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Is conflict adaptation adaptive? An introduction to conflict monitoring theory and the ecological problems it faces

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Abstract

Attending to a single stimulus (or dimension of a stimulus) requires filtering out distracting stimuli to avoid producing an incorrect response. The conflict monitoring (or conflict adaptation) account proposes that experience of conflict results in a shift of attention away from distracting stimuli and/or towards the target stimulus. The proportion congruent and congruency sequence effects are two findings often used to argue in favour of the conflict monitoring account. However, there are several potential limitations with conflict monitoring theory. This article explores some of the previously unarticulated (or rarely articulated) supplementary assumptions that must be made for the conflict monitoring account to be consistent with several important findings in the literature, some of which might undermine the initial intuitive appeal of the theory. Indeed, this opinion paper presents the view that conflict adaptation may not actually be particularly adaptive for performance. This article also discusses alternative interpretations of so-called "attentional control" phenomena. According to this view, participants may simply be learning regularities in the task structure that are unintentionally introduced when manipulating conflict (e.g., contingent regularities between distracting stimuli and responses). This sort of learning does benefit performance and is inherent for our functioning in the world, making this a more parsimonious view. Although simplicity is not everything, this article will present the case that the assumptions (often hidden or non-obvious) of conflict monitoring theory are non-trivial and, in many cases, imply relatively non-adaptive processes.

Keywords
Conflict monitoring; conflict adaptation; cognitive control; attention; learning; proportion congruent effects; congruency sequence effects

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> *RULE I*
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> *We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.*
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> *To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.*
>
> –Isaac Newton (1726/1846)

Introduction

In a world full of potential distractions, deliberate control over our attentional focus is crucial. To succeed at the task at hand, it will generally be necessary to control which elements in our environment that we want to process in detail and which elements that we need to filter (i.e., ignore). There are a great number of laboratory tasks designed to study our capabilities (and difficulties) to control attention to avoid distraction, generally grouped into the broad category of conflict tasks. The most well-known and heavily studied example is the colour-word Stroop effect (Stroop, 1935; for a review, see MacLeod, 1991), which I will use as the running example for this article (for other example tasks, see Eriksen & Eriksen, 1974; Simon & Rudell, 1967). In this task, the participant is asked to name the print colour of a colour word, which proves rather easy

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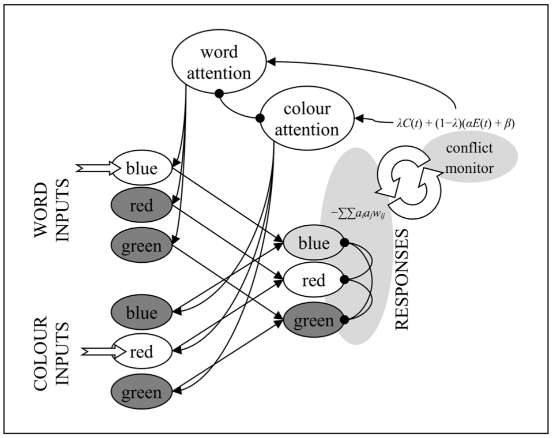

Figure 1. A minimal account of conflict monitoring.
Colour and word inputs are activated, which subsequently activate their corresponding responses, which can produce conflict. Notably, conflict must be measured somehow, and detected conflict must be used to adjust attentional distribution to targets and distracters.

when the word and colour are *congruent* in meaning (e.g., the word "green" printed in green) but rather difficult when they are *incongruent* (e.g., "red" in green). Though cognitive control over attention is sufficiently successful to (usually) avoid reading the word, the *congruency effect* (i.e., incongruent—congruent response times or errors) indicates that this control is not absolute: the distracting word has an impact on performance, indicating that it was not filtered entirely.

Of course, much work has focused on better understanding *how* and *when* cognitive control over attention is implemented. That is: with what mechanisms and in which situations. In the literature with conflict tasks such as the Stroop, one theory reigns supreme: the *conflict monitoring (or conflict adaptation) theory* (Botvinick et al., 2001). This is one of the most influential ideas in the recent history of cognitive psychology. According to the conflict adaptation account, each time we experience conflict between a task-irrelevant stimulus (e.g., a colour word) and a task-relevant stimulus (e.g., the colour that the word is printed in), this conflict is detected and triggers an upregulation of control over attention. In particular, conflict is said to trigger a decrease of attention to the distracting stimulus and/or an increase of attention to the target stimulus. Almost exclusively, evidence for conflict monitoring comes from two general experimental approaches: proportion congruent and congruency sequence manipulations.

First, consider the *proportion congruent effect (PCE)*. The PCE—in its simplest form—is the finding that the congruency effect is reduced when most of the trials are incongruent (e.g., 75% incongruent, 25% congruent), relative to when trials are mostly congruent (MC; e.g., 75% congruent, 25% incongruent; Logan et al., 1984; Logan & Zbrodoff, 1979; Lowe & Mitterer, 1982). The conflict adaptation account argues that this is the case because attentional control is upregulated to a greater extent when conflict is more frequent (i.e., more incongruent trials = more upregulations). Attention to the distracting word is reduced even more and/or attention to the colour is increased, thereby reducing the congruency effect. The idea is that the system is adjusting attention to avoid further conflict. In contrast, control is downregulated when conflict is rare, thereby *increasing* the congruency effect. As will be discussed in more detail later, the other necessary (but often more implicit) assumption of this view is that the latter downregulation of control in low-conflict situations is somehow useful as well.

Next, consider the *congruency sequence effect (CSE)*, often also referred to as the Gratton effect, sequential congruency effect, or (conflating behavioural observation with theoretical interpretation) the conflict adaptation effect. The CSE is the finding that the congruency effect is reduced following an incongruent trial relative to following a congruent trial (Gratton et al., 1992). According to the conflict monitoring hypothesis, this is due to increased attentional control following an incongruent trial (Botvinick et al., 1999). The logic, then, is that participants adapt to a *recent* experience of conflict (i.e., on an incongruent trial) by upregulating control to avoid further conflict. Thus, on the following trial, the word has a diminished impact on colour identification. Control over attention is said to be relaxed following a congruent trial (i.e., where conflict was minimal or absent), thus producing a larger congruency effect on the following trial.

The notion that selective attention is controlled in response to conflicting response tendencies is highly ingrained in the cognitive control literature (for reviews, see Bugg & Crump, 2012; Egner, 2008, 2014). This view was especially popularised by Botvinick and colleagues (2001) with their conflict monitoring computational model, illustrated in Figure 1.

The formulas in the figure are discussed in detail in the "Learning And Expectations About Conflict" section of the paper. More briefly, in this model, it is proposed that conflict is actively monitored by a conflict monitoring unit, proposed to be located in the anterior cingulate cortex. In the model, conflict is detected by measuring Hopfield energy, which is a value that increases to the extent that two or more response nodes are concurrently active (e.g., on an incongruent trial). Detection of conflict leads to a relative strengthening of attention to the target and weakening of attention to the distracter on subsequent trials. Because conflict is more frequent in the mostly incongruent (MI) condition, the control signal is higher than in the MC condition, allowing the model to simulate the PCE. In addition, because attentional control is adapted on a trial-by-trial basis, the CSE can also be simulated (albeit this was done with entirely different control parameters): after an incongruent trial, control is increased, and after a congruent trial, control is decreased.

It is difficult to overstate how popular conflict monitoring theory has become (e.g., even simply in terms of the number of citations key papers have accrued in a relatively limited amount of time; see Schmidt, 2019a). The tasks mentioned above (with their many variants) and the theoretical interpretation of said effects have further "spread their wings" far beyond their initial roots in cognitive psychology and neuroscience to clinical (Abrahamse et al., 2017; Liu et al., 2012; Praamstra & Plat, 2001; Steudte-Schmiedgen et al., 2014; Tulek et al., 2013), developmental (Iani et al., 2014; Lansbergen, Kenemans, & van Engeland, 2007), aging (Bugg, 2014b), and individual differences research (Hutchison, 2011; Lansbergen, van Hell, & Kenemans, 2007). This one idea has therefore had wide impact. The account has garnered many proponents over the years and the PCE and CSE have been replicated in a wide variety of conflict paradigms (e.g., Stroop, flanker, Simon, etc.).

