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ORIGINAL ARTICLE

Are there control processes, and (if so) can they be studied?

Glenn R. Wylie · James F. Sumowski · Micah Murray

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Abstract Generally, so-called control processes are thought to be necessary when we must perform one out of several competing actions. Some examples include performance of a less well-practiced action instead of a well-practiced one (prepotency); learning a new action (novelty); and rapidly switching from one action to another (task-switching). While it certainly is difficult to perform the desired action in these circumstances, it is less clear that a separate set of processes (e.g., control processes) are necessary to explain the observed behavior. Another way to approach the study of control processes is to investigate physiological dependent measures (e.g., electrophysiological or neuroimaging measures). Although these offer another avenue of inquiry into control processes, they have yet to furnish unambiguous evidence that control processes exist. While this might suggest that there are no control processes, it is also possible that our methods are insufficiently sensitive to measure control processes. We have investigated this latter possibility using tasks that are neuroanatomically distinct, though within the same modality (vision). This approach did not yield evidence for a separable set of control processes. However, recent works using a task-switching paradigm in which subjects switch between a visual and an auditory task suggest that switching both task and modality may be importantly different than switching task within a given modality. This may represent a way forward in the study of control processes.

The problem

Many experimental psychologists lead a double life. As scientific investigators of human cognition, we live by one of the primary assumptions of the scientific method: determinism. We observe and document the ways in which human performance is determined by contingencies we impose upon the laboratory environment. Our conviction in our deterministic beliefs is firm (c.f., Skinner, 1971). For example, we believe that if we were to perform an experiment for a second time, the results would be largely the same (within the constraints of statistical uncertainty and power). Thus, one of our double lives is bounded by a determinism that we base our careers upon, and that has been a constant companion since our days in graduate school.

In the second of our double lives, we suspend our belief in determinism. As soon as we leave the lab, we adopt the

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pervasive view that we have ‘agency’, that we are able to make choices freely, and that our actions are not completely determined by our environment (e.g., Chomsky, 1971). We believe, for example, that our choice to stop at the grocery store or the cinema on the way home from work is a choice freely made. We do not seriously think that such choices are completely determined by preceding events, or by cues in the environment. While we strongly believe that behavior in the lab—the behavior of our participants—is determined by our experimental manipulations, we somehow cannot bring ourselves to accept that our own actions in the world beyond the lab are similarly constrained (though by a much more complicated and subtle set of contingencies).

How can this uneasy double standard be resolved? While authors such as Chomsky have argued against the idea that our environment (and genetics) determines our behavior, they do not dispute the fact that in its reliance on, for example, replication, science assumes determinism. Nevertheless, what would count as evidence for or against our ability to control our actions? There are several instances when we believe we exert control over our actions, and we do this by using “control processes” (which is code for the will). Control processes in humans are often discussed in contrast to hard-wired stimulus-response contingencies in lower animals (“instinct”). For instance, as discussed by Marsel Mesulam, M.D. (Mesulam, 2000), a turkey hen will attack any animal present within her nest unless it makes the chirping sound of her chicks. Demonstrating the determinism of stimulus-response contingencies, a deaf turkey hen will kill its own offspring. As humans, however, we can often ignore quite salient stimuli to perform actions more appropriate to our circumstances. For instance, despite feeling hungry at a restaurant, we are able to wait until everyone at the table is served before eating our food. We can attribute this restraint to our evolved heteromodal cortexes (especially prefrontal cortex), which provide a buffer between lower needs of our limbic system (feeding) and stimulus-response programs of the unimodal association areas (eating). Such restraint makes us feel in control of our behavior (i.e., free). It is not surprising, therefore, that persons whose frontal lobes are damaged (e.g., victims of traumatic brain injury) or underdeveloped (e.g., children) are often not held to the same level of responsibility for their behavior in legal proceedings. The implication is that freedom lies within our ability to control our behavior. Control processes are needed when we inhibit inappropriate albeit desirable responses or refrain from performing a highly practiced action despite relevant stimulus cues (prepotency), when we learn to perform a new action (novelty), and when we switch from one action sequence to another (task-switching). Let us take these in turn.

