UiT The Arctic University of Norway

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The Influence of Verbal Instructions on Action Control

Investigating the Effect of Action–Effect Instructions from the Associative Learning Perspective

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Abstract

Ideomotor theory posits that actions are represented in terms of their perceptual effects. Past studies on action control have studied these representations as bidirectional associations in behavioral research. However, a parallel line of research has shown that verbal information is an alternative route to form these associations, even if a person has not performed any active behavior. The present thesis aims to investigate whether verbal instructions formulated in an effect–response manner can serve as an additional source of learning, resulting in the formation of associations between perception and action components. In a series of five experiments in three articles, the present thesis evaluates the effect of effect–response instructions via the compatibility effect, employing the experimental procedures from studies on action–effect associations in combination with learning and test phases. Article 1 investigates whether effect–response associations influence behavioral responses as response-priming effects. Article 2 addresses the question of whether these associations are based on the associative learning mechanism. Article 3 investigates the effect of effect–response associations on visual selective attention in terms of selection biases. The results from Article 1 show that the effect of verbal effect–response instructions primes behavioral responses as response biases. The results of Article 2 show that the effect of such instructions is more likely based on associations between perception and action rather than the mere saliency effect of these components. The results also indicate that a visual presentation of a priming stimulus is not a necessary precondition to observe the compatibility effect. Finally, Article 3 shows that effect–response instructions produced an instruction–compatibility effect; however, this effect is observed only within response errors. The results of reaction times showed that participants' responses were decelerated.

List of Articles

Article 1

Damanskyy, Y., Martiny-Huenger, T., & Parks-Stamm, E. J. (2022). Unintentional response priming from verbal action–effect instructions. *Psychological Research*. https://doi.org/10.1007/s00426-022-01664-0

Article 2

Damanskyy, Y., Martiny-Huenger, T., & Parks-Stamm, E. J. (2022). Associative learning from verbal action–effect instructions: A replication and investigation of underlying mechanisms. *Journal of Cognition.* https://doi.org/10.5334/joc.284

Article 3

Damanskyy, Y. (2023). Verbal instructions as selection bias that modulates visual selection. *Visual Cognition.* https://doi.org/10.1080/13506285.2023.2221046

Note. Printed Article 3, attached to the present thesis, contains errors. The online version of the article does not have those errors and contains a correction form.

1. Theoretical Background

Controlling one's own actions is a key component of cognitive control. People perform multiple actions daily, forming a continuous stream of actions to achieve their desired outcomes. However, studying these actions is challenging, first because human behavior involves an interplay between external factors pertaining to a specific situation in a given moment; second, because there is no clear-cut method of parsing people's streams of actions into separate segments. As a result, research varies in its focus on different aspects of action control, such as stimulus–response learning, intention, and motivation. Nevertheless, much research on action control shares a common premise: Actions are closely interlinked with perception, and these two components do not constitute separate systems but rather interact with and support one another.

Throughout history, human actions have been categorized into two lines of thought: the *sensorimotor* and *ideomotor* views. The sensorimotor perspective focuses mainly on actions generated by external stimuli (Guthrie, 1935). In this context, specific stimulus–response associations play a major role in organisms' behavior, determining their interaction with the environment. Sensorimotor theories define specific factors related to stimulus–response associations, such as the magnitude of rewards and the number of repetitions, as well as the influence of these factors on the strength of stimulus–response associations that predict future behavior.

The ideomotor principle, as an opposing viewpoint, extends the concept of action control by emphasizing the importance of internal causes for actions. The general concept of ideomotor theories implies that when a person interacts with the external environment, specific actions trigger perceivable changes in their surroundings that result in a formation of associative links between such actions and their perceivable consequences (action–effect association; (Elsner & Hommel, 2001). For example, pressing a light switch leads to the appearance of light in the room. Upon the repetition of that action (e.g., the action of pressing a light switch), the action becomes associated with its perceivable effect (e.g., light in the room). Then, a mental activation for that environmental change (e.g., light in the room) automatically triggers corresponding actions.

Table 1

Definition Table with Descriptions of Overlapping Concepts in the Thesis and the Articles

Concept	**Description**
Action–Effect vs Response–Effect	Both concepts mean one thing: an association between a response and its ensuing effect. However, action–effect is used more when discussing the general idea of action–effect learning according to Hommel 2001.
Effect–Response vs Stimulus–Response	Both concepts indicate an association between a given stimulus and response. However, the effect–response concept presupposes that stimulus is defined as an effect that followed a response.
Effect–Action vs Action–Effect	Alternate versions of the effect–response and response–effect concepts. However, in general, action-effect is a more common theoretical concept introduced by Hommel.
Response-effect sentence vs effect-response sentence	Both entail verbal instructions that associate a certain stimulus with a corresponding response. In the response-effect order, the response is presented first, whereas in the effect-response order, the order is

	reversed. Importantly, in both types of sentences, the stimulus is defined as a consequence of the specific response.
Learning Phase (Articles 1 and 2) vs Priming Phase (Article 3)	Both concepts indicate the same thing: the phase of an experiment in which participants learn specific associations between a given stimulus and response. “Learning phase” is commonly used within action control literature, whereas “priming phase” is more general and often used in other type of research like attention and other priming paradigms, as well as action control. However, in the context of the present thesis, these two concepts indicate the same thing.
Test Phase (Article 1 and 2) vs Probe Phase (Article 3)	Indicate the same part of an experiment in which researchers evaluate the effect of different primes.

1.1 The Origins of Ideomotor Theory

The origins of ideomotor ideas can be traced back to the 19th century from two roots: German philosophers (Herbart 1816; Lotze 1852; Harless 1861; as cited in Yun Kyoung et al., 2010) and British physiologists (Laycock, 1845; Carpenter, 1852; as cited in Yun Kyoung et al., 2010). The British root is considered a medical–physiological perspective, and the work of Laycock and Carpenter focused more on reflexive behavior to explain several occult phenomena on a physiological level. German scholars, on the other hand, were more interested in developing the theory of action control to overcome the explanatory gap between mind and body.

Herbart is often considered the pioneer of the fundamental idea of ideomotor principles, and his main idea can be narrowed down to the following principle: The human body makes its first movements for organic reasons. These movements lead to specific changes in the environment, which the cognitive system perceives. Then, when a desire arises for these environmental changes, the cognitive system automatically employs the corresponding actions that previously led to those changes. Although Herbart used concepts such as "soul" and "feelings of the soul," which are not considered scientific in contemporary psychology, the fundamental idea that the cognitive system can purposively employ specific actions via their perceptual effects remains the core concept of modern ideomotor theories of actions.

1.1.2 An Empirical Model of the Ideomotor Principle

One theoretical framework to test ideomotor theory originates from studies by Greenwald (1970a, 1970b) that were integrated by Elsner and Hommel (2001). One of the challenges in testing ideomotor theory stems from a lack of specific methodological methods to evaluate internal causes, intentions, and thoughts. In response to this challenge, Greenwald (1972) suggested an extension of the ideomotor principle based on his previous findings (Greenwald, 1970a, 1970b), which showed that perceiving an effect that one has previously learned as the an effect that followed from a particular action (e.g., a green light triggered by a keypress) can facilitate or promote the execution of that action. Thus, this extension postulates that if one presents a previously learned effect as an external stimulus (instead of asking participants to activate it internally), it can influence the responses that follow. This idea can be viewed as evidence showing that actions are represented in terms of perceptual effects (i.e., an indication of response–effect learning), and this idea is fundamental within the ideomotor approach.

This idea had a practical impact on the central framework used to test ideomotor theory, which Elsner and Hommel (2001) adopted and defined as action–effect. The empirical action–effect model involves two steps: (a) the learning phase (also known as the priming or acquisition phase), in which participants learn specific response–effect associations; and (b) the test phase, in which previously learned response–effect associations are inverted to evaluate their effect on actions in forced-choice or free-choice experimental tasks.

In addition to the framework of Elsner and Hommel (2001), a parallel line of research (Kunde, 2001) proposed an alternative framework for evaluating the response–effect compatibility effect. According to Kunde (2001), if an effect precedes a response, the demonstration of action anticipation is not possible because ideomotor theory explicitly claims that response selections are influenced not by presented effects but rather by anticipated effects (those that occur after a response is executed). Therefore, the experiments employed in Kunde's study focused specifically on this aspect of ideomotor theory, demonstrating that anticipated action–effects influence the free choice of actions and their execution latency.

For example, In Kunde's 2001 experiment, participants responded to colored dots on a screen by pressing keys, with each keypress lighting up one of four boxes. The experiment was designed to examine the compatibility between the keypresses (actions) and their resulting visual effects. When a key was pressed, one of four boxes on the screen would light up. The key aspect of the experiment was the mapping between the keypresses and the lighting boxes: In some trials, pressing a key would light up the box directly above it (corresponding mapping), while in others, a different box would light up (noncorresponding mapping). The results showed that participants responded more quickly and accurately in the corresponding mapping condition, where the lit box matched the pressed key. By contrast, response times were slower and accuracy was reduced in the noncorresponding mapping condition, where the lit box did not match the expected one.

Although the methodology proposed by Kunde aligns more closely with the foundational principles of ideomotor theory, it places less emphasis on assessing the underlying mechanisms of action–effect learning. Empirical approaches according to Kunde’s method involve straightforward tasks where the relationship between an action and its effect is clear and therefore does not require a learning phase. By contrast, the framework introduced by Elsner and Hommel (2001) facilitates the implementation of action–effect learning and focuses on how associations between responses and their effects are established.

Given that the studies in the present thesis are focused on the use of verbal instructions as an alternative learning approach, the methodological framework employed across all articles is more aligned with Hommel’s approach. This alignment is grounded in the idea that verbal instructions are a distinctive learning mechanism for establishing action–effect associations. Hommel’s approach presupposes the learning phase where participants develop specific relationships between perceptual and action components, which is suited for the objectives of this thesis. Therefore, the empirical model I adopted incorporates such a learning phase, followed by a testing phase to evaluate the strength and influence of those established associations on action control.

1.1.3 Stimulus–Response and Response–Effect Compatibility Effect

The fundamental concept of the stimulus–response compatibility effect is that the performance (e.g., speed or accuracy) of certain tasks is better or worse due to an overlap between specific stimulus and response sets (stimulus–response mapping; Kornblum et al., 1990). The compatibility effect presupposes two core concepts: *mapping* and *automaticity.* Mapping suggests that repeated responses to specific stimuli establish an association between that stimulus and response, known as the stimulus–response mapping rule (Fitts & Seeger, 1953). Later, when participants engage in a task with different mapping rules, a compatibility effect emerges when a previously learned stimulus–response mapping rule coincides with the rule in the current task, thereby facilitating responses. Another manifestation of compatibility is interference, which occurs when a previously learned stimulus matches with one presented in an experimental task, but the associated response differs from what the task requires. Such misalignment results in interference, leading to delayed responses or reduced accuracy.

Automaticity suggests that certain responses are activated without conscious intent. This concept aligns with some of the fundamental characteristics of automatic behavior as outlined in the literature (Kornblum et al., 1990; Logan, 1990; Schneider & Shiffrin, 1977). Such behaviors often arise from repeated exposure or practice. Therefore, an automatic response is one that is triggered involuntarily, without deliberate intention, and requires minimal cognitive effort.

The compatibility principle employed in action–effect research is similar to that of classical stimulus–response research, except that participants learn stimulus–response mapping in the reverse order (i.e., response–effect associations). Specifically, if action control depends on an overlap between actions and their ensuing effects, then action selection and the response to an imperative stimulus (which is a specific cue signaling the participant to perform a certain action) will depend on response–effect compatibility. For example, in the learning phase, by performing repetitive responses, participants learn that a specific keypress (e.g., left keypress) leads to the appearance of a red circle. The temporal overlap between these events results in the formation of a response–effect association (e.g., left keypress—red circle). Subsequently, in the testing phase, the presentation of an imperative stimulus would prompt the participant to execute the associated action (left keypress) in anticipation of the learned effect (appearance of the red circle).

