Searching for the Source of Executive Attention

Abstract

William James presaged, and Alan Allport voiced criticisms of cause theories of executive attention for involving a homunculus who directs attention. I review discussions of this problem, and argue that existing philosophical denials of the problem depend on equivocations between different senses of "Cartesian error". Another sort of denial tries to get around the problem by offering empirical evidence that such an executive attention director exists in prefrontal cortex. I argue that the evidence does not warrant the conclusion that an executive director can be localized in prefrontal cortex unless dubious assumptions are made, and that computational models purporting to support these assumptions either beg the question, or fail to model executive attention in terms of cause theories.

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A 2005 article in *Science* identified the biological basis of consciousness as one of the 25 most important questions facing science over the next 25 years (Kennedy & Norman, 2005, p. 75). This article claims that while consciousness has for centuries been "the exclusive purview of philosophers," a shift towards making the investigation of consciousness a scientific activity has begun (Miller, 2005, p. 79). Although consciousness proper may only recently have become a respectable research area within psychology and neuroscience, there has for many years been an abundance of research directly relevant to the field. As Alan Allport pointed out, the word "attention" has often been used by psychologists as a "code name for consciousness" (1980, p. 113).

The exact nature of the relationship between attention and consciousness is a matter of some debate (see Noë and O'Reagan, 2000; Hardcastle, 2003; Koch and Tsuchiya, 2007), but there is wide agreement that some close relationship obtains. Given that i) consciousness is a topic of great philosophical interest, ii) attention has been a very active topic of research in psychology for at least the last 30 years, and iii) a close relationship is thought to exist between consciousness and attention, it is strange that attention has received so little notice from philosophers of science (some exceptions are Mole (2005), as well as cursory mentions in Dennett (2005)). Since attention has been so well studied, and carries along with it somewhat less in the way of intuitive baggage than does consciousness, attention strikes me as

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a convenient starting point from which to approach broader questions in the study of consciousness.

Here I examine one branch of the vast attention literature: that dealing with executive attention. Within this literature, a pattern of inquiry has arisen similar to that now popular in the scientific study of consciousness. Philosophical debates are being set aside, and empirical work is being appealed to in order to settle conceptual questions. Furthermore, bold conclusions are being hinted at, or at least projected for such a time as imaging technologies have improved a little more, suggesting that an important aspect of mind will then have been successfully reduced to a particular piece of brain.

I begin by giving some background on types of attention, and their associated experimental practices. Next, I review philosophical arguments against conceptualizing executive attention as a causal agent, and assess the defenses that have been raised against these arguments. I then turn to some recent empirically-driven arguments from neuroimaging data and computational modeling that attempt to get around these conceptual difficulties. I argue that this attempt fails, since the question at stake – whether a brain region can pay attention – is not an empirical question, and furthermore, the empirically-driven arguments either suffer from logical errors, or in fact lend inductive support to there being a conceptual problem.

Types of Attention

It is common in reviews of attention research (Fernandez-Duque & Johnson, 2002, 153; Gazzaniga, Ivry, & Mangun, 2002, p. 247; Posner, 1994, p. 7398; Posner, 2004, p. 3; Tsotsos, Itti, & Rees, 2005, p. xxiv; among others) to begin by quoting William James's definition, "Every one knows what attention is. It is the taking possession by the mind, in clear and vivid form, of one out of what seem several simultaneously possible objects or trains of thought" (1890, pp. 403-404). Even though everyone knows what it is, in their review of the attention literature, Johnston and Dark complain of a "widespread reluctance to define attention" (1986, pp. 43). Part of the problem, as Allport points out, is that the terms *attention* and *selection* are used in confusing and ambiguous ways, without any consensus on what each refers to (1993, pp. 186). It seems clear that the field would benefit from a terminological cleanup, and perhaps some operational definitions, but I won't undertake this work here. I will, however, lay down some of the basic definitions found in the literature.

In the early years of attention research, the main distinction was between *early* and *late* attention. Attention was modeled as a selection filter, and the main research question was whether the selection filter operates before or after cognitive processing. Broadbent (1958) pioneered research in this field with his dichotic listening experiments. Kahneman and Triesman (1984) provide an overview of the early literature. Results like that of MacKay (1973) posed problems for filter theories, by showing that even unconscious stimuli can be processed to a high degree.

