

COMMENTARY

The Primacy of Behavioral Research for Understanding the Brain

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Understanding the brain requires us to answer both *what* the brain does, and *how* it does it. Using a series of examples, I make the case that behavior is often more useful than neuroscientific measurements for answering the first question.¹ Moreover, I show that even for “how” questions that pertain to neural mechanism, a well-crafted behavioral paradigm can offer deeper insight and stronger constraints on computational and mechanistic models than do many highly challenging (and very expensive) neural studies. I conclude that purely behavioral research is essential for understanding the brain—especially its cognitive functions—contrary to the opinion of prominent funding bodies and some scientific journals, who erroneously place neural data on a pedestal and consider behavior to be subsidiary.

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In an era of increasingly precise methods for measuring and perturbing neurons in the brain, it often seems that with more neural data, we will soon make untold breakthroughs in understanding the brain. Such anticipation has heralded neuroscience-data-focused projects such as the Brain Initiative and the Human Connectome Project. The focus on neural measurement has been accompanied by the demotion of animal and human behavioral research—the mainstays of psychology from where understanding of the brain and mind originally hailed—to being “only” behavior, that is, insufficient, and even irrelevant to the neuroscientific quest. This widespread neuroscience chauvinism even infiltrates the way we think about and discuss our findings: Decision making is often described as executed by neurons, not by animals, and whole experiments in awake behaving animals are sometimes described from the point of view of neurons, not the animal housing them: “Neurons were presented with images . . . ”

I believe that this approach is fundamentally flawed. Assuming that a process such as decision making can be understood solely by

looking at single neurons, or even their ensembles, is like attempting to understand why people in Australia drive on the left side of the road from examination of their DNA. Neural firing patterns are the wrong level for investigating many pressing questions in neuroscience. Even if we could measure all the neurons in the brain with arbitrary precision, without an incisive behavioral paradigm we would not be able to answer many neuroscientific questions. Indeed, the insights about the brain that my lab has gleaned from our own research have almost all come from behavioral data, which explains the progressive decrease of neuroimaging experiments that we choose to run.²

An extremely sobering reminder of the fact that a full neural description does not guarantee understanding of what the brain does or how it does it, is the nematode *Caenorhabditis elegans*, whose behavior we are still far from being able to predict despite it having only 302 neurons and probably the best characterized nervous system in the world (Bargmann & Marder, 2013). That full knowledge of a

¹ My arguments here have also been published, in similar form, as a chapter in Lerner et al. (2020).

² This is not because functional neuroimaging (fMRI) experiments are useless. fMRI data, like any other data, be it choice behavior, reaction times, eye movements, can answer some questions and not others. My research focuses on understanding what algorithms the brain uses for decision making, and how these are affected by mental illness (i.e., computational- and algorithmic-level questions, rather than implementation-level questions, according to Marr, 1982). For these particular questions, I often find behavioral data together with computational models to provide more constraints than do neural data, and am hard pressed to come up with questions for which the neural data will tell us something beyond the behavior. However, there are exceptions, and I have used neural data (always in conjunction with specifically tailored behavioral experiments and computational models) to formally compare between computational models (admittedly, arriving at the same conclusion as was seen in the behavioral data; Niv et al., 2015), and to contrast computational algorithms that could not be differentiated behaviorally (Niv et al., 2012). Different from Coltheart (2006), I am not arguing here that fMRI—or any neuroscience experiment—is useless, only that without incisive behavioral paradigms, neuroscience is not nearly sufficient for achieving the understanding of cognition that we aim for.

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circuit does not equate with understanding of that circuit suggests that when trying to understand the brain, in particular high-order cognitive functions, focusing on neurons to the exclusion of other levels of inquiry, and without careful thought about behavioral paradigms that allow us to meaningfully interrogate the neural substrate, might lead us down an expensive and less-than-revealing rabbit hole. For the sake of better understanding the brain, we should therefore reverse the current “hierarchy” (in which neural measurements are seen as basic and fundamental, and behavior is an optional component that cannot stand on its own) and restore behavioral research to its historical place of primacy and necessity.