During the test phase, when participants engage in a speed categorization task with varying task rules, the compatibility effect can manifest as either facilitation or interference, or both. Facilitation is defined by quicker and more accurate responses when a previously learned stimulus (e.g., a red circle) is presented, either as a target or an irrelevant stimulus, and the required response aligns with a previously associated response (e.g., left keypress aligns with a required left keypress). Conversely, interference occurs when a priming stimulus coincides with a target stimulus, but the associated response is different (e.g., red circle is associated with a left keypress but required response is a right keypress), leading to slower or less accurate responses. This compatibility effect reflects the dynamic interplay between previously learned associations and current task demands, influencing both the facilitation and inhibition of responses based on the congruence or incongruence of the stimulus–response pairings.

All experiments in this thesis incorporated diverse forms of priming, therefore, the compatibility effect is conceptualized as the facilitation or interference encountered in processing and responding to a target stimulus, following the presentation of a compatible or incompatible primes compare to control trials which did not involve any primes. In more explicit terms, responses to a target stimulus are anticipated to be faster and more accurate when they are preceded by a compatible prime. Conversely, the responses are expected to be slower and less accurate when they are preceded by an incompatible.

1.2 Language

The relationship between language and cognition has long been debated. Historically, the question about what type of role language plays in human cognition is a polarizing one. On one side of the debate, language is viewed—with a lot of nuances—as peripheral to the mind (Fodor, 1976; Pinker, 1997), meaning that language has nothing to do with cognition, and language is necessary only for making a thought explicit. On the other side is the view that language is conceptually necessary for thinking, and therefore, language is closely intertwined with cognition (Dummett, 1981; McDowell, 1994; Perlovsky & Sakai, 2014). Although this categorical distinction is oversimplified (Gomila, 2012), numerous studies have provided support for the idea that language is interconnected with cognition, including problem-solving (Glucksberg & Weisberg, 1966), the framing effect (Tversky & Kahneman, 1981), and attention (Knapp & Abrams, 2012; Schmidt & Zelinsky, 2009).

Furthermore, the question of how language influences action control also relates to this discussion. The relationship between language and action control has been primarily investigated from two research perspectives: implementation intention (Gollwitzer, 1999) and instruction-based research (Cohen-Kdoshay & Meiran, 2007). The general idea suggests that when verbal information is presented in an action-oriented manner (i.e., involving information about a specific stimulus and response), this information affects response selection when a previously verbally specified stimulus is encountered, indicating that language is closely intertwined with action control.

1.2.1 Research on Verbal Instructions

The theory of implementation intentions posits that behavior can be strategically controlled by forming a plan in an *if–then* format (Gollwitzer, 1999). This format increases the likelihood of executing planned behavior when humans encounter an anticipated situation (the *if* component), which will increase the likelihood of them executing the intended behavior (the *then* component). For example: "If I pass a supermarket, then I will buy fruit." In this case, the anticipated situation (i.e., the supermarket) is a critical cue that increases the likelihood of executing the planned action (i.e., buying fruit). Gollwitzer (1999) defined such if–then planning as "strategical automaticity," which has been studied from a variety of perspectives as a self-regulatory strategy (for a review, see Gollwitzer & Sheeran, 2006).

Numerous instruction-based research studies, such as those noted by Brass et al. (2017), have shown that instructions given in the form of stimulus–response mappings can affect performance in other tasks where such instructions are not directly relevant. The experiments in those studies were designed to test whether the effect of verbal instructions operates through working memory, operating on the basic assumption that when participants are given specific instructions, they store this information in their working memory. As they perform other tasks, these stored instructions can lead to reflexive behavior, which is an automatic, unconscious response that occurs without deliberate thought or intention. Essentially, the instructions held in working memory can trigger these reflexive responses, even in situations where they may not be relevant or beneficial.

The basic design of this type of research involves three stages: During the planning stage, participants are given particular priming instructions that are relevant to a specific task (e.g., “If the word is ‘cat,’ press left; if the word is ‘dog,’ press right”). However, before executing the task in which these instructions are to be applied (i.e., the *inducer task*), a preceding *diagnostic task* is introduced that shares stimuli (the words “cat” and “dog”) and responses (left/right keypress) with the inducer task, but with different instructions (e.g., “If the word ‘cat’ is printed upright, press left; if the word ‘cat’ is printed in italics, press right”). Using this design (Liefooghe & De Houwer, 2018; Liefooghe et al., 2012), the presence of the instruction-based compatibility effect in the diagnostic task was demonstrated when the required response and the stimulus matched with the instructions of the inducer task (e.g., “cat” was presented upright and required a left keypress response).

1.2.2 Verbal Instructions and the Action–Effect Paradigm

Several studies have also evaluated the effect of verbal instruction under the action–effect paradigm. For example, using the previously described two-stage empirical model, Eder and Dignath (2017) showed that verbal instructions can mediate the acquisition of response–effect associations during the learning phase. Therefore, verbal instructions can modulate learning during active behavior. In contrast, Theeuwes et al. (2015) evaluated the effect of verbal instructions on actions from a working memory perspective. Specifically, the authors showed that verbal instructions formulated in a response–effect manner (e.g., “the left key removes P”) could bias performance in a subsequent reaction-time task in which those instructions are irrelevant but features of the stimuli and response overlapped.

While the study by Theeuwes et al. (2015) tested the effect of verbal response–effect instructions from a working memory perspective as reflexive behavior (i.e., short-term effect), there is still a lack of empirical evidence showing that effect–response or response–effect instructions have an effect on actions from an associative learning perspective. As several studies have pointed out, verbal instructions based on associative learning principle can have a long-term effect (Pfeuffer et al., 2017; Pfeuffer et al., 2018); however, in these studies, verbal instructions were presented as simple verbal codes (e.g., “G + N”). Therefore, studying verbal instructions from an action–effect perspective can provide further insight into action–effect learning and test whether instructions may also have an effect if they are not kept in working memory.

1.3 Selective Attention

While previously mentioned studies investigated how verbal instructions influenced action selection, there remains another important part of action control that involves perception: The rapid detection of changes or cues in the environment is a key factor in adjusting and adopting ongoing behavior. Thus, research has long emphasized the relevance of selective attention to action control (MacKay, 1987; Müsseler & Hommel, 1997; Rizzolatti et al., 1987). *Selective attention* is the ability to select specific stimuli and responses, access particular memories, and navigate thoughts that are behaviorally relevant at a given moment (Maurizio, 1998). Although all perceptual domains are relevant to action control, visual selective attention may be a central topic to be investigated from not only a perception perspective but also from an action control perspective (Memelink & Hommel, 2013; Verbruggen et al., 2014; Weidler & Abrams, 2016).

Visual selective attention provides rich experimental flexibility that enables the investigation of attention across multiple visual domains (e.g., color, shape, location, motion; Carrasco, 2011). Often, visual selective attention is studied solely from a top-down (task-driven) or bottom-up (stimulus-driven) perspective. However, many studies have argued that selection history or a reward associated with a specific stimulus influences attentional control to a greater degree than the physical properties of the stimuli or the relevance of the behavior (Anderson et al., 2011; Anderson & Yantis, 2013; Bucker & Theeuwes, 2014; Failing & Theeuwes, 2016; Failing & Theeuwes, 2015; Libera & Chelazzi, 2006). These two sources of influence are defined as selection biases (Awh et al., 2012). From this perspective, previous behavior has an effect on selective attention rather than behavioral relevance or the physical properties of stimuli.

Comparing response–effect associations and selective attention, it follows that the experiences of past active behavior influence actions and selective attention. Furthermore, several studies have pointed out that actions themselves can navigate selective attention (Fagioli et al., 2007; Hommel, 2005; Wykowska et al., 2009). Behavioral research has also posited that associative learning impacts selective attention (Gottlieb & Balan, 2010; Le Pelley et al., 2011).

Several studies, such as those by Knapp and Abrams (2012), Schmidt and Zelinsky (2009) and Wolfe et al. (2004), have indicated that verbal instructions can influence selective attention, similar to their effect on action control. However, there is limited research on how associative learning from these verbal instructions impacts selective attention. It is vital to understand whether verbal instructions can replace selection history in learning. Investigating verbal instructions that link a response to a priming stimulus (action–effect) may offer deeper insights into how verbal instructions affect selective attention and action–effect learning.

2. Research Objectives

The present thesis aimed to increase our understanding of the effect of verbal instructions on cognitive control from an action–effect perspective. Because cognitive control is a broad topic, I focused our research on three distinct research questions that address the effect of verbal instruction on action control and selective attention from an associative learning perspective. The main research question of this thesis originated from the main premises of research on effect–response associations and verbal instruction, namely (a) actions are represented in terms of their perceivable effects (action–effect learning), and (b) verbal instructions provide additional sources of influence on action control similar to active behavior. Therefore, in the present thesis, I focused on effect–response instructions as an additional learning mechanism that could result in associative learning that links perception and action components similarly to action–effect learning based on active behavior. Therefore, the three main objectives are:

1. In Article 1, I, with my co-authors, investigated whether effect–response instructions can be considered an alternative method of forming effect–response associations similarly to action–effect learning based on active behavior.
2. In Article 2, I, with my co-authors, investigated whether the effect of effect–response instructions is based on the associative learning principle rather than the alternative saliency effect explanation. Furthermore, we also assessed whether a visual example of a verbally specified stimulus is a necessary precondition to observe language-based action–effect learning.
3. In Article 3, I investigated the effect of effect–response instructions on visual selective attention in terms of selection biases.

3. Methodological Approach

In this chapter, I will explain the methodological approach of this thesis. This chapter is divided into four sections: (a) a presentation of the general experimental design of this thesis, which provides a general overview of the experimental approach; (b) a description of the statistical analysis and data preparation used in each article; (c) a presentation of the online platform used in this study, which explains certain aspects related to online experimental settings; and (d) a discussion of ethical standards, which explains the general ethical principles employed in all three articles.

3.1 The General Experimental Design

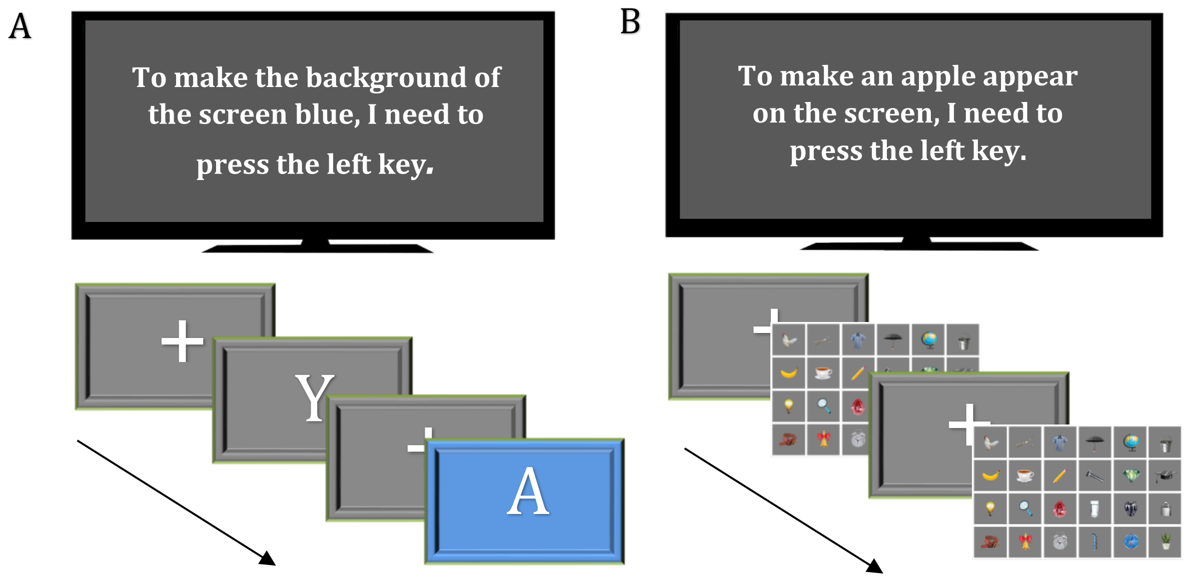
The experimental design, employed consistently in all three articles, was based on the two-phase model of Elsner and Hommel (2001) with one notable deviation in the learning phase: Unlike Elsner and Hommel's model—in which learning is achieved through performing of actual responses—the design of the present articles involved only the presentation of verbal instructions during this phase. The main idea was to explore whether effect–response or response–effect associations could be acquired solely through verbal instructions in which relationship between a response and its effect is clearly specified (e.g., "To make an apple appear on the screen, I need to press the left/right key.")