Another troublesome distinction is between *bottom-up* and *top-down* attention. Bottom-up attention is thought of as arising directly from outside stimuli such as flashing lights, sudden noises, or any stimulus that has either instinctive or learned importance, such as the sight of a predator, or the sound of your name. These stimuli evoke stronger neural responses than less 'salient' stimuli regardless of task, environment, or training. Interestingly, it is not enough for a stimulus to be right in front of your eyes for you to attend to it, as demonstrated by Rees, Russell, Frith, and Driver's (1999) "inattentional blindness" and Rensink, O'Reagan, and Clark's (1997) "change blindness" examples. Top-down attention is thought of as arising from internal stimuli such as memories and thoughts. Other names for similar distinctions are *automatic* vs. *controlled*, *stimulus-driven* vs. *goal-directed*, *passive* vs. *active*, and *exogenous* vs. *endogenous*. Unfortunately bottom-up and top-down have both literal and metaphorical meanings, and could either imply direction of processing, or level of automaticity, although the two meanings do not always match. There are automatic top-down as well as controlled bottom-up attentional effects (i.e. Duncan, 1984; and Egly, Driver, & Rafal, 1994). Another difficulty is that attentional effects can move backwards, in feedback loops, or as Dennett (2005, p. 133) says, "sideways", for example in the interactions between cells in Desimone's biased-competition model (see Chelazzi, Duncan, Miller, & Desimone, 1998).

Selective and Executive Attention

The main distinction used in recent literature is between *selective* attention, *executive* attention*,* and *vigilance*. I won't discuss vigilance here. A few definitions of selective attention from the literature are: "the differential processing of simultaneous sources of information" (Johnston & Dark ,1986, p. 44); "a system of cognitive processes that manages the burden of having too much to do at once by prioritizing among stimuli to be processed" (Carr, 2004, p. 56); and "the generic term for those mechanisms which lead our experience to be dominated by one thing rather than another" (Driver, 2001, p. 53). One point of agreement is that selective attention is a broad concept covering many distinct mechanisms operating in a variety of brain systems, if not the entire brain (Driver, 2001; Triesman, 1969/2003; Allport, 1993; Parasuraman, 1998; Mole, in press). Nevertheless some maintain the hope that selective attention might be drawn together under one specific function or mechanism (some suggestions are an "attentional organ system" (Posner, 2004), a "triangular circuit" (LaBerge, 2004), and the synchronization of activity between prefrontal, parietal and mediotemporal cortex (McKnight, 2007)). The custom in recent work on selective attention seems to be to remain agnostic as to what general function selective attention performs, as none of the popular models (bottleneck, spotlight, limited resource, biased competition) seem to fit every case included under the umbrella of selective attention.

Executive attention is even more loosely defined. Posner and Rothbart define it as "the regulation of thought, emotion and behaviour" (1998, p. 1915). Norman and Shallice include planning, conflict resolution, decision-making, error correction, and overcoming habitual responses to perform novel or difficult tasks as executive processes (1986, pp. 2-3). Much of the work on executive attention investigates patients with prefrontal lesions. These patients show impaired performance on various so-called executive tasks, such as the Wisconsin Card Sort Test, the Stroop task, and verbal fluency. Although it is agreed that prefrontal patients lack executive functions, and that the tasks used for diagnosis involve executive function, nobody seems quite sure how to isolate the executive element of a given task such that it can be studied independently (although work is being done).

The consensus seems to be that executive attention involves *centralized supervisory control*, and that in order to supervise and control, it is necessary to have information about, and some causal influence over the things supervised and controlled. According to Posner, two important functions of an executive are to be "informed about the processes taking place within the organization" and to "exercise some control over the system" (1994, p. 7400). It is not quite clear what sort of control is involved, and in what sense these control processes need to be central.

Effects and Causes

Allport points out that a "subtle equivocation” exists between attention conceptualized as a resource, which can be 'directed', and attention as a controller, that directs or allocates that resource (1993, p. 186). These are sometimes distinguished as *effect* theories and *cause* theories. The following quote illustrates both, "When attention is directed to one of the stimuli, this causes an attentional feedback signal to be directed to the neuronal population activated by the attended stimulus" (Reynolds & Desimone, 2000, p. 234). To give an admittedly ungenerous paraphrase, they claim that attention to a stimulus causes attention to the attended stimulus. The first instance of "attention" might be described by a cause theory, in which attention causes neuronal effects whereby some stimuli receive more processing than others. The second instance of "attention" might be described by an effect theory, in which attention *is* that neuronal effect of differential processing. An even more ungenerous paraphrase might say that the mental act of attention causes the neural event of attention.

