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Cognitive Load in Economic Decisions

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Abstract

Intuitive decision making has a large and often negative impact in economic decisions, but its measurement and quantification remains challenging. Following research from psychology, behavioral economists have often attempted to causally manipulate the balance of intuition and deliberation by relying on experimental manipulations as cognitive load. However, these attempts have resulted in mixed success, with many null results and no clear general pattern. We explain the possible reasons behind these developments and offer avenues for improvement. First, we show that a very simple formal model of decision processes offers a straightforward test to determine whether cognitive load has been successfully induced, hence disentangling failed inductions and true null results. Specifically, cognitive load in economically-relevant tasks must result in shorter response times. Second, we show that the intuitive arguments on the behavioral implications of cognitive load do not hold on closer, formal examination, unless strong assumptions are made that may or may not hold in typical economic experiments. We then report on seven economic experiments (joint $N = 628$) using different cognitive load manipulations and confirm the implications of the model. While the effect on response times is strong and pervasive, behavioral effects are weak and elusive. Our research serves as a warning on the differences between economic tasks and psychological experiments and the difficulties associated with importing methods uncritically.

Keywords: Cognitive Load · Intuition · Response Times · Economics and Psychology

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1 Introduction

Human beings routinely rely on their intuition, even for complex decisions. Extensive evidence from psychology shows that many human responses are based on impulses and habits, and involve little or no deliberation (Kahneman, 2003, 2011). Economic decisions are not an exception. Accordingly, the economics literature is paying increasing attention to the role of intuition in a large variety of areas. For instance, it has been often argued that self-control problems might be due to failures to inhibit intuitive reactions (Baumeister et al., 1994; Bernheim and Rangel, 2004; Kaur et al., 2010). The impulse to give in to immediate consumption might be behind intertemporal inconsistencies as those captured by hyperbolic discounting (Thaler, 1981; Ainslie, 1992; Laibson, 1997). These and other examples have given rise to a number of "dual-self" models (Thaler and Shefrin, 1981; Bénabou and Tirole, 2002, 2003, 2004; Benhabib and Bisin, 2005; Fudenberg and Levine, 2006, 2012), which reflect the view that economic behavior might result from the interplay between intuition and deliberation.

The role of intuition has also been intensely discussed for a number of important problems in interpersonal interactions. A large and heated debate has addressed whether cooperative behavior can be considered intuitive or not (Rand et al., 2012; Tinghög et al., 2013; Bouwmeester et al., 2017; Recalde et al., 2018). A similar debate has centered on whether fairness or rather selfishness is the default (intuitive) mode of behavior (Piovesan and Wengström, 2009; Fischbacher et al., 2013; Achtziger et al., 2016; Cappelen et al., 2016; Andersen et al., 2018). Several works have investigated whether honest behavior has an intrinsic value because dishonesty (and lying in particular) involves an active inhibition of intuitive tendencies (Cappelen et al., 2013; Fischbacher and Föllmi-Heusi, 2013; Gneezy et al., 2018). The recognition of the importance of intuition might also inform the design of behavioral interventions. For instance, Heller et al. (2017) designed randomized controlled trials to help youth at risk of engaging in crime "slow down and reflect on whether their automatic thoughts and behaviors are well suited to the situation they are in."

To study intuition and its consequences for economics, we need both correlational and causal evidence. Research from psychology suggests that deliberative processes rely on cognitive resources to a much larger extent than intuitive thinking (Baddeley and Hitch, 1974; Baddeley, 1992). Thus, if those cognitive resources are taxed or impaired, the balance between deliberation and intuition will be shifted toward the latter. This is the essence of *cognitive load* manipulations that causally reduce the amount of cognitive resources available for a task, hence impairing deliberation and boosting intuitive behavior. An extensive literature has shown the effectiveness of these manipulations in psychology (Baddeley et al., 1984; Shiv and Fedorikhin, 1999; Hinson et al., 2002; Lavie and de Fockert, 2005; Barrouillet et al., 2007). This is important, because the shift induced by cognitive load would be very consequential in many situations of interest in economics. In terms of decisions and performance, intuitive processes often correspond to cognitive shortcuts or heuristics, which might be aligned with deliberation in some or many situations, but might conflict with it, leading to biases, in economically relevant domains as decision making under risk or uncertainty. Thus, tilting the balance toward intuition allows to better understand such biases. In terms of preferences and motives, this might reveal intrinsic tendencies (sometimes informally referred to as a "default mode of behavior"), and hence a shift toward intuition might help uncover the roots of many economically relevant human tendencies as altruism or cooperation.

It is hence not surprising that a large number of works in behavioral economics have turned to cognitive load and related manipulations to causally influence reliance on intuition. However, the literature has achieved limited success and generally obtained mixed or null results. Cappelletti et al. (2011) found no effect of cognitive load on proposer offers in an Ultimatum Game. Similarly, Cornelissen et al. (2011) found no effects in a Dictator Game, although there was an interaction with Social Value Orientation (Murphy et al., 2011). Hauge et al. (2016) reported finding small or nonexistent effects in a series of Dictator Games. Benjamin et al. (2013) found no significant effects of cognitive load on time preferences or selfish behavior. Glaser and Walther (2014) reported that behavior in an investment task was unaffected by cognitive load. In a study on mixed-strategy play in games, Duffy et al. (2016) obtained counterintuitive results of cognitive load and concluded that the availability of cognitive resources might not affect behavior. Allred et al. (2016) studied strategic sophistication under cognitive load and concluded that the effects, if any, were inconsistent across games. Deck and Jahedi (2015) found that cognitive load increases risk aversion and impatience over money, but has no effect on impatience over consumption. Further, the effects are driven by the individuals most sensitive to the manipulation. Drichoutis and Nayga (2020) reported finding no effects of cognitive load on risk preferences or consistency of economic decisions.

Other studies, however, have found significant effects of cognitive load manipulations in economic tasks, sometimes in contrast with the studies quoted above. Milinski and Wedekind (1998) and Duffy and Smith (2014) found effects of cognitive load on behavior in a repeated prisoner's dilemma. Carpenter et al. (2013) provided evidence that cognitive load impaired strategic sophistication in games. Døssing et al. (2017) found increased cooperation under cognitive load in a repeated public good game. Schulz et al. (2014) used a series of mini-Dictator games and found that subjects under cognitive load react less to the degree of advantageous inequality. Samson and Kostyszyn (2015) showed that cognitive load reduces trust in a Trust Game. van 't Veer et al. (2014) found that participants under cognitive load were more honest in the die-rolling task of Fischbacher and Föllmi-Heusi (2013). Buckert et al. (2017) documented increased reliance on imitation under a manipulation closely related to cognitive load. Gerhardt et al. (2016) find increased risk aversion in lottery choices under cognitive load.

Overall, the picture is a blurred one, with mixed and often non-significant effects. It is also reasonable to assume that publication bias might have resulted in an additional number of unsuccessful studies not being circulated. In view of this, some researchers have even argued that economic rationality might be unaffected by temporary impairments in cognitive resources (Drichoutis and Nayga, 2020). Given the fact that cognitive load is a well-established, non-controversial manipulation in psychology, this situation is puzzling. In this work, we set out to provide answers to the puzzle and suggest avenues for possible improvement.

For this purpose, we first provide a very simple formal model of decision processes incorporating the postulated effects of cognitive load, namely that cognitive load tilts the balance toward more intuitive processes and away from deliberative ones. This simple model immediately delivers a useful prediction which can help improve future experiments relying on cognitive load. The reason is that, if a given cognitive load experiment finds no effect, it is not possible to conclude whether this is truly because a shift to intuition does not affect economic behavior, or rather because the particular cognitive load manipulation implemented has failed to tax cognitive resources to a sufficient extent. Our first prediction provides a manipulation check which allows to test whether the manipulation has been successful or not *independently of whether there are any effects on behavior.* Specifically, the model predicts that decisions under cognitive load must be *faster* than in its absence. The intuition for this result is straightforward. One of the fundamental characteristics associated with more intuitive (or more automatic) processes is that they are generally faster than more deliberative ones (Kahneman, 2003; Strack and Deutsch, 2004; Evans, 2008; Weber and Johnson, 2009; Alós-Ferrer and Strack, 2014). If a manipulation successfully induces a shift toward more intuitive processes, meaning that decisions arise from those more often, average response times must become shorter. However, while this effect will typically arise for economic tasks, we do not necessarily expect to observe it in the kind of tasks characteristic of cognitive psychology. The reason is that economics often deals in more complex decisions than psychology. Cognitive load is bound to cause a small increase in response times due to the more mechanical parts of decision making, e.g. those involved in perception, motor implementation, and process selection. For very simple tasks involving very short response times, those mechanical effects might dominate, resulting in longer response times (Gevins et al., 1998; Baddeley et al., 2001; de Fockert et al., 2001). For complex tasks as the ones of interest to economists, those mechanical effects will typically be negligible and the effects we describe here will dominate.

Our model allows us to critically examine the standard predictions ascribed to cognitive load in economic experiments. Essentially, the argument is that, if cognitive load induces a shift toward intuitive processes, a shift toward intuitive actions should result. On close examination, this argument rests on additional and possibly unwarranted assumptions. Again, for the simple tasks often used in cognitive psychology, processes are often straightforward stimulus-response mappings with little variability, and an identification between intuitive processes and intuitive actions might be unproblematic. For the complex decisions economists are interested in, however, processes are closer to behavioral rules, which depend on stimuli in a noisy way. It is simply not possible to conclude that a given action comes from a particular type of process without incurring in a reverse inference fallacy (Krajbich et al., 2015). Further, even though theories of intuition and deliberation often use those labels in a dichotomous way for simplicity, the underlying dimension (automaticity) is actually viewed as a continuum in psychology (e.g., Allport, 1954; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977; Bargh, 1989; Cohen et al., 1990). That is, few processes are purely automatic (or intuitive), or, in other words, many processes that an economist might view as intuitive are merely *less* deliberative than others and rely *less* on cognitive resources than those. Thus, it is by no means clear that, even if cognitive load shifts the balance toward more intuitive processes, those processes will remain completely unchanged under impaired cognitive resources. In our model, we incorporate natural assumptions on the how decision processes are affected (be they more deliberative or more intuitive), and find that the standard predictions regarding the effects of successfully-induced cognitive load on behavior fail to obtain. Restoring them entails a strong, additional assumption which essentially boils down to intuitive processes being completely unaffected by cognitive load.

After detailing the findings in our simple model, in this paper we report on seven independent experiments (joint $N = 628$), using a variety of cognitive load manipulations and different economic applications with reasonably-complex tasks (strategic interactions in Cournot markets, voting decisions in small committees, and belief updating tasks). In all experiments, we find robust effects of cognitive load on response times as predicted by the model. Hence, we conclude that all our cognitive load manipulations successfully induced process shifts as desired. However, effects on actual behavior are mixed and often nonexistent.

In conclusion, we propose that economists using cognitive load in future experiments deploy the response times test we provide here as a manipulation check, in order to be able to argue that their manipulation had the desired effect of inducing a shift toward intuition. At the same time, researchers should be aware of the fact that uncritically importing arguments from psychology on the actual effect on behavior might be unwarranted, and generally rests on strong assumptions on the nature of the involved intuitive processes. This is not to say that researchers in economics should abandon cognitive load entirely, but merely that the nature of the assumptions on underlying processes should be made clear, and their validity should be investigated.