The test phase was designed to assess whether previously learned effect–response associations influence behavioral responses. This was accomplished by measuring the magnitude of the compatibility effect. During this phase, I adopted a compatibility approach consistent with the compatibility principle outlined in subsection 1.1.2. The test phase in each article featured various categorization tasks. In these tasks, participants were instructed to categorize the presented stimuli according to the specific instructions of each task.

Therefore, in cases where a target stimulus requiring a response for the categorization task aligned with a stimulus and response specified in the verbal instructions (i.e., a compatible condition), I anticipated faster response times and/or fewer response errors. Conversely, when the priming stimulus was presented as a target stimulus, but the required response did not match the response in the verbal instructions (i.e., an incompatible condition), then slower response times and/or more response errors were expected. Additionally, trials where the target stimulus differed from the priming stimulus were treated as control trials. Figure 1 presents an overall schematic illustration of the general design of Articles 1 and 2 (A, left panel) and Article 3 (B, right panel).

Figure 1

Visual Presentation of the General Experiment Design



Note. The figure illustrates the general experimental design of Articles 1 and 2 (A) and Article 3 (B). The upper part illustrates a learning phase, in which participants memorized an effect–response sentence. The lower part illustrates a test phase, in which participants performed a simple categorization task (Articles 1 and 2) by categorizing the target stimulus as either a vowel or a consonant. In the visual search task (Article 3), participants needed to find the target stimulus and categorize it as either a fruit or a vegetable.

3.2. Statistical Analysis and Data Preparation

In Article 1, the data were analyzed using mixed ANOVA, primarily in response to editor's request for an analysis that is more familiar to our readers. We maintained this approach in Article 2 due to its close similarity to Article 1. However, in Article 3, I transitioned to mixed-model analysis. This shift was motivated by a desire to gain experience with a more sophisticated analytical approach, especially considering that mixed models are particularly suitable for designs involving multiple observations per participant (Winter, 2020). Data preparation across all articles was consistent, with a notable exception in Article 3; here, I applied stricter criteria (±2.5 SD compared to ±3 SD in Articles 1 and 2) to exclude individual response times that significantly deviated from the mean calculated by participant and within-participant conditions.

In addition, in Article 3, I excluded individual responses identified as having an intra-priming effect. This procedure is quite common in studies employing the visual search paradigm (Lamy & Kristjansson, 2013). The final analysis in Article 3 involved a boxplot analysis of response times and response errors, calculated by stimulus identity. Because the target stimuli in Article 3 were real objects that varied in visual dimensions (e.g., color, shape, saliency), the boxplot analysis served as a control measure to assess whether responses to a specific object greatly deviated from responses to other stimuli.

3.2.1 Compatibility Means vs Absolute, Raw Means

In this subsection, I aim to clarify the specific rationale for why Articles 1 and 2 did not use compatibility scores[1] and instead analyzed raw means scores. The use of descriptive means (which were analyzed as marginally estimated means in ANOVA) in Articles 1 and 2 is an alternative to the commonly employed compatibility scores utilized in Article 3 and other similar studies on verbal instructions (e.g., Liefooghe & De Houwer, 2018; Pfeuffer et al., 2018). The primary reasoning for employing raw means in data analysis was based on the idea that during the computation of compatibility scores, researchers average the data from both left and right responses according to their congruency or incongruency with verbal instructions, thereby merging at least two variables into one. This approach appears to be less transparent than the method of using actual raw means.

Although the choice of this analytical method might not be common, I want to emphasize that the difference between these different approaches does not impact the results or conclusions drawn from the present thesis. Despite offering a more transparent insight into the results, using absolute values can be more complex to interpret and describe. In contrast, compatibility scores (although less transparent) are simpler to comprehend and convey. Importantly, these two methods of analysis, despite their differences in presentation and complexity, ultimately lead to the same outcomes in results.

[1]Those scores shall not be confused with the compatibility gain score presented in Article 2. See the description of Figure 4 on page 32 for compatibility gain scores. They are made for illustration purposes only and cannot be used in statistical analysis.

For this reason of transparency, my colleagues and I chose to use actual raw scores instead of compatibility scores in Articles 1 and 2. At the same time, I also acknowledge that although this method provides more detailed information, it may inadvertently introduce extraneous statistical noise (Casella & Berger, 2002) due to the increased number of interaction effects, leading to complex high-order interaction analyses, such as four-way interactions. Therefore, I chose to use compatibility scores in Article 3.

3.3 Online Settings

The methodological procedures of all three articles used a new approach: an online assessment of behavioral responses. To my knowledge, before 2019, all previous studies assessing the effect of instructions in reaction-time tasks had been conducted in laboratory settings. However, laboratory settings increase the cost of such procedures, especially when a larger sample must be recruited. Most studies evaluating the effect of instructions (Braem et al., 2017; Eder & Dignath, 2017; Koban, Jepma, Geuter, & Wager, 2017; Liefooghe & De Houwer, 2018; Muhle-Karbe, Duncan, De Baene, Mitchell, & Brass, 2017; Pfeuffer et al., 2017; Theeuwes et al., 2015) did not have samples larger than 50, and none had a sample larger than 100.

Understandably, it was difficult to conduct studies online in the past due to technical limitations; for example, the standard deviation among responses could be high due solely to technical reasons (e.g., browser differences, internet speed). However, new technical possibilities have recently become available (Bridges et al., 2020). Furthermore, a direct comparison of online-based and laboratory-based platforms showed that an online platform can be as good as a laboratory platform for evaluating an experimental paradigm (Kim et al., 2019). Moreover, an essential benefit of conducting research online, as demonstrated in a comparison of online-based and laboratory-based platforms by Kim et al. (2019), is the feasibility of recruiting a more diverse and non-student sample from platforms such as Toluna and Prolific. This diversity is a significant advantage over the typical student samples assessed in laboratory settings on campuses, offering a broader and potentially more representative data set for evaluating experimental paradigms.

3.4 Sample Size

The first experiment of Article 1 was conducted with a sample of 43 participants. The determination of this sample size was not grounded on any hypothesis or computation; rather, it was influenced by the temporal constraints and available resources at the time, given that all participants were students. The second experiment of Article 1, however, incorporated 400 participants—the minimum sample size provided by the recruiting company. The participant count for Article 2 was guided by the outcomes of Article 1. Despite the relatively small effect sizes identified in Article 1 (Exp1, $\eta p^2 = 0.08$; Exp2, $\eta p^2 = 0.02$), we opted to maintain a comparable number of participants per condition as in Article 1, approximately ~200 per condition. Given that Article 2 comprised three main conditions, the final sample size amounted to 700, considering that a small part of participants will be excluded as outliers or due to technical reasons.

The effect size for Article 3 was determined through a simulation analysis specifically designed for mixed-model evaluations on a pilot study with five participants (Green et al., 2016). This analysis was performed on response times and on compatibility factor alone without considering interaction effect. The results indicated that 94 participants were required in order to achieve β = 88.20%. Table 1 details the outcomes of this analysis. Even though the standard beta (β) of 80% involved about 70–72 participants, I aimed for a slightly higher power (~88%). In addition, I rounded up the number of participants to 100 to account for potential outliers. This decision was also constrained by the limited resources allocated to this study. I will discuss the possible implications of this decision on the results in the limitations section of this thesis. However, this analysis is applicable to Experiment 1 alone. The sample size for Experiment 2 was driven by two factors, namely the outcome of the previously mentioned power calculation for Experiment 1 and limited resources available for that study.

Table 2

Results of Simulation Sample Size Analysis for Article 3: Experiment 1

Number of Levels in Participant	**Power for Predictor**	**95% Confidence Interval**	**Number of Rows**
3	10.80%	(8.94, 12.89)	271
16	25.40%	(22.73, 28.22)	1422
94	88.20%	(86.04, 90.13)	8343
120	94.30%	(92.68, 95.65)	10656

3.5 Ethical Principles

Every experiment in all three articles was conducted in accordance with the ethical principles stated in the Act on Research Ethics (2007) and Ethical Principles (APA, 2010). Specifically, in no experiment did I collect any sensitive data. The design of all articles also presupposed only behavioral responses. Additionally, I collected demographic information such as age and gender, and all data received from participants were anonymized. I also obtained informed consent from our participants by displaying the informed consent form as the first page of every experiment, and participants were asked to press the spacebar if they agreed with the information and were ready to participate in the study. Participants also knew that they could withdraw their data and terminate their participation at any moment without providing any explanation. Finally, the local ethical committee of the Arctic University of Tromsø approved this research (Article 1 - October 2019; Article 2 and Article 3 – March 2020; all with the archive reference: 2017/1912).

In my initial study, conducted in 2020 with my co-authors, a pre-registration was filed; here it is included as supplementary material in Article 1, aligning with emerging standards in research methodology. Subsequent studies, however, did not involve additional pre-registrations. Specifically, in Article 2, although the first two experimental conditions were pre-registered, a deviation occurred with the third condition. This condition, as initially planned, was found to contain a conceptual flaw; therefore, we could not use it for our research aims. It was subsequently replaced with a condition that did not contain that flaw. Despite adhering to the pre-registered data processing protocols and hypotheses, this modification in the experimental design was a notable deviation from the original pre-registration. In consideration of research transparency and ethical standards, we refrained from labeling this part of the study as pre-registered. Instead, we chose to disclose and link the original pre-registration, acknowledging the alteration made.

Article 3 did not involve pre-registration. The initial phase of Experiment 1 was conducted as part of students' bachelor project, involving collaboration with other students. This phase was later integrated into the research of the current thesis. At the time of this transition, data collection for Experiment 1 had already been completed; as such, I decided not to pre-register the experiment. The launch of Experiment 2 faced time constraints.

However, to ensure methodological consistency across the series of studies, I tried to adhere to data preparation procedures that were employed in previous two studies where it was applicable. I present this information here solely in the interest of transparency.

4. Article Summaries

4.1 Article 1: Unintentional Response Priming from Verbal Action–Effect Instructions

4.1.1 Background

Article 1 focuses on the theoretical concept of action–effect learning, which postulates that actions are associated with their perceivable outcomes through bidirectional associations. The concept of action–effect suggests that thinking about the effect of an action will activate the linked behavior that previously produced the effect. Traditionally, these associations were thought to be formed through actual behavior involving perception and actions. However, two areas of research, implementation intentions and instruction-based research, have shown that verbal information can also influence subsequent actions.

Past research has established that action–effect associations are typically learned through active behavior. However, the impact of verbal instructions on behavioral responses, particularly when formulated in a response–effect manner, remains an area of ongoing exploration. Article 1, conceptually akin to the study by Theeuwes et al. (2015), investigates the effects of verbal instructions from an associative learning perspective, hypothesizing that these effects can last longer than several seconds. A key distinction in our study is the time interval between the learning and test phases. Unlike the study by Theeuwes et al., where this interval spanned just a few seconds, our study extended it to a few minutes. This variation offers a new perspective on the durability of the influence exerted by verbal instructions.

