own hypotheses, but we must instead create them. Barbara had to come up with the idea that her competitor might be expanding T-widget production or might be introducing a new product. But there are many other ideas that never occurred to her at all, despite their logical possibility. The CEO of Acme could have accidentally ordered tin when he had instead meant to order aluminum; he could be doing a favor for his friend in the tin industry; he could be trying to corner the tin market in the tradition of the Hunt brothers; he might believe in numerology and admire tin's atomic number 50. But she did not think of these bad hypotheses and then take the effort to reject them; she just thought of the plausible ones. According to one view, we choose which hypotheses to consider by sampling the space of possible hypotheses according to their prior probability (e.g., Dasgupta, Schulz, & Gershman, 2017). But this begs the question of both how the hypothesis space itself is constructed and how prior probabilities are assigned to each hypothesis; even if true, this can only be a partial explanation. Several research programs have studied inductive biases that lead people toward certain kinds of hypotheses (Kalish, Griffiths, & Lewandowsky, 2007; Lagnado et al., 2007). For instance, people rely on knowledge of stable causal mechanisms (Johnson & Ahn, 2015, 2017), the accessibility of information in memory (Hussak & Cimpian, 2018), and the structure of events across time (Derringer & Rottman, 2018; Johnson & Keil, 2014; Lagnado & Sloman, 2006) as useful cues to generating hypotheses. We will see later on that thinking through stories can also prune the hypothesis space.

Second, *information limits* exist because the available data often underdetermines which hypothesis is correct. The CEO would love a peek inside her competitor's factory to test these hypotheses, but cannot do so without committing industrial espionage. People have an impressive capacity to generate evidence by marshalling other relevant information from memory and considering its implications (e.g., recalling a presentation from members of the engineering team who had mentioned possible advantages of tin components, conditional on other technological breakthroughs, and inferring that the competitor may have experienced such a breakthrough). Less impressively, people tend to infer evidence even where none exists by using irrelevant cues, and this often leads people to make erroneous inferences about hypotheses that make unverified predictions (Johnson, Rajeev-Kumar, & Keil, 2016; Khemlani, Sussman, & Oppenheimer, 2011).

Third, *specification limits* exist because Bayesian methods for evaluating hypotheses require precise numerical probabilities, which hypotheses typically do not wear on their sleeves—that is, we often operate in an environment of Knightian uncertainty. In the numerical example above, Barbara was able to estimate the prior probabilities of the widget-expansion hypothesis over the new-product hypothesis as 2-to-1, and the likelihood of the data as 1-to-4 under these two hypotheses. How did Barbara come up with such figures and why don't they have more decimal places? What database could Barbara have consulted, for instance, to calculate the prior probability that the competitor would expand widget production? One strategy people use for circumventing this problem is to use Occam's razor to infer simpler rather than more complex hypotheses, since simpler hypotheses tend to have higher prior probabilities, balancing this factor against the fact that more complex explanations often are better able to fit the data (Johnson, Valenti, & Keil, 2018; Lombrozo, 2007). This strategy, unlike Bayesian inference, need not be accompanied by precise probabilities, but nonetheless can flexibly address hypothesis-inference problems by weighting simplicity differentially across contexts.

Fourth, we face *capacity limits* if we try to use uncertain inferences about hypotheses to make further predictions. Suppose Barbara goes ahead and calculates a 70% probability that the competitor is introducing a new product. This fact is not itself what Barbara wants to know. Instead, she would like to know how this will affect her own market position. If Acme is introducing a new product, is it in a competing market? If so, which one and how much will this weaken Barbara's firm's position? If not, which one and can Barbara's firm enter that market to compete on price or quality? If Barbara's firm entered the new market, how long would it take to recoup this investment and would it have positive net present value? Each of these questions depends on the answers to the previous ones, and each possible answer raises new questions. Making optimal predictions from uncertain hypothetical inferences requires us to keep track of the uncertainty at each stage and propagate it across the chain of inferences—a task that compounds exponentially in computational complexity with the number of steps. People use a grossly simplifying heuristic to solve such problems, called *digitizing*—rather than treating probabilities as analog quantities between 0 and 1, they often treat them digitally, as though either 0 or 1, when making predictions (Johnson, Merchant, & Keil, 2018; Murphy & Ross, 1994; Steiger & Gettys, 1972). This means that people systematically ignore uncertainty and focus on a single predictive pathway to the exclusion of others. On the plus side, this allows us to use hypothetical inferences to make predictions. On the minus side, these predictions are systematically overconfident in the sense that they are too close to 0 or 1. This sort of dynamic may be responsible for boom-and-bust cycles in the macroeconomy, for instance when homeowners and investors ignore the low but non-zero probability that their homes values will decline (Gennaioli, Shleifer, & Vishny, 2015).