One might argue that the reason the majority of discoveries that have withstood time are due to behavioral research is that until recently we did not have access to incisive tools for neuroscientific research—noninvasive magnetic resonance imaging of humans and optogenetic manipulations are fairly new techniques. However, cutting-edge methods for recording and manipulating the brain have, for the most part, verified what we already knew from behavior, rather than led to truly novel insight. Of course, neuroscientific measurements (or brain perturbation studies) are necessary for answering questions about localization—*where* in the brain different (behaviorally identified) cognitive functions are implemented. But even today, behavior remains the standard by which theories about the *roles* and *computations* carried out by different neural structures are tested (even for theories that do not seek to model behavior directly, see, e.g., Wang, 2008). If we ask ourselves candidly “what has neuroscience taught us about cognition that we did not already know from behavior?” we realize that unfortunately the answer is “not very much.” Therefore, in the name of expediency, we have a moral obligation to our subjects of study, our taxpayer funders, and the patients waiting for cures, to reverse the trend that suggests that we should dispense with behavioral research and focus only on neural mechanisms.

What Has Behavior Taught Us About the Brain?

Long before the advent of neuroscience methods such as optogenetics and Designer Receptors Exclusively Activated by Designer Drugs (DREADDs), ingenious behavioral experiments made great strides in identifying the latent cognitive processes that underlie behavior—the ultimate output of the brain. For instance, even in low-level perception, arguably the area of neuroscience farthest from behavioral output, scientists inferred that color vision is due to the three types of neurons (retinal cones), and were even able to estimate the cones’ wavelength sensitivity from psychophysics experiments of color matching and chromatic adaptation, and studies in color-blind observers (e.g., Stiles, 1959; Stiles & Burch, 1959). This occurred decades before physiologists were able to patch and measure the wavelength response properties of individual cones and verify the original behavior-based predictions (Baylor et al., 1987).

At the other end of the spectrum, the elusive domains of high-level cognition and cognitive control, ideas about the role and information content of attentional signals feeding back into low-level perceptual processing areas were also derived from simple, but revealing, behavioral paradigms such as visual search and pop-out (Hochstein & Ahissar, 2002), and from experiments delineating the extent to which practice effects generalize across simple visual tasks (Ahissar & Hochstein, 1993, 1997). These two examples from the domain of vision are representative of others at all levels of research

into the workings of the brain and the mind. In this section, I describe in detail a small selection of results from behavioral studies that illuminate brain processes, each going beyond what would have been achievable even using unrealistic, whole-brain, single-cell-resolution neural measurements.

I will start with my personal favorite: A rat is trained to run in a T-shaped maze, from the base of the T to the right arm, to obtain food. At the critical junction, the rat may be choosing to go right due to internal, egocentric cues (i.e., turning right relative to its own body), or based on external, allocentric cues (turning toward a certain location in space, e.g., to the side of the room that has a window). Imagine you could record activity from anywhere in the rat’s brain during this experiment. How could you determine which of the two strategies the rat was using? Even knowing what we know about navigation, spatial maps in the hippocampus, and head direction cells, it is not clear what you should look for in your recordings. In particular, the fact that the external cues of the window and blinds are represented in some brain area does not mean the rat is *using* these cues to guide its actions. Arguably, even if you had recordings of every neuron in the brain during this task, it would be extremely difficult to answer this simple question regarding the rat’s strategy. And, the problem is not that recordings are correlational rather than causal. Perturbing a brain area and seeing an effect on behavior would also not reveal how the animal was making its decision when there was no perturbation. By manipulating the brain we can find out what brain areas *can* affect behavior on this task, but not what brain areas *do* affect behavior as the rat is making its right turn.

Instead, what Packard and McGaugh (1996) did was simple and cheap: They turned the maze around, so that if the base of the T had been pointing to the south, now it was pointing to the north. They then set the rat at the base of the T, and observed its behavior. If the rat continued to turn right, that would indicate an egocentric strategy. If the rat turned left, it must be following the peripheral external cues, and turning to the same side of the room. The latter is indeed what happened for most of the rats after 8 days of training, but not after 16 days of training, suggesting that decision-making strategies change with extended training. This elegant behavioral manipulation thus informed us of a neural strategy, suggested representations that are necessary for executing it (the rat must be representing the external cues), and allowed for a variety of follow-up experiments that would delineate the conditions under which rodents choose to use an allocentric rather than egocentric strategy. Understanding these conditions then led to computational models that specified the types of data that the animal must learn and use in each condition, and the computations that may support transitioning from one strategy to another (Daw et al., 2005). Indeed, neuroscientific experiments involving lesioning or inactivating brain areas continued to reveal some computational principles of this decision-making process (see the next section), and the transition between different behavioral strategies has been linked to mental health phenomena such as addiction and obsessive-compulsive disorder (Voon et al., 2015). However, the initial findings were due to cheap, and extremely clever, behavioral experiments.