Prepotency

Control processes are required when we refrain from performing a highly practiced action. For instance, patients with frontal lobe damage seem compelled to perform the action most often associated with common objects (utilization behavior, Archibald et al., 2001). Thus, when presented with a comb, such a patient will comb his hair, regardless of whether combing his hair is appropriate. Because you and I are able to not only refrain from performing such actions, but are also able to perform arbitrary actions with such objects (e.g., using a comb as a pointer, to indicate a word on a page), it is frequently concluded that we are free to perform any action we choose. Another example is when two coincident stimuli, or two dimensions of a single stimulus, compete for action. Perhaps the best-known example of this is Stroop stimuli (Stroop, 1935): color words (e.g., “red”) printed in an incongruent color (e.g., blue ink). It is notoriously difficult to refrain from performing the over-practiced task of reading the word (and all but impossible for patients with frontal damage to do so), and it is thought that this is because the color-naming task is performed far less often than the word-reading task (for striking evidence supporting this view, see MacLeod & Dunbar, 1988). The fact that we are able to name the color in the face of interference from the far stronger word-reading task is frequently cited as evidence for our ability to control or determine our actions. However, the fact that we are able to overcome learned tendencies—to name the ink color of a color word—is not evidence against determinism. Indeed, the reverse is the case. We perform the difficult, unpracticed action because we are told to. That is, our actions are determined by the experimenter who asks us to name the ink color. The fact that patients with frontal damage are less able to comply with such instructions may tell us something about the mechanisms that allow us to resolve interference from alternative stimulus-action pathways, and that might be what ‘control processes’ do, but it does not necessarily bear on the question of whether we are able to arbitrarily choose which action to perform at any point in time.

Novelty

Another instance when control processes are thought to be needed is early in practice. When we first start to learn a complex action sequence, we must effortfully sequence each component action until we acquire the new skill. Driving is often cited as a good example. However, just as in the case of performing the weaker of two potential actions, we perform the sequence in the correct order because we are told to. We have some external aid to guide our behavior—

be it a teacher or some sort of written instruction—that we rely upon until we internalize the sequence of actions. In those cases where we have no external guide, we learn by trial and error, and while it is true that this is a marvelous ability, it is an ability that any self-organizing system must have if it is to survive long.

Do we need to invoke control processes to explain how the brain is able to acquire a new skill? One might begin to think so when it is remembered that there is no one in the brain who knows how to translate a new stimulus into a new action. For example, when a subject comes into our lab and is presented with a task she has never had to do before, how is it that she is able to do the task reasonably well with relatively little instruction or practice? It is tempting to propose that control processes somehow know the goals (supplied by the experimenter) and the stimuli, and somehow direct processing such that an appropriate response is generated. However, while this may be intuitive, it is obviously flawed since control processes in this example turn out to be little more than a homunculus. Moreover, this is not how the brain appears to solve the problem of the acquisition of new skills. Functional neuroimaging studies that investigate practice show that early in practice, there is widespread brain activity. Later, when the skill is well-learned, brain activity is far more discreet, involving only those areas necessary for task execution (e.g., Jenkins et al., 1994; Toni et al., 1998). One interpretation of these data is that initially, the brain does not "know" how to translate the stimuli into responses, so it uses far more resources than necessary to get the job done: it "throws all it has" at the job. This may waste resources, but it does result in a response. Once a response has been generated, then a processing stream exists to translate the stimuli to responses and even if this stream is highly inefficient (and perhaps even quite error-prone), it represents a start. On subsequent trials, processes that are not necessary for successful task execution can be 'pruned', or dispensed with (i.e., practice), eventually leading to a processing network that is highly efficient and discreet. While control processes may be invoked to support some aspect of this process—for instance, to resolve the interference between competing (sub) processes involved in larger goal—it is not clear that they are needed to orchestrate, or direct, the learning.

Task-switching

Control processes are also thought to be required when one action sequence must be abandoned and another taken up. Generally, this occurs when we switch from one task to another, and this has led to the "task-switching" paradigm. In this experimental paradigm, subjects are given two or more tasks and on some trials they have to repeatedly perform each task while on other trials they must switch from one task to another (e.g., Jersild, 1927; Rogers & Monsell, 1995; Meiran, 1996; Wylie & Allport, 2000; Koch, 2003; Koch et al., 2010). One well-replicated finding is that subjects respond with longer latencies and make more errors when they switch than when they repeat a task. This is particularly true when more than one of the tasks can be performed with each of the stimuli used (e.g., Jersild, 1927). That is, when the stimulus does not itself serve as a cue for the relevant task, it is more difficult to switch to a new task.