Conflict monitoring theory is not without its critics, however. Alternative interpretations of PCEs and CSEs have been proposed, for example, that assume that neither effect is driven by cognitive control or even attentional modulations. Instead, simple learning biases—confounds unintentionally introduced into the design when manipulating congruency proportions or sequences—are responsible for the observed interactions (for reviews, see Schmidt, 2013b, 2019a). Still others have pointed out that, while the account does coherently explain PCEs and CSEs, the account does not seem to explain much else, including a range of well-known findings in conflict tasks (Algom & Chajut, 2019; Algom et al., 2022), a definite weakness relative to more integrative theories of conflict tasks (e.g., Melara & Algom, 2003). The general argument has been that these alternative explanations of the key phenomena appeal only to basic assumptions about learning processes that must be true anyway to explain a much wider range of phenomena, whereas the assumptions of conflict monitoring theory are less essential, applying almost exclusively to the PCE and CSE. As such, the law of parsimony (ala the Newton, 1726/1846, quotation) should, at minimum, give us some pause. Of course, conflict monitoring and more simple learning biases could potentially coexist or even be a part of the same learning mechanisms (Abrahamse et al., 2016; Egner, 2014), but the suggestion of the critics has been that the conflict monitor is superfluous.

Goals and scope

The goal of this article is not to provide a thorough overview of findings in the literature using the PCE and CSE (a number of such reviews already exist; e.g., Abrahamse et al., 2016; Bugg & Crump, 2012; Egner, 2008, 2014). The aim is also not for a thorough review and analysis of the existing empirical data attempting to distinguish between the conflict monitoring and alternative accounts (for some existing analyses like this, see Braem et al., 2019; Schmidt, 2013b, 2019a). Instead, the current article will attempt to do two things. First, it will be argued that, while seemingly intuitive at first glance, a conflict adaptation device might not actually be "adaptive" for performance. This raises the question of why such a process would exist in the first place. Second, the article will argue that, quite to the contrary, the alternative learning and memory account relies solely on mechanisms that are necessary for our interaction with the world (e.g., covariation detection, feature binding, and timing), mechanisms which also provide desirable performance benefits to participants. The goal is not to review whether current results do or do not support a role for conflict monitoring in performance, but rather to explore conceptual/theoretical reasons *why* it is reasonable to doubt this theoretical view (e.g., despite the initial theoretical appeal). It will more specifically be argued that, at least in the most typical cases, conflict monitoring does not actually seem to be "adaptive" for behaviour (e.g., in terms of aiding participants in becoming faster and/or more accurate). In this article, I will discuss some of the various ways in which the inherent (but sometimes hidden) details of the conflict monitoring perspective become much less intuitive (in some cases even rather bizarre) when the theory is further unpacked. It is important to stress that this article does not aim to conclude that, given these conceptual complications, conflict monitoring theory is necessarily wrong. Indeed, the present work will also explore some of the potential responses to the theoretical complications that I will propose, which may prove useful in exploring alternative conceptualizations of the conflict monitor in future research. Some of the minimal additional assumptions required by conflict monitoring theory will be explored in more detail than usual in this article.

Is conflict adaptation adaptive?

Perhaps the biggest draw of the conflict adaptation account is the initial intuitive appeal. Attending to the distracter leads to conflict, and conflict is bad for performance (i.e., due to slower response times and increased errors on incongruent trials). Thus, adapting attention away from the word is seemingly desirable. Following this logic further, if performance of a task with a conflict monitor is demonstratably better than performance of the same task without a conflict monitor, then we can, at minimum, say that there is a potentially useful purpose of the conflict monitor. Otherwise, we could argue that conflict adaptation reflects a *failure to learn* (viz., that the strategy is not useful or even harmful). This, I will argue, is exactly the case, at minimum for some of the key phenomena investigated in the literature.

The congruency sequence effect

The CSE is the best example of an effect that the conflict adaptation account proposes to explain in a way that is definitionally non-useful. For this effect, congruency proportions are not manipulated; instead, different sequences of congruent and incongruent trials are assessed (e.g., with a 50:50 congruent-to-incongruent trial ratio for all participants). Participants are said to adapt attention following conflict to avoid further conflict. It has been acknowledged before (including in the original report; Gratton et al., 1992; see also, Egner, 2007), but it bears repeating here that this mechanistic account assumes that participants *fail to learn* that:

$$P(\text{congruent} \mid n-1 \text{ congruent}) = P(\text{congruent} \mid n-1 \text{ incongruent})$$

And:

$$P(\text{incongruent} \mid n-1 \text{ congruent}) = P(\text{incongruent} \mid n-1 \text{ incongruent})$$

That is, previous-trial congruency is uncorrelated with current-trial congruency; the probability of a congruent versus incongruent trial is the same regardless of what occurred on the previous trial. In other words, conflict on trial $n-1$ is not predictive of conflict on trial n. In fact, net performance would be exactly the same if participants adapted attention away from the word after a *congruent trial* and towards the word after an *incongruent trial*, instead of the reverse. In this sense, upregulating attentional control following incongruent trials and downregulating control following congruent trials is no more or less useful than the exact opposite. It will not produce any performance benefit. Objectively speaking, it would be better to filter out the distracter on all trials (i.e., to the best of one's ability): consistently focusing attention on the target would decrease interference both on trials following an incongruent trial and those following a congruent trial. As such, at least in the context of the CSE, conflict adaptation is not behaviourally adaptive by any reasonable interpretation of the term and is (to some extent) not even really "learning."[1]

Of course, there are two possible (and related) responses to this point. First, it could reasonably be argued that this sequential adjustment is adaptive in the less-arbitrary outside world in which, for example, distraction tends to be followed by more distraction. Said processes are merely unadaptive in the more-arbitrary laboratory scenarios in which these regularities no longer apply. Thus, rather than arguing that conflict "adaptation" is unadaptive generally, we could argue that we merely fail to adapt to the new experimental situation. This might be because either (a) we fail to learn that conflict does not tend to follow conflict (i.e., more than conflict tends to follow an absence of conflict), or (b) that we are incapable of reconfiguring these sequential control processes (e.g., because they are hardwired into the system and cannot be "shut off"). In any case, the "conflict adaptation effect" (another term for the CSE) seems to be a misnomer.

There is another (related) possible response to the conceptual problem indicated above. Attention might be adjusted on each trial in response to the presence or absence of conflict to dynamically adjust control demands through experience (Aben et al., 2017). This might be easier (and faster) than trying to average across the task as a whole. As such, the CSE might be regarded as a logical byproduct of these local adjustments (Botvinick et al., 2001). In other words, one might argue that the CSE is merely a short-term consequence of an attentional strategy that is more useful over a longer timescale (e.g., similar to the relation between stimulus-response binding and contingency learning explored by Giesen et al., 2020; Schmidt et al., 2016, 2020). In other words, adaptations on one trial might not be particularly useful for the immediately following trial (CSE), but the accumulation of these local adjustments might be useful for dealing with task-wide conflict frequency (e.g., PCE). One problem with this notion is that it implies that PCEs and CSEs are strongly related to each other, but the evidence seems to argue against this. The two effects seem to be uncorrelated and dissociable (Funes et al., 2010; Torres-Quesada et al., 2013; Torres-Quesada, Lupiáñez, et al., 2014; Torres-Quesada, Milliken, et al., 2014). A further complication is that this notion assumes that conflict adaptation *is* useful in a PCE experiment, which brings us to the following section.