In fact, the most fundamental observation about learning—that it proceeds through error-correction—was based on behavioral findings. In particular, if you train an animal that a neutral stimulus, say, a flashing light, predicts the availability of food, the animal will learn this relationship, as can be measured from the animal’s salivation response or food-cup approach once the light begins flashing.

Similarly, if a tone is paired consistently with food, the animal will show similar responses to the tone. However, in a series of carefully controlled experiments in the 1960s, Leon Kamin showed that if the light is first paired with food until learning asymptotes, and only *then* the tone is added to the light (still with food following presentation of both), the animal will not learn to respond to the tone (Kamin, 1968). This phenomenon of “blocking” showed that presenting a neutral stimulus and a motivationally significant outcome in a predictable temporal relationship is not sufficient to engender learning. Instead, learning requires a “prediction error”—the motivationally significant outcome (i.e., the food) must not already be fully predicted (in this case, by the light).

Based on these and other seminal behavioral findings, Robert Rescorla and Alan Wagner proposed a computational model of learning based on prediction errors (Rescorla & Wagner, 1972), which is, to this day almost 50 years forward, the most influential account of trial-and-error learning in animals and humans. Together with later computational models of reinforcement learning showing how one can use prediction errors to optimally learn the sum of future rewards predicated on a certain state or stimulus (Barto et al., 1983; Sutton, 1988), and recordings of activity of dopaminergic neurons while monkeys learned to associate cues with motivationally significant outcomes (Ljungberg et al., 1992), these findings led to the influential identification of phasic dopaminergic signals with a reward prediction-error signal (Barto, 1995; Montague et al., 1995, 1996), widely heralded as the poster child of computational neuroscience. This success story—a normative computational theory that explains and predicts key neural signals with precision (e.g., Tobler et al., 2003; Waelti et al., 2001), drilling in all the way from Marr’s (1982) computational level, through an algorithmic solution and to its neural implementation (Niv, 2009; Niv & Langdon, 2016)—began with behavioral findings (Sutton & Barto, 1990), and is a prime case of behavior and computation informing our understanding of neural mechanisms, not the other way around.

It goes without saying that ideas about stimulus competition and learning through prediction errors, first identified through blocking, have had far-reaching implications for mental health research. For instance, findings showing impaired blocking in schizophrenia (Martins Serra et al., 2001) inspired the hypothesis that positive symptoms of schizophrenia may, in part, be due to spurious associations between stimuli that should otherwise have been subject to blocking. Linking this to aberrant dopamine prediction errors provides another explanation for the beneficial effects of neuroleptics (dopamine antagonists) for treating positive symptoms.

More recently, behavioral experiments have led to more insight regarding these fundamental learning circuits by demonstrating a distinction between prediction-error signals due to receiving less reward than expected and prediction-error signals due to making a motor error that misses the rewarding target: While the former prediction errors affect learning of the value of the rewarding option, the latter do not, suggesting a neural dissociation between the two types of prediction errors (McDougle et al., 2016). In particular, the findings suggested that not receiving an expected reward due to one’s own reach error does not activate the same type of neural prediction errors—a hypothesis supported by follow-up work using functional magnetic resonance imaging (fMRI) of neural prediction-error signals in the striatum (McDougle et al., 2019).

In the domain of memory, the behavioral phenomenon of “retrieval-induced forgetting” shows that when trying to recall an item, memory