In the study by Theeuwes et al. (2015), verbal instructions were updated every four, six, eight, or 16 trials. After reading these instructions, participants were evaluated on their responses within a short time frame, approximately 7.5 seconds later. In contrast, Article 1 adopted a different approach: We presented response–effect and effect–response instructions as a single sentence, delivered just once before the participants engaged in a categorization task. This method enabled us to examine the impact of verbal instructions over a longer duration, assessing their effect after an interval of 2–3 minutes.

4.1.2 Study Aims

The main objective of Article 1 was to investigate action–effect learning from a verbal instruction and associative learning perspective, emphasizing the extended timing interval and the reduced necessity for participants to keep the instruction in their working memory for the entire duration of the categorization task. Additionally, in an exploratory manner, we examined whether the formulation of response–effect sentences influence the compatibility effect. In Article 1, verbal instructions were formulated in two distinct ways: (a) effect–response (Experiments 1 and 2) and (b) response–effect (Experiment 2). In the effect–response format, a priming stimulus (e.g., a blue patch) was verbally presented as an effect of a priming response (pressing the left/right key) but still preceded the response in the verbal formulation, thus resembling the classical stimulus–response order with perception preceding action. In contrast, the response–effect format reversed this order, as seen in typical action–effect patterns (e.g., "I need to press the left key to make the screen blue"), where the priming stimulus follows the priming response.

The central aim was to test whether verbal action–effect learning has behavioral consequences. Given the substantial evidence that verbal stimulus–response plans (perception–action order) can influence behavior, we focused on this perception–action (i.e., effect–response) order in Experiment 1 and replicated it in Experiment 2. This approach allowed us to specifically examine the potential behavioral impact of verbal instructions in the effect–response format.

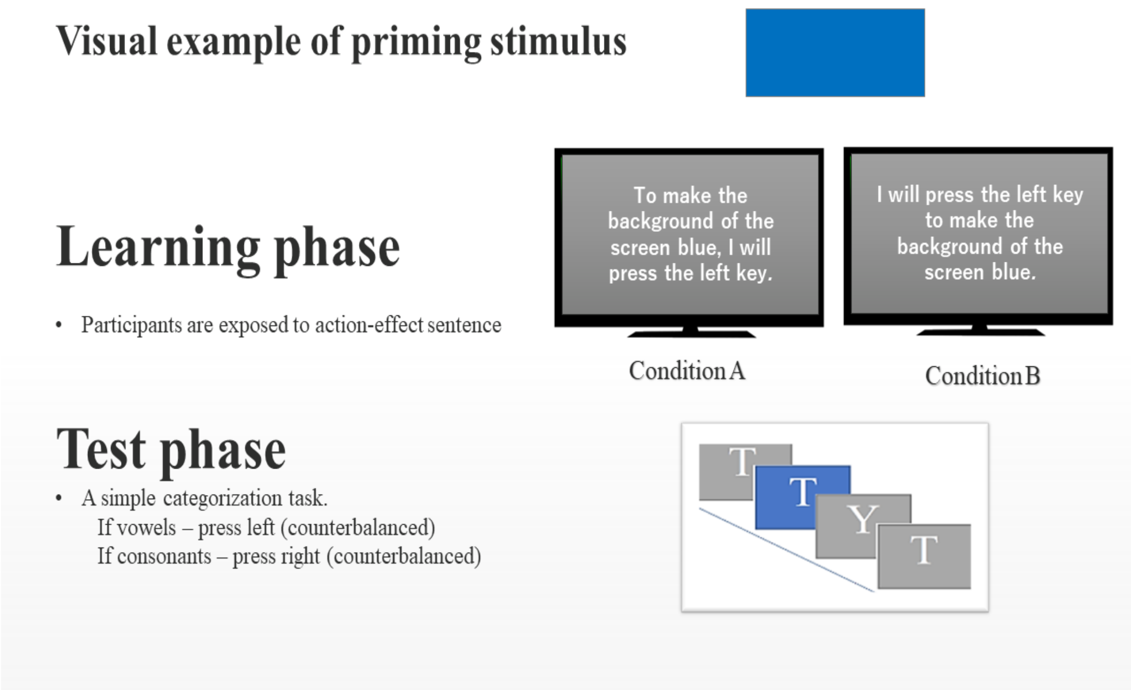
4.1.3 Methods

Article 1 featured two online experiments, with 43 participants in Experiment 1 and 400 in Experiment 2. During the learning phase of both experiments, participants were tasked with memorizing a sentence that established an effect–response relationship. For example, in an effect–response order, the sentence was phrased as "To make the screen blue, I need to press the left key." In Experiment 2, we introduced two conditions: The first half of the participants memorized a sentence in the same effect–response order as a replication, whereas the second half memorized a sentence in the reversed, response–effect order (i.e., "I need to press the left key to make the screen blue").

During the test phase, participants performed a vowel–consonant speed categorization task, where they had to categorize a presented stimulus as either a vowel or consonant by pressing either left or right keys (A and L, respectively). In a quarter of the trials, the background color changed to blue along with the displayed letter, serving as the prime stimulus (e.g., blue background). When this prime stimulus matched the response linked to it from the learning phase (e.g., blue background requiring a left key press for a vowel) and coincided with the required response for the categorization task, these trials were considered compatible. For example, a vowel presented on a blue background would be compatible if the learning phase linked blue to a left response. On the other hand, trials where the stimulus matched the prime stimulus but necessitated a different response (e.g., a consonant on a blue background requiring a right key press, contrary to the blue–left association) were defined as incompatible. Trials with a neutral gray background, where the prime stimulus (blue) was absent, were considered control trials, not predisposing any specific response. Figure 2 illustrates the general design of Article 1.

Figure 2

Visual Presentation of the Design of Article 1



Note. Prior to the learning phase, participants were made aware that any mention of the color blue would specifically indicate that color. In the learning phase, under Condition A (applicable to Experiment 1 and half of the participants in Experiment 2), participants encountered an action–effect sentence presented in an effect–response format. For Condition B (applying to the other half of participants in Experiment 2), the action–effect instruction was presented in a response–effect order. Following the learning phase, participants performed a basic categorization task that involved categorization of a given stimulus as either a vowel or a consonant. In one third of these trials, the background color changed to blue (critical trials).

4.1.4 Main Findings

The two experiments in Article 1 consistently showed—albeit with relatively weak evidence—that verbal instructions in the effect–response order had an influence on participants' subsequent behavior as measured by response accuracy. This consistency was particularly apparent in the prime-present trials, where the target stimulus was displayed on a blue background. In these instances, participants made fewer response errors when the required response for the categorization task aligned with the priming response indicated in the effect–response sentence. In contrast, the results indicated that there were more errors in trials where the required response conflicted with the priming response (e.g., left–right). In the control trials, where no prime was present, no such compatibility effect was evident. The distinct patterns of fewer errors in facilitated conditions versus more errors in interfered conditions, across both experiments, highlight the impact of the verbal instructions on behavior. However, these findings were observed only when the action–effect instructions were formulated in an effect–response manner. Furthermore, in terms of reaction times, the findings did not demonstrate that the action–effect instructions influenced participants' speed in the categorization task.

4.1.5 Conclusion

The results regarding response times in Article 1 showed no compatibility effect; instead, they indicated an overall deceleration of responses in the critical trials. It is important to note that response times and error rates are not independent variables; rather, both measure the same underlying facilitation/interference assumption. An observed effect in either response times or error rates is sufficient to support our conclusions, provided there is no evidence of a speed–accuracy trade-off. Evaluating the response errors, the findings demonstrated that effect–response associations can indeed be established through verbal instructions.

Consequently, verbal instructions can be considered a viable alternative method for acquiring effect–response associations, thereby bridging perception and action components. However, these findings were specific to the condition in which the action–effect sentence was formulated in an effect–response manner. In contrast, in the condition where the action–effect sentence was reversed (i.e., response–effect formulation), the results did not provide statistical evidence to support the formation of action–effect associations. A more detailed discussion of the interplay between response times and error rates in relation to facilitation and interference assumptions may be found in the general discussion section. However, because the interaction effect between the two order conditions was not significant, we cannot conclusively argue that there is a difference between the two order conditions.

4.2 Article 2: Associative Learning from Verbal Action–Effect Instructions: A Replication and Investigation of Underlying Mechanism

4.2.1 Background

Article 2 served as a continuation of Article 1. However, although Article 1 showed evidence suggesting that verbal effect–response instructions have behavioral consequences on response accuracy, there were still several unanswered questions. A key point of interest was the use of a visual example of the priming stimulus (i.e., a blue patch) in the verbal instructions. Specifically, the question remained as to whether such a visual demonstration of the priming stimulus is essential to observe the compatibility effect of response–effect instructions. Furthermore, the compatibility effect observed in Article 1 might have been influenced by the saliency of both the perception (e.g., the visual example of a blue patch) and action (e.g., the part of the verbal instructions specifying the direction of the response ["press the left/right key"]) components. In Article 2, we aimed to investigate these aspects more systematically in order to discern whether the visual demonstration of the priming stimulus significantly contributes to the observed effects.

Article 2 was a continuation of Article 1. However, although Article 1 showed that verbal effect–response instructions prime the responses' accuracy, the findings left several questions unanswered. First, in Article 1, participants saw a visual example of the priming stimulus (i.e., a blue patch) that was specified in the verbal instructions. Thus, it remained unclear whether such a visual demonstration of the priming stimulus is a necessary precondition to observe the compatibility effect of effect–response instructions. Second, the compatibility effect could also have occurred due to the saliency effect of perception (e.g., a visual example of a blue patch) and action (e.g., part of the verbal instructions that specified the direction of the response ["press the left/right key"]) components without establishing any link between the components.

4.2.2 Study Aims

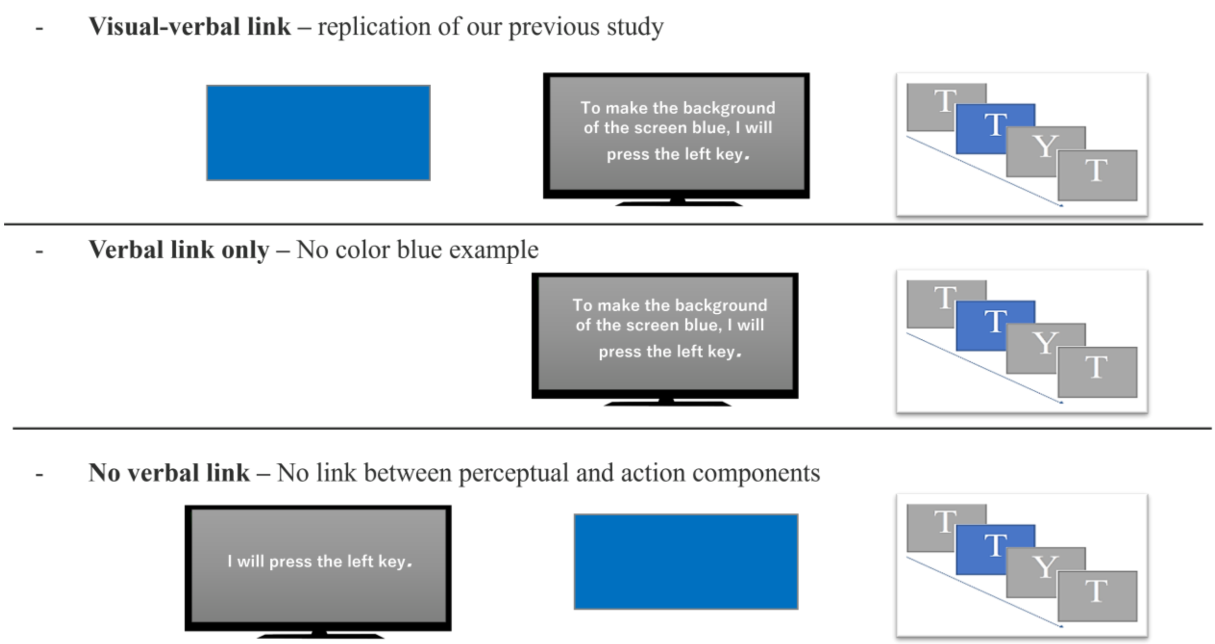
Article 2 further investigated two central aspects related to the effect of verbal action–effect instructions. First, we tested whether verbal effect–response instructions establish associative links by comparing them to the saliency-based alternative explanation. Second, we investigated whether a visual example of the verbally specified priming stimulus is a necessary precondition to observe the compatibility effect of verbal effect–response instructions.