Digitization may also help to explain the excessive volatility found in stock prices (Shiller, 1981). For example, stock market futures performed a strange dance on the night of Donald Trump's election in November 2016. As the exit polls increasingly came to favor Trump over Clinton, S&P 500 futures sank in value, nearly 4% in a few hours. In the early hours of the morning, futures prices began to rise as steadily as they had sank, coinciding with Trump's uncharacteristically gracious victory speech as Clinton conceded the race. Futures prices rose as fast as they had sank, and by the time trading opened the next morning the price had recovered to the level of the previous evening's close. The *Wall Street Journal* explained this event as the market's oscillation between adopting a "Bad Trump" hypothesis (protectionist, unpredictable) versus a "Good Trump" hypothesis (tax-reforming, regulation-slashing). Perhaps if we knew with certainty which Trump would govern, this really could explain 4% of the present-discounted future dividends of American companies. But this market swing was evidently based on almost no information—a speech which provided little in the way of policy but which managed to avoid protectionist rhetoric. This might raise our credence in Good Trump from 45% to 55%, but surely not from 0% to 100%. Unless, of course, the stock market in aggregate tends to digitize, "rounding up" probabilities like 55% closer to 100% and "rounding down" probabilities like 45% closer to 0%. We will never know what investors were thinking that night, but experimental evidence shows that in general, lay (and possibly even professional) investors appear to reason about stock prices just this way (Johnson & Hill, 2017).

I conclude that hypothesis-inference problems are widespread both in everyday cognition and in economic decision-making; that these problems, posed abstractly, could be solved effectively by Bayesian calculations; but that these calculations, when even modest elements of realism are introduced, prove practically and even conceptually impossible. Humans have evolved heuristics to circumvent these inherent limitations, and while these heuristics do introduce (sometimes systematic) errors, they perform well enough in real-world settings to allow humans to get by. A growing experimental literature finds that these heuristics emerge early in childhood (Bonawitz & Lombrozo, 2012; Johnston et al., 2016) and that these same heuristics guide basic cognitive processes such as causal thinking (Johnson et al., 2016; Khemlani et al., 2011; Lombrozo, 2007), category-based reasoning (Johnson, Merchant, & Keil, 2015; Murphy & Ross, 1994; Sussman, Khemlani, & Oppenheimer, 2014), and visual tasks (Johnson, Jin, & Keil, 2014), as well as intuitions in applied settings such as stereotyping (Johnson, Kim, & Keil, 2016), consumer choice (Johnson, Zhang, & Keil, 2016), and finance (Johnson & Hill, 2017). My hope is that future work can help to refine and formalize these information-processing strategies as well as to broaden their relevance to economic phenomena. If economists tend to look for their keys under the mathematical spotlight, then the only hope of finding them is to make that spotlight bigger or the surrounding darkness brighter.

Stories

A second tool that people use is narrative thinking—the process of taking a sequence of events, imposing a causal and temporal order on it (a story), and using that story to predict what will happen next. This is related to, but distinct from, the hypothesis-inference problems I described above. Those problems typically revolve around identifying a causal hypothesis that explains some data, with knowledge of that cause in turn being useful for action directly or for making a prediction about some other thing it affects. Stories are richer than this. They are chains of causation with a distinct temporal order, with goal-directed activity at their center, and which by their nature predict events yet-to-come in that causal–temporal chain.

The role of stories in decision-making (Pennington & Hastie, 1992) and economic activity (Akerlof & Snower, 2016; Shiller, 2017) has been increasingly acknowledged in recent years. David Tuckett has proposed *conviction narrative theory* (CNT) as a sociological and psychological theory of how humans use narratives to think, decide, and communicate (Tuckett, 2011; Tuckett & Nikolic, 2017). Tuckett identifies several functions of narratives in economic decision-making—making sense of situations in order to identify opportunities; simulating the consequences of potential actions; communicating the rationales underlying choices to gain social support; and maintaining conviction for a chosen action in the face of uncertainty (see Akerlof & Snower, 2016; Mercier & Sperber, 2018).