This pattern of results has led to the wide use of the task-switching paradigm in studies of control processes (e.g., DeJong, 2001; Rubinstein et al., 2001; Waszak et al., 2003; Yeung & Monsell, 2003; Brass & von Cramon 2004; Wylie et al., 2004b). Moreover, patients with frontal damage have difficulty in switching task. For example, in the Wisconsin Card Sorting paradigm, where subjects must switch the rule they use to sort the cards based on feedback provided by the experimenter, patients with frontal damage frequently fail to switch, despite having declarative knowledge that a switch is required (Demakis, 2003).

While the task-switching paradigm has much to recommend it, it is not immune to the concerns raised in relation to the other paradigms for studying control processes: subjects are generally cued to perform one task or another. That is, because subjects are changing their action based on an external cue, it is not clear that this paradigm represents a way to straightforwardly measure control processes. The cues used range from a symbol presented before every stimulus that informs subjects of the relevant task on each trial to an instruction given at the beginning of a block of trials to switch task after a certain number of trials of each task (e.g., perform two trials of one task then switch to the other task for two trials, then switch back to the first task, and so on). In this latter case, a secondary cue is often used to ensure subjects do not lose track of which task is currently relevant (e.g., Rogers & Monsell, 1995; Wylie & Allport, 2000; Koch, 2003; Wylie et al., 2003a, b), but subjects are able to keep track of when a switch is required, even without such secondary cues (Jersild, 1927; Logan, 2007). In either case, the task-sequence is determined by the experimenter.

Recently, a 'voluntary' task-switching paradigm has been developed to answer exactly this criticism. In this paradigm, subjects are told at the beginning of a block of trials to switch between two tasks "at will", but to try to do so randomly (e.g., not to switch on every trial, or on every other trial, or only once in the entire run of trials). The results of these experiments are largely consistent with those of cued task-switching experiments, which has been taken as evidence that cued task-switching experiments do indeed represent a good way to study control processes (e.g., Arrington & Logan, 2004; Forstmann et al., 2006; Mayr & Bell, 2006; Forstmann et al., 2008; Arrington et al., 2010; Yeung, 2010). However, while the voluntary task-switching paradigm has good face validity, it may represent a variant of the more traditional task-switching paradigm in which subjects are instructed to switch after a certain number of trials of each task. In the more traditional paradigm, the number of trials preceding each switch is determined by the experimenter, while in the voluntary task-switching paradigm, it is determined by whatever criteria each subject uses to achieve a "random-seeming" series. However, the mechanisms used to achieve the switch may be very similar in these two paradigms, since in both cases subjects switch after a certain number of trials of a given task have been performed, and it might be this similarity that generates the similarity in results. On this interpretation, it is not the voluntariness of the switches that distinguishes this paradigm (since subjects are told to switch in both this and in more traditional paradigms), but rather the estimation of a random-seeming series—a difficult task that has itself been proposed to require control (executive) processes (e.g., Baddeley, 1996).

Switch costs

While it might be the case that switches of task are always cued (either internally or externally), it nevertheless remains the case that subjects are slower and less accurate on switch trials relative to non-switch (task-repetition, or repeat) trials. Thus, one approach could be to argue from the data: we think task-switches require executive processes; there seems to be some process (es) active on switch trials that results in poorer performance; perhaps executive processes are active on switch trials. While this might be the case, there are other, simpler explanations that have proven remarkably robust. For example, the slower, less accurate performance on switch trials could be due to interference from the task subjects are switching away from (for a good review, see Kiesel et al., 2010). This interference account has been used to explain not only the results from purely behavioral task-switching experiments, but also from studies that have used neurophysiological measures such as EEG/ERP or fMRI (e.g., Wylie et al., 2003a, b, 2004a, 2006). Not only is the interference view able to explain the data without reference to control processes (and is therefore a better explanation when Occam's razor is applied), but it is able also to explain patterns of data that are difficult to account for otherwise. For example, even when subjects are provided sufficient time to prepare for a forthcoming switch of task, they are still slower and less accurate than on repeat trials. This has led even those who contend that switch costs index control processes to propose that interference from previously adopted task sets does occur (e.g., Rogers & Monsell, 1995). On the other hand, the fact that increasing preparation time results in smaller switch costs has been taken as evidence that control processes are active on switch trials, and that this is one way to measure their operation. However, the fact that performance benefits as a function of preparation time is not incompatible with the interference account, and therefore it remains difficult to argue for the existence of control processes simply from this pattern of results.