Historically, bottom-up or automatic varieties of attention have been thought of as effects, while top-down, controlled, or voluntary varieties of attention have been thought of as causes. William James admitted that "*immediate sensorial attention*" and "*Derived attention, where there is no voluntary effort*" both are mere effects (1890, pp. 448-449), but he saw the "*effort to attend*" as an "*original force*" (p. 453). In 1990, Allport claimed that, "Practically all current theories of attention are cause theories" (1993, p. 186). Since then, there has been a shift towards effect theories, especially in accounts of selective attention. Fernandez-Duque and Johnson (2002) classify theories based on the older spotlight and limited resource metaphors as cause theories, since they involve a controller who moves the spotlight or decides which stimuli are salient, and theories based on the newer competition metaphor as effect theories.

Although selective attention is now usually described in terms of effects, executive attention is often still conceptualized as a causal agent. Posner refers to the supervisory system as having access to the “overall representation of the environment and the goals of the person" (Posner & DiGirolamo, 1998, p. 402). Elsewhere he discusses "the correct theory of attentional control" as involving finding "the source of attention" (Posner, 1994, p. 7402). It is not clear how a brain mechanism could have access to a person's goals, and serve as the source of attentional effects based on these goals without being imbued with intentional properties. Even in cases where the terminological shift away from causes is complete, it is not always clear that a reconceptualization has occurred; the burden of the cause theory is sometimes shifted to related concepts like consciousness or working memory. For example, according to Posner, Edelman "distinguishes what he calls attention (which is the increase in neural activity within brain areas currently of the organism's concern) from consciousness, which is the source of these activations" (Posner, 1994, p. 7401).

Within executive attention research, the main alternative to the central executive view exemplified by Posner is the conflict monitoring view of Botvinick, Cohen, and colleagues. There is a growing literature on particular sub-mechanisms of executive attention such as task switching, and cognitive control. For example, the task switching experiments of Monsell (2003) and Rubinstein et al. (2001) ask subjects to switch between several tasks during a trial, and look at the behavioral effects such as switch costs in reaction time, and the biological correlates of task switching, such as changes in the pattern of EEG or fMRI activations. As Botvinick, Braver, Barch, Carter, and Cohen (2001) note, most of this work focuses on “the nature of the influence exerted by control” while “very little is yet known about how the intervention of control processes is itself brought about” (p. 624). For example, Serences, Liu, and Yantis identify the BOLD signal as an attentional control signal, and investigate the effects it modulates, but they have little to say about “the ultimate origin of attentional control signals”, except to speculate that it is probably to be found somewhere in prefrontal cortex (2004, p. 40).

Both these lines of research (not to mention the rest of cognitive psychology) typically help themselves to the assumption that there is an ultimate origin of executive control to be found somewhere in the brain, probably in prefrontal cortex. That there might be something fishy about this assumption is, at the same time, often acknowledged. Botvinick et al. claim that without an account of how control is triggered, “control remains a sort of homunculus that ‘just knows’ when to intercede” (2001, p. 624). This homunculus is frequently treated as something that can and will be demystified with a little bit more work, and improved fMRI technology. In the next sections I argue that the problem is not just a matter of digging deeper into the brain to find the ultimate source of control, but that this conceptualization of executive attention as a causal agent is wrong-headed. An ultimate source of executive control is not the sort of thing that one could localize to any particular brain region.

Philosophical Arguments

In this section I discuss some of the conceptual problems at play in executive attention research. First I review several arguments that have been made against cause theories of executive attention. Next I clarify what the homunculus problem is, and discuss the related issue of centrality. Finally I examine some of the counter-arguments that have been offered against the charge that cause theories of attention involve a homunculus, and conclude that these depend on logical errors and equivocations.

Arguments Against Cause Theories

James recognized in 1890 that cause theories of attention raise philosophical problems. Although he subscribed to the view that some varieties of attention are causes, he insisted that the question of whether attention can be a cause could not be settled empirically. According to James, cause theories of attention must be dualist theories, and he was drawn to them for religious rather than empirical reasons: "As mere *conceptions*, the effect-theory and the cause-theory of attention are equally clear; and whoever affirms either conception to be true must do so on metaphysical or universal rather than on scientific or particular grounds" (1890, p. 448).

Few contemporary researchers are willing to endorse dualist theories, so it is not surprising to find a number of them expressing unease about cause theories. In 1980 Allport gave an influential critique of then current theories of attention. In this paper he argues against theories in which attention or consciousness appear as an essential information process, but in which the process so labeled is left unspecified. Such theories, he says, “can not seriously be distinguished from *homunculus* theories: theories of a little man inside the head” (Allport 1980, p. 113). Allport threw down the gauntlet in his 1990 *Attention and Performance XIV* address. In this critical review of 25 years of attention research, he declares that, "qua causal mechanism, *there can be no such thing as attention*" (1993, p. 203). He continues, “no comprehensive causal mechanism of attention has ever been specified, even in barest outline. Reference to attention (or to the central executive, or even to the anterior attention system) as an unspecified causal mechanism explains nothing” (1993, p. 204).