As just one example of stories in economics, narrative thinking appears to play an outsized role in how the world's investment capital is allocated across equities. Tuckett (2011) interviewed dozens of fund managers, collectively responsible for managing over $500 billion. These interviews uncovered numerous examples where managers used stories for all these purposes: To spot opportunities (e.g., situations in which market prejudice led a company to be undervalued), to imagine what would happen if they acted (e.g., predicting the price will revert to reflect fundamentals after a delay), to justify these choices to others (e.g., investors or subordinates), and to maintain conviction (e.g., to hold onto a stock after a decrease in its price). This last function—maintenance of conviction—is analogous to the "belief digitization" results described above. That is, the managers consider multiple possible stories, and adopting the one they consider likeliest rather than trying to integrate across the probabilities of all the different stories when they act (if indeed these probabilities were even calculable in any meaningful way). This is probably crucial for avoiding paralysis in the face of profound uncertainty.

Ongoing experimental work has been examining the cognitive underpinnings of narrative thinking, as well as its consequences for financial decision-making. This work has supported a number of insights broadly consistent with the more qualitative research mentioned above. Three broad conclusions can be reached on the basis of this work.

First, *people automatically supply stories when using data to form expectations*. One study focused on how investors use explanatory information (e.g., from analysts) in predicting prices. Participants read about companies whose stock price had either increased or decreased (Johnson, Matiashvili, & Tuckett, 2018a). When these price changes were explained as occurring due to an internal cause (e.g., poor management), participants were more likely to extrapolate the trend into the future, compared to when the explanation invoked an external cause (e.g., a supply shock). This is consistent with the idea that internal causes would be perceived as more stable over time. However, either type of explanation led people to extrapolate trends more compared to a condition in which no explanation was given. In a follow-up study, even unexplained price trends were treated more like signal than like noise, particularly price increases. Such beliefs could potentially lead to stock prices that are rigid downwards, because price decreases require more evidence to be perceived as "real" compared to increases.

A related study looked at how investors use price history information in predicting prices. There is evidence that investors tend, by default, to extrapolate past trends linearly, such that recent price increases are expected to give way to future price increases, and vice versa (De Bondt, 1993). But recent research has found that people actually reason about price trends in a more sophisticated way that relies on pattern-matching (Johnson, Matiashvili, & Tuckett, 2018b). Although people do assume that linear price changes will be followed by further price changes in the same direction, people are much less likely to extrapolate trends when the price history includes previous periods of reversion (the price both increased and decreased) or a period of stability (the price held constant at one level). This was true for a variety of different prices in addition to stock prices (e.g., foreign exchange rates, futures contracts, consumer goods), occurred under incentive-compatible conditions, and, like the effect of attributions described above, was observed among finance experts. Such pattern-based expectations should be accounted for in models of investor behavior, as they can in principle lead to feedback loops among price changes, price expectations, and investing behavior.

Second, *narrative-based expectations produce downstream consequences for other thoughts and behaviors*. For example, one study looked at how participants use company news to predict the company's future stock price (Johnson & Tuckett, 2017). Participants predicted future prices in light of a positive or negative piece of news about a company (e.g., positive or negative earnings surprise) which was either about the future or the past (e.g., last quarter's actual earnings or next quarter's predicted earnings). Mainstream financial theory (Fama, 1970) says that such information should have no impact on stock prices, assuming some gap between when the announcement was made and when we learn about it (so that the market prices in the information). So if people follow rational expectations, positive or negative news should be treated similarly. A second possibility, motivated by behavioral finance, is that people would instead predict stock prices in a way consistent with known econometric trends (Bernard, 1992), namely that stocks have short-term momentum (prices overreact relative to the long-term trend) followed by mean-reversion in the longer-term (reverting to the original trend).

But in fact, participants followed a third model—their expectations became increasingly extreme at longer time horizons, such that they predicted a modest difference in stock prices between positive versus negative news at a 2-week interval, but much larger differences at a 1-year interval.