The remainder of the paper is organized as follows. Section 2 provides an overview of the psychological theories behind cognitive load manipulations. Section 3 spells out our formal model. Section 4 presents three experiments on strategic behavior in Cournot markets. Section 5 discusses three experiments with different voting rules in committees. Section 6 presents an experiment on belief updating with two different cognitive load manipulations. Section 7 discusses the results and presents suggestions for future research.

2 Working Memory and Cognitive Load

Understanding cognitive load manipulations requires a discussion of *working memory*, which can be described as the set of functions and resources governing the selection and execution of decision processes. To understand the origins of cognitive load manipulations, we briefly introduce the working memory model of Baddeley (1986, 1992, 1996, 2000), which is a standard reference in cognitive psychology. This model describes how different working memory components might be responsible for automatic and controlled processes and their selection. It suggests a supervisory system that controls the switch between processes. The model distinguishes a *central executive system* from several subordinate memory systems (components) that are modality-specific. These components are the *phonological loop*, the *visuospatial sketchpad*, and the *episodic buffer*. Each of the working memory components has only limited (cognitive) capacity. Accordingly, cognitive load manipulations work by overloading these components' resources.

The *phonological loop*, also known as *verbal working memory*, is responsible for the retention of verbally coded material, independently of whether it is presented in written or auditory form. It refreshes stored information through inner-voice repetition or subvocalization (see, e.g., Gathercole and Baddeley, 1993). The assumption is that the cognitive resources required by the phonological loop are used more intensely by more controlled processes. Most of the cognitive load manipulations employed in previous economic research target the phonological loop (typically, keeping certain numbers in memory), and accordingly, so did several of our manipulations. The *visuospatial sketchpad* is responsible for the retention of graphically coded material, as e.g. images. Some cognitive load manipulations in psychology avoid the phonological loop and target this subsystem instead by, e.g., asking participants to keep a configuration of dots in memory. One of our experiments in Section 5 included a manipulation of this type. The *episodic buffer* is the most-recent addition to working memory theory (Baddeley, 2000), and is assumed to be responsible for the temporary storage and manipulation retrieved from long-term episodic memory.

Last, the *central executive* integrates information from various sources and is also seen as the supervisor or controller of the other working memory components, consuming a large part of the cognitive resources associated with working memory (Norman and Shallice, 1986). It plays the role of a supervisory system switching between controlled and automatic processes. More generally, it is assumed to govern the controlled selection or development of strategies in situations which are new in the sense that no specific rules have yet be learned, i.e. when automatic processes are not available. It is also responsible for allocating attention to complex controlled processes and implementing them. Hence, successfully performing complex cognitive tasks (e.g. by inhibiting automatic processes) can be assumed to rely on functions of the central executive. Cognitive load manipulations targeting the central executive are seen as particularly demanding. The experiment in Section 6 included a manipulation of this type.

3 A Simple Formal Model

The model builds upon previous models incorporating multiple behavioral rules, but extends them to incorporate cognitive load (Achtziger and Alós-Ferrer, 2014; Alós-Ferrer, 2018; Alós-Ferrer and Ritschel, 2020). It assumes that two behavioral rules codetermine behavior, a more deliberative one and a more intuitive/impulsive one. In this manuscript, we present multiple experiments in different settings involving different behavioral rules.

3.1 The Basic Model

Consider a given decision problem, where a decision maker has received some information on the available alternatives. On the basis of possibly-different parts of that information, different behavioral rules deliver prescriptions. Suppose further that only finitely many options are available (as will be the case in the experiments). Denote by X the finite set of options, with typical element $x \in X$.

In any cognitive load experiment, the researcher will have some candidate for deliberative and intuitive behavior. Let D and I denote the more deliberative/controlled and more intuitive/impulsive behavioral rules, respectively, and let x^D denote the deliberative and x^I the intuitive choice. However, behavior is noisy, and hence we assume that all rules are stochastic in nature, i.e., they carry an amount of noise, resulting in deviations from the rule's prescription. Note that, hence, the deliberative rule can select x^I and the intuitive one can select x^D, and any of them could select actions $x \neq x^D, x^I$. That is, x^D is the option most frequently selected by the deliberative process and x^I is the option most frequently selected by the intuitive process, but the processes themselves are noisy. If $P^D(x) > 0$ and $P^I(x) > 0$ denote the probabilities with which each rule selects $x \in X$, conditional on the rule being the one which actually determines the response, then $P^D = P^D(x^D)$ is the probability with which the deliberative rule indeed selects the deliberative choice, and $P^I = P^I(x^I)$ is the probability with which the intuitive rule selects the intuitive alternative. By definition of x^D and x^I, and assuming no knife-edge ties, one has that, for each decision situation, $P^D > P^D(x)$ for all $x \in X, x \neq x^D$ and $P^I > P^I(x)$ for all $x \in X, x \neq x^I$. That is, the prescription of a rule (x^D or x^I) is the rule's *most frequent* (modal) selection, but in the multi-alternative case this does not even imply that the prescription is selected more than half of the time.

If a researcher has decided to implement a cognitive load manipulation, it will be because he or she wants to make use of the fact that cognitive load induces a shift in (unobservable) decision processes. To formalize this assumption, we adopt the view that which of the two rules will actually determine behavior is a stochastic event. Let $\Delta > 0$ be the probability that the actual response is selected according to the more intuitive rule (or, alternatively, the latter is not inhibited by the central executive in favor of more deliberative ones), and $1 - \Delta$ the probability that it is selected according to the more deliberative one. The parameter Δ thus reflects the balance between more intuitive and more deliberative processes. The essence of cognitive load is hence captured by the following assumption.

(L) Δ increases under cognitive load.

Response times are also assumed to be stochastic. Let $R^D = E[RT|D]$ and $R^I = E[RT|I]$ denote the *expected* response times conditional on the response being selected by the more deliberative or the more intuitive rule, respectively. For simplicity, we assume that expected response times do not depend on the actually-selected response. Naturally, since the more automatic rule is thought to be faster *in expected terms*, we assume

(R) $R^D > R^I$.

For some of the results below, we will further assume that

(P) $P^I > P^D$,

i.e. the deliberative process is noisier than the impulsive/automatic one, while the latter is more *consistent*. This is natural since automatic processes are assumed to rely more strongly on associative stimulus-response patterns. A simple way to think of the model is to conceive of the intuitive rule as a swift cognitive shortcut, while the deliberative rule is a slow, deliberative process which depends on actual computations and is hence more error-prone.[1]

The model described so far encompasses the one in Achtziger and Alós-Ferrer (2014), which however was restricted to binary choices, and extends it to include cognitive load. Assumptions (R) and (P) have been given a micro-foundation in Alós-Ferrer (2018), where the behavioral rules are instantiated as diffusion processes as in the drift-diffusion model (DDM) of Ratcliff (1978) and Ratcliff and Rouder (1998), which has been recently further analyzed by Fudenberg et al. (2018) and is standard in cognitive psychology and neuroscience (e.g. Shadlen and Shohamy, 2016). In this model, evidence accumulation (internal to the decision maker) is captured as a diffusion process with a trend μ and two barriers. Whether the process chooses an option or the other corresponds to whether the upper or the lower barrier is hit first. The response time is the time at which the first barrier is hit. Alós-Ferrer (2018) shows that assumptions (R) and (P) above follow immediately if one assumes that the drift rate of the more automatic process is larger in absolute value than the drift rate of the more deliberative process, which in turn simply captures that the former is swifter than the latter.

It is important to emphasize that the response time of a given behavioral rule can never be actually observed, because any given choice (even if it is the choice most likely selected by a given rule) might originate from any behavioral rule. Thus, predictions can not rely on an assignment of choices to rules without falling prey to a reverse inference

[1] Here "error" just means a response other than the one prescribed more often by the rule, and it is not taken in a normative sense. An "error" for the intuitive rule might be to choose the same option as the deliberative rule when both are in conflict.

fallacy (Krajbich et al., 2015). This problem can be avoided by concentrating on averages which do not condition on particular choices (e.g., response times under high vs. low cognitive load, across all decisions). A different way to derive testable predictions rests on the concepts of *alignment* and *conflict*. Recall that x^D and x^I are the choices made more often by the rules D and I, respectively. In this sense, they are the *prescriptions* of the rules, even if those do not always select them. We speak of *conflict* if the behavioral rules make different prescriptions ($x^D \neq x^I$), and of *alignment* if both behavioral rules make the same prescription ($x^D = x^I$).

This distinction is important. First, in some experiments, the prescriptions of the behavioral rules might be clear beforehand, hence observable. For example, a myopic best reply can be computed *ex ante*, even if a noisy best-reply rule does not always select it. An imitative rule will prescribe to follow the alternative with the highest observed payoffs, even if the actual choice sometimes deviates from that prescription. A rule approximating normatively optimal behavior will deliver clear prescriptions, even if the actual rule is error-prone. Thus, once the experimenter has focused on two particular rules, whether a specific decision happens under conflict or under alignment might be *ex ante* observable.

Second, in any experiment relying on cognitive load, the assumption is that the shift to more intuitive processes will result in an observable change in behavior. This might not always be the case, however. Intuitive processes are in themselves not flawed: rather, they have evolved because they economize cognitive resources while delivering a good response in evolutionarily typical situations. Hence, in many cases, they will actually prescribe the same response as more deliberative processes (alignment). It is only when they are used in an evolutionarily new situation that they will conflict with the latter and prescribe erroneous or suboptimal responses. In particular, no effects on behavior (e.g., performance impairments) should be expected in a situation of alignment.

3.2 Response Times Effects

Our first result is straightforward. Even though the response times of individual processes (conditional on process selection) are unobservable (because any choice might have been selected by any process), observable response times are a convex combination of the response times of the different processes. The effects of cognitive load on response times for tasks in the economic domain are then rather intuitive. Cognitive load shifts the balance toward more impulsive/automatic processes, that is, the percentage of decisions accruing to such processes increases. Since automatic processes are faster, one immediately obtains the apparently paradoxical conclusion that response times must *decrease* under cognitive load. This is captured by the following straightforward result.

Theorem 1. *Assume (R) and (L). Under cognitive load,*

(H1a) the expected response time decreases; and

(H1b) the expected response time conditional on either conflict or alignment decreases.

Proof. The expected response time is $(1-\Delta)R^D + \Delta R^I = R^D + \Delta(R^I - R^D)$. Since $R^I < R^D$ by (R), this quantity decreases under cognitive load by (L). This is independent of whether one conditions to conflict or alignment. $\square$

Note that (H1b) is still true if one allows for differences in Δ across conflict and alignment situations (for instance, it might be reasonable to assume that Δ is smaller in case of conflict, reflecting conflict detection and resolution by the central executive). In this latter case, (H1a) also holds, provided the experiment avoids confounds which would alter the proportion of decisions of each type across cognitive load treatments. As we will show below, behavioral effects under cognitive load should only be expected (if any) in case of conflict, and hence we consider it preferable to concentrate on the conditional prediction (H1b) in experiments where the distinction between conflict and alignment is observable, and revert to (H1a) if not. In the experiments we report on below, we will face both kinds of situations.

We also remark that, to keep the model simple, we have assumed that *process* response times in themselves are unaffected by load. This can be easily generalized. In particular, a natural model of cognitive load in terms of drift-diffusion processes would be to assume that the process barriers are lowered, resulting in lowered process consistency (more randomness). This immediately results in *faster* process response times, which adds to the effect shown above.