Johnston and Dark argue against cause theories of attention on the grounds that all of the important empirical results in attention research can be accounted for with effect theories, and add that viewing attention in terms of effects "has the important advantage of avoiding the homunculus problem" (1986, p. 69). One place they see this problem is in Schneider et al. (1984), where the automaticity with which subjects detect targets is explained by subjects being equipped with an "attention director" (1986, p. 68). Johnston and Dark remark that, "the same questions that were asked about how individuals pay attention now have to be asked about how the attention director pays attention" (1986, p. 68).

Dennett makes similar points about consciousness and meaning in the course of criticizing the “Cartesian Theatre” of the mind in his 1991 book *Consciousness Explained*. He later applies this to attention too. In the course of explaining his "fame in the brain" metaphor of consciousness, Dennett says, "There is no literal searchlight of attention, so we need to explain away this seductive metaphor by explaining the functional powers of attention-*grabbing* without presupposing a single attention-*giving* source" (2005, p. 138).

In the introduction to *Control of Cognitive Processes: Attention and Performance XVIII*, Monsell and Driver remark that despite 20 years of these criticisms, the control homunculus, the “default solution” to problems in executive attention, “has continued to parade about in broad daylight” (2000, p. 3-4). They point out that most work has been concerned with “*what* is controlled… rather than *how* that control is exercised.” They further note that the assumption is generally made that “if control is exercised, then there must be a controller” (Monsell & Driver, 2000, p. 6). They remain optimistic that a strategy of deconstruction and fractionation of control homunculi into simpler mechanisms, inspired by Dennett's pandemonium of demons picture (1991, p. 237), will prove successful, and put forward the volume's contents as illustrative of this approach.

Many attention researchers simply dismiss the homunculus problem out of hand. It has neither been elaborated in much detail, nor defended against with much conviction. I will try to remedy this in the next sections by fleshing out both what the worry consists in, and how arguments defending attention against the worry might be intended to work.

The Homunculus Problem

The homunculus problem is most commonly associated with the problem of how perception is possible. The naive explanation of visual perception begins by suggesting that another version of what is visually experienced is played internally for the 'mind's eye' to behold. This implies that there must be something like a little man (or homunculus) sitting inside watching a little movie being played in the mind. The problem with this explanation of perception is that exactly the same demands for explanation could be asked of this little man's perceptions as of yours, so this explanation does not do any work (see Pylyshyn, 1973). The homunculus problem could more generally be attached to any explanation of how a mechanism achieves a given ability that involves the assumption that some part of the mechanism has the ability. Dennett's pandemonium model gives each homunculus a simpler job than the whole, so does not commit this error, but neither is it meant as a denial that most explanations involving homunculi are invalid.

This becomes a problem for theories of attention when top-down, or executive attention is thought of as a cause rather than an effect. As the following example illustrates, one folk notion of attention is that it is something that you can direct and turn on or off at will. If an arithmetic teacher tells his or her students to "pay attention", the students try to focus something like their effort or thoughts on the topic at hand. This is not a case of a flashing light grabbing attention, or even of a learned idea or disposition affecting processing through feedback networks or biasing competition in a population of neurons. This is instead a case where an agent wills that more attention be allocated to their sums than to their passing fancies. In this case, it is natural to say that the student paid attention, and that this caused certain brain processes suitable for doing sums to kick in at the expense of brain processes suitable for daydreaming. The sticky question is not to figure out what happened in the student's brain in terms of processing of sums beginning and processing of daydreams ending, but to figure out what the student did in order to achieve this effect. The tempting answer is that some bit of their brain made the appropriate changes happen by 'directing the attention'. Directing attention, however, is just the psychological phenomenon to be explained. If we explain the student directing his or her attention by virtue of some part of his or her brain directing its attention, we haven't explained much.

This problem is related to, but distinct from, the question of whether there is a central location or process in the brain that performs the functions associated with executive attention.

Centrality

The question of whether there is some centralized attentional system in the brain is treated as an important project in studies of attention. Nobody claims that the attention system must be spatially central. Instead it is often claimed that this system is the source of attentional control, or an essential node in the hierarchy of decision-making. These notions of centrality could be characterized as having Cartesian, and Broadbentian roots, respectively.