Neurophysiology

Because it is possible to dispute the claim that purely behavioral paradigms can unequivocally shed light on control processes, one might attempt to circumvent this difficulty by investigating neurophysiological responses when control is thought to be required. The logic is the same as for purely behavioral experiments: if it could be shown that there was some process that was specific to switch trials, then—because we believe control processes are required on switch trials—this process might represent control processes. The only new thing that neurophysiology provides is a new set of dependent variables. For example, using ERPs, one might attempt to show that some component occurred at a specific time after stimulus onset and that this component was unique to switch trials. Indeed, this claim has been made (e.g., Nicholson et al., 2005; Nicholson et al., 2006). However, this claim becomes difficult to maintain when one considers the ERPs on repeat trials. The componentry of these repeat-trial ERPs looks essentially the same as that of switch trials; it is only the amplitude of the components that differs (e.g., Wylie et al., 2003a, b, 2009). Do these differences in amplitude reflect a process unique to switch trials (e.g., control processes), or do they represent merely a modulation of the same processes active on repeat trials? This has proven difficult to ascertain.

Another avenue of inquiry is to use fMRI to see if there are any processes that are used on switch trials that are spatially distinct from those used on repeat trials. Initial attempts at finding anything like this failed (e.g., Kimberg et al. 2000; Dreher et al., 2002), but there now exists a growing literature that shows that there is more activity in frontal and parietal areas when subjects switch task relative to when they repeat a task (e.g., Dove et al., 2000; Sohn et al., 2000; Brass & von Cramon, 2002; Gurd et al., 2002; Sylvester et al., 2003; Yeung et al., 2006). However, one question that is seldom asked is whether the areas that are active on switch trials are uniquely active on switch trials. That is, whether the differences shown between switch and repeat trials are due to activity on switch trials that is absent on repeat trials, or to an augmentation of the activity of the same processes that are active on repeat trials. Because a subtractive methodology is nearly always employed this question is seldom addressed, but the answer is critical to understanding whether the 'activation' shown on switch trials represents processing that is not required on repeat trials (e.g., control processes) or merely an augmentation of the same processing that is required on repeat trials (e.g., overcoming interference).

In our work, we have found that there do not appear to be any processes that are uniquely active on switch trials relative to repeat trials (Wylie et al., 2003a, 2006). For example, in one study (Wylie et al., 2006), we found the same pattern of activity on switch trials and repeat trials, including frontal and parietal areas, as well as lower-level, more task-related areas in visual cortices. In agreement with other research, there was more activity in frontal and parietal areas on switch trials than on repeat trials, but the same network was active regardless of trial-type. In other work, we have investigated this issue using EEG (Wylie et al., 2009). In that experiment, we again showed that the network of areas (indexed by the topography of voltage across the scalp) on switch trials was statistically indistinguishable from that on repeat trials. Again, there was a difference in the strength of the activity, such that the activity on switch trials was stronger than that on repeat trials, and this difference appears to account for the differences in ERPs that have been previously reported in the literature.

While these experiments support the idea that there is no qualitative difference between switch and repeat trials, and therefore go some way towards the disquieting conclusion that the task-switching paradigm may not be the most well-suited paradigm to study control processes, there are some reasons to doubt that they tell the whole story. Perhaps the strongest of these reasons is that the tasks used were broadly similar to one another. Although we took some pains to choose tasks that relied on spatially separable neural networks, yet all the tasks used were visual tasks (for a similar approach, see Yeung et al., 2006). It could be that control processes are inextricably associated with the tasks they control, and that if two visual tasks are used, then very similar control processes will be recruited. For example, the control processes used to switch task (on switch trials) and to mitigate interference (on switch and repeat trials) may be indistinguishable using the neurophysiological tools that are currently available (fMRI, EEG and MEG). Therefore, a stronger test of the idea that the same processes are used on both switch and repeat trials would be to use a paradigm in which subjects switch between two tasks that are in two separate modalities (e.g., a visual task and an auditory task).

Methodological benefits of inter-modal task-switching paradigms

The use of an inter-modal, task-switching paradigm confers several important advantages, particularly when brain imaging or other neurophysiologic measures are to be acquired simultaneously. The basic rationale is that tasks performed in different sensory modalities will recruit spatially distinct brain networks, making it easier to tease apart those regions/networks specifically involved in control processes. By also including switching within and between sensory modalities, there is the added benefit of being able to assay the specificity of control processes to the modality and/or the task at hand.