In tasks proper of cognitive psychology, where response times are extremely short, the effect identified in Theorem 1 is likely to be small and other, more mechanical effects might dominate. However, shorter response times under cognitive load have been observed in a few studies using complex tasks (most cognitive load studies in economics do not report response times). Specifically, Whitney et al. (2008) observe this effect in a study on framing under phonological-loop cognitive load, and Gerhardt et al. (2016) report shorter response times in lottery choices when using a cognitive load manipulation targeting the visuospatial sketchpad. However, those studies delivered no explanation for the effect. Whitney et al. (2008) conjectured that participants speeded up their decisions "in order to maintain high accuracy" (see Section 7 for further discussion).

3.3 Behavioral Effects

The effect of cognitive load on choice frequencies, however, is less than straightforward. It is often argued that cognitive load should increase the frequency of those decisions prescribed (selected most frequently) by the more impulsive behavioral rules. This intuitive conclusion, however, depends on additional assumptions and might be false in general. To substantiate this claim, we start by noting that, in addition to the process shift captured by (L), cognitive load is likely to affect choice frequencies for individual processes. According to the literature reviewed in Section 2, processes relying on cognitive resources will be selectively impaired. This leads to the following assumption.

(B1) P^D decreases strictly under cognitive load, and $P^D(x)$ increases weakly for all $x \neq x^D$.

This assumption states that the more deliberative behavioral rule becomes more noisy, hence selecting the deliberative choice less often (and all other options at least as often). This assumption is natural. In the domain of cognitive psychology, where automatic processes are pure stimulus-response reflexes, it is also natural to assume that they do not rely on cognitive resources and should be unaffected by cognitive load. Even though dual-process theories often speak of deliberative and automatic processes for simplicity, the automaticity dimension is actually viewed as a continuum (e.g., Bargh, 1989). The actual postulate is that decision processes in the human mind differ in their degree of automaticity. We subscribe the view that the intuitive rule is a *more* automatic behavioral rule than the deliberative rule (hence our assumptions (R) and (P)), but we would not assume that it is void of any cognitive/deliberative content.

Alas, if the intuitive behavioral rule can also be affected by cognitive load, then no predictions can be made in terms of choice frequencies, as the following example shows.

Example 1. Consider a situation of conflict, $X = \{x^D, x^I, y, z\}$ with $x^D \neq x^I$. Let $P^D = 0.4$, $P^D(x) = 0.2$ for all $x \neq x^D$, $P^I = 0.7$, and $P^I(x) = 0.1$ for all $x \neq x^I$. Denote choice probabilities under cognitive load with the subscript L. Let $P_L^D = 0.25 + 3\varepsilon$, $P_L^D(x) = 0.25 - \varepsilon$ for all $x \neq x^D$, $P_L^I = 0.4$, and $P_L^I(x) = 0.2$ for all $x \neq x^I$, with $0 < \varepsilon < 0.05$. Further, let $\Delta = 0.25 - \delta$ and $\Delta_L = 0.5$, with $-0.25 < \delta < 0.25$. This example fulfills (L), (B1), and (P) both with and without cognitive load. Further, x^D is the modal response of the deliberative process both with and without load, and analogously for x^I. The probabilities of an intuitive choice with and without cognitive load are

$$\begin{aligned} P(x^I|\text{Load}) &= 0.25 - \varepsilon + 0.5(0.4 - 0.25 + \varepsilon) = 0.325 - 0.5\varepsilon, \\ P(x^I|\text{No Load}) &= 0.2 + (0.7 - 0.2)(0.25 - \delta) = 0.325 - 0.5\delta. \end{aligned}$$

Thus,

$$P(x^I|\text{Load}) - P(x^I|\text{No Load}) = 0.5(\delta - \varepsilon)$$

which can take positive, negative, or zero values in the admissible ranges of ε, δ.

The conclusion that cognitive load should lead to more intuitive choices in case of conflict, however, can only be reached under the strong additional assumption that the intuitive rule is purely automatic and hence unaffected by cognitive load.

(B2) The probabilities $P^I(x)$ are unaffected by cognitive load.

The following result makes this observation explicit. However, we remark that we do not expect the data to conform to this prediction because we consider (B2) unwarranted.

Theorem 2. *Assume (P) holds with and without cognitive load. Under (L), (B1), and (B2),* *(H2) in case of conflict, the frequency of intuitive choices increases under cognitive load.*

Proof. Let all probabilities under cognitive load be denoted with the subscript L. The probability of intuitive choices under cognitive load is

$$P(x^I|\text{Load}) = (1-\Delta_L)P_L^D(x^I) + \Delta_L P^I = (1-\Delta_L)P_L^D(x^I) + (\Delta_L - \Delta)P^I + \Delta P^I$$

where $\Delta_L - \Delta > 0$ by (L) and the probability of x^I under the intuitive process is unaffected by load by (B2). Note that $P^I > P_L^D > P_L^D(x^I)$ by (P) and the definition of x^D. Hence,

$$P(x^I|\text{Load}) > (1-\Delta)P_L^D(x^I) + \Delta P^I \geq (1-\Delta)P^D(x^I) + \Delta P^I = P(x^I|\text{No Load})$$

where the second inequality follows from (B1). □

Even under the strong assumption (B2), however, the prediction does not extend to situations of alignment, as the following example shows.

Example 2. Consider a situation of alignment, $X = \{x^D, y, z, w\}$ with $x^D = x^I$. As in the previous example, let $P^D = 0.4$, $P^D(x) = 0.2$ for all $x \neq x^D$, $P^I = 0.7$, and $P^I(x) = 0.1$ for all $x \neq x^D$. Denote choice probabilities under cognitive load with the subscript L. Let again $P_L^D = 0.25 + 3\varepsilon$, $P_L^D(x) = 0.25 - \varepsilon$ for all $x \neq x^D$, with $0 < \varepsilon < 0.05$, and $\Delta = 0.25 - \delta$ and $\Delta_L = 0.5$, with $-0.25 < \delta < 0.25$. Contrary to the last example, assume $P_L^I(x) = P^I(x)$ for all $x \in X$. This example fulfills (P) both with and without cognitive load, and also (L), (B1), and (B2). The probabilities of an imitative choice with and without cognitive load are

$$\begin{aligned} P(x^I|\text{Load}) &= 0.25 + 3\varepsilon + 0.5(0.7 - 0.25 - 3\varepsilon) = 0.475 + 1.5\varepsilon \\ P(x^I|\text{No Load}) &= 0.4 + (0.7 - 0.4)(0.25 - \delta) = 0.475 - 0.3\delta. \end{aligned}$$

Thus,

$$P(x^I|\text{Load}) - P(x^I|\text{No Load}) = 1.5\varepsilon + 0.3\delta$$

which again can be positive, negative, or zero in the admissible ranges of ε, δ.

Theorem 2 and Examples 1 and 2 show that, in economic multi-alternative decision making, cognitive load might often fail to produce measurable results on choice frequencies. First, the natural hypothesis in the choice domain follows only if the strong assumption (B2) is made, or equivalently if the postulated intuitive process is of purely automatic nature, that is, it places no demands on cognitive resources (or, by continuity, very low demands). Second, even under that assumption, the result only follows in case of conflict and might not obtain if conflict and alignment are not clearly distinguished. This observation is of independent interest given that cognitive load manipulation often fail to deliver results in economic tasks.

In summary, we view the strong prediction (H1a,b) derived from Theorem 1 as a manipulation check which can be used to verify that cognitive load was successfully implemented. Once this is established, we view the additional prediction (H2) derived from Theorem 2 as a test of the additional assumption (B2) on the nature of the intuitive behavioral rule.

4 Experiments 1–3: Cournot Markets

In this section we discuss three different experiments where participants took the role of firm managers in Cournot oligopolies. In this particular strategic setting, previous evidence suggests that two specific behavioral rules are particularly important. On the one hand, *myopic best reply* captures one-step payoff maximization and can be seen as a simple proxy of deliberative thinking. On the other hand, a large strand of research has suggested *imitation* of successful strategies as an alternative rule governing behavior. Theoretical results by Schaffer (1989) and Vega-Redondo (1997) have shown that imitation in Cournot oligopolies mimics maximization of relative payoffs and, if firms follow imitative behavioral rules and make infrequent mistakes, the resulting stochastic dynamics converges to the Walrasian equilibrium (and not to the Cournot-Nash equilibrium). This result extends to a larger class of economic interactions (aggregative games Alós-Ferrer and Ania, 2005). A number of laboratory experiments on Cournot oligopolies have found partial convergence to Walrasian outcomes, which can be taken as indirect evidence for the presence of imitative behavior (Huck et al., 1999; Offerman et al., 2002; Apesteguía et al., 2007, 2010). Buckert et al. (2017) conducted a Cournot oligopoly experiment adding an additional task which required attention in some trials (which could be interpreted as a form of cognitive load), and found evidence compatible with increased reliance on imitation. However, Bosch-Domènech and Vriend (2003) found no stronger reliance on imitation in a Cournot oligopoly experiment when cognitive demands were increased by implementing time limits and describing payoff tables in an inconvenient way. Alós-Ferrer and Ritschel (2020) measured response times in a Cournot oligopoly experiment and found evidence of multiplicity of behavioral rules along the lines of myopic best reply and imitation.

In all three experiments below, the prescriptions of myopic best reply and imitation can be determined *ex ante* for each individual decision. Thus, tests can be made conditional on conflict or alignment. The experiments used different cognitive load tasks and within vs. between designs. For ease of exposition, we first present the shared experimental design and then the cognitive load manipulations. Finally, we discuss the results for response times (predictions H1a,b) and choices (prediction H2) for all three experiments.

4.1 Shared Experimental Design

Participants in Experiments 1–3 interacted in 4-player Cournot oligopolies (tetrapolies). The design (except for the cognitive load manipulations discussed in the next subsection) followed Alós-Ferrer and Ritschel (2020). Subjects participated in three different oligopolies (parts), with 17 rounds each (total of 51 rounds). For each part, we computed a payoff table using a Cournot oligopoly with zero costs and a linear inverse demand function of the form $P(Q) = a - Q$, where $P(\cdot)$ is the inverse demand function, a the saturated demand, and Q the total quantity in the market. During the experiment a neutral framing was used and neither firms nor quantities were mentioned. We reduced the action space to four possible actions, i.e. A, B, C, and D with either increasing or decreasing quantities from A to D.[2] Hence, the whole payoff table had dimensions 4×20, with four rows representing the possible actions and 20 columns labeled AAA to DDD representing the possible actions of the opponents.

Payoffs were expressed in points, with an exchange rate of 20 Eurocents per 1000 points. The points achieved in all 51 rounds were accumulated and paid at the end of the experiment. After the first round the participants were informed about the outcome of the previous round. Before making the next choice, participants saw the full payoff table, their own choice and earnings from the previous round, and the previous choice and earnings from the other three group members. The first round in each part did not provide any information on the previous round and was therefore dropped for the analysis, yielding 16 rounds in each part for a total of 48 rounds.

4.2 Experimental Procedures and Cognitive Load Manipulations

Experiments 1–3 were conducted at the Cologne Laboratory for Economic Research (CLER), University of Cologne, and programmed in z-Tree (Fischbacher, 2007). Participants were recruited using (Greiner, 2015), and were students from the University of Cologne excluding those with majors in Psychology, Economics, or Advanced Business Administration. They received a performance-based payment plus a show-up fee of 2.50 Euro.