The proportion congruent effect

In that vein, consider the PCE. According to the conflict monitoring theory, control is increased (upregulated) after each experience of conflict and decreased (downregulated) in the absence of conflict. It might be argued that conflict monitoring is adaptive in this context because: (a) upregulating control diminishes incongruent-trial interference, (b) incongruent trials are much more frequent in the MI condition, and therefore (c) increasing control in the MI condition has a higher payoff than in the MC condition. Although all three of these premises are correct, it still does not follow that conflict adaptation is adaptive in a PCE experiment. From a purely performance-based perspective, conflict adaptation is not adaptive, unless:

$$f(d)\cdot P(\text{con} \mid \text{MC}) + g(d)\cdot P(\text{inc} \mid \text{MC}) < f(u)\cdot P(\text{con} \mid \text{MC}) + g(u)\cdot P(\text{inc} \mid \text{MC})$$

And:

$$f(d)\cdot P(\text{con} \mid \text{MI}) + g(d)\cdot P(\text{inc} \mid \text{MI}) > f(u)\cdot P(\text{con} \mid \text{MI}) + g(u)\cdot P(\text{inc} \mid \text{MI})$$

where *f*() and *g*() are functions[2] that determine congruent and incongruent response times, respectively, when attentional control is downregulated, *d*, or upregulated, *u*, which must of course be multiplied by the relative trial frequency, *P*, of congruent (con) and incongruent (inc) trials in the MC and MI conditions. Stated more simply, if we ignore the distinction between congruent and incongruent trials and take a measure of mean response time in the task, then is it the case that mean response time will accelerate in both the MC and MI incongruent conditions with a conflict monitor active relative to a situation where the conflict monitor is not active? If the conflict monitor makes performance worse in either condition, then this is problematic for the theory. Mean response time will most certainly be faster in the MI condition (second formula) when attentional control is upregulated (right half of formula), relative to when control is downregulated (left half of formula) or left in an intermediate state. This is consistent with the idea that conflict monitoring is beneficial. However, this argument falls apart when considering the MC condition (first formula). It is unlikely that mean response time will be faster in the MC condition when attentional control is *downregulated* (left half of formula), relative to when control is upregulated (right half of formula) or left in an intermediate state. That is, the mechanism proposed by the conflict monitoring account is by definition not helpful in the MC condition, making participants slower rather than faster.

It is relatively clear why upregulating control is useful in the MI condition. Conflict occurs frequently in the MI condition, so it is useful to do as much as possible to reduce the impact of the word on print colour identification. Interference, normally quite large, will be reduced for incongruent trials, and congruent trials will be largely unaffected (i.e., given that congruent-trial facilitation is rather small relative to incongruent-trial interference). Why would a downregulation in the MC condition not be similarly useful? First, because interference on incongruent trials vastly exceeds any facilitation on congruent trials (for a review, see MacLeod, 1991). As such, increasing attention to the word might increase congruent-trial facilitation, but would also increase much more the much-larger interference effect (i.e., unless functions *f*() and *g*() can be proposed that would suggest otherwise). Indeed, it is generally agreed upon that a base assumption of conflict monitoring theory is that incongruent trials should be more affected by attentional modulations than congruent trials, with more symmetric effects generally taken as evidence against conflict monitoring (e.g., Bugg et al., 2011). Given this, the proportion of incongruent trials would have to be infinitesimal for an *increase* of attention to distracters to ever be useful. That is, the small congruent-trial facilitation benefits due to downregulation of control would be outweighed by large incongruent-trial costs, unless incongruent trials were exceedingly rare.

I mentioned above that congruent-trial facilitation tends to be notably smaller than incongruent-trial interference. One complication in measuring these two components of the congruency effect is determining the appropriate baseline (Parris et al., 2022). For instance, facilitation is notably larger when compared with a neutral (colour-unrelated) word baseline than to a letter string baseline (e.g., XXXX), because the former contains lexical conflict (e.g., see Brown, 2011). Similar complications exist with the Simon and flanker tasks, though some evidence suggests that facilitation is completely absent (Evans & Servant, 2022). Complications aside, it is generally assumed that interference is much larger. However, there may exist exceptions, tasks in which facilitation is relatively large. Experiments manipulating congruency proportions (or congruency sequences) rarely include baselines to even attempt to determine whether this might be the case. Still, even in the case of larger facilitation, some other results might nevertheless suggest that decreasing attentional control will still not help in a MC condition. For example, some results suggest that congruent-trial facilitation is substantially less affected by manipulations that do affect incongruent-trial interference (Brown, 1996), and this includes proportional manipulations (Tzelgov et al., 1992). Furthermore, facilitation metrics are often unintentionally confounded by a contingency bias when congruent pairings are presented more often than incongruent or neutral pairings, meaning that facilitation might have been exaggerated in many past works (see Lorentz et al., 2016). But perhaps there might exist some task variants in which the result of the above-mentioned formulas *would* suggest a beneficial effect of conflict monitoring, even in the MC condition, and perhaps this might determine in which situations conflict monitoring is used and in which situations it is not.

The above analysis applies to response times. The situation is even worse when considering error rates, where downregulating control will always increase errors. In particular, *increasing* attention to the task-irrelevant stimulus—which can potentially lead one astray—and/or *decreasing* attention to the task-relevant stimulus—which always points to the correct response—can only possibly increase error frequency. Strictly mathematically, then, downregulating control can only increase error frequency and can never help. The optimal strategy will therefore always be to attend to the distracter as little as possible and the target as much as possible in a fixed and stable way (i.e., with congruency proportions being irrelevant). A mechanism that proposes dynamic up- and downregulations of attention is only better if both the up- *and* downregulations are useful. Thus, conflict adaptation does not represent a successful exploitation of the environment (unlike, for instance, stimulus-response contingency learning, to be discussed later in this article).

Always on control and cognitive misers

To clarify, it is *not* the suggestion of the current article that controlling attention to reduce attention to (potentially) distracting stimuli is a useless strategy. Quite the contrary, the argument is that this strategy is extremely useful *regardless of whether the proportion of congruent trials is high or low* (and in the CSE context: regardless of whether the previous trial was congruent or incongruent). Indeed, it is generally accepted that randomly varying distracting stimuli produces a cost generally referred to as Garner interference (Garner, 1974; for reviews, see Algom et al., 2022; Algom & Fitousi, 2016) and that incongruency produces further interference. The interference from task-irrelevant (distracting) stimuli is subjectively striking enough that attempts to control attention are rather intentional. In other words, conflict adaptation may not provide an objective benefit to performance above that achieved by simply maintaining the task goal of attending to the target and ignoring the distracter as best as possible on all trials and in all conditions. Schmidt (2019a) referred to this latter view as the "light switch" model of cognitive control. In the light switch model of cognitive control there are no micro-adjustments of attentional control. Rather, participants make one large macro-adjustment (specifically: an upregulation of control) as soon as they realise that the task is difficult and requires a strong focus on the colour and filtering of the distracting word (e.g., after the first incongruent trial, or even earlier when given task instructions). Of course, the macro-adjustment will never be perfect, and the word will therefore continue to affect target colour identification, but performance will be globally improved in the context of a conflict task. It is also worth noting that increased control will not be useful in all contexts (e.g., in non-conflict tasks or in tasks in which non-target stimuli are informative). Rather, it is merely the proposition that the type of downregulation of control implied by the conflict monitoring model is not useful in the context of a conflict task. It is also worth pointing out that this "light switch" model of control does not represent an account of the PCE (or CSE), as it will not adapt differently to MC and MI situations.

Fundamental to the conflict adaptation view, however, is the assumption that we adapt to conflict *some* of the time (e.g., in MI blocks or after an incongruent trial). With this comes the corollary assumption that some of the time we *do not* adapt (e.g., in MC blocks or after a congruent trial). This is conceptually confusing, as reducing the impact of the distracting word is *also* useful in the MC condition. There is likely no meaningful *benefit* for allowing attention to the word, since the Stroop effect is almost entirely interference driven (for a review, see MacLeod, 1991). How a relative increase of attention to the distracter in the MC condition is adaptive for performance is thus unclear. Indeed, the only result will be a *cost* for incongruent trials. While incongruent trials may be less frequent, being slack with attentional focus will result in very large impairments on these incongruent trials. It would be more adaptive to focus attention as best as possible on the target colour to reduce these large impairments, just like in the MI condition. Reducing attention to the distracter and processing the target (the explicit task goal) is always a better strategy.