This is inconsistent with both rational expectations (there should be no difference at any time horizon) and also standard behavioral accounts (there should be a larger difference at a short time horizon and smaller difference at a longer time horizon). Instead, participants appeared to rely on narrative thinking, inferring some underlying cause that will lead to stable price increases or decreases into the future. Moreover, this trend was exacerbated by news that was future- rather than past-oriented, consistent with the idea that narrative thinking involves an important temporal component. Similar results were observed for a group of participants highly knowledgeable about investing (e.g., PhD students in economics, MSc students in finance, and professional financial analysts).

In addition to confirming again the role of narrative thinking in expectations, this study looked at how these expectations influence choices and the emotional dynamics mediating this process. These beliefs about future trends indeed translated into decisions—participants were more likely to include stocks in a hypothetical portfolio when those stocks had more positive future expectations, and this was true even though standard financial advice would give very different advice, both under efficient markets assumptions (Malkiel, 2015) and under behavioral assumptions (Jegadeesh & Titman, 1993). These choices were in turn mediated by emotional processing, which is an integral part of narrative-based choice according to conviction narrative theory (see Damasio, 2006). Participants' choices to include a security were accompanied by a prevalence of positive, approach emotions over negative, avoidance emotions. An economic role for emotion is also confirmed by large-scale econometric analyses of news databases, finding that the prevalence of excitement- versus anxiety-related words in the financial press predict macroeconomic aggregates such as GDP and output (Nyman et al., 2018).

Finally, *people attend carefully to trusted sources to inform their expectations and choices*. Indeed, endorsement from trusted sources can even override direct evidence (Johnson, Rodrigues, & Tuckett, 2018a). In one study, participants decided which stocks they wanted to include in a portfolio, where each stock was accompanied by information about its industry as well as conflicting opinions by two expert stock analysts. The companies' industries could be either prototypically associated with politically left (e.g., electric cars) or right (e.g., oil companies) sensibilities, and were endorsed by stock analysts with either more left- or right-leaning ideological views. The alignment between the experts' and participants' political views strongly predicted portfolio allocation choices, and completely swamped any effect of the companies' industries. That is, politically left participants would eagerly invest in oil companies if endorsed by a liberal analyst, and politically right participants would seize on the opportunity to invest in electric cars if endorsed by a conservative analyst. Source information is equally critical in guiding consumers' decisions about products, including cultural products such as books as well as seemingly apolitical products such as blenders (Johnson, Rodrigues, & Tuckett, 2018b). Thus, stories are likeliest to be adopted when they come from a trusted source. Since stories are bundled in a digestible form for communication, these trust dynamics likely govern their spread through social networks. Socially-mediated story adoption can lead to positive outcomes, such as coordination, as well as negative outcomes, such as herding and panic (Shiller, 2000; Tuckett, Smith, & Nyman, 2014). Studying the relation between social and economic realities is a particularly crucial area for future research, since their alignment has a profound influence on the economic consequences of narrative thinking.

Although this work focuses mainly on financial decision-making, it is likely that analogous principles characterize behavior in a variety of other economic contexts, such as managerial strategy and consumer choice. In our earlier example, we considered a CEO deciding among competing explanations for her competitor's behavior. One of the major problems she faced was determining which hypotheses to even consider. This problem may be simplified through narrative thinking. For example, prototypical patterns of events—such as scripts (Schank & Abelson, 1977), schemas (Rumelhart, 1980), or causal mechanisms (Johnson & Ahn, 2017)—may be identified as skeletons, to which recent evidence can be added to flesh out a story. These complex stories may then, in turn, be evaluated based in part on the same hypothesis-inference heuristics used to evaluate simpler hypotheses, and then to form expectations along the lines of the studies of financial decision-making just described. Such hybrid patterns of thought may be ubiquitous in characterizing complex, real-world decisions such as strategic choices. Such reasoning strategies are difficult to study experimentally because they contain a multitude of moving parts; but such study will be increasingly important as we try to marry cognitive science with real-world economic choice.