traces that compete for recollection but eventually lose the competition are subsequently weakened (Anderson, 2003). For example, imagine learning the word-pairs *fruit-pear*, *fruit-kiwi*, and *fruit-apple*. In a later rehearsal phase, you are requested to complete the stem *fruit-pe__* (presumably with the word “pear”). Because “apple” is the quintessential fruit in some countries, and moreover, it bears some semantic similarity to a pear (round-ish, palm-sized, tree-growing fruit that is sometimes green), it may come to mind as you recall the word “pear” and compete for that recollection. Behavioral findings show that if the competition is sufficiently strong (but “pear” still wins, as “apple” cannot complete the word stem), the memory trace for “apple” is weakened. As a result, subjects are less likely to remember the pair *fruit-apple* in a later recall test, or to complete the stem *fruit-a__* with “apple”; moreover, they may even tend to not remember the word “apple” in other pairs (Anderson et al., 1994). This effect is item specific (it does not affect “kiwi,” a fruit that was less likely to come to mind and compete with “pear”), it depends on competition for retrieval (just rehearsing the pair *fruit-pear* does not weaken “apple”), and its boundaries in terms of dependence on practice, memory strength, etc., have been extensively characterized (Anderson & Spellman, 1995). The behavioral phenomenon of retrieval-induced forgetting imposes useful constraints on the implementation of memory systems, suggesting specific forms for networks of memories and how and when they are updated. In particular, it has suggested that the function of oscillating inhibition seen in cortical semantic memory networks and hippocampal attractor networks of episodic memory may be to strengthen weak memories and punish competitors (Norman et al., 2006, 2007)—insight that would be much harder to glean from neural recordings absent the rich behavioral data and clever paradigm.

In my own lab, Nina Rouhani recently completed a series of behavioral studies that revealed more about the organizational structure of episodic memories than we could have hoped for even with invasive neuroscientific techniques. Rouhani asked participants to report reward predictions during learning, and subsequently tested their memory for trial-unique visual items (scenes) that had been presented throughout the task. Her results showed that surprising outcomes (either much more or much less reward than expected) increase memory accuracy for items that coincide with the surprising rewards (Rouhani et al., 2018). The findings were compatible with two different computational and mechanistic processes: Surprising events may be encoded more strongly, or they may be better differentiated from other events, making them easier to recall. By introducing sporadic large change points in reward outcome and testing for recognition priming (the phenomenon of faster recognition memory for an item when it is “primed” by an event that was linked to it in memory storage), Rouhani was further able to show that memory for a highly surprising event is separated from both the preceding and following items, even while serving as a connection node in the overall network of memories (Rouhani et al., 2020). These results inform our understanding of the neural organization of memories and potential effects of norepinephrine and dopamine on hippocampal encoding, and further suggest that the source of dopamine may not be the same midbrain dopamine that affects learning (Rouhani et al., 2018), all using behavioral paradigms and computational modeling.

Another example is research into the format and structure of working memory (Miller, 1994), in particular visual working memory (Brady et al., 2011). Here, too, progress on the key question of whether working memory takes the form of a limited number of

discrete slots or rather is limited due to a shared resource—questions that seem most reliably about the neuroscientific implementation of a computational component of perception—has relied on behavioral psychophysics research as well as computational modeling (Bays & Husain, 2008; Brady & Alvarez, 2015; Keshvari et al., 2013; van den Berg et al., 2012, 2014). Characteristically, neuroscientific evidence for hypotheses derived from computational modeling of behavioral findings played a confirmatory role, rather than revealing the phenomenon (Ma et al., 2014).

Finally, detailed hypotheses about how attention is deployed and controlled have also been inspired by, and tested in behavioral data. In particular, recent work suggests that even as we attend to an object or a location in space, our attention spotlight is not stationary—it moves around briefly, scanning the environment and returning back to the location of focus, at a frequency of 8 scans/s (Fiebelkorn et al., 2013). This “blinking of the attentional spotlight” may be related to other 8 Hz (theta rhythm) processes characteristic of environmental sampling. It also constrains the search for brain areas that are responsible for deployment of attention. In particular, further neural data from nonhuman primates implicated the lateral intraparietal cortex and frontal eye fields—two prominent parts of the classic frontoparietal attention network—in both maintaining attention and suppressing shifts away from its focus, and periodically shifting attention to rapidly scan the environment (Fiebelkorn et al., 2018). Important for our discussion, Fiebelkorn et al.’s (2013) findings that initially revealed the existence of this fascinating cognitive process did not involve any neural measurements. Moreover, those findings could hardly have been predicted even knowing that other environmental sampling processes operate at 8 Hz. Indeed, it seems that no amount of neural data on attention could have led us to speculate this pattern of behavior with confidence, whereas a single behavioral study was sufficient to establish it.

What Have We Learned From the Brain That Behavior Had Not Already Taught Us?