4.2.3 Methods

Article 2 included one experiment with the final sample size *N* = 655. This experiment had three main conditions: (a) the *visual–verbal link* condition was an exact replication of the effect–response condition in Article 1; (b) *verbal link only* was a condition under which participants did not see an example of the priming stimulus (i.e., a blue patch) and only comprehended it in verbal form; (c) the *no verbal link* condition separated the perception and action components—under this condition, participants only received instructions for a specific response that was not associated with blue, and an example of the color patch was shown after the effect–response instructions.

Figure 3

Visual Presentation of the Main Design of Article 2



Note. The *visual–verbal link* condition involved the same procedure that was employed in Article 1. In the *verbal link only* condition, the procedure was the same as in the first condition, with the exception that participants were not exposed to an example of the blue-colored patch. In the *no verbal link* condition, the priming action–effect sentence did not contain information about the stimulus. Furthermore, the example of the blue-colored patch followed the priming action–effect sentence, thereby separating the perception and action components.

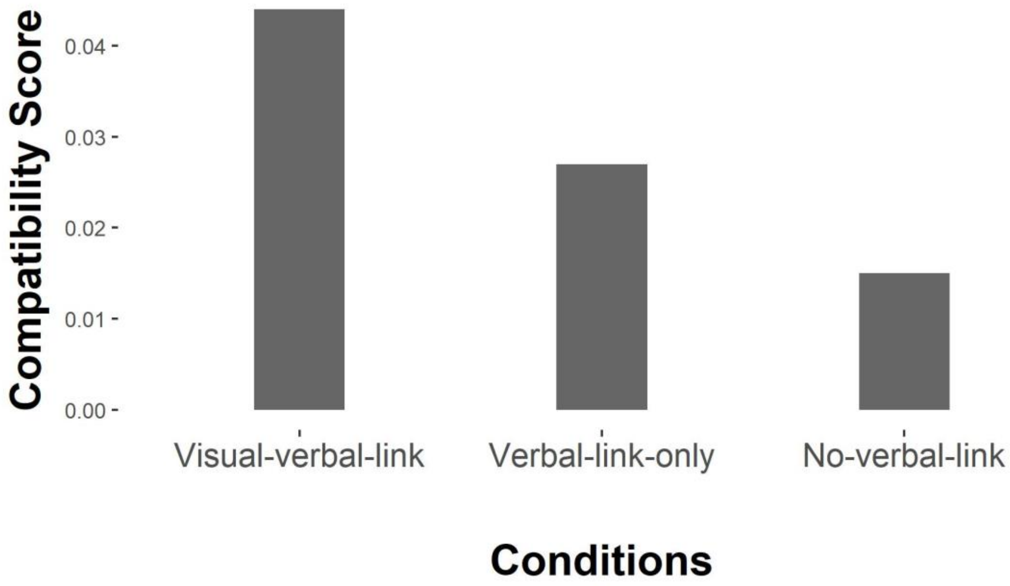
4.2.4 Results

Although the main four-way interaction analysis showed no statistically significant difference between three main conditions, we conducted a planned analysis of each condition separately. This decision was made to ensure that the findings of Article 1 were reflected in direct replication in the visual–verbal link condition. The results of the visual–verbal link condition replicated the findings of Article 1 (i.e., the effect–response condition). The results also showed that under the verbal link only condition, the response–effect instructions also influenced participants' accuracy. Notably, in the prime-present trials, participants made fewer errors when the required response matched the priming response. They also made more errors in the opposite case when the required response did not match the priming response. However, in descriptive terms, this effect was less pronounced than under the visual–verbal link condition.

Additionally, the results from the no verbal link condition showed that when the perception (i.e., a blue patch) and action (e.g., "I need to press the left/right key") components were not linked with each other; they did not produce the same compatibility effect as was observed under the visual–verbal link and verbal-link-only conditions. Similar to the results of Article 1, the overall results did not indicate that participants' response times were affected by the action–effect instructions. Figure 4 illustrates the overall compatibility gain scores of the response errors calculated separately for each condition.

Figure 4

Compatibility Gain Scores of Response Errors



Note. Scores were derived by summing the compatibility effects under the critical prime-present condition and then subtracting the equivalent sum from the control condition (i.e., prime absent). If the control condition exhibited the same result pattern as the critical experimental condition, this subtraction led to an overall score of zero, indicating no effect. A higher positive compatibility score indicates a pronounced facilitation effect from compatible configurations and/or a greater interference due to incompatible configurations. Source: adapted from "Associative learning from verbal action–effect instructions: A replication and investigation of underlying mechanisms" by Y. Damanskyy, T. Martiny-Huenger, & E. J. Parks-Stamm, 2022, *Journal of Cognition*. Licensed under CC BY 4.0.

4.2.5 Conclusion

Overall, the results replicated the central findings of Article 1 regarding the effect–response condition. In addition, the results also showed descriptive tendency, suggesting that the impact of response–effect instructions may not rely on the visual presentation of a verbally specified stimulus. This effect appears also less likely to stem from the saliency of the presented stimulus. However, given that the main four-way interaction was not significant, these findings require further investigation and replication.

4.3 Article 3: Verbal Instructions as Selection Bias that Modulates Visual Selection

4.3.1 Background

While much research has investigated the selective attention solely from bottom-up and top-down perspectives, a large body of researched has highlighted that attention guidance is guided by previous selection history that results in repetition priming (Theeuwes & Van der Burg, 2011, 2013; for a review, see Lamy & Kristjansson, 2013). Awh et al. (2012) defined selection history as selection bias; repetition priming in this context is defined as a change in reaction time or accuracy to a stimulus due to its previous presentation as well as performed responses to that stimulus (Henson et al., 2014). In other words, a stimulus becomes more salient for selection attention when it has been previously encountered.

In, addition, the effect of selection bias shares characteristics with the impact of response–effect associations that arise from active behavior. Specifically, this impact is automatic and effortless, and it can either facilitate or inhibit responses, depending on the compatibility effect. Taking into account research on verbal instructions, which suggests that verbal information influences actions similarly to a direct experience linking perception and action components, the influence of verbal instructions on perceptual areas makes them a plausible candidate for investigation as selection bias (Awh et al., 2012).

Previous research has indicated that verbal information can influence selective attention (Knapp & Abrams, 2012; Schmidt & Zelinsky, 2009; Wolfe et al., 2004). However, these studies have not examined the effect of verbal information in the context of effect–response or response–effect associations. Introducing a priming effect in the form of a verbal effect-response sentence allows for not only investigating the impact of these verbal instructions on perception but also evaluating whether this effect serves as a unified (i.e., perception–action) priming mechanism. Verbruggen et al. (2014) suggested that cognition operates under three processes of executive functions during an experimental task performance: signal detection, action selection, and action execution. By combining a visual search task with a classification task, it is possible to assess whether the action–effect sentence—as a unified priming mechanism—can help surpass these stages and provide a more direct route for responses. This aligns with the scientific perspective that perception and action are not separate systems but instead interact with and support each other (MacKay, 1987).

4.3.2 Study Aims

The primary focus of Article 3 was to explore whether response–effect instructions impact visual selective attention in the form of a selection bias. Specifically, if verbal instructions function as a selection bias, their influence should exhibit characteristics similar to biases from direct experience. Using the facilitation paradigm, I aimed to explore whether a visual search can be facilitated when the priming stimulus specified in the verbal instructions matches the target stimulus in a visual search task. Moreover, I hypothesized that this facilitation should be influenced by the response associated with the priming stimulus. As a result, when the priming stimulus matches the target stimulus and requires a response that also matches the priming response (i.e., the compatible condition), I expected faster and more accurate responses compared to the opposite pattern—that is, when the priming stimulus matches the target stimulus, but the required response is different (i.e., the incompatible condition).

4.3.3 Methods

Article 3 included two experiments (N = 88, Experiment 1; N = 90, Experiment 2). The first experiment evaluated the effect of verbal action–effect instructions on selective attention within the facilitation paradigm. The second experiment replicated Experiment 1 while accounting for the potential influence of its most repeated stimulus (i.e., frequency). As in Articles 1 and 2, both experiments in Article 3 included learning and test phases.

During the learning phase, participants memorized a specific effect–response sentence, such as “To make an apple appear on the screen, I need to press the left/right key.” During the test phase, participants performed a visual search categorization task. They had to find the target stimulus as quickly as possible, and categorize it as either a fruit or a vegetable by pressing either the left key (the A key on a keyboard) or the right key (the L key on a keyboard) (the conditions were counterbalanced). Other stimuli in the search array belong to different categories. Under the compatible condition, the target stimulus and required response matched the priming stimulus and associated response from the learning phase (e.g., apple–apple, left–left/right–right). Under the incompatible condition, on the other hand, the target stimulus matched the priming stimulus, but the required response was different (e.g., apple–apple; left–right). The control trials did not contain a priming stimulus.

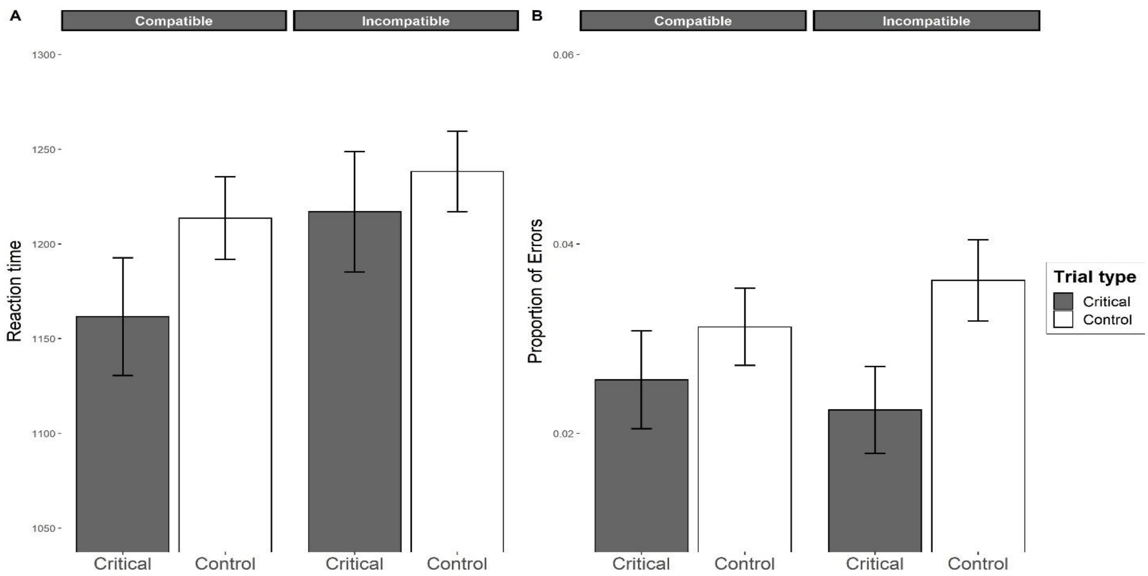
Experiment 2 addressed a central issue from Experiment 1 that could distort results and prevent the drawing of conclusions; this issue related to the effect of the most frequent stimulus presentation (Hout & Goldinger, 2010). In the first experiment, the priming stimulus (apple) appeared at a proportion of 1:3 compared to the rest of the control stimuli, mirroring the proportions used in Articles 1 and 2. However, when considering each individual control stimulus, this proportion was unequal, with the apple appearing three times more frequently than any individual control stimulus. This disparity made it challenging to distinguish the effect of the verbal effect–response sentence from the effect of the most repeated stimulus. Consequently, in Experiment 2, this unequal proportion was addressed by equalizing the appearance rate of critical and control stimuli (approximately 940 trials per stimulus). Additionally, to assess whether the frequency of stimulus appearance influenced the results in Experiment 1, I introduced an additional condition wherein one of the control stimuli appeared most frequently (at a 3:1 ratio compared to each other control stimuli). However, this stimulus was not verbally specified in the effect–response sentence as in Experiment 1 and participants were not aware of this unequal proportion.