Intuitive Theories

Hypothesis-inference heuristics and stories are useful for understanding an individual event, acting on it, and predicting the future. But humans often wish to go beyond individual experiences and form more generalized knowledge. Several interconnected literatures in developmental psychology examine the *intuitive theories* that children have of how the physical, biological, and social worlds work. The astonishing result of this decades-long research tradition is that children, even infants, have remarkably rich understandings of these domains (Spelke, 2000). Babies understand, for example, that unsupported objects fall down (Needham & Baillargeon, 1993), that one plus one equals two (Wynn, 1992), that living things have solid insides (Setoh et al., 2013), and that people will act on false beliefs rather than unknown truths (Onishi & Baillargeon, 2005). It is reasonably clear why natural selection would have built such intuitions into our minds—even primates share some of our intuitions about physics (Santos & Hauser, 2002), and given humans' ecological niche it makes sense that we have uniquely well-tuned instincts about the social world (Frith & Frith, 2007).

But in recent millennia, human culture has advanced exponentially faster than biological evolution, and society has given us fantastically sophisticated technological tools and social institutions (Boyd, Richerson, & Henrich, 2011). But this is a story of collective success in the face of widespread individual failure (Sloman & Fernbach, 2017). Despite the illusion that we understand how complex artifacts such as locks or toilets work, most individuals have amazingly shallow knowledge about their underlying causal mechanisms (Rozenblit & Keil, 2002). Our ability to piece together sophisticated intuitive theories is remarkably poor for domains where biological evolution has not prepared us. Yet, the cultural evolution of technology can be considered a triumphant success because it is usually unimportant for people to know how the gadgets around them work, so long as they can use them.

But it is not all triumph. In a market economy, and a democratic one especially, it *is* important for people to understand how socially agreed institutions work. Humans, as Adam Smith wrote, are not pieces on a chessboard to be pushed around at will, but "in the great chess-board of human society, every single piece has a principle of motion of its own" (Smith, 1759). Institutions, then, exist in a feedback loop among our individual human nature as market participants, the emergent forces that govern the economy within a particular set of institutions, and the beliefs and choices we make within those institutions to shape them. Our knowledge of economic institutions can affect our behavior as market participants, and as voters our ignorance can damage or destroy those institutions.

This topic has received some attention in political economy. For example, Bryan Caplan (2007) argued that voters' systematic deviations from economists' views constitutes a serious obstacle to effective democracy. He compared economists' versus laypeople's answers to the same questions (Caplan, 2002), and identified four biases suffered by laypeople relative to economists—*make-work bias* (conflating economic growth and employment), *anti-foreign bias* (dismissing the benefits of interacting with foreigners), *pessimistic bias* (unduly negative perceptions of current economic conditions and their improvement relative to the past), and *anti-market bias* or *emporiophobia* (distrusting market mechanisms; see Rubin, 2014). While the evidence for these biases is compelling and their political effects undeniable, Caplan's analysis does little more than to supply labels for them—a deeper analysis would probe *why* people hold these systematic misconceptions and whether there is potential to correct them. This is precisely the kind of work cognitive scientists do.

Until recently, cognitive scientists paid little attention to our intuitive economic theories. However, an explosion of interest promises to accelerate our understanding greatly (Boyer & Petersen, 2018; Leiser & Shemesh, 2018). This emerging research tradition has probed laypeople's mental models of a wide range of economic phenomena, though the research on any one topic is typically very thin. This includes basic economic concepts such as supply and demand (Leiser & Halachmi, 2006), marginal utility (Greene & Baron, 2001), exchange (Cosmides & Tooby, 1992; Fiske, 1992), trade-offs (Fiske & Tetlock, 1997), profit (Bhattacharjee, Dana, & Baron, 2017), and property (Blake & Harris, 2009; DeScioli & Karpoff, 2015; Friedman, 2010); major economic phenomena including inflation (Leiser & Drori, 2005), unemployment (Furnham, 1982), inequality (Gandy & Baron, 1998; Starmans, Sheskin, & Bloom, 2017), poverty (Furnham, 1982), and financial crises (Leiser, Bourgeois-Gironde, & Benita, 2010); and policy issues such as taxation (McCaffery & Baron, 2003, 2006), public goods (Kemp, 2002), redistribution (McCaffery & Baron, 2005; Petersen, Sznycer, Cosmides, & Tooby, 2012; Skitka & Tetlock, 1993), regulation (Haferkamp et al., 2009; Hirshleifer, 2008), immigration (Hainmuller & Hiscox, 2010), and international trade (Baron & Kemp, 2004; Hiscox, 2006; Kemp, 2007). Further, a tiny but fascinating literature looks at how people believe these concepts relate to one another, particularly in macroeconomics (Leiser & Aroch, 2009; Williamson & Wearing, 1996).