Studying the brain is undoubtedly useful for neuroscience, especially inasmuch as many questions in neuroscience revolve around how different computations are implemented in neural hardware. The question I am posing here is whether neuroscientific measurements are *sufficient* for making progress on understanding cognition, so much so that their import overwhelms that of behavioral findings, and studies that do not employ neuroscientific measurements should be categorized as “only” behavior and considered to not be relevant to understanding the brain. I also ask whether neuroscientific studies are *necessary* for answering some questions in cognition. That is, is it at all possible that a study can answer a question about the brain absent any neural measurements?

I pose these questions not as straw men. Although neuroscience and behavior need not be at two opposing ends of acceptable research—in fact, they are most effective when done in tandem, ideally also with theoretical modeling—unfortunately, in recent years, the rise of neuroscience has been accompanied by a disdain of behavioral research to the point of rejecting its usefulness altogether. Meetings (e.g., *Computational Systems Neuroscience*), journals (e.g., *The Journal of Neuroscience*—a society journal that supposedly represents the whole field), and, perhaps most importantly, funders (e.g., the *National Institute of Mental Health*) all share the implicit or explicit assumption that behavioral studies

cannot lead to valid neuroscientific findings, and therefore are irrelevant to understanding the brain. This leads to rejection of abstracts, articles, and grant proposals, sometimes without review, and often with the claim that they are not topically relevant³.

However, the list of true discoveries about perception and cognition from neural measurements or perturbations that were not already known from studies of behavior is embarrassingly short. Of course, beginning from the Hodgkin–Huxley model (Hodgkin & Huxley, 1952)—a fundamental breakthrough in understanding how neurons fire action potentials and communicate that is of course not attributable to any behavioral findings—and continuing with many investigations of anatomy, physiology, and recordings of single neurons while animals were performing different tasks, we have learned a lot about the workings of neurons in different areas of the brain. Still, the breakdown of different functions and cognitive processes into modules that can be expected to be realized independently in neural hardware, and that we should therefore be looking for in different brain areas, is predominantly thanks to behavioral studies of healthy individuals and those suffering from brain damage.

Arguably, the most interesting findings from neural recordings or perturbations derive from experiments in which these methods are coupled with incisive, hypothesis-driven behavioral task designs. It seems that neuroscience alone is not nearly as revealing as are a combination of a telling behavioral design, neural recordings or perturbation of neural function (e.g., through lesion, inactivation, or stimulation), and a computational model that states the hypotheses to be tested precisely. For this combination to reach its peak performance, work on developing and testing behavioral paradigms, as well as computational models, is as important as work on developing new neuroscience methods. My argument is therefore that pure behavioral research is not only critical for understanding the mind—but also it is the cornerstone of understanding how mental processes are implemented in the brain.

Assuming that one overarching goal of neuroscience is to understand how the brain processes information, it is relevant to ask: What have we learned about information processing from neuroscientific research alone? Unfortunately, even with the aid of my entire lab at the time I first conceived of this article (see the Author footnote), I could not come up with more than a very short list of such findings to highlight here. The finding that face perception is special, for instance, was driven by behavioral findings (Kanwisher & Yovel, 2006)—comparing perception of right-side-up and up-side-down faces (Diamond & Carey, 1986; Yin, 1969)—long before face cells were discovered in infrotemporal cortex (Quiroga et al., 2005). Of course, determining whether there are specialized modules for processing faces versus other objects is aided by neuroscientific data (Grill-Spector et al., 2004; Kanwisher et al., 1997; Yovel & Kanwisher, 2004); however, even those distinctions are often easier

³ As examples, a program officer at the National Institute for Mental Health (NIMH) requested that a colleague withdraw her funding application from consideration prior to review, saying that regardless of reviewers’ evaluation, the clearly mental-health relevant research will not be funded as it does not include a neural component; I have similarly been told, on consultation with several program officers, that a computational psychiatry center that focuses on behavioral measures is not of interest to NIMH unless we include neuroscience methods such as fMRI or MEG, despite research so far showing little return for such techniques in understanding mental illness (Roiser, 2015), not to mention in developing clinically feasible tools for diagnosis and treatment selection.

to make behaviorally rather than neurally (Leder & Carbon, 2006; Robbins & McKone, 2007). Another example, moving from visual perception to higher cognitive functions, is “theory of mind.” In this domain, the concept, its breadth, and its development were all successfully studied without looking into the brain (e.g., Wellman et al., 2001). Neural studies have revealed potential brain areas underlying theory of mind (Saxe et al., 2006; Saxe & Kanwisher, 2003; Young et al., 2010), but have not done much to explain the cognitive process of theory of mind as a series of representations and computations over these. Even domain specificity and dissociations between false beliefs and false photos were seen behaviorally through developmental work before their respective neural counterparts were pinpointed (as reviewed in Saxe et al., 2004).