These discussions of centrality often take as their aim either pointing to, or defending against so-called Cartesian errors. Theories of attention raise worries about the mind-body problem, which is associated historically with Descartes's suggestion in *Passions de l'Ame* that the pineal gland might be the place through which mind and body communicate (I, 33, pp. 352-3). The doctrine of the pineal gland is seen by psychologists as the paradigm mistake to be avoided in contemporary theories of mind, although the exact nature of the mistake is not always made clear, and it may not always (or ever) be an error Descartes committed (see Damasio, 1994, pp. 148-149). Two of the many possible interpretations of what it means to commit a Cartesian error are: claiming that a mental process is controlled by a central point, or claiming to have explained something mysterious by pointing to a small part like the pineal gland, or a black box in a cognitive psychologist's diagram, and saying "the magic is in there". These are independent. The first variety – whether a mental process is centrally controlled, rather than distributed – is an empirical question that may be true of some brain areas and false of others. The second variety, however, is the homunculus problem.

Christopher Mole provides a discussion of how the early work on attention by Broadbent (1958) shaped the field of attention research and still wields an influence even though researchers now largely agree that Broadbent's assumptions were flawed. Mole first outlines Broadbent's assumptions: "(1) Attention was seen as the allocation of limited capacity processing resources; (2) the cognitive architecture of perception was seen as largely linear... and (3) conscious control was seen as being intimately related to the paying of attention" (Mole, in press, p. 4). Further, Mole diagnoses a widespread disagreement in the psychological literature as to which of Broadbent's assumptions to relax, and how exactly to do so (10). Broadbent's second assumption, linearity, is the one I will focus on. Linearity is directly related to the idea of a centralized attention system, since architectures with multiple branching paths or loops need not have any central point through which all processing must pass.

As Mole points out, Broadbent's linearity assumption implies that there is a single locus of selection (the uniqueness assumption). One important notion of centrality is this idea of there being a single locus in the brain where selection occurs, thus separating pre-attentional from attentional processing. This is an assumption that most psychologists now reject, although there are a number of alternatives to this notion of centrality. Top-down attention as feedback loops would violate linearity, but not necessarily uniqueness. In contrast, Desimone's biased-competition model is an example where selection is both widely distributed and non-linear (Chelazzi et al., 1998). Mole points out that there could be another form of uniqueness, where for each instance of attention, there exists some point where the selection happens (Mole, in press, p. 13). This opens yet another alternative: keeping the idea of a single bottleneck, but denying that it has a stable location. Lavie (1995) defends such a "moving-bottleneck view of attention" (Mole, in press, p. 14). These differing views on linearity and uniqueness relate to the first variety of Cartesian error: the error of centralized control.

Although it is an empirical question, whether attentional systems are distributed or centralized is often treated as a critical question on which the coherence of the field hangs. It seems an equivocation is at work wherein empirical evidence relating to the centrality of attention systems is offered as a defense against the homunculus problem. In the next section, I review two arguments that set themselves up as being denials of the homunculus problem, but seem to defend against something else instead.

Defenses Against the Homunculus Problem

Because so-called Cartesian errors are not always clearly specified, it becomes possible to defend oneself against one variety of Cartesian error while inadvertently committing another. Baars, for example, does just this in a defense against Dennett's Cartesian Theatre argument. First Baars denies that searchlight theories of attention posit a "single-point center," with the implication that no Cartesian error is committed (Baars, 1998, p. 59). In the next breath Baars cites neuroscientific evidence suggesting that there is "strong convergence" of conscious information in various brain areas, with the (somewhat contradictory) implication that the supposed Cartesian error is no error after all, since the empirical evidence bears it out (Baars, 1998, p. 59). Either the denial that a central point is posited in searchlight theories of attention, or empirical evidence suggesting that a central point in fact exists would be convincing evidence against the charge of having committed a Cartesian error, if that error was the first variety mentioned above: claiming that a mental process is controlled by a central point. Dennett's Cartesian Theater argument, however, is meant to illustrate the homunculus problem (see Dennett, 1991, p. 229), and not centralized control, so Baars's defense is off-target.

In the introduction to *The Attentive Brain*, Parasuraman mentions Posner's causal theory of attention, and the commitment of others, including Desimone and Duncan, and Johnston and Dark, to effect theories (1998, p. 12). The reason he suggests for their skepticism about cause theories is "this feeling that postulating a specialized attentional control area in the brain creates a homunculus in the brain" (1998, p. 12). Parasuraman then brushes this worry aside, saying, "However, as discussed earlier in this chapter, the notion of attentional control does not necessarily require a homunculus" (1998, p. 12). Looking back to his earlier discussion, it is not clear that he has successfully defended cause theories against the homunculus problem. There is only one passage in the chapter that discusses the homunculus problem. Below I quote that passage, and provide its context within the text.