4.3.4 Results

The findings from Experiment 1 suggest that participants in the compatible group tended to respond more quickly to the critical stimulus compared to those in the incompatible group, although this difference was not pronounced. Additionally, the speed of participants' responses to the critical stimulus did not differ significantly from their responses to control stimuli in either group. The analysis of response errors did not provide statistical evidence to support the idea that the priming action–effect sentence had a significant impact on participants' accuracy in the visual search task. A notable observation, however, was in the incompatible group, where participants showed somewhat higher accuracy in responding to the critical stimulus compared to the control stimuli. However, because the most frequent stimulus was identical to that specified in the effect–response sentence, it did not allow any conclusion to be drawn as to whether these results are due to the verbal response–effect sentence or the most repeated stimulus—or a combination of both.

Figure 5

Illustration of the Mixed-Models Analysis for Experiment 1



Note. The plot illustrates the results from the linear mixed model (A) and generalized linear mixed model (B), with the confidence intervals derived from these models. Notably, the means on both graphs represent marginally estimated means. Source: adapted from "Verbal instructions as selection bias that modulates visual selection," by Y. Damanskyy, 2023, *Visual Cognition*, 31(3), p. 8. Published by Informa UK Limited, trading as Taylor & Francis Group Licensed under CC BY 4.0.

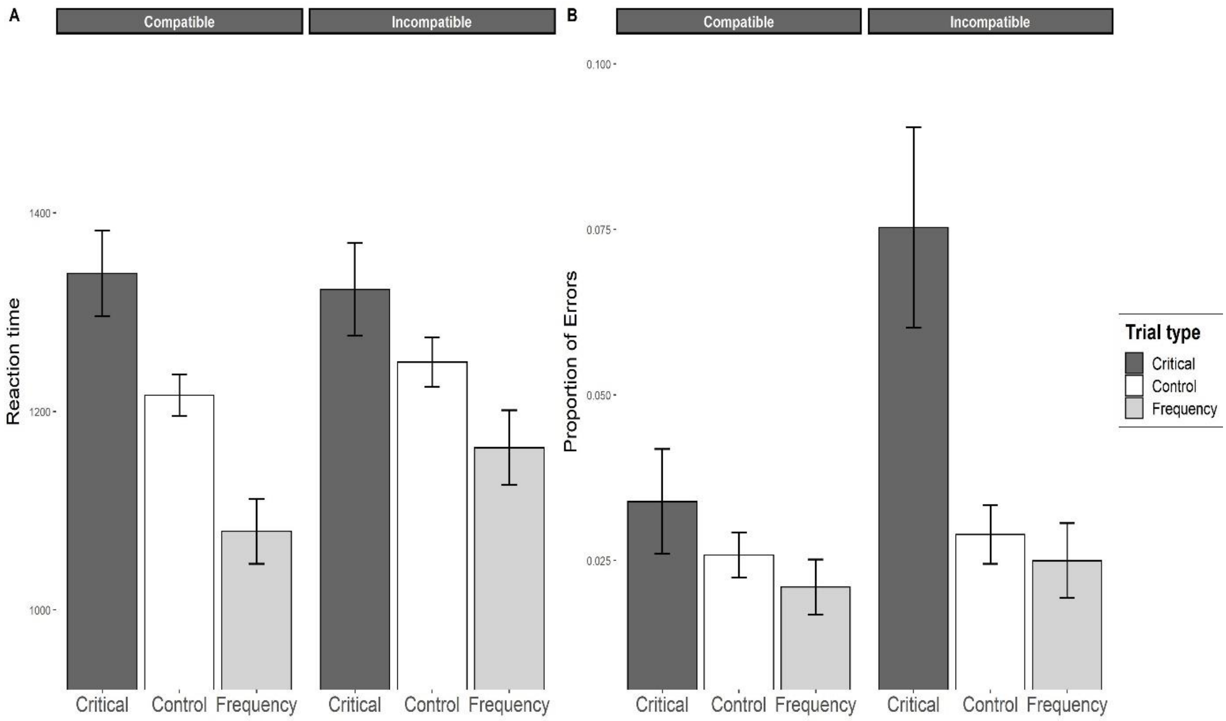
The results of Experiment 2 show that when a target stimulus matched with a stimulus specified in the verbal effect–response sentence (i.e., apple), it decelerated participants' responses, independently of whether the associated response from the response–effect sentence matched with the required response in the visual search task. The results also showed that the most frequent stimulus (i.e., carrot) resulted in shorter response times (just like the tendency in Experiment 1). Participants responded to it significantly faster than to the critical (apple) or control stimuli. The findings from the frequency condition replicate the previous findings from the visual search paradigm in that the most repeated stimulus facilitates visual search (Hout & Goldinger, 2010).

However, the findings of Experiment 2 do not replicate the findings of Experiment 1, suggesting that the observed effect in Experiment 1 was likely a combined effect of the most frequent stimulus and verbal effect–response sentence that could not be differentiated in Experiment 1. In addition, the results in Experiment 2 also showed an unexpected significant interaction effect between the most frequent stimulus and verbal effect–response sentence. In other words, the part of the verbal effect–response sentence that specified the response direction (i.e., "press left"/"press right") showed an interaction effect with the most frequent stimulus (i.e., carrot).

For response errors, the results showed a marginally significant interaction effect between compatibility and stimulus, which suggests that the accuracy among frequency, critical, and control stimuli varied between the compatible and incompatible groups. Specifically, the analysis highlighted that the accuracy of participants in critical trials within the incompatible group differed statistically from their accuracy in frequency or control trials. In addition, the accuracy of participants in the critical trials of the compatible group differed significantly from the accuracy of participants in the incompatible group. These observations are consistent with the instruction–compatibility effect found in Articles 1 and 2. However, the observed effect should be interpreted with caution, as the high-order interaction was only marginally significant (discussed further in section 5.1).

Figure 6

Illustration of the Mixed-Models Analysis for Experiment 2



Note. The plot illustrates the results from the linear mixed model (A) and generalized linear mixed model (B), with the confidence intervals derived from these models. The means on both graphs represent marginally estimated means. Source: adapted from "Verbal instructions as selection bias that modulates visual selection," by Y. Damanskyy, 2023, *Visual Cognition*, 31(3), p. 11. Published by Informa UK Limited, trading as Taylor & Francis Group. Licensed under CC BY 4.0.

4.3.5 Conclusion

The results of Experiment 1 were most likely driven by a combination of the response–effect sentence and the frequency effect, which prevented us from making a clear distinction between them. The results of Experiment 2 showed an interference effect of the response–effect sentence on visual search performance in terms of reaction time. The results also showed that the most frequent stimulus caused facilitations of response times that align with the theory of repetition priming, which suggests that frequent exposure to a stimulus can facilitate its processing.

In terms of response errors, the findings revealed (as expected) a significant instruction–compatibility effect of the effect–response sentence, which influenced participants' accuracy. This pattern of this effect is similar to that observed in Articles 1 and 2; although, in this study, participants had the additional task of identifying a target stimulus among distractors. However, the differing results between response times and response errors present a challenge in drawing definitive conclusions. Specifically, the experimental design of Article 3 did not facilitate a clear distinction between selective attention and response selection, a topic explored further in the General Discussion section. To further clarify these aspects, future research may benefit from a design that specifically addresses these distinctions, potentially leading to more conclusive insights into the mechanisms of attention and response selection.

5. General Discussion

This thesis sought to evaluate the effect of verbal action–effect instruction on action control and selective attention. In three articles, I studied this topic with the aim of answering three distinct research questions: (1) Do effect-response instructions establish effect–response links that unintentionally prime responses? (2) What are the underlying mechanisms of this priming effect? (3) Do effect-response instructions make visual selective attention more sensitive to verbally specified stimulus and response? Articles 1 and 2 employed a procedural approach involving a single stimulus presentation, focusing mainly on how verbal instructions influence response selection. In contrast, Article 3 adopted a more complex design that involved both searching for the stimulus and responding to it.

In terms of response errors, the findings across all articles showed the anticipated effect of effect-response instructions. Article 1 illustrated that effect-response instructions acted as induced unintentional response bias, indicating the presence of language-based action–effect learning. The outcomes of Article 2, in addition to central replication of the findings from Article 1, also suggested that the impact of effect–response instructions is less likely to be explained by the alternative saliency explanation and does not necessarily require a visual presentation of a verbally specified priming stimulus. The findings from Article 3 also showed that participants' accuracy in the compatible condition exceeded their accuracy in the incompatible condition, even if the critical stimulus had to be found first.

Regarding response times, the data suggested that the priming effect-response sentence interfered with participants' responses, leading to slower reaction times. Given the different patterns observed in response errors and response times, I will discuss these findings separately in the following sections. In addition, the methodology used in all articles warrants certain reflections that I will discuss in the Methodological Approaches and Limitations section.

5.1 Response Times

The reaction-time findings from all three articles give rise to several potential interpretations. In Articles 1 and 2, my colleagues and I suggested that the slower response times on priming trials might be attributable to a sudden alteration in background color. Although the effect–response's priming effect could still be present, a rapid background color change could induce one of four scenarios: (1) interference alone, (2) a combination of facilitation and interference, (3) facilitation alone. Without a control condition to distinguish the impact of the sudden background color modification from the effect of the effect-response sentence, it is difficult to form any conclusion as to how the effect–response sentence influenced response times.

The argument from Articles 1 and 2 (i.e., that a sudden shift in background color might interfere with participants' responses) is not applicable to the findings in Article 3. In these experiments, background color was not used as a priming stimulus, and the priming stimulus (apple) appeared for the same amount of time as the control stimuli (Experiment 2). The findings from Article 3 suggest the possibility that the effect–response sentence may have led to some interference with participants' response times. These findings appear to diverge from those observed in similar studies. For example, studies by Braem et al. (2019), Liefooghe and De Houwer (2018), and Theeuwes et al. (2015) all reported facilitation of responses on critical compatible trials. However, differences in study design complicate any attempt at direct comparison with these studies. Specifically, those studies were designed to test the effect of verbal instructions from working memory perspective (see Section 4.1.1 for a description of Theeuwes et al., 2015).

In addition, in the case of Article 3, which used visual search, I wish to emphasize that it is not possible to definitively determine whether the interference with participants' responses was directly related to the impact of the effect–response sentence on participants' search templates, or whether it was more related to their response selection. Several possibilities are possible: The participants' search templates may have been primed, allowing them to find the critical stimulus faster; however, the verbal effect-response sentence may also have caused interference with response selection, leading to slower responses. Alternatively, the participants' search templates may have been distorted initially by the verbal effect-response sentence, resulting in slower response times.