Although the literature on each of these topics is individually thin, one major and consistent finding is that people construe these issues in moral terms to a much greater degree than economists (Coase, 1960 is a classic example), consistent with standard behavioral economics results documenting attitudes toward price fairness that are quite foreign to economists' analytical toolkit

(Kahneman, Knetsch, & Thaler, 1986). This supports Rubin's (2003) contention that folk economic thinking focuses almost exclusively on the distribution of wealth rather than its creation. Indeed, moral concepts even appear to creep into views of macroeconomic causation, which are dominated by the notion that "good begets good" (Leiser & Aroch, 2009), such that "bad" economic phenomena are causally related (e.g., unemployment and inflation) and inversely related to "good" phenomena (e.g., growth). Macroeconomists disagree on much, but saltwater and freshwater alike can agree that this is nonsense. But if inflation expectations are a key driver of inflation itself (Friedman, 1968; Solow, 1969), such nonsensical beliefs can become self-fulfilling prophecies.

Let's zoom in on one of these issues in order to better understand the sorts of fruits this research can offer. Rubin (2003) suggests that one of the deep differences between economists' versus laypeople's mental models of economic activity is that economists view most transactions as positive-sum, whereas laypeople view them as zero-sum (explaining, incidentally, why lay economic theories prioritize distribution above production). There has been surprisingly little empirical attention paid to the possibility that people have a zero-sum mental model of economic transactions, though zero-sum beliefs have been found in other domains (Burleigh, Rubel, & Meegan, 2017; Meegan, 2010; Newman, Gorlin, & Dhar, 2014; Pilditch, Fenton, & Lagnado, 2019; Rozycka-Train, Boski, & Wojciszke, 2015; Smithson & Shou, 2016). However, it seems intuitive that people would think in this way, is consistent with a good deal of political rhetoric, and would help to explain some of the biases Caplan (2007) identified, such as anti-foreign bias (see also Bazerman, Baron, & Shonk, 2001). Does zero-sum thinking in fact explain protectionist attitudes in trade policy, and if so, what cognitive factors lead people to think in this way?

Taking the latter question first, my own research has found that there are at least two principal drivers of zero-sum thinking. A first reason is people's tendency toward *intuitive mercantilism*. Mercantilism is, of course, the pre-Smithian notion that wealth should be identified with money rather than with useful goods and services. One series of experiments tested this idea by describing extremely simple, everyday economic transactions to research participants (Johnson, Zhang, & Keil, 2018a), such as Sally buying a $30 shirt from Tony's store, Eric getting a $15 haircut from Paul's barber shop, or Vivian and Tommy swapping their McDonald's and Burger King hamburgers. Economics, along with Smithian common sense, tells us that for the monetary transactions, the buyer and seller both benefit, since they otherwise would not have agreed to the transaction, and likewise both barter partners must be benefitting or they would not have traded. But laypeople do not share these intuitions. They believe that sellers benefit at buyers' expense, while neither trader benefits from a barter. This is bizarre in a Smithian world, but not in a mercantilist one where the notion of benefit is restricted to monetary benefit. Buyers do indeed lose money (though they value it less than what they buy) while sellers gain money (which they value more than what they sell), while no money is exchanged at all in a barter.

(The second reason for zero-sum thinking, less relevant perhaps to international trade, is that people often fail in spontaneous perspective-taking (Lin, Keysar, & Epley, 2010). Smith teaches us that "it is not from the benevolence of the butcher, the brewer, or the baker, that we can expect our dinner," but equally it is not from the buyer's benevolence that they purchase their dinner. Such insights require us to take the perspective of the buyer and seller to recognize their own motivations, and failing at this can exacerbate the zero-sum thinking produced by mercantilism. This is supported by experimental evidence. In experiments similar to those described above, giving explanations for the buyers' actions—even empty ones ("Sally made the purchase because she wanted the shirt")—greatly reduced the rate of zero-sum thinking.)