Parasuraman admits that the notion of attention as control has been criticized for two reasons, first because it embodies too many diverse functions, and second because it "raises the specter of a homunculus". Immediately following his identification of these problems, he writes the following:

> Yet no one would deny the importance of control processes in the development of skill and in the maintenance of efficient task performance. Furthermore, one aspect of attentional control provides possibly the only support for the argument that attention involves a special function that is quite distinct from other functions such as perception and memory. (1998, pp. 7-8)

He goes on to give examples of other brain mechanisms that interact with attention to illustrate how the "separate status of attention is put in jeopardy", but does not mention the homunculus problem again until near the end of the chapter where, as quoted above, he claims that attentional control does not require a homunculus.

This is a curious argument. Parasuraman makes two points: that clearly executive control is necessary for a number of our psychological capacities, such as skill development; and that the notion of attention as control is the only thing that might save attention from lacking a special function. The first point may be true, but it does little to support his argument. Control may indeed be important for skill development, and it seems clear that we have this psychological capacity to control our attention. This only shows that there is a phenomenon here in need of explanation. Pointing out the need for an explanation is not the same as providing one, however, and does not justify the use of any old explanation, no matter how problematic. The almost unlimited explanatory utility of homunculi is exactly why they are so tempting, so Parasuraman's first point simply affirms that homuncular explanations would be convenient, if only they worked.

The second point seems like wishful thinking. He says that the control variety of attention may be our only defense against the criticism of attention lacking a special function separate from other cognitive functions. The implication is that if control did bring along with it a homunculus, thus disqualifying control as a viable cognitive function, we would have to admit that attention is not a coherent field of study (a worry Parasuraman mentions on p. 4). Parasuraman understandably does not want it to come to this, so he rejects the premise that control involves a homunculus in order to avoid the unpalatable conclusion. Parasuraman makes it clear why we might *want* this premise to be false, but he provides no argument for why we should *believe* that it is false. What is more curious is that this plea for the preservation of attention's uniqueness is found in a book claiming to champion the idea of a diverse concept of attention.

The strangeness of this argument seems to stem from a misconstrual of the homunculus problem's target. Parasuraman's argument makes much more sense as a defense of the existence of executive control in our repertoire of psychological abilities; however, the purpose of raising the homunculus problem is not to demonstrate that executive control does not exist. Instead it is to demonstrate that although executive control appears to exist at the level of folk psychology, it may be a mistake to try to attribute this ability to a part of the brain. Perhaps Parasuraman is implicitly committing the mereological fallacy. If he assumes that properties of the whole must be properties of some part, he might conclude that if there is no part capable of attention, the whole cannot be capable of attention.

Both Baars and Parasuraman take themselves to be defending cause theories of attention against the homunculus problem, but both instead offer arguments against other problems. The worry that cause theories of attention are misconceived remains intact from the philosophical perspective. In the next section I address the empirical evidence that has been amassed suggesting that an area of frontal cortex contains the executive control system invoked by cause theories of executive attention.

Empirical Arguments

Recent papers in the literature try to sidestep the philosophical problems elaborated above by offering empirical evidence. One way in which empirical evidence is deployed against criticisms of the central executive view is to deny that the executive system is centralized. Studies examining whether split-brain patients can perform two attentional tasks at a time are one example (see Handy & Gazzaniga, 2005). Alvarez and Cavanagh offer "evidence for independence in the capacity to attentively track targets in the left and right visual hemifields" (2004, p. 637) even for normal observers. Crick offers anatomical evidence from the connections between the reticular complex and thalamus to suggest that there "may be several separate searchlights" of attention (2003, p. 267). This sort of empirical evidence does little to dispel worries about homunculi. Suggesting that there are two or more searchlights of attention does not help to explain how these searchlights are directed. Having two little men in the brain leaves just as much unexplained as just having one. Fully distributed models of attention, where no part is supposed to possess the function of directing or controlling attention do not fall prey to this error, but these are not cause theories. In the final section, I will discuss the possibilities available for doing without cause theories.

A second strategy is to offer empirical evidence suggesting that a central attention system, as described by a cause theory, in fact exists, which constitutes a denial of the philosophical result from the previous section. Posner writes:

> Dennett provided a strong philosophical critique of those who implicitly cling to a view that there is an arena of consciousness or what he calls the Cartesian Theater of the Mind. Nonetheless, the [empirical evidence] above seemed to identify cingulate activation with aspects of awareness in so much tighter a way than previous efforts that it seems reasonable to set them down with as much clarity as possible. (1994, p. 7401)

Parasuraman notes that identifying the "causal source(s) of attentional effects, which would presumably implement the component of attentional control" has proven elusive, but claims that neuroscience can provide evidence for causes and not just for effects (1998, p. 12). Whether empirical evidence can in principle overturn philosophical results is a controversial question, but I am willing to entertain the possibility.