Obviously, questions such as “what area of the brain is involved in process *X*” can only be asked at a neural level. However, my claim is that understanding what process *X* is—what computations it embodies—is rarely done at the neural level alone, if at that level at all. In a sense, the term “computational neuroscience” is a misnomer, as computational discoveries about the brain can hardly skip the psychological–behavioral level. Some exceptions can be found in the domain of perception. One can convincingly argue that measuring the receptive fields of neurons at different levels of the hierarchy of visual processing areas in passive, nonbehaving animals has helped explain how visual processing proceeds from building blocks to percepts (although we are still far from understanding even this basic process, and recently computational models, not neuroscientific data, seem to be providing most of the breakthroughs).

But is perception the “highest” cognitive function for which neuroscience without behavior can inform our understanding of cognitive processes? Although few and far between, there are some instances where neuroscientific research has led to insights about higher cognitive functions that would perhaps not be available otherwise. This has been mostly by way of lesion studies (or, in animals, reversible inactivations) that illuminated the separable components of cognitive processes once thought to be unitary. In memory research, for instance, the groundbreaking case of Henry Molaison (more commonly known as patient H.M.) first suggested a separation between episodic long-term memory and other forms of memory and learning, revolutionizing the study of memory and the hippocampus, and spurring the field of cognitive neuropsychology (Augustinack et al., 2014; Scoville & Milner, 1957). Notably, the findings relied on testing with appropriate behavioral tasks that demonstrated the different dissociations, for instance between forming new episodic memories and learning new skills (Milner, 2005). Similarly, although behavioral studies such as Packard and McGaugh’s (1996; discussed above) suggested that animals transition between decision strategies as they become more familiar with a task, lesion studies in rodents paired with theoretically sophisticated behavioral paradigms revealed that both strategies—goal-directed decision making that relies computationally on planning in a mental model of the world, and habitual responses that lean exclusively on past experience (Daw, 2018; Dickinson, 1985; Dickinson & Balleine, 2002; Drummond & Niv, 2020)—are learned in parallel and, in principle, available for use at any given time (Balleine, 2005; Killcross & Coutureau, 2003; Yin et al., 2004, 2005).

Other questions regarding apparent (or true) equivalence between categories of events still await resolution by way of neuroscientific measurements. For instance, is not getting an expected reward

equivalent to losing money? Behavior suggests that these two situations are similar, but whether they truly are equivalent is a question best answered at the implementational level. Another question of this same flavor is as follows: Do we have two antagonistic motivational systems, an appetitive one and an aversive one (Konorski, 1967), or rather is motivation controlled by one system with two poles? This class of questions can possibly only be answered at a neural level, albeit coupled with a suitable behavioral task. Indeed, these are prime examples for which a generic behavioral test (e.g., extinction learning or conditioned place preference) will not be nearly sufficient, and neural investigations must rely on a clever behavioral task specifically tailored to the question at hand (see below). Finally, questions about social behavior and its reliance on specially tailored versus general-purpose neural mechanisms are another class of studies that combine all of Marr’s (1982) levels. As Lockwood and colleagues convincingly argue, such questions about which of several algorithms is implemented in the brain can be best answered with neural measurements during properly controlled behavioral experiments tailored to the algorithms being contrasted (Lockwood et al., 2020).

I therefore want to make clear that I am not arguing for the futility of neuroscientific research. I am calling for a true merger between psychological cognitive science (ultimately interested in understanding behavior) and neuroscience, or at least cognitive neuroscience (ultimately interested in explaining the brain). The study of behavior cannot afford to ignore such an important source of information as the brain—why would we be interested in measurements such as response times and eye movements but *not* be interested in accompanying neural measurements? And similarly, the study of the brain must rely on understanding of behavior if it has any chance of making rapid progress.