If people extend these zero-sum beliefs about sellers (exporting countries) "winning" at the expense of buyers (importing countries), then precisely the same logic underwriting zero-sum beliefs about individual transactions would produce anti-trade attitudes in the context of the global economy. Once again, this is confirmed by multiple lines of converging experimental evidence (Johnson, Zhang, & Keil, 2018b). First, beliefs about importing and exporting countries precisely mirror those about individual buyers and sellers, with exporting countries as "winning" and importing countries as "losing." For example, if participants are told that "Some people, who live in the United States, order pairs of Nike running shoes from Thailand. They each pay $150 for the shoes and receive them in the mail," participants tend to believe that the U.S. is made worse-off while Thailand is made better-off. Second, these beliefs even extend to *domestic* trade—states that import goods from other states are seen as "losing" at the other states' expense—although these intuitions are less strong than country-level intuitions. Finally, these effects can be largely undone by invoking the concept of *balance of payments* (going back to Smith's friend and intellectual fellow-traveler David Hume, 1752). That is, dollars in must equal dollars out as a matter of accounting, so that dollars paid for imports must return from exports or investment (e.g., purchasing U.S. debt). When this concept is made salient (e.g., "The sellers of the shoes used the dollars they received to purchase U.S. products and invest in the U.S. economy"), imports are deemed much less harmful. This is good news from the standpoint of challenging erroneous views, but additionally it is strong support for the notion of intuitive mercantilism—such arguments undercut trade-skepticism by highlighting the fact that even mercantilist views (identifying wealth with money) imply that trade can be neutral as long as the money ultimately comes back home one way or another.

This explanation of trade-aversion in terms of intuitive mercantilism differs from several others on offer for why people are averse to international trade. Although these alternative explanations are not mutually exclusive—there is indeed evidence for all of them—none are conceptually or empirically equipped to dispel intuitive mercantilism as the main driver of trade aversion (Johnson, 2018).

First, as noted previously, humans and even some non-human animals have evolved intuitions about physics, and humans may also have evolved intuitions about exchange (Cosmides & Tooby, 1992; Pinker, 2003). But such intuitions would have evolved in an environment of hunting, gathering, and barter among small bands, not a globalized economy intermediated by money. Consequently we may have strong but erroneous intuitions about the economy. Although this does explain why we do not have accurate evolved intuitions about trade, it does not explain why we have the specific erroneous intuitions we do. Our ancestors presumably would ridicule exporting countries for giving up valuable resources in exchange for useful bits of paper, not admonish them for exploitation.

Second, people may fail to understand the concept of comparative advantage (Baron & Kemp, 2004). Indeed, those with poor measured understanding of comparative advantage are more likely to favor trade restrictions. Interestingly, New Zealand participants tended to outperform Americans, possibly because living on a small island makes one's comparative disadvantages more salient.

Third, humans have strong evolved intuitions about supporting their in-group or tribe while battling their out-group or competing tribes (Boyer & Petersen, 2018). On this view, trade is aversive because it involves transferring resources to the out-group, even though one's own group also gains. This predicts that only *international* trade would be seen as aversive, whereas we have seen that even domestic trade (across states) and exchanges between individual consumers and retailers are seen as zero-sum. However, since zero-sum tendencies are indeed *stronger* for international trade, it is likely that coalitional thinking exacerbates existing mercantilist tendencies.

Like so many other economic issues, people thinking about trade appear to focus on distribution (allocating the pie) rather than efficiency (expanding the pie). And once again, this results in moralistic attitudes creeping into economic thinking. In experiments, people not only claim that imports are economically harmful, but that the consumers who choose to import these goods are behaving *immorally*. This is particularly true for imports from developing countries, which appear to trigger the paradoxical belief among some people that such trades are lose–lose. These moralistic attitudes are worrying for at least two reasons. First, they could very well drive public policy, both because politicians may hold similar attitudes, which may be further exacerbated through their selection by voters, and because voters may enforce them even among politicians who do not privately agree with them. Second, even in a regime of unfettered free trade, consumers who incorporate a moral cost into purchases of foreign goods may, at the margin, be less inclined to purchase foreign products even if foreign production is economically efficient. The price system leads to efficient outcomes because it coordinates the behavior of producers and consumers. If consumers experience an intangible, and economically illusory, moral cost to purchasing foreign products, international trade may be less efficiency-enhancing in practice than it appears on paper.