The first step in this strategy is to gather imaging evidence suggesting that parts of prefrontal cortex (PFC) are active during executive tasks, and that these parts are missing in patients with executive dysfunction. The next step is to argue that a controller must have certain anatomical properties, which the PFC can be shown to have. From this it is concluded that the PFC must contain the executive controller. Further evidence from computational models is then put forward to show that the mechanisms believed to exist in PFC are capable of producing the output expected of an executive control system. In the following sections I proceed through these steps examining the arguments, and conclude that the empirical evidence does not, and further evidence of the same kind cannot settle the issue of whether causal theories of attention are correct.

Evidence for PFC Function

A large number of neuroimaging studies using PET, and fMRI, have been performed suggesting that anterior cingulate cortex (ACC) and a few related structures are the main areas involved in executive attention (Kastner & Ungerleider, 2000; Schneider & Chein, 2003; and many others). Kastner and Ungerleider note that "both studies of patients suffering from attentional deficits due to brain damage and functional imaging studies of healthy subjects performing attention tasks have given insights into a distributed network of higher-order areas in frontal and parietal cortex that appear to be involved in the generation and control of attentional top-down feedback signals" (2000, p. 327).

A common response to this evidence from researchers still suspicious of homunculi is to point out problems with the imaging data. The ACC is not the only brain area that lights up in fMRI images of subjects performing executive tasks; dorsolateral prefrontal cortex (DLPFC) and some parietal areas also tend to light up. Furthermore, serious questions have been raised about the validity of interpreting fMRI images as showing the locations at which cognitive functions are performed (see Bogen, 2001). At this point it is unclear whether these interpretations will be strengthened or called into question as imaging technology improves, and more evidence is accrued. In any case, poking holes in the evidence is perhaps not the best strategy to take against the claim that the anterior cingulate functions as the source of attentional control, because it concedes too easily the point that the question can be answered empirically.

The next step in the argument is providing necessary properties that an executive controller must have. The first necessary property put forward is that the system be centralized rather than distributed. Localizing attention to a specific region is taken as essential to showing that attention could be a causal agent. Parasuraman for example says that whether attention is a causal force or the effect of other processes is "closely related to the question of whether there exist attentional *systems* that are separate from other sensory and motor systems in the brain" (1998, p. 12).

The second necessary property put forward is that this unified region must be well connected. Miller and Cohen suggest that a control system requires bidirectional connections to diverse areas of cortex, so that the total state of the brain might be monitored, and signals might be sent giving instructions (2001, p. 171). Posner expects "the source of attention to lie in systems widely connected to other brain areas, but not otherwise unique in structure," and concludes that, "this appears to be the basic organization of frontal midline networks" (Posner, 1994, p. 7402). It is nowhere claimed that high connectivity is a sufficient condition for being able to perform supervisory functions.

Anatomical evidence is then offered showing that regions of PFC are well connected to other cortical areas in the way expected of a supervisory region. Both the Miller and Cohen, and Posner studies mentioned above offer this sort of evidence. Posner and DiGirolamo conclude from this that frontal areas are the source of executive attention: "the cingulate and other midline frontal areas are involved in producing the local amplification in neural activity that accompanies top-down selection" (1998, p. 411). They go on to say, "We believe that the cingulate, in conjunction with other midline areas, is responsible for those top-down effects, in that it provides a boost in activation to items associated with the expectation" (1998, p. 411).

Is the Evidence Sufficient?

The most obvious problem with the argument sketched above is that the patterns of connections found to exist between the ACC and other areas of cortex were put forward as necessary properties of an attentional control system, but not sufficient ones. Instead of coming up with necessary properties beyond well connectedness, the strategy is to argue that the ACC is the *only* part of the brain with these necessary properties. The imaging studies seem to play an important role here, by demonstrating that a few frontal areas are the only active brain areas that all executive tasks have in common, and that lesions in this area nearly always lead to deficits on executive tasks. In other words, the anterior cingulate may be the only essential node in the chain of causes leading to executive attention, if these imaging studies are correct. Inferring from fMRI studies that only the areas detected are involved in a task is exactly the sort of conclusion that Bogen (2001) criticizes most harshly.