4.2.1 Experiment 1: High-Demand Load (Between)

In Experiment 1, we ran 6 sessions with 24 participants each for a total of $N = 144$ (87 females; age range 18–39 years, mean 23.2 years). The experiment was conceived as a between-subject manipulation, with 72 subjects in a *Load* treatment and the remaining 72 in a *No Load* treatment (three sessions each). Average earnings, including the show-up fee, were 13.61 Euro and 20.12 Euro under No Load and Load, respectively.

[2] The three parts were implemented to avoid that data would be rendered meaningless by convergence to the Walrasian outcome, since after convergence occurs, there is no behavioral variance. Payoff table 1: $P(Q) = 150 - Q$, $A = 37.5$, $B = 33.25$, $C = 30$, $D = 18.75$ (or reversed); Payoff table 2: $P(Q) = 175 - Q$, $A = 43.75$, $B = 38.875$, $C = 35$, $D = 21.875$ (or reversed); Payoff table 3: $P(Q) = 200 - Q$, $A = 50$, $B = 44.5$, $C = 40$, $D = 25$ (or reversed).

Participants in the Load treatment earned more due to the additional earnings in the cognitive load task; excluding those (earnings from the main decision in the Load treatment 14.06 Euro), average earnings were not significantly different across treatments (MWW, $N = 144$, $z = -1.489$, $p = .1365$). A session lasted around 85 and 105 minutes in the No Load and Load treatments, respectively.

In the Load treatment, participants were asked to memorize a seven-digit number which was displayed for 10 seconds before each Cournot oligopoly decision, and recall it after that decision (within 10 seconds). Memorizing a number is a common cognitive load task targeting the phonological loop and has been implemented in a variety of experiments (Roch et al., 2000; Hinson et al., 2002; Lavie and de Fockert, 2005; Carpenter et al., 2013). Correct recall was incentivized with an additional 750 points. As a comparison, participants earned an average of 1200 points per round from the Cournot oligopoly decision. In the No Load treatment, no load was present during the whole experiment.

4.2.2 Experiment 2: High-Demand Load (Within)

Data was collected in two sessions with 28 and 32 participants, respectively, for a total of 60 participants (36 female; age range 18–70 years, mean 26.3 years). Average earnings were 17.67 Euro (ranging from 12.70 to 21.70 Euro including the show-up fee). A session lasted about 105 minutes.

The cognitive load manipulation was the same as in Experiment 1, but was implemented within-subject. In each of the three parts, 8 rounds corresponded to Load and 8 to No Load. The very first round of each part, excluded from the analysis, was also under No Load. Again, correct recall was incentivized with an additional 750 points. Rounds without cognitive load included no memorization task.

4.2.3 Experiment 3: Low-Demand Load (Within)

Data was collected in two sessions of 32 participants each for a total of 64 participants (28 female; age range 18–33 years, mean 24.6 years). Average earnings were 18.45 Euro (ranging from 15.00 to 26.50 Euro, including the show-up fee). A session lasted about 105 minutes.

As in Experiment 2, we implemented two within-subject treatments, *Load* and *No Load*, but relied on a lower-intensity (easier) cognitive load manipulation targeting the phonological loop. In each of the three parts, 8 rounds corresponded to Load and 8 to No Load. The very first of each part, excluded from the analysis, was also under No Load. For rounds with cognitive load, participants were asked to memorize a single-digit number which was displayed for 5 seconds before the Cournot oligopoly screen appeared. During the Cournot oligopoly decision task, the participants heard another single-digit number via headphones which was played at a random time between 1 and 10 seconds. After the Cournot decision was made, participants had and enter the sum of the two numbers in a new screen.[3] The cognitive load task was incentivized and each correct answer earned an additional 750 points. Rounds without cognitive load included no additional task.

4.3 Results: Response Times

To test predictions H1a,b, we computed the individual-level average response times for decisions taken in the No Load and Load treatments, for each of the Experiments 1–3. First, we confirm prediction H1a for all three experiments. In Experiment 1 (between), participants in the Load treatment were on average faster (9.43 s) than those in the No Load treatment (13.15 s; Mann-Whitney-Wilcoxon test, MWW, $N = 144$, $z = -4.962$, $p < .0001$). In Experiment 2 (within), participants took on average 9.89 s for rounds under load and 14.90 s for those without load (Wilcoxon Signed-Rank test, WSR, $N = 60$, $z = -6.589$, $p < .0001$). In Experiment 3 (within), using the easier cognitive load task, participants took on average 14.34 s under load and 15.22 s without load. This is a smaller but still significant difference (WSR, $N = 64$, $z = -3.110$, $p = .0019$).

Figure 1 displays the averages of the individual-level average response times for decisions taken in the No Load and Load treatments, for each of the Experiments 1–3. Data is split according to whether the decisions in each round were made under conflict or alignment (that is, whether the imitative choice and the myopic best reply differ or coincide, respectively), as required to test for Prediction H1b.

Indeed, we confirm Prediction H1b in conflict situations for all three experiments. The predicted relation holds between subjects for Experiment 1 (Load, 9.30 s; No Load treatment, 13.10 s; MWW, $N = 144$, $z = -5.110$, $p < .0001$), and within subjects for Experiment 2 (Load rounds, 9.77 s; No Load rounds, 14.85 s; WSR, $N = 60$, $z = -6.522$, $p < .0001$) and Experiment 3 (Load rounds, 14.22 s; No Load rounds, 15.28 s; WSR, $N = 64$, $z = -3.397$, $p = .0007$). The prediction also holds for alignment situations in Experiment 1 (Load, 10.41; No Load, 13.32 s; MWW, $N = 144$, $z = -3.548$, $p = .0004$) and Experiment 2 (Load rounds 10.28 s; No Load rounds, 15.78 s; WSR, $N = 57$, $z = -5.145$, $p < .0001$).[4] In Experiment 3, relying on the easier cognitive load task, participants were also faster on average in Load rounds (14.60 s) compared to No Load rounds (15.33 s), but the difference was not significant (WSR, $N = 63$, $z = -1.280$, $p = .2005$).

4.4 Results: Behavior

The previous subsection shows that the cognitive load manipulations were implemented successfully in Experiments 1–3. Following the standard logic of cognitive load manipulations, one would expect a shift toward more intuitive decisions, which in this case

[3] This design makes the manipulation closer to Buckert et al. (2017), who used a concurrent "distraction" task. We thank Ronald Hübner for suggesting this manipulation.

[4] The number of observations changes across tests because not all subjects faced decisions in alignment situations.

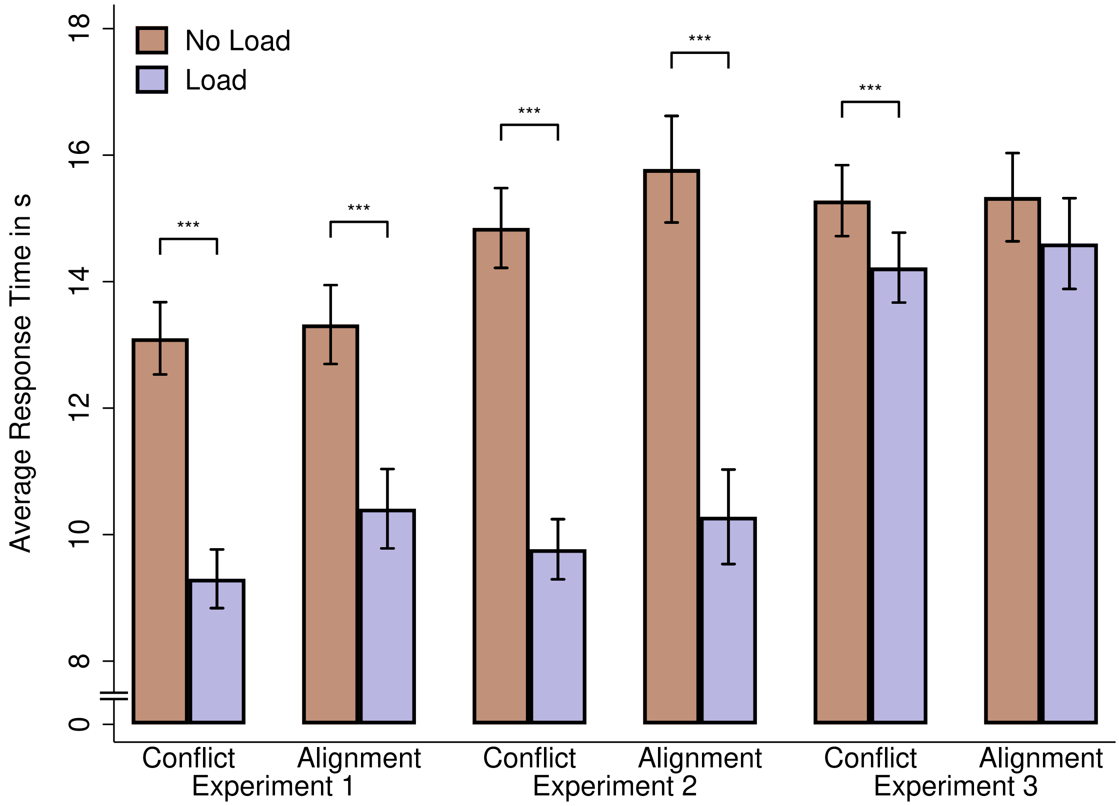


Figure 1: Cournot oligopoly experiments. Average response times of decisions under load and no load in conflict and alignment, Experiments 1–3. MWW (Experiment 1) and WSR tests (Experiments 2 and 3), * $p < .1$, ** $p < .05$, and *** $p < .01$.

means more imitative choices. By virtue of Theorem 2, our model would actually support this prediction, but only for decisions under conflict, and only if we accept the strong additional assumption (B2). In our Cournot oligopoly experiments, this assumption states that imitation should be unaffected by cognitive load, which we consider implausible. Imitation can be assumed to be *less* deliberative than myopic best reply, but it is unlikely to be a purely automatic process not relying on any cognitive resources.

Figure 2 displays the relative frequency of imitative choices in conflict situations for Experiments 1–3, across (between or within) treatments. There were, however, no significant differences in Experiment 1 (Load subjects, 37.49% imitative choices; No Load subjects, 34.97%; MWW, $N = 144$, $z = 0.452$, $p = .6516$) or in Experiment 3 (Load rounds, 30.42%; No Load rounds, 31.22%; WSR, $N = 64$, $z = -0.174$, $p = .8620$). In Experiment 2, the relative frequency of imitation did increase significantly under cognitive load (Load rounds, 34.96%; No Load rounds, 31.79%; WSR, $N = 60$, $z = 2.05$, $p = .0403$). In summary, results are mixed and do offer only weak or no support for prediction (H2) and assumption (B2).