The cognitive underpinnings of economic intuitions have been studied most thoroughly for trade, in part because it is topical and politically contentious. But numerous other critical issues loom large, including policy issues such as taxation, regulation, and macroeconomic policy, and a full picture of economic activity must embrace the feedback loops between ordinary economic processes and the internal conceptions of these processes by economic agents.

Conclusion

We're the last couple left in the restaurant; the kitchen is long closed. We should wrap things up so that the wait staff can go home. On the way out, let's consider: How did this first date go? Shall we schedule another?

I argued that cognitive science and behavioral economics need one another. Cognitive science focuses on characterizing internal states and understanding how they produce behavior; it seeks to paint a rich picture of human nature; it is interested equally in the successes and failures of human cognition, preoccupying itself with the question of how hunks of organic material could have come to tie their shoes, much less dominate the planet ("What a piece of work is a man!" quoth Hamlet); it plays nicely with fields of inquiry across the academy; and, for all its blind spots, they are different from those of behavioral economics. Cognitive science is especially promising as a new body of knowledge and method to be applied to the problems of characterizing human preferences and expectations, and how humans manage in an environment not of risk, but of Knightian uncertainty.

As one example of how cognitive science can enrich economics, I've pointed to research on sense-making, describing three sets of tools humans are known to use. For thinking through "causal forks" (A or B could cause X; which is it?), people rely on a set of fallible yet broadly truth-tracking heuristics, which combine to make cognition possible in the face of apparently insuperable challenges, such as informational and capacity limits. For thinking through "causal chains" (A causes B, which causes C, which causes X; what could A, B, and C be?), people rely on stories, which allow us both to make sense of past information and, because of their temporal orientation, let us form expectations about the future. And for thinking through "causal webs" (A, B, C, and D are causally related in some way; what causes what?), people rely on intuitive theories, which, despite their shallowness, guide our beliefs and attitudes. I've provided examples of how these patterns of thought can influence aspects of economic behavior ranging from business strategy to consumer behavior to financial decision-making. And my suspicion is that, just as the economic implications of the heuristics and biases of Kahneman and Tversky took time to simmer within the economics profession, the fundamental role of sense-making in economics will become increasingly inescapable.

Where else might cognitive science play an important role? Sense-making is, after all, just one corner of the field. The vast literature on learning can be a critical addition to areas of behavioral economics that seek to characterize how behavior changes with experience, such as in behavioral game theory (Camerer, 1997). The literature on motivation not only points out a variety of drives not encompassed by rational choice theory (e.g., Ariely & Loewenstein, 2006), but also proposes alternative unifying theories of human motivation that might be profitably studied in economic contexts (Eccles & Wigfield, 2002). The literature on emotion tends to undercut the dominant view of emotion in behavioral economics as a biasing factor, instead identifying it as an integral part of ordinary human decision-making, which serves to mediate between goals and action (Damasio, 2006).

One particularly promising area is in moral psychology, which has been advancing at a breakneck speed in the past ten years. For example, one promising approach argues that humans are hard-wired intuitively for cooperation, but can override these intuitions and behave selfishly if the motivation and cognitive resources are available (Rand et al., 2014). Another new approach argues that people bargain "virtually"—that is, that humans solve coordination problems nonverbally by imagining what solution would emerge if verbal bargaining were possible (Misyak et al., 2014). And a variety of papers have looked at specific moral intuitions that people hold, including many we may not consider entirely rational upon reflection (e.g., De Freitas & Johnson, 2018; Sunstein, 2005), which can inform demand-side behavior and create supply-side responses.

If behavioral economics and cognitive science are one day to marry, I hope they will have a child—a *cognitive science of markets*. I have many hopes for that child. I hope that she is pluralistic, crying "yes!" to the intellectual world, embracing diverse methodologies and bodies of knowledge, even those that are far-flung from our ordinary notions of what economics or cognitive science are about. I hope that she joins cognitive science in recognizing the importance of measuring and characterizing internal states, and economics in studying matters that are of great importance to the affairs of the world. And I hope that she feels encouraged to study things that are difficult to study, even if this means we must look beyond ideas and tools that we find comfortable.

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