There are numerous ways that this issue could be studied: one could manipulate the input modality, one could manipulate the output modality, or some combination of the two. While studies have investigated the manipulation of output modalities (e.g., Philipp & Koch, 2005) or the mappings between input in a given modality such as vision and different response modalities (e.g., Philipp & Koch, 2010), the strongest test would be to manipulate the input modality, since the sensory systems devoted to vision and audition (for example) rely on spatially distinct brain networks. Indeed, there has been some investigation of this in the field (e.g., Lukas et al., 2010a, b; Stephan & Koch, 2010). These studies have used paradigms in which the task-relevant stimulus is either visual or auditory, and subjects must judge whether this stimulus is on the left or right side. While these studies have shown that switch costs remain when subjects switch from one modality to another, the task has remained constant: subjects have been asked to make a left–right judgment regardless of the modality of the task-relevant stimulus. This suggests at least two hypotheses: either (some portion of) the interference shown in these studies is due to conflict at a more abstract level (e.g., the categorical level, c.f. Hommel, 1998; Brass et al., 2003; Schneider & Logan, 2010), or that when any aspect of a task changes, interference results (e.g., Philipp et al., 2008). We will return to this below.

Inter-modal task-switching

In our previous work investigating inter-modal task switching, we required subjects to discriminate either the identity (manmade vs. natural) or spatial location (right vs. left) of environmental objects that were presented either visually or acoustically on any given trial (Murray et al., 2009). The following sequence of events occurred on each trial. First, an auditory-visual cue stimulus was centrally presented for 500 ms and signaled which of the two tasks should be completed on each trial. In Experiment 1 the cue was uninformative regarding the sensory modality of the task stimulus, whereas in Experiment 2 the cue also reliably conferred information about the sensory modality of the upcoming task stimulus (note that similar results were obtained on both Experiments). This was followed by 650 ms of no stimulation (i.e. central fixation cross only). Then, the target stimulus (either visual or auditory) was presented for 500 ms. This was in turn followed by a variable inter-trial interval of 2,500–3,000 ms, during which time participants responded manually.

These specific tasks were chosen because of the now abundant evidence supporting the existence of partially segregated functional and anatomic pathways for processing the identity and location of stimuli within both the visual system (e.g., Ungerleider & Mishkin, 1982; Haxby et al., 1994) as well as the auditory system (e.g., Rivier & Clarke, 1997; Romanski et al., 1999; Kaas & Hackett, 2000; Alain et al., 2001; Maeder et al., 2001; De Santis et al., 2007). By using tasks that are thought to recruit functionally specialized pathways in each sensory modality and by varying across trials the sensory modality in which the task was performed, we attempted to minimize the interference, or cross-talk, between the brain activity associated with the tasks subjects performed on successive trials. If a large portion of the switch cost is indeed due to such interference, then reducing the interference should result in a reduction in the switch cost.

We showed smaller switch costs when subjects were switching both task and modality relative to when they had to switch only the task (Murray et al., 2009) (for more studies on component task-switching see, e.g., Allport et al., 1994; Kleinsorge & Heuer, 1999; Hubner et al., 2001; Hunt & Kingstone, 2004; Philipp & Koch, 2010). One way to interpret these results is that there are separable control processes for switching task and for switching modalities. The underlying logic here is that if there were a common mechanism for switching task and modality, and if switch costs represent some index of this mechanism, then the switch costs would always be larger when it was necessary to switch both task and modality than when only one (task or modality) had to be switched. Inasmuch as the switch costs are smaller (in one case, even absent) when both task and modality had to be switched, this might support the idea of separable control processes for task- and modality-switches. However, there is another interpretation. This interpretation starts from the observation that while switch costs were indeed smaller when both task and modality switched, this was only true because it took subjects longer to respond on task-repeat trials in this condition. That is, when the modality switched, subjects were not able to benefit from a repetition of the task. This is consistent with a hypothesis forwarded by Koch and colleagues (Philipp et al., 2008), the essential premise of which is that when any aspect of a task switches—be it the goal, the S-R mappings, etc.—interference results. For example, in the study by Philipp and Koch (2010), a switch or repetition of task was manipulated independently from a switch or repetition of response modality. While a switch of either task or modality resulted in an RT cost, the pattern was under-additive. That is, if neither task nor response modality switched, subjects were far faster than in any other condition. In relation to our study in which we investigated switches of task and modality (Murray et al., 2009) this hypothesis would predict that subjects would respond most quickly when both task and modality repeated. Our data are completely consistent with this view.