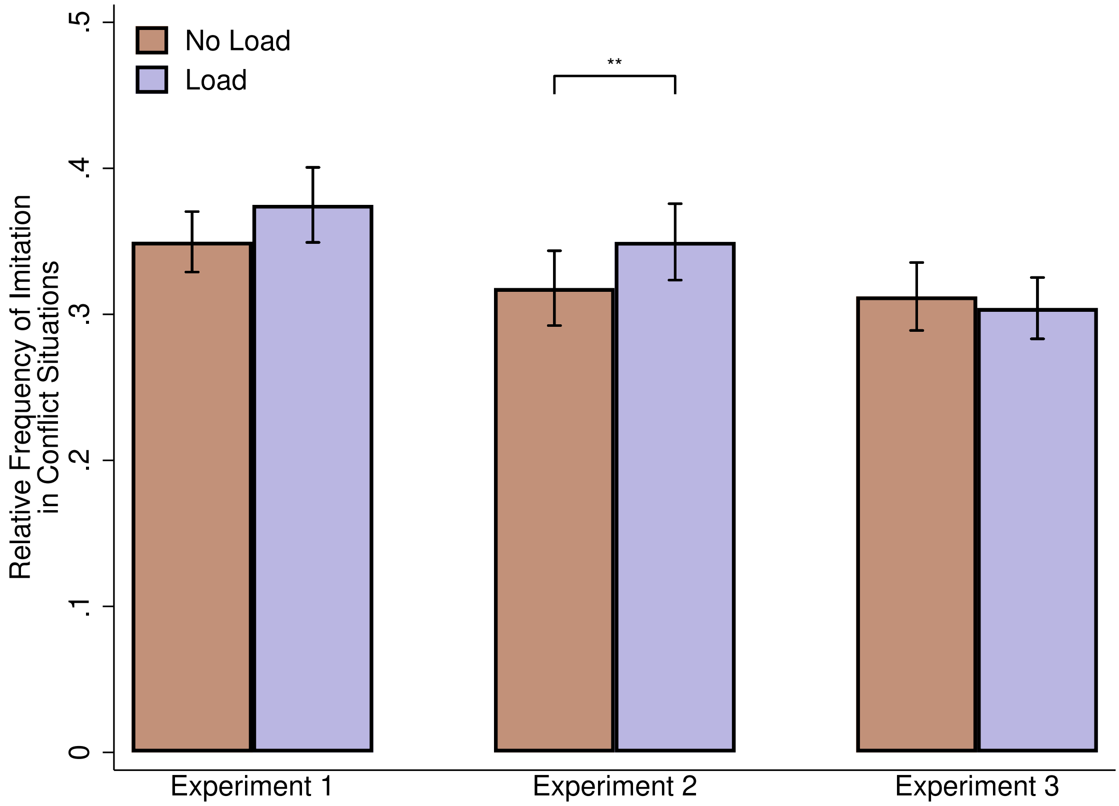


Figure 2: Cournot oligopoly experiments. Relative frequency of imitation decisions in conflict situations, Experiments 1–3. MWW (Experiment 1) and WSR tests (Experiments 2, 3), * $p < .1$, ** $p < .05$, and *** $p < .01$

4.5 Discussion (Experiments 1–3)

Cournot oligopoly experiments with a large payoff table deliver an example of economically relevant but relatively complex individual decisions. In three separate experiments using both between and within settings and relying on two different cognitive load manipulations, we show that decisions under cognitive load are, as predicted by (H1a,b), faster under cognitive load. The difference in response times remains as expected when disentangling decisions according to whether they were made under conflict or under alignment.

The cognitive load manipulation we use in Experiments 1 and 2 is widely used in the literature. Using this manipulation, the effect on response times is relatively large. The effect is much smaller (although still generally significant) in Experiment 3, suggesting that the manipulation we used in this case was indeed weaker. To substantiate this claim, we computed the individual-level difference in average response times between No Load and Load in case of conflict in Experiments 2 and 3 (which both involved within-subject manipulations; we focused on conflict because the basic effect is significant in both experiments in this case). The difference was significantly larger in Experiment 2 (5.08 s) compared to Experiment 3 (1.06 s; MWW test, $N = 124$, $z = 5.980$, $p < .0001$).

In particular, we conclude that our manipulations successfully impaired cognitive resources. In spite of this, actual effects on behavior were obtained only in Experiment 2, yielding weak support to the conventional wisdom that impairing cognitive resources should increase the frequency of the actions (most often) prescribed by (more) intuitive processes. This is, however, compatible with the view that cognitive load might also partially affect the inner workings of more (but not fully) intuitive processes, for in this case assumption (B2) is unwarranted and prediction (H2) does not necessarily follow.

5 Experiments 4–6: Voting Decisions

In this section, we discuss three voting experiments where participants took the role of committee members and voted for different options according to two voting methods. One reason to use voting experiments is that, as we will discuss below, they constitute an example of a complex situation where, even though there are natural candidates for two different behavioral rules, the actual prescriptions do not suffice to distinguish conflict and alignment *ex ante*. However, our results still make a clear prediction (H1a) and the standard logic behind cognitive manipulations still suffices to identify an expected behavioral effect.

An important objective of a voting method is to elicit and represent the electorate's preferences faithfully. However, theoretical results in social choice theory have shown that any voting method within a wide family is manipulable and creates incentives to vote strategically, misrepresenting the own actual preferences (Gibbard, 1973; Satterthwaite, 1975). This is especially true for Plurality Voting, where each voter casts a single vote for his or her most-preferred alternative, a method which forms the basis of most actual electoral methods in use in Western societies. A particular problem is the "wasted vote" effect, where voters refrain from supporting their actually-preferred candidate or party in the belief that its winning chances are too small, supporting a popular alternative instead not because they actually prefer it, but because it is the least-disliked among those likely to win.

An alternative method which partially escapes manipulability (because it does not belong to the class covered by the results mentioned above) is Approval Voting (Brams and Fishburn, 1978; Alós-Ferrer and Buckenmaier, 2019). In this method, voters can vote for ("approve of") as many alternatives as they see fit, with the winner determined by simple majority of approvals. In particular, Approval Voting escapes the waste vote effect, since approving of a non-favorite option can be accomplished by merely moving the approval threshold without misrepresenting preferences, and, in particular, without disapproving of the favorite option. Voting field experiments have provided evidence that election outcomes might greatly differ if Approval Voting were used instead of more-established methods (Laslier and Van der Straeten, 2008; Alós-Ferrer and Granić, 2012, 2015).

In the context of voting, hence, the natural behavioral rules to consider are *sincere voting* vs. *strategic behavior.* Since the latter requires reasoning about the likely behavior of others, it should correspond to a more deliberative mode of thinking. This is also in agreement with the more general view that sincerity is an intuitive reaction, e.g. as compared to dishonest behavior (Cappelen et al., 2013; Fischbacher and Föllmi-Heusi, 2013).

In contrast to the experiments in the previous section, the actual prescriptions of one of the postulated behavioral rules are unclear. This is because there is considerable heterogeneity in strategic behavior (Stahl and Wilson, 1995; Ho et al., 1998), and hence the actual prescriptions in this case would depend on a variety of individual correlates including cognitive capacity. Thus, although in the experiments below it is always possible to determine whether a decision was sincere or not, it is not possible to classify decisions as happening in conflict or alignment. This, however, is no obstacle for our analysis, because prediction (H1a) does not rely on this classification. As for effects on behavior, though, this is an example where conventional wisdom would expect a shift toward more intuitive behavior (in this case, sincere voting) under cognitive load, but actual theoretical results are lacking, since prediction (H2) does hinge on decisions being made under conflict.

The experiments again used different cognitive load manipulations, but were all within-subject. As in the previous section, we first present the common experimental design, then the cognitive load manipulations, and finally the results for response times and voting decisions for all three experiments.

5.1 Shared Experimental Design

For Experiments 4–6, we considered a complex voting decision. The decision task was strictly individual, because no feedback on voting outcomes was provided until the end of the experiment. We relied on the standard design of voting experiments following Forsythe et al. (1993, 1996) (see also Granić, 2017). Specifically, the main decision task was to cast a vote using different voting methods, implemented in separate blocks.

Participants were allocated to groups of six voters each and cast their votes for four possible alternatives, A, B, C, and D. In each group, there were three voter types, with two participants randomly allocated to each type. They were confronted with "societies" represented by payoff profiles which consisted of a payoff outcome for each possible alternative and each type, i.e. a 3×4 payoff table. Votes were cast according to either Plurality Voting or Approval Voting. Participants voted multiple times in two different voting blocks, one per method. The order of methods was counterbalanced across participants.

Under Plurality Voting, each participant voted for exactly one of the alternatives and the alternative with the most votes won. Under Approval Voting, each participant voted for as many alternatives as she approved of and the alternative with the most

Table 1: Voter Profiles, Experiments 4–6. Societies 1 and 2 were used in experiment 4; Societies 3 and 4 were used in experiments 5–6.

Society 1 (Exp. 4)

Voter	#	A	B	C	D
Type 1	2	60	50	70	80
Type 2	2	70	80	50	60
Type 3	2	70	60	80	50

Society 2 (Exp. 4)

Voter	#	A	B	C	D
Type 1	2	50	70	80	60
Type 2	2	50	80	70	60
Type 3	2	80	70	50	60

Society 3 (Exp. 5,6)

Voter	#	A	B	C	D
Type 1	2	60	50	70	80
Type 2	2	70	80	50	60
Type 3	2	70	60	80	50

Society 4 (Exp. 5,6)

Voter	#	A	B	C	D
Type 1	2	50	60	80	70
Type 2	2	50	80	70	60
Type 3	2	80	70	50	60

approvals won. Ties were broken randomly.[5] At the end of the experiment, one voting round was randomly drawn and the winning alternative was determined according to the voting method and the votes of all members of the group.

Experiment 4 used the payoff profiles of Societies 1 and 2 in Table 1, while Experiments 5 and 6 used Societies 3 and 4. The exchange rate was 12 Eurocents per point. In each experiment, each payoff profiles was used four times per voting method, but the payoffs were jittered using small random perturbations which did not alter the ordinal relation among outcomes. Furthermore, the names of the alternatives were shuffled and the rows in the payoff profile rearranged to avoid demand for consistency. In Experiment 4, each voter's (actual) type also changed across voting decisions, while in Experiments 5 and 6 it was fixed.

5.2 Experimental Procedures and Cognitive Load Manipulations

Procedures and recruitment for Experiments 4–6 were as those for Experiments 1–3, including software platforms. Participants were students from the University of Cologne excluding those with majors in Psychology, and those who had participated in previous voting experiments. They received a performance-based payment of 4 Euro (as the lab-mandated fee had increased with respect to Experiments 1–3).

5.2.1 Experiment 4: High-Demand Load (Within)

In Experiment 4, we ran 2 sessions with 30 participants each for a total of $N = 60$ (38 females; age range 18–32 years, mean 23.1 years). Average earnings were 18.29 Euro

[5]The experiment included a third "voting method" in the form of an incentive-compatible preference elicitation task as in Alós-Ferrer and Buckenmaier (2020), but there were no qualitative differences between elicited preferences and payoff-induced preferences.

Table 2: Order of Cognitive Load Rounds and Payoff Profiles Within a Voting Block. Top: Experiment 4; Bottom: Experiments 5–6.

Round	1	2	3	4	5	6	7	8
Load	No	Yes	No	Yes	Yes	No	No	Yes
Society	1	2	2	1	2	1	2	1

Round	1	2	3	4	5	6	7	8	9	10
Load	No	Yes	Yes	No	No	No	Yes	No	No	Yes
Society	3	4	3	4	Filler	Filler	3	4	3	4

(ranging from 12.00 to 22.20 Euro including the show-up fee[6]). A session lasted around 75 minutes.

The cognitive load manipulation was the same as in Experiment 2, and also implemented within subjects. The payoff for correct recall was 40 points (one round was randomly selected for payment). Table 2(top) details the order of payoff profiles and treatments within each block of voting decisions. Payoff profiles were jittered independently each voting round.

5.2.2 Experiment 5: High-Demand Load (Within)

In Experiment 5, we ran 4 sessions with 30 participants each for a total of $N = 120$ (68 females; age range 18–30 years, mean 23.3 years). Average earnings were 15.53 Euro (ranging from 9.60 to 18.80 Euro including the show-up fee). A session lasted around 75 minutes.