One might argue, however, that focusing attentional resources on just one stimulus dimension is an effortful process (Kahneman, 1973), and one that is difficult to maintain all of the time. Thus, control is only engaged when it is thought to be most needed (e.g., a task with frequent conflict). In the case of the PCE, then, this effort account must assume that effort is expended much more *overall* in the MI condition. Without this implied assumption, of course, the conflict adaptation account cannot explain the PCE. However, the implication would then be that participants in the MC condition do not use up their finite cognitive control resources, despite the fact that doing so would be highly effective for improving performance on incongruent trials. In other words, reducing the redistribution of attention in the MC condition may reduce effort, but results in an increased impairment of performance in conflict trials. Failure to use available attentional resources to deal with this exacerbated conflict due to the reduced frequency of conflict may or may not be a sensible account of the PCE. Indeed, even though conflict is relatively less frequent in the MC condition, it is still quite frequent in an absolute sense (e.g., one in every four trials with a 25% incongruent manipulation). Furthermore, it could even be argued that conflict adaptation would produce a cost for *congruent* trials in the MC condition if attention were reduced to the target (e.g., less rapid processing of task-relevant information and increased errors).

In the case of the CSE, the effort account faces different conceptual problems. The amount of effort expended would vary from trial to trial, rather than from one set of participants to another. But as already discussed, the probability of further conflict is no higher following an incongruent trial relative to a congruent trial. Thus, variance in effort following incongruent relative to congruent trials might be able to explain the pattern of data in the CSE, but it is not clear what utility this sort of dynamic adjustment of effort would proffer. Of course, this may nevertheless be the case, but it would mean that the conflict adaptation mechanism is actually not adaptive in improving performance. It would also mean that the cognitive system is extremely poorly calibrated to learn about the regularities in the environment, adjusting the effortful use of finite cognitive resources in suboptimal ways.

Given the above, there are only three ways forward for the conflict monitoring account. First, one could simply accept that conflict adaptation is not useful in the context of our typical laboratory experiments (PCE and CSE), perhaps with the additional assumption that it is useful elsewhere. It would also have to be assumed that participants *fail to learn* that conflict adaptation is not helping or, at minimum, that they learn but cannot "shut off" the monitor. That is, the conflict monitor may be "hardwired" into the system (e.g., because it serves other purposes), and it will influence behaviour even when not useful. Of course, this view would be fundamentally incompatible with the last resort account (e.g., see Bugg, 2014a)—which assumes that the conflict monitor is used but only when other cues (e.g., stimulus-response contingencies) are not detected—as this account requires the assumption that the conflict monitor *can* be shut off. This "hardwired" monitor view would also have difficulty explaining the findings from dissociation procedures (e.g., Hazeltine & Mordkoff, 2014; Schmidt, 2013a, 2017; Schmidt & Lemercier, 2019) where evidence against an attentional control contribution was observed (i.e., exactly the cases[3] that the last resort account attempts to explain away).

The aforementioned caveats aside, consider one reason why the conflict monitor might be hardwired into the system. By default, the cognitive system might be wired to attempt to process as much information from the environment as possible so as to better prepare us to deal with unattended information if it becomes pertinent (e.g., when hearing your name in another conversation at a cocktail party; see Cherry, 1953; Moray, 1959). Restricting attention too much could be costly since it would mean reduced incoming information to learn from. This is related to the exploration-exploitation dilemma (March, 1991; Schumpeter, 1934). The conflict monitor may, therefore, be hardwired to "turn on" only when increased focus is needed, and it may simply be suboptimal in determining that increased control would be equally useful in a MC list.

Second, one could argue that conflict adaptation is "useful" in the cognitive miser sense (Fiske & Taylor, 1984). Human behaviour quite frequently deviates from the optimal, especially when an imperfect approach to a problem requires much less effort than the optimal approach. It has therefore been argued (e.g., Braver, 2012; Braver et al., 2007) that maintaining a high level of control (termed *proactive control*) is computationally/metabolically costly, whereas only upregulating control in the moment when needed (termed *reactive control*) is much less so. Applied to conflict paradigms, then, the cost of maintaining sustained attentional control (the light switch model of control discussed above) is only deemed worthwhile when conflict is frequent (MI). In contrast, when conflict is less frequent (MC), the "lazy" approach of dealing with conflict reactively when it occurs is deemed a better balance between effort and performance. This is the only view of conflict monitoring that I am aware of that makes any sort of sensible case for why conflict monitoring might exist in the first place. However, there remain potential problems with this view.

For example, it must be assumed that the computational or metabolic cost of simply maintaining focus on task-relevant information over task-irrelevant information is more costly than actively assessing/monitoring conflict on every trial and actively up- or downregulating the focus of attention on each of the two stimulus dimensions after each conflict/non-conflict experience. Whether this is true or not is unclear,[4] but the case could at least be made that conflict monitoring may not actually be the cognitive miser solution. In addition, the cognitive miser argument only seems particularly compelling in the case of the *list-level PCE (LLPCE)*, the observation of a PCE resulting from the overall levels of conflict in the task as a whole (which also assumes non-item-specific generalisation of control). In this case, sustained proactive control in the MI but not MC condition can be proposed. However, most other phenomena studied in the literature with congruency proportion manipulations cannot be explained by conflict adaptation unless control is assumed to be rapid and reactive. In these cases (discussed in the next section), it is unclear how much "savings" actually exist. In the case of the CSE, conflict monitoring is objectively not resource-sparing within the task, as upregulating control after an incongruent trial and downregulating control after a congruent trial is clearly not useful (i.e., as discussed in the "Congruency Sequence Effect" section). Indeed, it is objectively damaging to resource efficiency, as supplementary resources are needed to monitor conflict and adapt control. However, the CSE could be regarded as a short-term consequence of adaptations to congruency proportions that is simply poorly calibrated and fails to learn that there is no predictability in congruency sequences (as also discussed in the "Congruency Sequence Effect" section).

Before turning to these other phenomena, consider one final response to the conceptual problem highlighted in the current section. As mentioned above, always-on control will typically be objectively superior to the up- and (more importantly) downregulations of control implied by a conflict monitor. Although resource sparing or laziness might be defensible arguments for why said mechanism nevertheless exists, another possibility is that the mechanism is simply a bad learner that forgets quickly and does not learn well from its mistakes. Such a mechanism could potentially still be viewed as performing approximately reasonably at its job. Specifically, "always-on control" is objectively best, but "sometimes-on control" is still better than "always-off control." Thus, imagine that the conflict monitor is hardwired into the system to increase control in cases where potential conflict occurs (not always the case; e.g., in non-conflict tasks), and rapidly defaults back to a "lazy mode" when conflict has not been experienced recently. Said mechanism would need to have a very rapidly decaying memory store (something like severe anterograde amnesia) to explain the rapid downwards adjustments occurring after congruent trials in the CSE, for example. Even for the PCE, it would have to be assumed that the mechanism quickly forgets that it is repeatedly presented with incongruent information every three or four trials (depending on the PC manipulation, of course), thus failing to avoid the response time and accuracy costs that this entails. Even though such a mechanism might be said to have "the memory of a goldfish," it will still do better than having no control mechanism at all, by avoiding at least some future conflict. If the present analysis is correct but a conflict monitor nevertheless exists, then it may make most sense to stop referring to said mechanism as an "adaptive control" device. Instead, it may be better to conceptualise it as perpetually lazy, poorly calibrated, or an incompetent learner.