We also performed correlational analyses with these data (Murray et al., 2009): we correlated switch costs for the two tasks (visual and auditory) when the switches were either within-modality (task switches within the visual modality correlated with task switches within the auditory modality) or across-modality (switching task and switching from the auditory to the visual modality correlated with switching task and switching from the visual to the auditory modality). We reasoned that if there is some process that must be completed on all switch trials then both within- and across-modality switches should be correlated. This is because the control process should be active when a subject switches task and the modality repeats and when the subject switches task and the modality switches. However, if a large part of the switch cost represents the time taken to overcome interference from the other task, one might expect correlations only between within-modality switches and not between across-modality switches. This is because there should be more interference within a modality than across modalities. In fact, we observed that switch costs were positively correlated between vision and audition when the sensory modality of the target repeated across trials (in Experiment 1 and 2, $r = 0.522$ and 0.698, respectively), but not when it switched across trials (in Experiment 1 and 2, $r = -0.109$ and -0.072, respectively).

The fact that within-modality switch costs are correlated may suggest there to be a common mechanism that is called into play whenever there is interference from a competing task—in this case, another task within the same modality. This mechanism could be conceptualized as a ‘central executive’, or indeed as any mechanism that serves to increase the influence of the currently relevant task-set (goal) when interference from other possible task-sets is high. Thus, this result accords well with the extant literature, but does not inform the question of whether control processes exist. However, the fact that there is no correlation when subjects must switch both task and modality suggests that this sort of switch may be qualitatively different from within-modality switches. Are control processes to be found in inter-modal switches? If so, one might expect them to be correlated too, since control processes are thought to be central. However, it may be that because the tasks are differentially demanding (performance was faster for the visual task than the auditory task), control processes may have been needed to switch from one modality to the other (e.g., audition to vision), but not back. Alternatively, it could have been that the disparity in task demands resulted in the inhibition of the stronger task in order to perform the weaker (e.g., Allport et al., 1994; Allport & Wylie, 2000), and that this asymmetry in inhibition—and subsequent need to undo the inhibition on switch trials in one 'direction' (i.e., towards one modality but not the other)—resulted in the observed lack of correlation.

One possible way to disambiguate these alternatives is to use a neurophysiological measure such as EEG or fMRI in combination with an inter-modal, task-switching paradigm. We have begun this work, with some encouraging results: there does indeed appear to be a process unique to switch trials when subjects switch both task and modality. If these preliminary results are confirmed, inter-modal task-switching may prove to be a powerful tool for the study of control processes.

Conclusions

We study control processes because we are interested in the will. However, notions of the will may not be compatible with the strong determinism that is central to the scientific method. In this article, we have suggested that there is little support for the idea of the will in the existing literature on executive control, principally because, as experimentalists, we have to instruct our subjects how to behave in our experiments. This makes it somewhat difficult to support the idea that changes in performance on, for example, switch trials are necessarily due to the imposition of the (free) will. However, in nearly every instance where we think the will to be active (prepotency, practice, task-switching), we find strong evidence for interference resolution. This suggests a possible solution to our unhappy conclusions about the will: it might be that to look for the will using the methods of experimental psychology and/or neuroscience is to make a category mistake. Differences in response time (and/or error rates) and in patterns of brain activity may provide information at a different level of explanation than we require for questions involving the will. If this is the case, then the resolution of interference may be exactly what we mean when we speak of exertions of the will. This line of reasoning fits quite well with our phenomenological experience: a clear choice requires no exertion of the will and interference is absent; when a choice requires the intervention of the will, this is because there are at least two competing alternatives and the interference generated by these alternatives must be resolved before we can act. Moreover, this line of reasoning has the reassuring conclusion that we can indeed study control processes, and that we have been doing so for a great many years, through the study of interference resolution. The study of intermodal task switching represents a fruitful continuation of this line of enquiry.

However, if we reject the idea that the study of interference resolution is what we mean when we speak of control processes, or the will, our conclusions are less sanguine. We might be tempted to do this because this formula leaves out any mention of 'free' will. Thus, while equating interference resolution with the will might enable us to square our scientific method with our use of language, we are no closer to reconciling our notions of agency (free will) with the strong determinism that is central to the scientific method. And so we return again to our double life—determinism at the lab, free will at home—and the interference that results from these two conflicting alternatives provides ample scope for the application of one conception of the will, if not the other.

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