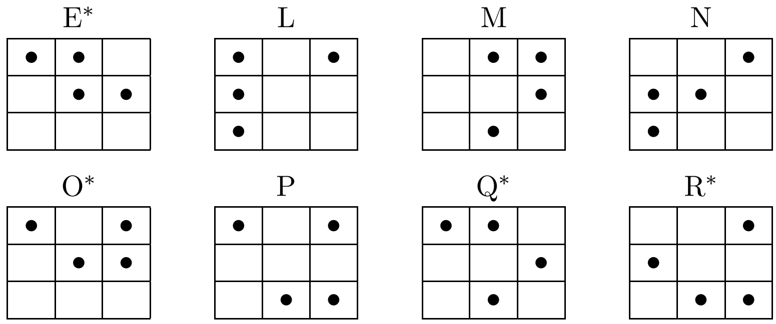
The cognitive load manipulation was as in Experiments 2 and 4, and also implemented within subjects. The payoff for correct recall was reduced to 30 points (one round was randomly selected for payment). Table 2(bottom) details the order of payoff profiles and treatments within each block of voting decisions. There were two filler rounds with additional (different) profiles without load. Payoff profiles for Societies 3 and 4 were jittered twice, so that the exact same profiles were presented after and before the filler tasks (and each profile was faced with and without load), but participants saw four different profiles before the filler tasks, and four different profiles after them.

5.2.3 Experiment 6: Taxing the Visuospatial Sketchpad

In Experiment 6, we ran 4 sessions with 30 participants each for a total of $N = 120$ (74 females; age range 17–58 years, mean 23.1 years). Average earnings were 15.40 Euro (ranging from 9.20 to 18.80 Euro including the show-up fee). A session lasted around 65 minutes.

[6]Due to a programming error, each participant received 12 extra points at the end of the experiment.

Figure 3: Visual Load Grids, Experiment 6.



The voting task and voting block design was identical to Experiment 5. The cognitive load task, however, substantially differed from all previous experiments. We switched to another subsystem of working memory, the visuospatial sketchpad. The task we used required memorizing a visual pattern which cannot be easily (and silently) articulated as a number sequence as in the previous experiments. This task is widely used in the psychological literature (Bethell-Fox and Shepard, 1988; Miyake et al., 2001; De Neys, 2006; Franssens and De Neys, 2009; Trémolière et al., 2012; Johnson et al., 2016). The visual pattern consisted of a dot matrix displayed as a 3×3 grid containing 4 black and 5 white dots (see examples in Table 3).[7] The matrix was presented for 1 second and had to be recalled (by activating black dots in an empty grid) after the voting decision. The rest of the implementation details (including payment) were as in Experiment 5.

5.3 Results: Response Times

Since we cannot disentangle decisions in conflict and in alignment, we compute individual average response times differentiating decisions under Load and No Load. Prediction (H1a) then states that, if cognitive load has been successfully induced, decisions under Load must be significantly faster. Figure 4 displays the average of the individual average response times conditional on treatment, for each of the Experiments 4–6. Data is split according to voting method (PV=Plurality Voting, AV=Approval Voting).

We confirm prediction H1a for Experiments 4 and 5 under both voting methods. In Experiment 4, decisions under Load were on average faster than those under No Load both for Plurality Voting (Load, 15.32 s; No Load,21.77 s; WSR, $N = 60$, $z = -5.683$, $p < .0001$) and for Approval Voting (Load, 15.25 s; No Load, 22.01 s; WSR, $N = 60$, $z = -5.897$, $p < .0001$). The same holds for Experiment 5 (Plurality Voting: Load, 18.25 s; No Load, 23.18 s; WSR, $N = 120$, $z = -6.474$, $p < .0001$; Approval Voting: Load, 19.09 s; No Load, 24.49 s; WSR, $N = 120$, $z = -6.822$, $p < .0001$).

[7] The patterns were rotated versions of the following base patterns taken from Bethell-Fox and Shepard (1988): $E^*, L, M, N, O^*, P, Q^*, R^*$.

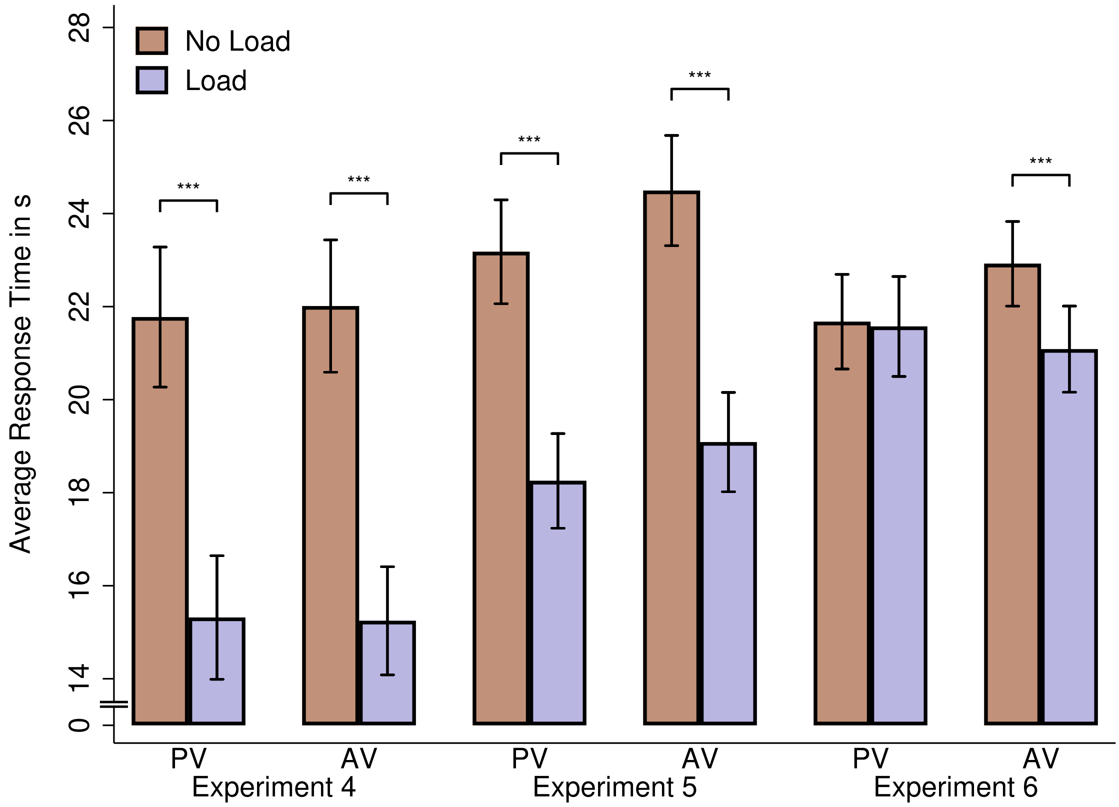


Figure 4: Average response times of voting decisions under load and no load in Plurality Voting (PV) and Approval Voting (AV), Experiments 4, 5, and 6. WSR test, * $p < .1$, ** $p < .05$, and *** $p < .01$.

In Experiment 6, prediction (H1a) was also confirmed under Approval Voting (Load, 21.09 s; No Load, 22.92 s; WSR, $N = 120$, $z = -3.245$, $p = .0012$), although the difference was of smaller magnitude. There was, however, no significant effect for Plurality Voting (Load, 21.57 s; No Load 21.68 s; WSR, $N = 120$, $z = -.720$, $p = .4714$).

5.4 Results: Behavior

The previous subsection shows that the cognitive load manipulations were implemented successfully in Experiments 4–6. In this case, the received logic behind cognitive load manipulations would lead us to expect a shift toward more sincere voting, reflecting the more deliberative nature of strategic behavior. However, our Theorem 2 would only support this prediction for decisions under conflict, and only if we accept the additional assumption (B2).

Sincere voting under Plurality Voting corresponds to voting for the most-preferred alternative. Under Approval Voting, a ballot is sincere if it includes all alternatives strictly preferred to any alternative in the ballot. Figure 5 displays the relative frequency of sincere votes for Experiments 4–6, across treatments and voting methods.

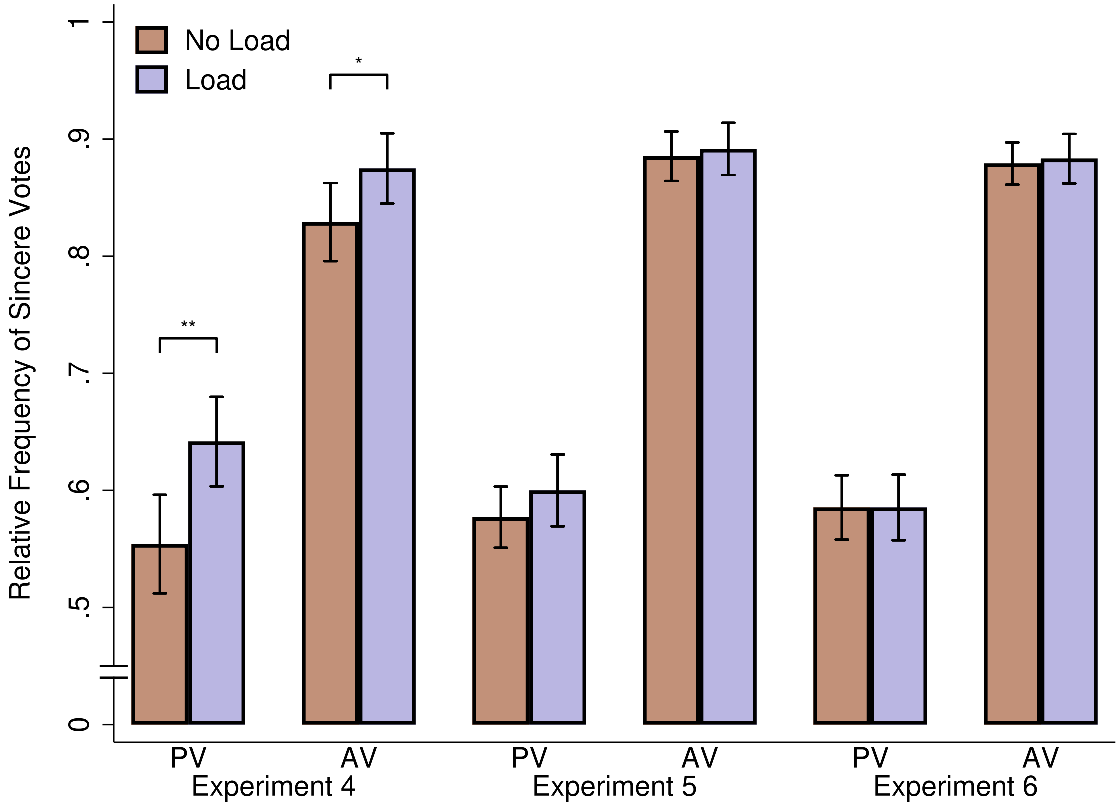


Figure 5: Relative frequency of sincere votes, Experiments 4, 5, and 6. WSR, * $p < .1$, ** $p < .05$, and *** $p < .01$

We find significant effects in Experiment 4. Under Plurality Voting, in this experiment, 64.17% of the decisions under Load were sincere, compared to 55.42% sincere votes under No Load (WSR, $N = 60$, $z = -2.260$, $p = .0238$). There was a marginally significant difference in the expected direction also for Approval Voting (Load, 87.50%; No Load, 82.92%; WSR, $N = 60$, $z = -1.683$, $p = .0924$).