Item- and context-specific proportion congruent

The previous sections have explained some of the potential difficulties with viewing the conflict monitoring mechanism as behaviourally adaptive in the LLPCE and CSE cases. The present section considers two other variants of the PCE and a different type of oddity that conflict monitoring interpretations of these latter effects entail. In particular, this section will explain how the conflict monitoring explanations of the item- and context-specific proportion congruent effects (to be defined shortly) imply a somewhat circular logic. In fact, in the worst case, the standard interpretations could be viewed as practically parapsychological, as precognition (with the effect preceding the cause). Or, in a less exaggerated but still bizarre case, conflict monitoring interpretations of these phenomena require assuming extremely fast, recursive, and non-perceptual attentional control.

In the *item-specific proportion congruent (ISPC)* procedure, some distracting stimuli (e.g., the words "blue" and "red") are presented most often in their congruent colour, the MC items, and others (e.g., green and orange) are presented most often in incongruent colours, the MI items. Congruency effects are larger for the MC items than for the MI items (Jacoby et al., 2003). What is interesting about ISPC effects is that they cannot be explained by adaptation to conflict in the task as a whole. Because all trials are intermixed, it is impossible to know in advance how to adjust attention prior to knowing the identity of the distracter. This obviously rules out advanced proactive control. As illustrated in Figure 2, adjusting attentional control on a word-by-word basis *requires* that the cognitive system knows what the word is (or has at least identified features of the word) before control can be implemented. This leads to potentially circular logic: *the word has to be attended and identified to determine whether or not to attend to the word and identify it*. Of course, modifications might reasonably be made to conflict monitoring theory to allow this to work (e.g., see Blais et al., 2007; Verguts & Notebaert, 2008). For instance, after a word is known and identified as MI, control over central attention might be adjusted to, for instance, inhibit the internal word representation. Thus, the assumption must be that participants can process the word enough to estimate control demands and then up- or downregulate attention to the word itself to deal with potential upcoming conflict. All this needs to occur before conflict onset (otherwise, anticipating whether conflict is likely is obviously useless and reactive control should have kicked in already). The "window" of time in which computational/metabolic costs might be saved is therefore quite small.

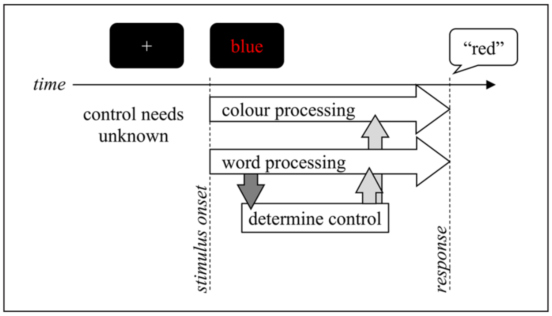

Figure 2. Illustration of an item-specific proportion congruent procedure unrolled over time.
Colour and word processing starts on stimulus onset but determination of item-specific control demands must wait until the distracter is known, after stimulus onset and partial processing of the word (dark grey arrow), then be applied to ongoing colour and word processing (light grey arrows).

Similarly, in the *context-specific proportion congruent (CSPC)* procedure, illustrated in Figure 3, the same stimuli are MC in one context (e.g., top display location) and MI in another context (e.g., bottom display location). The congruency effect is larger in the MC context (Corballis & Gratton, 2003; Crump et al., 2006, 2008). This particular CSPC design is particularly interesting. As with the ISPC effect, control cannot be implemented either task-wide or at the start of the trial. Importantly, control demands cannot be determined on the basis of the distracting word, either. Control demands can only be determined when the context (e.g., target location) is known. In addition to there being little time to save resources and no objective performance benefit when *not* upregulating control, the distracting word itself appears and disappears before the context (location) is known.[5] As such, *attention to the word cannot be adjusted until after the word has already been removed from the screen*. Thus, if there was any ambiguity in whether attention to the distracting word is adjusted at the perceptual input stage in an ISPC design, there is no ambiguity in the CSPC design. A conflict monitoring interpretation of this effect must assume that attention to the word is modulated *after* the word is already gone. For instance, it might be assumed that attentional control acts on the internal representation of the word (e.g., inhibiting the

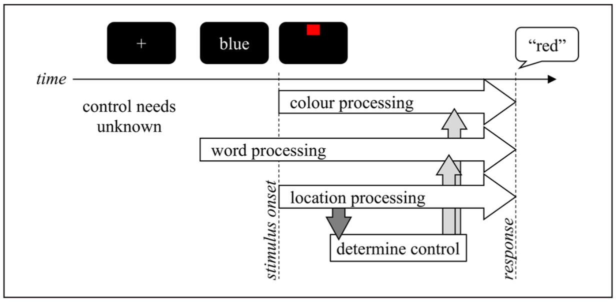

Figure 3. Illustration of a context-specific proportion congruent procedure unrolled over time.
Determination of context-specific control demands must wait until the target location is known, which can only occur after target stimulus onset and the disappearance of the distracting word (dark grey arrow), then be applied to ongoing colour and word processing (light grey arrows), the latter of which is no longer perceptually present.

representation of the word if the colour target is presented in the MI location). Control over percept-level attention is decidedly ruled out.

As one added caveat, *transfer effects* are observed in LLPC (e.g., Hutchison, 2011), ISPC (Bugg et al., 2011; Bugg & Hutchison, 2013; Spinelli & Lupker, 2020; Spinelli, Morton, & Lupker, 2022), and CSPC paradigms (e.g., Crump et al., 2017; Crump & Milliken, 2009; cf., Hutcheon & Spieler, 2017), where congruency proportions are manipulated with some items and effects of the list- or context-specific congruency proportions are observed for non-manipulated transfer items. If these effects are due to conflict adaptation, then they, too, could be taken to indicate a failure to learn: in this case, of which items a “control regularity” do and do not apply to. Of course, another, less negative way of interpreting transfer effects is as a generalisation of learning, though it remains the case that participants are applying a sort of rule to a situation in which said rule does not apply, despite having more than sufficient information to determine that the rule does not apply for transfer stimuli. This is particularly unusual in the CSPC case: control is proposed to be up- or downregulated on the basis of the target location, even when a word has already been presented in advance that is *not* predictive of conflict likelihood.

Of course, none of the above discussion necessarily implies that conflict adaptation does not occur. However, the above considerations do raise some questions about the potential utility of a conflict monitor. Certainly, the reader may simply accept that conflict adaptation is not useful in the context of conflict tasks but occurs anyway (which would likely require an alternative solution to the ambiguities that the last resort account aims to resolve). Relatedly, the reader may assume that conflict monitoring *is* an effective cognitive miser solution to conflict tasks. In the latter case, this does entail added assumptions (explored above) that may or may not seem reasonable to all.

Alternative explanations: parsimonious and adaptive

Although the adaptations proposed for the conflict monitoring account might be possible, competing accounts of the origin of the PC effect and CSE also exist (for reviews, see Schmidt, 2013b, 2019a; see also, Algom & Chajut, 2019; Algom et al., 2022; Schmidt et al., 2015). These alternative accounts argue that PCEs and CSEs have nothing to do with higher-order cognitive control at all. Instead, unintentional regularities (e.g., stimulus-response contingencies) exist in the key procedures, which can produce learning and memory biases. Critically, PCEs and CSEs are often eliminated or drastically reduced when measures are taken to control for these confounds. It is not the goal of the present analysis to consider these accounts thoroughly. Rather, here, I discuss how these alternative views are both more parsimonious and based on learning processes that are behaviourally adaptive.