Clever Behavioral Experiments Allow Causal Conclusions Despite Correlative Measures

One last point I would like to make, while on this soapbox, regards a related strong bias in neuroscience: the preference for causal manipulations rather than correlational measurements. This predisposition suggests that because an fMRI signal is correlational, it is, by design, inferior to a technique that manipulates the neural hardware, for instance, optogenetically. This bias similarly renders behavioral measures in humans absent a brain manipulation (for instance, using transcranial magnetic stimulation (TMS), or due to a lesion) automatically inferior. However, I believe that clever behavioral designs have two advantages: They allow behavioral studies to sidestep the causation/correlation pitfall and they help us utilize resources judiciously.

We can start by taking a cue from how the brain goes about making sense of the world: indeed, we often infer causal constructs (e.g., in perception: “What I am looking at is a table surrounded by four chairs”) from noisy, low-dimensional, correlational measurements (e.g., the two-dimensional images that fall on my retina) together with prior knowledge about the generative process (i.e., how we expect different objects to manifest in such measurements). So, inferring causality from convergent correlational measurements is not an ultimate sin.

The brain also utilizes perturbations—we can move our head to get a different image on our retina if an obscured object seems particularly ambiguous. Or we can walk over to the object in

question and move it to verify that all the parts that we thought were connected really do belong to the object. Inferences are therefore reliant on correlational methods very commonly, and on costly perturbation methods in extreme cases. We can similarly construe scientific inquiry: In trying to infer the causal structures of the world around us (Why does application of a painful stimulus generate long-lasting fear responses? Why are humans so prone to assuming, even on first glance, that some people are more trustworthy than others?), it would be wise to combine both correlational measurements and causal perturbations.

The next step is to realize that causal manipulations in neuroscience are not limited to silencing or activating a set of neurons, or lesioning a part of the brain. A behavioral task that requires a cognitive process can effectively apply a causal manipulation, turning a neural process (and its underlying hardware) “on” and “off” through changes in task demands. As an example, imagine the *N*-back task in which a participant views a series of letter stimuli and has to respond whenever the current letter is identical to the one viewed *N* trials back (with *N* being set in advance, for instance, to 2). This task requires constant maintenance of working memory, introducing and removing stimuli from the “recent *N* items” set, and comparing the current item to the contents of working memory. Changes in task demands could turn working memory mechanisms on or off, depending on the specifics: setting *N* to 1 would place minimal requirements on updating of working memory as the task of identifying repetitions can be solved even at the level of iconic visual working memory; or the task can be changed to “respond to up-side-down letters,” which does not require working memory at all. Of course, task performance requires more than one cognitive process (e.g., in the *N*-back task: reading, memorizing, comparing the current stimulus to the content of memory, deciding on a response), so a clever, well-controlled experimental design is necessary to single out one function and not others. But, this is exactly what the rich legacy of experiments in psychology has taught us to do well. The superpower of psychologists is their ability to design behavioral experiments that isolate and manipulate a process non-invasively, within a whole behaving organism.

As illustrated by the examples in prior sections of this article, this means that using behavior alone, we can investigate even the neural implementation of working memory. For instance, by assessing the capacity of working memory for colors, orientations, or their conjunction, Luck and Vogel (1997) showed that visual working memory was stored at the level of whole objects (i.e., in higher order visual perception areas) and not at the level of individual features. More recently, Katus and Eimer (2018) used the quintessential “causal” behavioral manipulation of dual-task requirements: They presented participants with visual and tactile stimuli, separately varying the number of items in each modality, and testing for working memory of only one modality in each trial. The two tasks effectively activated the neural mechanisms responsible for visual and tactile working memory, allowing the researchers to assess whether these mechanisms are shared or separate. The behavioral results—no reduction in working memory accuracy for one modality with the increased demands on the other modality—suggest that independent memory storage exists for each modality, and that capacity constraints on working memory do not result from a shared higher-level control process (Cowan, 2010).