In contrast, there were no significant differences for either method, neither in Experiment 5 (Plurality Voting: Load, 60.00%; No Load, 57.71%; WSR, $N = 120$, $z = -1.266$, $p = .2055$; Approval Voting: Load, 89.17%; No Load, 88.54%; WSR, $N = 120$, $z = -0.512$, $p = .6087$) nor in Experiment 6 (Plurality Voting: Load, 58.54%; No Load, 58.54%; WSR, $N = 120$, $z = 0.152$, $p = .8789$; Approval Voting: Load, 88.33%; No Load, 87.92%; WSR, $N = 120$, $z = -0.710$, $p = .4776$). Thus, results are again mixed and offer no strong support for the expected behavioral effects of cognitive load.

5.5 Discussion (Experiments 4–6)

Voting experiments involving even small committees (six members in our case) involve complex, strategic decisions which interact with the voting method in place. In three separate experiments using two different voting methods (Plurality and Approval Voting) and two different cognitive load manipulations, we show that decisions under cognitive load are, as predicted by (H1a), faster under cognitive load. The experiments are an example of a setting where, even though there exist clear candidates for the involved intuitive and deliberative processes, individual heterogeneity precludes identifying the prescriptions of the latter and hence differentiating conflict and alignment. However, Theorem 1 still delivers a prediction, which we readily find in the data.

Experiment 6 relied on a manipulation targeting the visuospatial sketchpad, instead of the phonological loop as most of our experiments. Thus, it is difficult to compare the strength of this manipulation with those of other ones from an *ex ante* point of view. However, our results show that the predicted difference in response times obtains only for one of the voting methods, and is of smaller magnitude than that found in other experiments, suggesting that the manipulation indeed differs from those targeting the phonological loop, and is most likely weaker. To substantiate this observation, we computed the individual-level differences in average response times between No Load and Load in Approval Voting in Experiments 5 and 6 (we focused on AV because the basic effect is significant in both experiments for this method). The difference was larger in Experiment 5 (5.41 s) than in Experiment 6 (1.83 s; MWW test, $N = 240$, $z = 3.777$, $p = .0002$). This is of independent interest, since the particular manipulation used in Experiment 6 is frequently used in the psychological literature.

We conclude that our manipulations also successfully impaired cognitive resources in our voting experiments. However, effects on behavior reflecting conventional expectations were obtained only in Experiment 4. As in Experiments 1–3, the overall picture is compatible with the view that cognitive load might also partially affect the more-intuitive processes at work in this paradigm.

6 Experiment 7: Bayesian Updating

In this section, we discuss an experiment which differs from the previous ones along several dimensions. First, we focus on a task which, although arising from the economics literature (Charness and Levin, 2005; Achtziger and Alós-Ferrer, 2014), involves much shorter response times (with averages between 1 and 3 s) than the ones in Experiments 1–6 and hence might be closer to experiments in cognitive psychology in this sense. Second, the task is completely non-strategic, in the sense that it does not involve thinking about other agents' decisions, but it is still relatively complex (as reflected by high error rates). Third, the experiment includes three treatments, a control condition and two cognitive load manipulations, and one of the latter is particularly taxing compared to previous ones (a "central executive" load).

Specifically, we rely on a belief-updating task using an urns-and-balls paradigm as typical of the judgment and decision making literature (e.g., Kahneman and Tversky, 1972; Grether, 1980, 1992), developed by Charness and Levin (2005) to study the possible conflict between Bayesian updating of beliefs and a simple win-stay, lose-shift reinforcement heuristic. This paradigm is interesting because participants can update their beliefs

State (Prob)	Left Urn	Right Urn
First (1/2)	●●●●○○	●●●●●●
Second (1/2)	●●○○○○	○○○○○○

Figure 6: Schematic representation of the task in Experiment 7.

in a normative way on the basis of received information, but the latter carries a win-loss frame, as is typical in many economic applications (project success vs. failure, firm's profits vs. losses, stocks going up or down, etc.). This frame cues basic reinforcement behavior, giving rise to a focus on past performance and well-known behavioral anomalies as *outcome bias* (e.g. Baron and Hershey, 1988). Charness and Levin (2005) showed that error rates in this paradigm are particularly high, and Achtziger and Alós-Ferrer (2014) used response times to show that the high error rates originate on reinforcement behavior. Achtziger et al. (2015) investigated the neural foundations of reinforcement behavior in this paradigm, and a number of other works have relied on it for further research (Charness et al., 2007; Hügelschäfer and Achtziger, 2017; Alós-Ferrer et al., 2017; Li et al., 2019).

In Experiment 7, thus, the behavioral rules we consider are a deliberative one implementing optimal decisions following Bayesian updating of beliefs (or simply "Bayesian updating" for short), and a more intuitive win-stay, lose-shift rule implementing a reinforcement-based heuristic. This experiment is an example of a paradigmatic comparison between deliberative and intuitive/automatic processes. On the one hand, it is well-known that human beings have notorious difficulties updating beliefs in a normative way (e.g., Kahneman and Tversky, 1972; Grether, 1980, among many others), and hence behavioral rules supporting normative behavior in this setting can be safely considered deliberative. On the other hand, evidence from neuroscience shows that reinforcement learning bears all the markers of automaticity and is associated with very fast and often-unconscious brain responses (e.g., Schultz, 1998; Holroyd and Coles, 2002).

6.1 Experimental Design

The decision task was as follows. There were two urns (left and right), each containing 6 balls, which could be black or white. Each participant completed 60 independent trials. In each trial, a state of the world (first or second) was realized, with probability 1/2 for each state (see Figure 6). In the first state of the world, the left urn consisted of 4 black and 2 white balls and the right urn of 6 black balls. In the second state of the world, the left urn consisted of 2 black and 4 white balls and the right urn of 6 white balls. All this information (but not the actually-realized state of the world) was known by participants.

In each trial, participants decided whether the left or the right urn should be used to extract a single ball, and received a payment of 18 Eurocents if and only if the ball was of a pre-specified color (say, black).[8] The extracted ball was replaced into the original urn, and participants had to choose an urn again, with a new ball being extracted and resulting in payment as in the first extraction. The focus of the analysis is on this second decision within each trial, as a rational decision maker should use Bayes' rule to update his or her beliefs on the state of the world on the basis of the feedback (black or white ball) from the first decision, but a reinforcer could use a simple "win-stay, lose-shift" heuristic and stick to the previous choice if and only if it was successful.

The composition of the urns was such that both behavioral rules (Bayesian updating and reinforcement) were always in conflict if the first extraction was from the left urn (i.e., Bayesian updating prescribes "win-shift, lose-stay"), and always in alignment if that first extraction was from the right urn (as the composition of the urns in that case revealed the state of the world); see Charness and Levin (2005) or Achtziger and Alós-Ferrer (2014) for details.

6.2 Experimental Procedures and Cognitive Load Manipulations

The experiment was carried out at the Social Psychology laboratory at the University of Konstanz (Germany), with each participant being measured individually and independently. Participants were 60 university students (21 female), randomly allocated to three different treatments. They earned 11.62 Euro on average (the cognitive load manipulations were not incentivized) and a session lasted around one hour.

In the *No Load* treatment, participants were not placed under any load. In the *Phonological Load* treatment, participants completed the main task while repeating the word "and" (German: "und") every 1.5 seconds, following the rhythm given by a physical metronome placed on the table. This manipulation is known to specifically block the phonological loop, which should lead to quick information decay (Baddeley, 1986; Gathercole and Baddeley, 1993) similarly to memorizing a long sequence of digits. In the *Central Executive Load* treatment, participants completed the main task while naming random numbers (from zero to nine) aloud at the rhythm of the physical metronome. This is a rather-strong manipulation which is known to seriously impair central executive functions and, in addition, tax working memory capacity (e.g. attention) to a strong extent (Baddeley, 1966).

In all cases, participants received careful instructions on both the decision task and the cognitive load task. They practiced the load task in the presence of the experimenter and were instructed that successfully conducting this secondary task was a precondition for payment in the main task. Their speech during the task was recorded and checked to make sure that they complied with the manipulation (no participants neglected the load task; however, recordings failed for two participants). They also went through five practice trials of the main task under load.

[8] The actual colors were counterbalanced. Following Charness and Levin (2005) and Achtziger and Alós-Ferrer (2014), in the first 30 trials the first decision was forced, following an alternating left-right pattern.

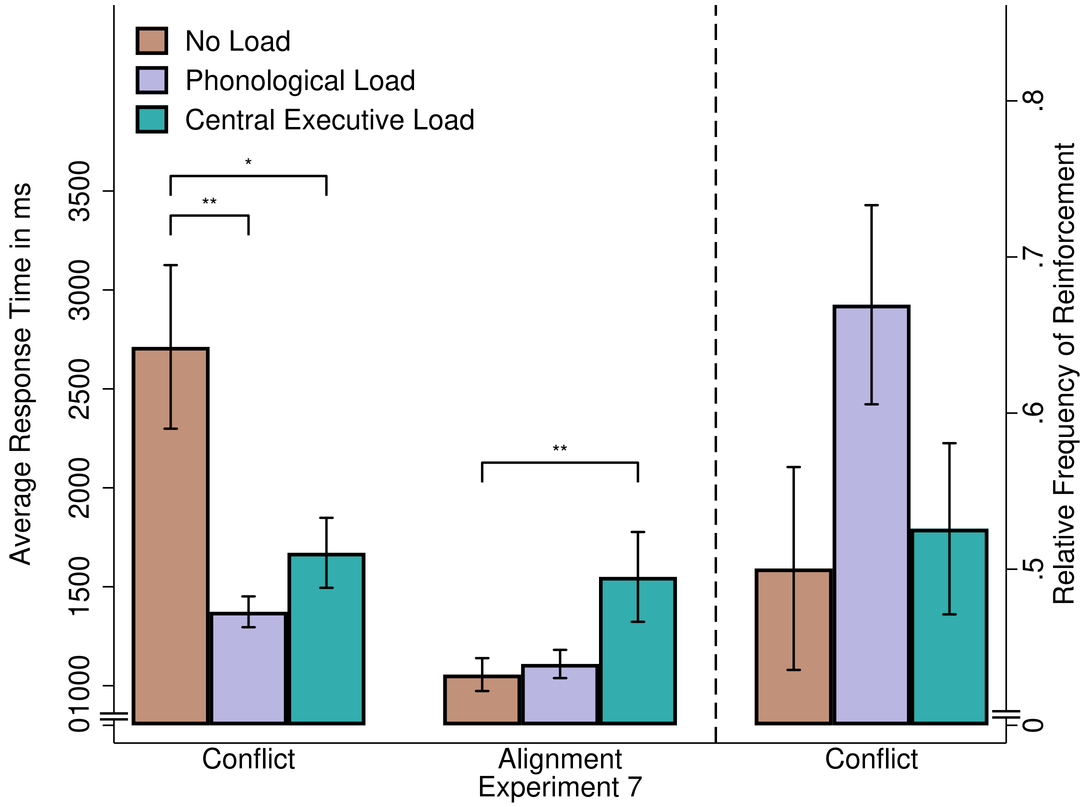


Figure 7: Average response time in conflict and alignment situations (right-hand side) and relative frequency of reinforcement decisions in conflict situations (left-hand side), Experiment 7. MWW test, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6.3 Response Time Results

The left-hand side of Figure 7 displays the average (of individual average) response times of the second draw in the No Load, Phonological Load, and Central Executive Load treatments, conditional on conflict and alignment. The average response times of decisions in case of conflict were $2,712$ ms in the No Load treatment, $1,374$ ms under Phonological Load, and $1,672$ ms in the Central Executive Load treatment. Confirming (H1b), the decrease in response times under load was significant according to Mann-Whitney-Wilcoxon tests, adjusted for multiple comparisons according to the Holm-Bonferroni method (Phonological Load vs. No Load, $N = 40$, $z = -2.624$, $p = .0174$; Central Executive Load vs. No Load, $N = 40$, $z = -1.894$, $p = .0583$). The average response times in alignment were $1,056$ ms, $1,110$ ms, and $1,550$ ms under No Load, Phonological Load, and Central Executive Load, respectively. The difference between Phonological Load and No Load was not significant (MWW, $N = 40$, $z = 0.974$, $p = .3302$), and the difference between Central Executive Load and No Load was significant in the opposite direction, that is, decisions in alignment under Central Executive Load were slower (MWW, $N = 40$, $z = 2.245$, $p = .0495$).