For example, the simplest proportion congruent manipulations introduce stimulus-response contingency confounds, as discussed by Schmidt and Besner (2008; see also, Dishon-Berkovits & Algom, 2000 ; Mordkoff, 1996). By increasing or decreasing the proportion of congruent trials, the *intention* is to decrease or increase the amount of conflict in the task. However, an unintended result of this manipulation is that certain stimulus-response pairings become much more frequent than others. Problematically, this affects the MC and MI items in opposing ways—and in exactly the direction of a PCE. Specifically, congruent pairings become extremely frequent in the MC condition (e.g., “blue” is presented very frequently in blue), and incongruent pairings become very rare (e.g., “blue” is presented rarely in green). The word therefore becomes “predictive” of the correct response, speeding response times significantly, but only on congruent trials. This will increase the congruency effect in the MC condition. These same contingency biases will produce a *smaller* congruency effect in the MI condition. There are different variants of a MI manipulation, but if “orange” is presented most often in red, then “orange” becomes predictive of an incongruent “red” response. This will facilitate incongruent rather than congruent response times, thereby decreasing the congruency effect. In this way, a PCE can emerge due to simple stimulus-response learning. No assumptions need to be made about conflict monitoring or attentional control.

Again, it is not the goal of this article to thoroughly review the empirical data for or against an account of PCEs exclusively in terms of contingency learning (see the above-mentioned review articles). However, there are a few things that are notable about this type of theoretical perspective. First, the learning and automatic use of contingent regularities is an uncontroversial proposition, as said processes are fundamental to our interaction with the world. There is also a consensus that such contingency biases do confound PCEs (Braem et al., 2019). Debate only exists as to whether these contingency biases are the whole story (Levin & Tzelgov, 2016; e.g., Abrahamse et al., 2013; Atalay & Misirlisoy, 2012; Bugg et al., 2011; Bugg & Hutchison, 2013; Hazeltine & Mordkoff, 2014; Schmidt, 2013a; Spinelli & Lupker, 2020, 2021, in press; Spinelli, Morton, & Lupker, 2022). This is unlike the conflict monitoring account, where the mechanism itself is in question.

Second, contingency learning processes are unambiguously adaptive for behaviour. For instance, if we were unable to learn contingencies, then we would be incapable of detecting signal from noise. The world would appear chaotic to our eyes. Learning regularities associated with cues (e.g., "distracter" words) and using them to anticipate probable outcomes facilitates performance considerably. For instance, responding is greatly speeded overall with a predictable series of responses relative to an unpredictable one (Nissen & Bullemer, 1987). Furthermore, contingency learning processes explain a wide array of behaviour from a multitude of (conflict and non-conflict) paradigms (e.g., for reviews of contingency learning work with a non-conflict Stroop-like task, see MacLeod, 2019; Schmidt, 2021a, 2021b; see also, Miller, 1987; for a comprehensive review of the Garner task, see Algom & Fitousi, 2016), not just a select set of so-called cognitive control phenomena.

The same holds true for alternative interpretations of the CSE. For instance, it has been argued that the CSE is biased by *feature integration* confounds. Specifically, performance is strongly affected by the repetition (vs. change) of stimulus and/or response features from the previous trial and these biases produce an interaction of the same form as the CSE (Hommel et al., 2004; Mayr et al., 2003). For example, responding is especially fast when both the target and distracting stimuli repeat (e.g., "red" in blue followed by "red" in blue), termed a *complete repetition*. These sorts of feature integration biases are proposed to occur because participants retrieve from memory the just-encountered stimulus (e.g., "red" in blue), which aids in more rapidly identifying an identical stimulus (i.e., "red" in blue) and preparing the identical response. Somewhat incidentally, these feature integration biases produce a CSE on their own. For instance, a complete repetition can *only* occur on congruent trials following congruent trials (e.g., "blue" in blue followed by "blue" in blue) and incongruent trials following incongruent trials (e.g., "red" in blue followed by "red" in blue). If the current trial is congruent and the previous trial incongruent, or vice versa, then a complete repetition is impossible. Thus, the congruency effect is increased following a congruent trial (due to congruent trial benefits) and decreased following an incongruent trial (due to incongruent trial benefits), producing a CSE. It is notoriously difficult to remove all traces of repetition (e.g., repetitions from Trial $n - 1$, Trial $n - 2$, etc.), though it is possible to create a situation in which any remaining repetition biases should, at least theoretically, be orthogonal to previous congruency.

Feature repetition biases not only help to explain the CSE but also a wide array of other findings, such as negative priming (Rothermund et al., 2005) and binding effects in non-conflict tasks (e.g., Frings et al., 2007; Hommel, 1998; for an integrative framework of a wide range of action control phenomena, see Frings et al., 2020). More generally, feature repetition biases are the simple consequence of binding together stimulus and response features into a coherent representation of an event into episodic memory. These episodic traces themselves are required for subsequent contingency learning. These memory encoding and retrieval processes are not only necessary to mentally represent and learn about experienced events in the world but they also produce a performance benefit when applied in both conflict and non-conflict tasks. For instance, episodic encoding allows for faster retrieval of responses on subsequent trials, producing the very steep learning curve typically observed early on in an experiment (Logan, 1988).[6]

Briefly, another potential bias in PCEs and CSEs is *temporal learning*. Frequently, we conceive of our manipulations from the perspective of the *content* of a task: which stimuli were presented, which keys were pressed, whether there is conflict in meaning between stimuli, etc. With notable exceptions for some fields (e.g., music psychology, timing and time perception, etc.), the temporal (time) dimension is often forgotten (Grosjean et al., 2001) despite the systematic effects it can have on behaviour (Gilden, 1997, 2001; Gilden et al., 1995). This is unfortunate, as *all* stimulus and response events occur in time and it is likely that all or most information stored in memory is inherently temporal in nature (M. R. Jones, 1976).[7] The notion is that participants not only anticipate *what* to respond (e.g., which response) but also *when*. Participants anticipate (probably implicitly) when to respond and this produces a performance benefit if evidence for a response is sufficient at the anticipated time (Schmidt, 2013c; Schmidt & Weissman, 2016). On a congruent trial in the MC condition (or after a congruent response), then, performance is facilitated, whereas there is not enough evidence at the anticipated time on incongruent trials. In the MI condition (or after an incongruent response), it is the reverse: incongruent trials fit the pace, and evidence for a congruent response is attained before the participant is prepared to respond.

There is more debate about whether the empirical evidence supports a role for temporal learning as a confound in the PCE and CSE (Cohen-Shikora et al., 2019; Schmidt, 2017, 2019b; Spinelli et al., 2019; Spinelli & Lupker, 2022). More specifically, there is little debate that rhythmic timing plays a role in behaviour, but uncertainties about whether and to what extent such timing biases confound the key effects in conflict tasks. This empirical question aside, temporal learning is also behaviourally adaptive. In addition to being required for causal perception (Michotte, 1943/1963), temporal learning can explain blocking effects (for a review, see Los, 1996), rhythmic behaviour (Grosjean et al., 2001), and any form of timed behaviour (e.g., music performance). From a slightly different perspective, learning about the optimal time to respond effectively balances the speed of responding with an acceptable accuracy rate (M. Jones et al., 2009; Kinoshita et al., 2008, 2011). Performance will be maximised in both the MI condition (i.e., by avoiding too many errors) and the MC condition (i.e., by speeding responding). In this way, the PC effect is the direct result of the optimization of response timing.

The learning of time-related information need not be considered as much more complex than learning about stimuli or responses. In the Parallel Episodic Processing model, this is achieved in a, perhaps, artificial way by coding response times into memory traces (Schmidt, 2016a) and biasing response thresholds dynamically at retrieval. However, a more elaborated model could conceivably code events *in* time, such that retrieval of both what and when to respond are intrinsically related. Consider a music learning example. One does not necessarily need to learn what note to play next *plus* when and how long to play it, but merely what to do at a given point in time. We could think of the events that are experienced over the course of a trial being recorded into memory like a film (rather than like snapshots in a strictly episodic model) and re-experiencing a stimulus event like hitting “replay.” In this sense, re-experiencing a stimulus will not only bias us to make a similar response as before, but *when* we are biased to make those responses will be influenced by the passing of time (e.g., time since stimulus onset; Schmidt, 2016b).