Thus, a relatively simple experiment can answer a question about the segregation of different types of information in working

memory, without ever measuring neural signals. The equivalent neural perturbation experiment is dauntingly difficult and less incisive: One would presumably perturb the activity of a brain area (e.g., by TMS) and look for effects on working-memory performance. However, finding that the perturbation does not affect task performance would not mean the brain area is not involved in the task (there could be redundancy in the system), and finding that performance is decreased would not specifically imply deterioration of working memory (here, again, one has to design careful control conditions to rule out other cognitive processes that are involved in the task). Finally, it is not clear what perturbation or neural measurement would tell us conclusively whether tactile and visual working memory rely on shared or separate neural substrates. This is therefore another example where a purely behavioral experiment can answer a neural implementation question more readily than neuroscientific measurements and perturbations can.

To be sure, here again I am not suggesting that neuroscientific measurements are irrelevant, or that there is no conceivable neuroscientific experiment that would answer the above question about working memory. What I am arguing is that the opposite is not true: It is not the case that pure behavioral experiments, using clever experimental designs, and behavioral output such as choices and reaction times, cannot possibly answer a question about neural mechanism. To the contrary, the latter may be better suited to answering that question with little time and effort. Considering behavioral measurements as inferior a priori is detrimental to neuroscientific progress.

So Why Do We Chronically Devalue Behavioral Work?

I have illustrated above the stark asymmetry between behavioral research and neuroscientific investigation in understanding cognition: Behavioral work contributes more to our understanding of the brain than neuroscience has contributed to understanding the mind. Interestingly, this is similar to the immense unidirectional contributions of the fields of machine learning and computational modeling to neuroscience, all while the former fields have learned relatively little from the brain. Perplexingly, in both cases funding decisions—which are essentially prioritization exercises—suggest the exact opposite.

This reversed perception of what field contributes meaningfully to another field may be fueled by several misconceptions. One is a prevalent illusion that neuroscientific data are in some sense “objective,” whereas using a (computational) theory to interpret (behavioral) data is more “subjective” and less scientific. Another, not unrelated widespread misconception is that behavior is “solved” or not interesting, whereas the “real” questions are ones in neuroscience. But, we are still very far from explaining behavior and closing shop in all departments of psychological science (Rescorla, 1988). A question remains, then, what is the best way to make progress in understanding behavior? Although behavior is generated by the brain, I have argued that it is not the case that understanding the brain is the best way to understand behavior. Behavior and neural data are informative about different things: If you are interested in understanding the provenance of behavior (as are many of us in psychology, cognitive science, and neuroscience), you should study behavior.

This observation suggests that we need a prioritization of *questions*, not of *techniques*. Once questions of interest are clearly defined, for instance “what is a reliable diagnostic phenotype of bipolar disorder” (to take a translational example that seems most readily yielding to neural biomarkers), we can consider what are the best techniques to answer the question expeditiously.

Which brings me to the final illusion: that because behavioral work is sometimes easier (and very often cheaper), its findings are worth less than findings from neuroscientific research. This is a common illusion in economics—people (and animals) value an expensive good more than they do a cheap one (even when the goods are identical) and prefer a reward that they have earned through more effort to one achieved more easily (Aronson & Mills, 1959; Clement et al., 2000; Plassmann et al., 2008). But, this preference is irrational. Sunk costs do not actually make something more valuable, only more . . . expensive. Do we not, therefore, have a moral obligation to do the behavioral experiments—those that may lead to faster answers at a lower cost—first? Concretely, if a series of behavioral tasks, coupled with computational modeling of the cognitive processes underlying behavior on these tasks, can quantify the aspects of bipolar disorder that fall outside the norm defined by healthy patients, should we not pursue this route before subjecting patients to expensive neuroimaging scans, not to mention invasive techniques?

Conclusion

In basic cognitive psychology research, the role of neuroscience has been suggested to be that of “constraining theories of representation and computation” (Cushman, 2020). It is wholly unclear, however, whether the most effective constraints come from neuroscientific data or from behavioral data. The brain is complex, and it is forgiving—it represents many quantities that may be ancillary to a specific function, and can solve a specific problem through several, often redundant mechanisms. As a result, neural data are often only weakly constraining, if at all. It may be our limitation as researchers (working with the cognitive abilities that our neurons afford us), but historically we have distilled more insight into cognitive processes from contrasting behavior in well-crafted experiments than we have from measuring or perturbing the brain. If we are to accelerate the progress of understanding the human mind, we therefore should restore behavior to its rightful place as the firm base of neuroscience (Griffiths, 2015) on which other findings and types of data can rely, but without which one cannot build a thesis.

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