6.4 Behavioral Results

The right-hand side of Figure 7 displays the relative frequency of reinforcement ("win-stay, lose-shift") decisions in the three treatments in conflict situations. The frequencies were 50.04% in the No Load treatment, 66.94% in the Phonological Load treatment, and 52.59% in the Central Executive Load treatment. The increase in the relative frequency of reinforcement under Phonological Load compared to No Load was in the conventionally-expected direction, but missed significance (MWW, $N = 40$, $z = 1.922$, $p = .1093$). There was no significant difference between Central Executive Load and No Load (MWW, $N = 40$, $z = 0.473$, $p = .6358$).

6.5 Discussion (Experiment 7)

Experiment 7 involved a complex decision task for which, however, response times are usually much shorter than in our previous experiments. The predicted effect of cognitive load on response times is readily found for two different cognitive load treatments, but only in case of conflict. In case of alignment, response times are particularly fast and the effects are either negligible or go in the opposite direction, reflecting the more mechanical aspects of having to perform an additional task during the main one. This serves as a reminder of the fact that the domain of application of the effects we discuss is limited to relatively complex tasks where response times are large enough for the differences between processes to be dominant relative to more mechanical effects. This is likely to include most tasks in economics, but few in more classical, cognitive-psychology ones.

Theorem 2 predicts an effect of cognitive load on behavior for decisions in case of conflict. In this case, we do obtain a clear, significant difference in response times (which is also of a large magnitude in relative terms) confirming that cognitive load was successfully induced. The expected effects on behavior narrowly miss significance for Phonological Load, and would have been significant in the absence of a statistical correction due to the presence of a third treatment. This is consistent with the view that reinforcement-based processes are highly automatic, and hence assumption (B2), on which Theorem 2 rests, might be warranted in this case. However, the results are absent for Central Executive Load, which suggests that strong-enough load manipulations have the potential to alter the characteristics of even this kind of processes, with the result that the conventionally-expected effects on behavior do not obtain.

7 General Discussion

Cognitive load is firmly established in psychology as a causal manipulation to study reliance on more intuitive or more deliberative decision processes. As interest on the role of intuition in decision making spread to economics, researchers started relying on this manipulation with the expectation that the balance between intuition and deliberation would be shifted toward the former under load, hence revealing fundamental components of economic preferences. However, the literature can be described as an accumulation of mixed results, with some studies finding the expected shifts in behavior, and others finding no effects. A particular problem is that, in the absence of behavioral effects as predicted, it is not possible to say whether the shift toward intuition was not as expected, or rather the cognitive load manipulation was simply unsuccessful.

In this paper, we offer an explanation of the mixed results in the literature and a possible avenue for improvement. Researchers should keep in mind that the more-cognitive branches of psychology, which have found cognitive load to be a useful tool, typically rely on simple, stylized tasks where the intuitive processes involved are quintessentially automatic, in particular relying on very few or no cognitive resources. At the same time, taxing cognitive resources in such simple tasks will often mechanically (and unsurprisingly) produce longer response times as decision makers conduct additional cognitive operations during a main task.

None of this observations apply to the tasks typical of economics. In this field, tasks are generally complex, and associated with relatively long response times. This has several consequences. The first is that the differences between more intuitive and more deliberative processes might be generally larger. In terms of response times, there is more room for the differences to become noticeable. One of the fundamental characteristics of more intuitive processes is that they are faster on average than more deliberative ones. Thus, if cognitive load shifts the balance toward more intuitive processes, it must also reduce observable response times. This is the content of our Theorem 1, which offers a straightforward manipulation check for cognitive load: response times must be shorter under (successfully-induced) load than in its absence. Somewhat paradoxically, this effect is unlikely to occur in the classical domains of application of cognitive load, as, in the latter, response times are too short and leave little room for the differences between processes to offset mechanical effects.

The second consequence of the higher complexity associated with economic tasks is that what economists typically consider "intuitive" will generally correspond to behavioral rules and decision processes with significant cognitive components. Those rules are likely to be "more automatic than" their deliberative alternatives, but unlikely to be "purely automatic." As a consequence, those processes will also be affected, possibly in complex ways, by the reduction in the availability of cognitive resources accruing to cognitive load manipulations. Our Theorem 2 shows formally that the conventional wisdom that load induces more intuitive behavior does obtain, but rests on the additional assumption that intuitive processes remain unaffected by load. The latter is likely to hold in psychological domains of application where intuition corresponds to highly-automatic, stimulus-response processes, but is also likely not to hold for at least part of the tasks which are of interest to economists.

In a series of experiments (total $N = 628$), we have shown that different cognitive load manipulations significantly reduced response times in several complex, economic decision tasks. The latter include very different economic paradigms: behavior in Cournot oligopolies, voting in committees under different methods, and belief-updating tasks. These observations confirm the prediction of Theorem 1, and suggest that our response-time test can be used as a manipulation check for cognitive load in economic tasks. Importantly, this test is independent of whether behavioral effects are found as predicted or not, and hence allows to disentangle studies where cognitive load was not successfully induced from those where the manipulation did work, but the effect of a shift in the nature of decision processes was not as expected.

In our experiments, and even though we do know that our manipulations were successfully induced, we find partial or no evidence for the conventional prediction that cognitive load should result in more intuitive choices (more imitation, more sincere choices, or more reinforcement-based decisions). We conclude that the additional assumption that the more-intuitive processes involved in the decisions we study are unaffected by load might be unwarranted.

As commented above, it is not surprising that previous work in psychology has not reported a systematic shift in response times as the one we predict and find here, since we target a different kind of tasks from the ones studied there. However, a handful of studies have used cognitive load on relatively complex tasks *and* reported response times. Whitney et al. (2008) analyzed the impact of cognitive load (memorizing a five-letter string and recalling a specific letter) on framing effects in decisions under risk (choosing between a gamble and a sure outcome). They report that response times decreased significantly from $2,950$ ms without load to $2,796$ ms with this kind of phonological-loop load. Gerhardt et al. (2016) investigated risk attitudes in a lottery-choice experiment with cognitive load, employing a visuospatial-sketchpad load manipulation (memorizing a dot pattern). They reported that response times decreased significantly from $3,835$ ms without load to $3,449$ ms with load. Those papers did find behavioral effects of cognitive load (less gambling and lower risk aversion, respectively). Both report the observed effect on response times to be unexpected, and the authors speculate that participants might have tried to speed up their decisions in order to maintain accuracy in the cognitive load task. Although this speculation does not affect any of the conclusions in those works, we offer a simpler explanation: the manipulations in those papers successfully shifted the balance toward more intuitive processes, which are associated with shorter response times, hence bringing overall observed response times down.

Nevertheless, one might speculate that the additional incentives provided in our cognitive load manipulations might somehow have induced participants to consciously speed up their decisions. This is unlikely, since, for example, we also observe the effect in Experiment 7, where the cognitive load manipulations (repeating the word "and" or generating random numbers aloud) were not incentivized. Also, Duffy et al. (2016) and Duffy et al. (2020) conducted two different experiments contrasting high cognitive load (remembering 6-digit sequences) with low load (instead of no load; remembering 1-digit numbers), both of which were incentivized. They also found that high load resulted in faster decisions (in Duffy et al., 2020, 10.081 s under low load vs. 9.586 s under high load), although the effect was unexpected in those studies.

In several of our experiments, the effect on response times is of a large magnitude in relative terms (Experiments 1, 2, 4, 5, and 7 in case of conflict). In Experiment 3, the effect is only significant in case of conflict, and the magnitude is substantially smaller than in Experiments 1–2, which used the same main task. The difference is that Experiment 3 used a different load manipulation (adding up a previously-read single-digit number with another, just-heard single digit). In Experiment 6, the effect is only significant for one of the voting methods, and again its magnitude is substantially smaller than in Experiments 4–5, which used the same main task. Again, the difference is that Experiment 6 used a different cognitive load manipulation (remembering a dot pattern). This suggests that the difference in response times, which we have proposed here as a test, might potentially be used to develop a metric of the comparative strength of different cognitive load manipulations.

Related to this, Experiment 7 offers an additional, potentially-interesting insight. In this experiment, a manipulation targeting the phonological loop produces the predicted effects on response times in case of conflict, and also (although significance is narrowly missed after corrections for multiple testing) the expected increase in intuitive, reinforcement-based choices. However, the main task involves much shorter response times than other experiments, especially in case of alignment. In the latter, we indeed do not observe any effect on response times, possibly suggesting that the task is close to the boundary of the domain of applicability of Theorem 1. In the same experiment, we also employed a particularly strong manipulation focused on central executive functions, which again yielded a reduction of response times, as predicted, but only in case of conflict. However, the effects of this stronger manipulation on behavior were markedly weaker (than those of phonological load). We argue that this is *not* paradoxical. The intuitive process we focus on in Experiment 7, reinforcement, is relatively automatic, but still rests on cognitive functions (associating success to decisions). A manipulation which does not tax away cognitive resources inordinately will affect reinforcement to a small extent, or not at all. A much stronger manipulation, in contrast, might affect both the more deliberative and the reinforcement process, invalidating the necessary assumptions behind the predicted behavioral effect. In particular, under central executive load, decisions in the binary main task approach 50% for each option, suggesting random behavior.

To summarize, in this paper we offer a warning on the risks of uncritically importing even well-established techniques across disciplines. This is not to say that economists interested in intuition and deliberation should abandon cognitive load. On the contrary, the very first conclusion of our analysis is that economists have a new tool at their disposal, allowing them to determine when a cognitive load manipulation has successfully induced a shift in the nature of decision processes employed by experimental participants. At the same time, we warn that researchers should be aware of the fact that conventional wisdom on the behavioral effects of cognitive load rests on additional assumptions on just how automatic the postulated intuitive processes are. Using cognitive load to causally test the role of certain processes in economic decision making requires a careful, prior analysis of the actual cognitive characteristics of those processes. If the cognitive difference between the postulated more deliberative and more intuitive processes is small or unclear, it is unwarranted to predict any behavioral effects. If the research question involves heuristics or processes of a clearly-automatic nature, or there are objective reasons to expect large differences (in cognitive terms) between the processes at work, the researcher will be fully justified to invoke our Theorem 2 and expect a shift toward more intuitive behavior. In this case, in addition, our Theorem 1 will provide the researcher with a test to ensure that possible null effects are not due to a failure in the manipulation.

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