The premise of Article 3 was based on the idea that verbal instructions, by linking perception with action, should prime specific responses to a given stimulus and also enhance the search template's sensitivity to the priming stimulus. Consequently, I hypothesized that the compatible condition would result in the fastest and most accurate responses. Nevertheless, the observed deceleration in response times suggests a significant influence of factors beyond selective attention, particularly the role of response selection. In Article 3, I proposed that an action–effect sentence could potentially trigger a memory retrieval when priming stimulus is encountered, which might then interfere with participants' response times. This proposition is informed by studies on implementation intentions and, more specifically, prospective memory (Rummel et al., 2012). Research within this paradigm suggests that formulating a verbal action plan in a stimulus–response manner leads to the immediate recall of that action plan upon encountering the stimulus specified in the plan. However, to investigate the validity of that suggestion would require additional study addressing this specific research question.

Response times are a common metric in visual search studies, particularly in experiments that rely solely on behavioral measures without the incorporation of eye-tracking or advanced neuroscientific methodologies. These response times not only reflect the efficiency of identifying a target which is related to selective attention, but also involve the process of deciding on and executing a response, known as “response selection.” However, the interplay between response selection and target identification is complex (Starreveld et al., 2004; Yaron & Lamy, 2021), underscoring the need to consider response selection as a potential influencing factor in visual search tasks. Although response times provide valuable insights, they may not fully isolate the attentional process from the aspects of response selection, necessitating a nuanced interpretation of these results.

In summary, the reaction-time data across the three articles present a complex picture of how effect–response sentence may influence action control. The interference observed in all articles suggests that the effect–response sentence can have a subtle yet significant impact on response times, likely operating below the level of conscious awareness due to the brief response windows employed in the experiments. This effect appears to be distinct from the facilitation typically reported in the literature, highlighting the nuanced role that verbal instructions play in action control. Although the precise mechanisms remain to be fully elucidated, the evidence points to an automatic, involuntary bias that can modulate response selection and execution.

5.2 Response Errors

In the context of response errors, the results across all articles demonstrated that effect–response instructions produced an instruction-based compatibility effect. This compatibility effect refers to the alignment between the presented stimulus, the response required by the task, and the response instructed by the effect–response sentence. This effect was replicated in all articles. Specifically, Articles 1 and 2 highlighted fewer response errors in prime-present trials where participants' responses matched the priming response, compared to their responses in the same prime-present trials where their responses did not match the priming response. Furthermore, this pattern of compatibility was not observed in the control trials. Article 3 replicated this pattern, showing a similar instruction–compatibility effect, although in a more complex task where participants had to search for a stimulus before responding to it.

In addition, I wish to add that the effect observed in all three articles is less likely to involve consciousness thinking, given that the response windows in each article were relatively short (Article 1: 1.5 seconds; Article 2: 1.5 seconds; Article 3: 3 seconds). These short response windows, especially in the cases of Articles 1 and 2, are less likely to provide enough time for deliberate, on-task consciousness consideration as to which response to make.

While some previous research has indicated that the effect of verbal instructions can be observed in both reaction times and response errors (Liefooghe & De Houwer, 2018; Pfeuffer et al., 2018), this is not always the case (Eder & Dignath, 2017; Theeuwes et al., 2015). The latter studies are but two examples of a sizeable body of research with differing findings, suggesting that the instruction-compatibility effect is not consistently present in both reaction time and accuracy. This pattern can vary from study to study based on factors such as methodology, paradigm, and employed analyses (Liesefeld & Janczyk, 2019). The differences between studies can be significant, making it difficult to provide precise explanations for these variations without conducting a series of studies that directly address this research question. However, one potential factor may be closely related to how participants are instructed to respond to the target stimulus, which is, in turn, related to the discussion of speed–accuracy trade-off (Heitz, 2014).

In general, participants are instructed to respond as quickly and accurately as possible. However, as Liesefeld and Janczyk (2019) noted, an experimental effect may be observable through either reaction time, response error—or both—for various reasons, such as participants' decision to focus more on speed or accuracy, experimental procedure, specific sample, or different experimental conditions. In all three articles, my co-authors and I checked for potential influences of the speed–accuracy trade-off and found no statistical evidence of its influence (for a review, see Heitz, 2014). Therefore, I do not consider it problematic that the impact of the effect–response sentence was evident only through response errors, and not response times.

The findings also showed that overall participants' accuracy in compatible trials was not better than their accuracy in control trials. These findings do not necessarily conflict with the main research questions, as the overall number of response errors in all three articles was relatively low. This suggests that the task may have been relatively straightforward, allowing participants to achieve high accuracy even without the assistance of a priming response–effect sentence (i.e., floor effect; Dixon, 2008).

5.3 Instructions as an Extension of the Ideomotor Principle

Language shapes cognition in multiple ways. For example, it endows humans with certain cognitive abilities that nonverbal organisms cannot possess. Language can also shape thinking through guidance and saliency (Lucy, 1997). The overall spectrum of language's influence on cognition is a broad topic that extends beyond the scope of this thesis (for an extended discussion and overview, see Gomila, 2012). Within this spectrum, I wish to highlight one potential other key function of language—namely, that it serves as a toolkit for controlling behavior.

As stated in the Theoretical Background chapter, studying human actions is challenging. In experimental settings, a stream of human actions is reduced to simple stimulus–response and response–effect representations that a researcher can study through different paradigms, but in real life, the matter is more complex because actions are represented in sequential, hierarchical structures (Cooper & Shallice, 2006; Kruglanski et al., 2002). Furthermore, these structures can have abstract meanings with high-level, abstract goals (e.g., drinking coffee) that are expressed by low-level actions (e.g., finding a cup, grasping the cup). One way to view this complexity is to adopt the suggestion of Frings et. al. (2020) that actions are represented as stimulus–response–outcome representations. In this case, the interactions between stimuli and responses are expressed not only in stimulus–response or response–outcome relationships but also in stimulus–outcome links.

Although this thesis did not study actions in the context of complex stimulus–response–outcome models, it can be argued that the findings from all three articles provide insight into the larger construct of language-based action control. In addition, by focusing on the stimulus–response aspect, as seen in instruction-based and implementation intention research, this thesis also examines the effect–response dimension. Although the three articles suggest that verbal instructions may have distinct impacts on response times and response errors, they collectively indicate that verbally formulated effect–response sentences can affect response selection in a manner akin to learning observed in active behavior.

Furthermore, the findings of Article 1 related to the format of the formulation of the effect–response sentence leave several questions unanswered suggesting new directions for investigations. Although the main four-way interaction was not significant, and does not allow to draw any specific conclusions, the findings suggested a descriptive tendency that the order in which a verbally specified stimulus and response (i.e., effect–response vs response–effect) are presented might influence the connection between perception and action components. Although the findings of Theeuwes et al. (2015) demonstrated that the verbal sentence formulated in response–effect order has an impact similar to that of the verbal stimulus–response formulations found in other studies on implementation intention and instruction-based research, several factors may still relate to the linguistic aspect of this matter. Factors such as causality (Wolff, 2007), grammatical structure and clarity (Slobin, 1996), or even the specific language in use could play a role (Boroditsky, 2001).

However, I wish to emphasize that in all articles in the present thesis, the impact of the effect–response sentence was examined based on the idea that verbally linking perception and action components functions similarly to temporal proximity based on active behavior. That is, a link is established between perception and action components when an action is performed, and its observed effect is temporally proximate. Although some studies using verbal codes in a simplified manner have shown that these verbal links can function similarly (Pfeuffer, Moutsopoulou, et al., 2017; Pfeuffer et al., 2018), more complex formulations may have a more complex effect on perception and actions.

5.4 The Underlying Mechanism of Verbal Instructions

Research on action–effect associations that are based on active behavior suggests that on a physiological level, *neural priming* underlies action–effect associations (Waszak et al., 2012). Neural priming (also known as repetition suppression) implies that repeated exposure to the same stimulus results in suppressed activity in the perceptual, prefrontal, and motor areas (Maccotta & Buckner, 2004), which are associated with perception and the improvement of behavioral performance concerning that stimulus. Therefore, the suppression of neural activity in these sensorimotor areas may indicate associative learning (Henson et al., 2014).

I, with my co-authors, suggested that the effect of verbal instructions is based on simulation theory (Hesslow, 2011), which posits that the human brain can simulate the activity in sensorimotor areas in ways similar to the activity that occurs when perceiving a real object or performing an action with it. According to this theory, there are multiple sources for a simulated activity (e.g., the imagination, memory recall, the observation of others' actions). However, as Martiny-Huenger et al. (2017) stated, comprehension of verbal instruction also triggers simulated activity in the brain, resulting in the formation of associations between perception and actions. This idea is based on the embodied cognition perspective (Barsalou, 1999, 2010) and studies demonstrating that sensorimotor areas are activated by the processing of verbal sentences involving action words (Arbib, 2008; Pulvermüller & Fadiga, 2010). Thus, when a person comprehends verbal instructions, associative links are formed between sensorimotor areas—which, in turn, can cause unintentional bias in behavior.

Although all three articles' results related to response errors can be placed within the framework of language-based simulated activity theory (Hesslow, 2011; Pulvermüller & Fadiga, 2010), the results from response times also indicated the potential presence of an additional layer of complexity caused by instruction-based effects, thereby highlighting a gap in our current understanding. Without a specific answer as to what caused this interference in the studies, it is difficult to place the findings from response times fully within existing theoretical frameworks. The divergence in results between response errors and times may indicate the need for a more nuanced perspective and research investigating possible interference effects caused by instructions.

6. Methodological Approaches and Limitations

In this chapter, I will discuss a particular aspect related to methodological approaches and highlight specific conclusions that can be derived from this thesis. Additionally, I wish to acknowledge and discuss certain limitations affecting all articles, including potential biases, which must be borne in mind if the studies are to be replicated.

6.1 Significant Effect

Articles 1 and 2 employed similar research designs and, consequently, they shared certain limitations that arose from the statistical analysis. Specifically, in Article 1, the main comparison among different formulations of action–effect sentences yielded no significant difference. Similarly, in Article 2, the main four-way interaction analysis indicated no significant differences among the three primary conditions. I consider these results a consequence of employing complex four-way interaction analyses. Four-way interaction is a complicated procedure whose results are not always easy to interpret, prone to adding statistical noise (e.g., overfitting of the model; Frost, 2020), and may lead to challenges related to power, multiplicity, sparse data when employing complex statistical models (Cohen et al., 2003; Sinacore, 1993).

In all papers, I employed the term “marginal significance” to interpret certain *p* values that exceeded the commonly accepted threshold of 0.05 but remained below 0.10. On the one hand, there are compelling arguments against employing such terminology. For example, results that are defined as “marginally significant” have a reduced likelihood of replication (Benjamin et al., 2018). At the same time, taking into account that the central effect of effect–response verbal instructions has been replicated twice in the same design (Articles 1 and 2) and once more in a more complex design (Article 3, Experiment 2), I also wish to clarify why such terminology was used.

The practice of interpreting *p* values between 0.05 and 0.10 is not uncommon in psychology (Olsson-Collentine et al., 2019). This was used with an idea that the 0.05 level is often used without clear justification, leading to problems such as inconsistent interpretations and a false sense of certainty (Gelman & Stern, 2006; Wasserstein & Lazar, 2016). This becomes particularly problematic when *p* values hover near the threshold. Critics advocate for a more nuanced comprehension of statistical evidence, emphasizing the importance of context, experimental design, and practical significance over rigid thresholds (Nuzzo, 2014).