Even if improved technologies made it unproblematic to conclude that only the areas detected by fMRI are involved in a given task, and the evidence didn't show that several additional areas are also involved, the conclusion still would not follow. Identifying the only part of the brain that has a necessary property of an executive controller would only warrant the conclusion that this area is an executive controller if we had some prior reason for believing that there exists an executive controller somewhere in the brain. The psychological phenomenon of executive control may be difficult to deny, but it does not follow from this that a part of the brain is the controller, just as it does not follow from the psychological phenomena of belief that there must be a part of the brain that is the believer.

What we have instead is a reason to believe that there is not an executive controller in the brain, and that cause theories of attention need to be reconceptualized. Whatever is going on in the anterior cingulate, we can be sure that it is not creating the volition to attend out of thin air. No mental act or neural event can be said to do this without invoking a homunculus, and to suppose otherwise is to commit the second variety of Cartesian error: purporting to explain something mysterious by pointing to a black box and saying "the magic is in there".

Computational Models

Sometimes additional evidence is given from computational studies that model aspects of attentional control. Reynolds and Desimone (2000) present a neural net implementation of Desimone and Duncan's (1995) biased-competition model of top-down attention. Their model only implements the effects of executive control, however. Simulating only the effects of control and not the source or cause seems to be the norm in computational models of executive control. Garforth, McHale, and Meehan report on a simulated robot featuring an executive attention system. Their willed attention module includes a control module that "receives input from a goal module, which encodes the action to be performed and provides a biasing attentional signal" (Garforth et al., 2006, p. 1928). The supervisory system "expresses willed intention" by modulating the salience of possible actions (Garforth et al., 2006, p. 1932), so actions are not so much chosen as chosen between. The “generate function” of the system, which is supposed to "create novel plans," has not been implemented, however. Human input specifies the "intended goal of the robot" (Garforth et al., 2006, p. 1934). These computational models are not effective as arguments defending cause theories of executive attention. That these models do not implement the “executive” itself, but only the way its input is used to choose between tasks, lends inductive support to Allport’s claim that, "qua causal mechanism, *there can be no such thing as attention*" (1993, p. 203).

Another sort of computational work does seek to implement the source of executive control, but in rather a different way. O’Reilly agrees that “to state simply that the PFC houses our internal ‘executive’… only labels and locates the mystery” (2006, p. 91). Recent papers from O’Reilly’s lab describe cognitive control as an “emergent phenomenon” (O’Reilly, 2006, p. 91) or “emergent function” (Hazy, Frank, & O’Reilly, 2006, p. 107). Emergence is a term that is used to mean a variety of different things, but here they seem to mean by emergent that a property or function is not explicitly programmed in. They set their approach in contrast to conceptualizations of controlled processing in which memory is "a passive store," and executive function is performed by a separate “module” (Hazy et al., 2006, p. 106). Instead of explicitly including an executive control module, they list finer-grained functions that the overall system must implement, and constraints that it must meet. Instead of built-in or human-input decision criteria, the model must learn its own decision criteria based on training experience (Hazy et al., 2006, p. 110). Although there is no executive controller in these systems, they simulate the behavior of one. This second variety of computational models do not constitute support for cause theories of executive attention. They are not implementations of cause theories, since they do not include any executive controller. They do provide possible hints as to how the psychological phenomenon of executive control can be retained without the executive controller.

Conclusion: Doing Without Cause Theories

I have reviewed philosophical arguments denying that a source of executive attention could exist in our brains (without resorting to dualism), and pointed out the flaws in the counter-arguments. Further I have shown that empirical evidence from neuroimaging and neuroanatomy do not manage to get around this difficulty, and that computational models of executive attention either beg the question by providing the executive’s decisions as input, or reconceptualize executive control such that there is no executive controller. Cause theories of executive attention, therefore, do not work.

Certainly willing oneself to pay attention is part of the repertoire of psychological phenomena belonging to all normal humans, and presumably psychological phenomena are in some way grounded in brains. Nevertheless, it seems impossible that there could be any bit of the brain that performs this function, so the relationship between the psychological and the possible neurological implementations remains something of a problem. We can say this much at least: When we decide to pay attention, there is activity in the anterior cingulate. We remain, however, stuck with the problem of how personal-level phenomena relate to the sub-personal. Christopher Mole’s solution is to describe attention as a manner rather than as a process (see Mole, 2005). Something seems right about this. Another possible course is to keep the assumption that there is a mechanism performing this function, but relax the assumption that it is confined to the brain. Perhaps an extended cognition approach could shed some light here. The problem with cause theories of attention is not that they suppose there to be causes of the phenomenon, but that they suppose there to be a privileged or first cause in the brain. If there is something that could be pointed to as the cause of volitions, it must be something like the whole life history of the person, as recorded in their brain and body. The emphasis on learning mechanisms in the computational models of O'Reilly's group points in a similar direction.

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