It is general consensus that learning biases like those discussed in this section do contribute to the key effects of interest. Debate has focussed on whether these biases form a complete account of the phenomena in question, or whether conflict monitoring also contributes. What is elegant about this *learning and memory* view is that it starts with the processes that surely must exist and stops there. Furthermore, the processes themselves are adaptive for performance. They provide benefits to events that are consistent with contingent or recently coded stimuli and help to optimally time responses. Such processes are further generally accepted as necessary for basic cognitive functioning and evidence for said processes is observed across a wide range of tasks. This differs from the conflict monitoring perspective, which is of debateable utility and does not seem to explain anything other than the PCE and CSE.

It could be freely admitted that conflict monitoring accounts are limited in focus and only concern themselves with how conflict is handled and not with how conflict is generated or with other factors influencing performance (e.g., contingency learning). It could then be proposed that conflict monitoring theory could be supplemented with other theories to fully explain performance in conflict and other tasks. The major caveat with this defence is that conflict monitoring theory would presumably have to be supplemented with exactly those theories that are its competitors, theories which explain all or most of the key phenomena anyway, making the conflict monitor itself superfluous. Of course, this is the very heated debate that is explored in the literature (i.e., whether the conflict monitor is fully or only partially superfluous), but there is a qualitative difference between a general theory that explains some key phenomena with the same general cognitive mechanisms that it needs anyway to explain a wide variety of other phenomena and a hyper-specific theory that exists only to explain the key phenomena and nothing more.

Learning and expectations about conflict

In the preceding sections, I discussed reasons why conflict monitoring might not be viewed as adaptive and how this contrasts with alternative perspectives of PCEs and CSEs. The current section takes a slightly different approach and considers the question of whether “learning” about conflict is a small inferential leap from a learning perspective based on stimulus-response or timing information. Fundamental to the learning and memory account is the assumption that participants can learn about the regularities present in the task, and behaviour can resultantly be influenced by this learning. This claim is probably uncontroversial. One might suppose that conflict monitoring is similar, only that learning is based on more abstract information. Instead of learning to associate stimulus and response features, one might argue that we learn associations between stimuli and “control states,” “attention settings,” or “task sets” (e.g., see Abrahamse et al., 2016; Egner, 2008). For instance, Egner (2014) suggests that conflict adaptation can be viewed as merely learning at a higher level of abstraction (i.e., not about concrete stimulus or response features, but about internal states). Or alternatively, conflict could be viewed as a teaching signal, as has been forwarded with the *adaptation-by-binding* account of conflict adaptation (Verguts & Notebaert, 2008, 2009; for a recent review, see Abrahamse et al., 2016).

It is, of course, correct to point out that everything from stimulus-response learning to conflict monitoring involves learning/memory (e.g., Abrahamse et al., 2016). However, this does not imply (or even really *suggest*) that two different hypothetical learning mechanisms are equally plausible. Certainly, when regularities are simple enough, participants will readily pick up on them (whether implicitly or explicitly) and optimise their behaviour accordingly. Whether it is covariations between task-irrelevant and -relevant stimuli (Miller, 1987; Schmidt et al., 2007), event sequences (Nissen & Bullemer, 1987), artificial grammars (Reber, 1967), or temporal regularities (Ellis & Jones, 2010; Schmidt, 2016b), participants are especially adept at exploiting their environment, often in ways that were not intended by the researcher (Dishon-Berkovits & Algom, 2000). But there are also limits. Economic and psychology research on complex systems, for instance, has indicated that participants (even highly educated participants given ample time to reflect on their decisions) fail to learn even moderate complexities in the reward structure of an experiment (for a review, see Sterman, 1994). To take a more complex example, there are clear rules to learn about how certain move sequences affect subsets of pieces on a Rubik's cube (e.g., with commutators of the form ABA′B′). As many have already discovered, however, it is quite challenging to figure out how to manipulate the cube to a solved state, and most people resultantly give up.[8] Whether we learn about conflict in a given task of interest and, more importantly, whether this knowledge influences our behaviour in the way proposed by conflict monitoring theory is not self-evident.

Of course, participants surely do learn about congruency proportions—how much conflict is present in the task—at least in some scenarios. For instance, Blais et al. (2012) presented participants with blocks of trials that varied in congruency proportions. After each block, participants were asked to guess whether there were more or less congruent than incongruent trials. Not surprisingly, participants were able to do this with some degree of accuracy. This gives a clear indication that participants did in fact learn the conflict proportion of the task. However, being aware of these regularities in a task does not logically imply that participants use said information to adapt attention. Note that the conflict monitoring account does not *only* assume that participants learn about the amount of conflict in the task. It further assumes that the human cognitive system has a conflict monitoring device that measures the amount of conflict experienced during each trial, then uses this information to signal an attentional adaptation device that rapidly redistributes attention to the target and distracting stimulus dimensions. These hypothetical processes are not a necessary result of congruency proportion awareness.

Regarding the CSE, the idea that participants should be able to learn about conflict regularities is even more problematic. As previously mentioned, there is no regularity to learn (i.e., because the probability of a conflicting incongruent trial is equivalent following congruent and incongruent trials). Thus, it is difficult to evoke the concept of "learning about conflict" in this case. That said, it may nevertheless be the case that participants expect a congruent trial following a congruent trial, and an incongruent trial following an incongruent trial (as initially proposed by Gratton et al., 1992). Our expectancies do not always correspond to objective probabilities. One example of this is the gambler's fallacy (Jarvik, 1951; Tversky & Kahneman, 1971). For instance, if a fair coin is tossed nine times in a row and comes up heads every time, then participants will (incorrectly) expect that a tails is "due." Thus, their prediction for tails on the 10th coin toss will be >50%, even though this is incorrect.[9]

The idea that participants might incorrectly expect repetitions of congruency is thus not entirely implausible. Determining whether or not this is true might be difficult, however. Simply asking participants their expectancies (e.g., Duthoo et al., 2013; Jiménez & Méndez, 2013, 2014) is problematic, because such verbal reports may not accurately reflect the cognitive processes underlying response time and error performance. Furthermore, participants might have no real expectancies while performing the task but might report one when probed. Such reported expectancies may be driven in part by demand characteristics, potentially based on trivial factors such as merely repeating as a prediction the last type of trial they just experienced. Indeed, there is at least some evidence to suggest that explicit expectancies can be poorly predictive of performance (Perruchet et al., 2006).

Whether expectancies for conflict do or do not play a role in the CSE is still a matter of some debate (e.g., Duthoo et al., 2013; Duthoo & Notebaert, 2012; Jiménez & Méndez, 2013, 2014; Weissman & Carp, 2013). Even if participants do have an actual expectancy of a repeat of the same type of congruency, it is not necessarily the case that this information will be used to adjust the distribution of attention to the target and distracting stimulus dimensions. It should also be noted that accounts that propose the adjustment of attention in response to expectancies of conflict are not entirely the same as the conflict monitoring account. The conflict monitoring account proposes that participants adapt to conflict (not expectancy of conflict) to reduce further conflict. The expectancy and conflict monitoring accounts, however, might be regarded as two subtypes of the broader attentional adaptation idea. Regardless, both perspectives face some conceptual challenges.

In any case, conflict monitoring theory implies more than simple learning of associations between, say, stimuli and "control states." Further assumptions are required. Figure 1 presents what might be considered a "minimal" conflict adaptation account. First, to modify attention on the basis of conflict, there must be a mechanism to actively *measure* the amount of conflict experienced. In Botvinick and colleagues (2001), this was achieved by multiplying together the activity level, a, of each pair of potential response alternatives, i and j, by their connection weights, w_{ij}, and taking the sum of all such pairwise comparisons (Hopfield energy, E):

$$E(t) = -\sum_{i=0}^{n}\sum_{j=0}^{n} a_i a_j w_{ij}$$

That is, we need a system that "watches" for the extent to which there are more than one potential response alternatives being considered by the decision mechanism. Evidence for only one response (small *E*) indicates no conflict, whereas evidence for two or more responses (large *E*) indicates conflict. This conflict measure must then be translated into some form of a control signal. In Botvinick and colleagues (2001), this was computed with *C*:

$$C(t+1) = \lambda C(t) + (1-\lambda)(\alpha E(t) + \beta)$$

That is, *C* is a value that adjusts after each trial by some rate, λ, as a function of a transformation of *E*. The result of such computations must then be able to be employed somehow to actually increase or decrease the attention given to each stimulus component, such as by activating task/attention nodes. If we want to assume more than simple passive carryover of control from recent events (Egner et al., 2010; Hubbard et al., 2017; see also, Hazeltine et al., 2011)—required for learning that specific stimuli or contexts are associated with more control than others—then we must further assume that (a) the "control state" (e.g., *C*) is stored into memory along with the stimuli and responses, and (b) that subsequent presentation of a stimulus retrieves the "control state," influencing the control calculation and/or attentional settings.

Of course, none of the above assumptions need be regarded as inherently unreasonable. Rather, the purpose of the above is merely to highlight the fact that the assumptions underlying conflict monitoring are less "simple" than the basic reinforcement principles guiding the "simple learning" view (i.e., what fires together, wires together; Hebb, 1949). In other words, conflict monitoring is not "just" simple learning but with more abstract information. Some additional "machinery" seems necessary. In particular, we must assume (a) a conflict monitor that watches for conflict, which (b) triggers control over attentional settings to distracters and targets. Further assumptions are required to explain other phenomena, such as the need to store the conflict states associated with individual stimuli, retrieve these control states, and adjust control settings on the basis of what was retrieved. Such an account certainly can be proposed, but it is not as simple as, for example, activation of a stimulus leading to reactivation of the response that it was previously linked with. At minimum, it should not be assumed that any hypothetically learnable regularity will be learned, especially when said regularities are not very simple.

Final thoughts

In this article, I have aimed to illustrate several things. First, the fact that conflict adaptation involves some form of learning/memory says little or nothing about the plausibility of the notion. Related to this, it is not clear whether conflict adaptation does represent an "adaptive" (i.e., behaviourally-useful) strategy at all. Resultantly, if it is a process that does occur, then it is one that is driven by faulty perceptions of the task contingencies or cognitive laziness (i.e., monitor-as-a-cognitive-miser), and it leads to unproductive behaviour. The ecological utility of a mechanism specifically designed to produce no real benefit is thus questionable. Some of the conceptual limitations with conflict monitoring that I explored in this article may seem obvious to the reader (at least in hindsight), but the frequent discussion of conflict adaptation as an "adaptive" attentional strategy in the literature does suggest that explicit consideration of these issues is warranted. Indeed, we might consider replacing the term "adaptive control" with something more neutral, such as "conflict-induced control," which makes no implicit claim about the beneficial nature of control.

As previously mentioned, the goal of the present analysis was not to evaluate the empirical evidence for or against a conflict monitor. Rather, the goal was to explore theoretical considerations that make the theory seem dubious. At minimum, some reconceptualization of why such a mechanism might exist seems warranted. As the present analysis highlighted, it seems difficult to view this proposed mechanism as behaviourally adaptive. Some readers may nevertheless not be ready to abandon the conflict monitor. This may be for empirical reasons (i.e., existing data that the reader finds to compellingly support the presence of a conflict monitor) or for theoretical reasons (i.e., if the reader finds the challenges posed by this article surmountable). For such readers, the present work still suggests that it would be useful to reconceptualize the conflict monitor. Reconceptualizing the conflict monitor as, for example, an effort-minimising cognitive miser might provide useful new direction to theoretical and empirical work on conflict monitoring.

Alternatively, future research might explore the idea that conflict monitoring can be behaviourally adaptive, at least in some situations. For example, there may be some task environments where reducing control *does* produce sufficient congruent-trial benefits in, for example, a MC list to offset incongruent-trial costs, and this might be proposed to be a key factor determining the coming-and-going of conflict monitoring (e.g., complementary to the "last resort" logic of Bugg, 2014a). Globally, the goal of the current analysis is not to present the case that conflict monitoring is necessarily wrong, but rather to (a) highlight potential oddities in the way in which the purported mechanism is often discussed that cast doubts on what might initially seem to be an intuitive idea and (b) indicate alternative (but not caveat-free) ways in which the mechanism might be reconceptualised (e.g., cognitive miser, slow learner, etc.).

This sort of reconceptualization of the conflict monitor is also important for our understanding of the applications of CSE and congruency proportion manipulations to the type of individual differences work mentioned in the Introduction. For example, if we observe that a given group of participants shows a larger (or smaller) CSE than a control group, should this be interpreted as good or bad? That is, does a larger effect indicate more efficient control, or less? This ambiguity has already appeared in some research. For instance, Grundy et al. (2017) compared monolinguals and bilinguals on a flanker task. Although the overall congruency effect was similar for the two groups, a CSE was only observed for the monolinguals. The authors concluded that the absence of a CSE for the bilinguals represents an advantage: bilinguals can disengage attention from the previous trial more rapidly than monolinguals. However, Goldsmith and Morton (2018) responded with a completely contradictory argument, suggesting that the results demonstrate a bilingual disadvantage: bilinguals *failed* to engage in the normal learning processes that produce a CSE. Another study by Spinelli, Goldsmith, and colleagues (2022) took a separate measure of working-memory capacity (a presumed index of attentional control) and found that higher working-memory capacity was only related to a lower overall main effect of congruency and was unrelated to LLPC and ISPC effects. As we can see with this analysis, the magnitude of a CSE or PCE could be interpreted as positive, negative, or completely neutral. Determining which of these three interpretations is correct should be a key question to answer moving forward.

I have also aimed to argue that the "simple" learning/memory view *is* simpler than conflict monitoring and relies only on mechanisms needed to learn about regularities in our world and to interact with it accordingly. Of course, parsimony is not everything. The simpler of two ideas is generally preferred if a simpler idea appears just as sufficient as a more complex idea (i.e., the *law of parsimony* or *Occam's razor*), but a more complex idea can be the correct one. In that sense, the present article has not aimed to argue that conflict monitoring does not exist (an empirical question), but rather why it might not if it does not or why it might if it does (i.e., for different reasons than traditionally assumed).

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Notes

1. That is, in the sense that there is no regularity to learn.
2. In the simplest (probably oversimplified) case, we might assume $f(x) = C - ax$ and $g(x) = C + bx$, where C is a constant base response time, a and b are facilitation and interference multipliers, respectively, and x is the attentional multiplier. That is, facilitation and interference will increase (in this case, linearly) as attention to the distracter increases.
3. We cannot have our cake and eat it, too.
4. Perhaps especially when conflict trials are experienced every couple of seconds or so even in the typical mostly congruent conditions.
5. Note that there are other CSPC variants in which the context and distracter are presented simultaneously (e.g., Bugg et al., 2008).
6. Impairments can occur for some trials, such as in negative priming or the much smaller distracter repetition costs when a response repeats (Frings et al., 2007), but the net effect is a positive one.
7. Imagine trying to perform almost any real-world behaviour (playing music, juggling, riding a bike, throwing a baseball, whisking a bowl of cream, etc.) with only knowledge about the actions to perform and their sequence, but no knowledge of the timing of each action.
8. Indeed, one is unlikely to passively learn a viable solution by randomly turning the cube. Instead, strategic trial-and-error hypothesis testing or pre-existing knowledge of either cubing algorithms or group theory (mathematics) are probably required. This is especially the case when reaching the end of a solve, where permuting the few remaining pieces without destroying what you already solved requires increasingly longer algorithms.
9. As a minor aside, the gambler fallacy actually makes the reverse prediction of the conflict monitoring account, namely, the experience of a certain outcome (e.g., tails, or an incongruent trial) predicts a *non-repetition* (e.g., heads, or a congruent trial).

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