Therefore, that we obtained the same pattern of results across all articles (in terms of response errors), although not always reaching a commonly accepted level of significance ($p < .05$), suggests that we are less likely to have committed a type 1 error when interpreting our results as marginal significance (Article 1: Experiment 1, $p = .08$; Article 1: Experiment 2, $p = .05$; Article 3: Experiment 2, $p = .09$). This consistent pattern, even with slightly higher *p* values, reinforces the potential validity of the findings. This approach was based on the importance of considering the broader evidence that is based on consistency rather than focusing solely on conventional, categorical interpretation of *p* values.

6.1.1 Significance Testing Using Mixed Models

Although mixed models are commonly described as a mere extension of regression models that also account for random factors, it is important to note that significance testing in mixed models is more advanced (Winter, 2020). The likelihood ratio test allows for the comparison of entire models and determines whether adding additional effects would enable a model to better fit a given data set. Mixed models also offer the possibility to obtain *p* values from Wald *t* values (i.e., *t*-as-*z* approach; Luke, 2017). Moreover, mixed models provide opportunities to obtain *p* values based on parametric bootstrapping, Kenward–Roger, and Satterthwaite approximations for degrees of freedom (Luke, 2017; Mehmetoglu & Mittner, 2022). Each of these methods involves complex mathematics, discussion of which would well exceed the scope of this thesis (see Luke, 2017 for an extended discussion on this topic). However, based on the results of Article 3, there are a few points that merit at least some reflection in this discussion.

Interpretation of significance in Article 3 was based on Satterthwaite approximations, taking into account the recommendation of Luke (2017), who emphasized that this method provides an acceptable type 1 error rate, in addition to being somewhat preferable to the likelihood ratio test. In addition, the results of Article 3 also provide confidence intervals derived from parametric bootstrapping to give additional insight into the data. Mehmetoglu and Mittner (2022) recommended greater reliance on confidence intervals produced by parametric bootstrapping, rather than *p* values produced by Satterthwaite approximations, because a degree of freedom based on approximations is not always reliable.

At one point in this thesis in Article 3, the results offer a flexibility of interpretation that is open to criticism. Specifically, the results for response errors in Experiment 2 showed that “overall two-way interaction between prime and compatibility was marginally significant ($b = -0.37$, 95% CI [−0.83, 0.06], $p = .091$).” Such results, especially upon introducing the marginal significance term, entails certain complications for interpretation. As these results could also be interpreted as non-significant, instead of marginally significant. The term “marginal” is not applicable with confidence intervals procedure because it is not clear, even in arbitrary terms, when the results should be considered as either marginally significant or non-significant. Therefore, a more clearly predefined criteria is needed to avoid such ambiguity in interpretations.

In addition, a question arises as to whether overall interpretation would change if I were to rely solely on confidence intervals. Cases in which different significant testing analysis might produce different results are rare, being more common in studies with very small sample sizes (Luke, 2017). In the particular case of Article 3, the interpretation is not a problem because the main results for compatibility effect are not affected by this problem (i.e., $b = 0.84$, 95% CI [0.20, 1.54], $p = .008$), as is shown by both bootstrapping and Satterthwaite methods. To avoid potential challenges with interpretations, one solution would be a full preregistration—not only of what is typically preregistered (e.g., sample size, outlier criteria) but also of a method to interpret significant levels, in line with the recommendations of Luke (2017) as well as Mehmetoglu and Mittner (2022). Because the analytical procedures in Article 3 were not preregistered, the presence of marginal significant interpretation can be considered a limitation because it introduces some variability into how a given result can be interpreted.

6.2 Sample Size

The sample size for Article 1 was based on both our sample size evaluation of previous studies within the field (e.g., range 50–100) as well as the resources available to us (e.g., how many participants can be recruited per study). In Article 2, we determined our sample size based on findings in Article 1 without power analysis; for Article 3, the sample size was determined via simulation analysis for response times. In this section, I aim to reflect on certain points related to sample size and analyses of response errors. Given the relatively high accuracy rate in all studies (Article 1 [Exp 1 ~ 94%, Exp 2 ~ 96%]; Article 2 [~ 96%]; Article 3 [Exp 1 ~ 96%, Exp 2 ~ 97%]), there are potential limitations related to statistical power.

Specifically, the analysis of response errors may be underpowered due to low percentage of response errors. Generally, the accuracy in experiments like those in all three articles are related to: (1) given instructions (e.g., focus more on speed or accuracy or both); (2) task complexity; or (3) individual factors (participants may prioritize more speed versus accuracy, or vice versa). However, if the experimental task itself is relatively easy, the overall accuracy will be high, and even if the hypothesized effect is still present in the within response errors, then the amount of observation should be sufficient to demonstrate that effect. Given that findings and conclusions in all three articles are based on response errors, it remains an open question as to whether the studies had enough power.

In specific case of Article 3, Experiment 1, in which sample size was calculated based on the power simulation analysis of a pilot study, the calculation was made solely on response times. Furthermore, because Experiment 1 did not incorporate a frequency predictor into its model when power calculation was performed, it remains unclear to what degree that would change the outcome of power simulation analysis.

6.3 Memory Task

Unlike the study by Theeuwes et al. (2015), this thesis is based on the idea that verbal instructions involve associative learning and can have a lasting effect on behavior. Therefore, an inability to recall the priming sentence does not necessarily indicate that the effect of the verbal instructions is absent. However, such a design would be challenging because it would necessitate a much longer interval between presenting the priming instructions and testing their potential effect.

The methodologies used in Articles 1 and 2 did not include measures to determine whether participants had processed the priming instructions. Our rationale was based on the idea that if we introduced an evaluation checkpoint immediately after participants memorized the verbal instructions (e.g., by asking them to type out the memorized sentence), it might interfere with the priming procedure because the participants would then have to perform a specific action mentioned in the verbal instructions. Additionally, evaluating whether participants can recall the verbal instructions after completing the task is tied to certain aspects (discussed further in this section) that preclude a precise assessment of whether participants indeed read the instructions.

Nonetheless, the effects observed in all three articles may have been distorted by a subset of participants who did not read the instructions thoroughly—or at all. Specifically, if they did not read the effect-response sentence, the responses were not influenced by and do not contain valuable information in prime present trials. However, the memory task employed in Article 3 identified whether a participant could recall the priming effect–response sentence at the end of the experiment, this measurement could not differentiate participants who managed to forget the sentence by the end of the visual search task from those who never read it in the first place. Thus, it remains possible that participants who overlooked the priming effect–response sentence section may have affected the overall findings.

The supplementary materials for Article 3 include a table that shows a post-hoc analysis similar to the one presented in the main body of the article. However, this analysis is based solely on a subset of participants who correctly recalled the priming effect–response sentence at the end of the experiment. For Experiment 1, regarding reaction time, the analysis revealed that the marginally significant difference between compatible and incompatible trials, previously observed, was no longer significant (p = .156). The results for response errors remained consistent with the original findings. Nevertheless, considering that the results from Experiment 1 in Article 1, these results do not allow differentiate the effect of the most repeated stimulus (appearance frequency) and verbal instructions, therefore, it becomes challenging to ascertain the implications of this change in significance.

In Experiment 2, the results of the subgroup of participants who successfully recalled the verbal effect–response sentence indicated a significant interaction between prime and compatibility (p = .022), contrasting with the original results (p = .091). Aside from this, the fixed-effects results remained unchanged (values of ps and bs were, of course, slightly different because the number of participants was different). However, the more pronounced compatibility effects, as well as the notable impact of the two-way interaction in this subgroup, suggest that implementing a task similar to that in Article 3 is beneficial. This is because the original results may have been influenced by participants who did not recall, or did not read, the verbal effect–response sentence.

However, taking into account that the memory measurement is post-hoc (originally, it was implemented to avoid deception; see Appendix B of Article 3), as well as how the memory task was implemented, does not provide a clear opportunity to draw definitive conclusion. Participants were simply asked to press a corresponding key to make an apple appear on the screen (the critical stimulus; slide 11 in Appendix B of Article 3). This task could have acted as a prompt for participants to recall the priming sentence, because without a prompt, some participants might not have been able to recall the effect–response sentence.

Nevertheless, the results of this post-hoc analysis suggest a good reason to implement a similar memory check in future studies. For example, a more extended design that can differentiate between participants who (a) read the instructions, (b) read but could not recall the instructions, or (c) did not read the instructions at all may provide better insight as to whether the effect of verbal instructions relies on such memory recall. In the subsection Time Spent on Completing the Task, I also discuss several methodological points that are relevant to such a memory check.

6.4 Online Settings

In addition, I wish to highlight several important methodological aspects related to studies conducted online. One advantage of using a laboratory setting is that the researcher can properly supervise the participants. This possibility is limited when participants participate remotely and are guided by instructions in text format. Therefore, we cannot be certain that all participants properly understood the instructions of our studies. Although various companies provide a range of possibilities for recruitment that can decrease the number of random interference factors, I emphasize the importance of presenting all instructions as clearly as possible in an online experiment.

6.4.1 Participant Pool

Companies such as Prolific provide rich methodological kits from which one can choose specific criteria for the recruitment of potential participants. However, it is impossible to control how many times a participant has already participated in similar categorization tasks during other studies, which can have a decisive impact on their response time scores independently of the priming paradigm. As Wong et al. (2017) noted, performing different types of reaction-time tasks improves overall response-time scores. Thus, reaction times may also reflect the responder's habits rather than the computational parameters of a specific task. Although this situation may change over time, and recruiting companies may start providing such information for recruiting criteria, until this has been done, this point must be acknowledged.

6.4.2 Time Spent on Completing the Task

The time participants take to complete a task can vary due to natural differences among them, such as age and the time of day they take the test. In our articles, the average time participants took to complete a task was around 10 minutes. Although I excluded data from participants who deviated significantly from this average (e.g., ~30min), I did not set a specific time limit for the learning phase. Monitoring this duration may serve as an alternative measurement to control whether participants read the priming verbal instructions.

Furthermore, even though participants were informed that they should carefully read all instructions, I did not employ any technical or statistical methods to verify whether they followed these instructions. As such, some participants may have rushed through the learning phase without allocating sufficient time to memorize the effect–response sentence. I wish to stress that in similar online studies, especially when the presentation of verbal instructions is crucial, controlling or evaluating the time participants spend on the learning phase can be an important variable to identify potential outliers. Such an approach may be an alternative solution to implementing the memory task discussed previously.

6.5 Generalizability

I used real objects as target and control stimuli in Article 3 to increase ecological validity (Bravo & Farid, 2004). However, to complement what is written in Article 3 about the advantages of using real objects, I wish to emphasize two potential disadvantages as well. First, the use of real objects may bring standardization challenges due to variations in size, color, texture, and other characteristics that could introduce variability into the data and thereby distort the results. Second, it complicates replicability by necessitating not only access to the same objects, but also control over their size.

Another notable limitation in our study is that the critical target stimulus (i.e., that which participants were required to find and categorize) was not systematically counterbalanced. Although Experiment 2 of Article 3 sought to replicate Experiment 1 while also accounting for a possible effect of the most repeated stimulus (frequency appearance), the possibility remains that the specific visual features of the critical target stimulus may have influenced the results. This possibility is supported by the boxplot analysis from Experiment 1, which indicated that certain stimuli could deviate significantly in terms of response time or response errors.

One way to resolve—or, at least, mitigate—that limitation would be to introduce an additional random effect to account for potential variations among different stimuli. This approach was applied in the reaction-time analysis, where stimulus identity was included as a random intercept. However, for response errors, the analysis revealed that stimulus identity did not significantly contribute as a random factor (SD = 0.00, $\chi2(1) = 0.0$, $p = 1$ in Experiment 1; SD = 0.00, $\chi2(1) = 0$, $p = .99$ in Experiment 2). It is important to note that this outcome arose not because stimulus identity lacked explanatory power, but rather due to the insufficient number of observations of response errors for each stimulus, which prevented the mixed model from accounting for this random effect. This aspect ties back to overall accuracy in the experiment and the sample size discussion previously addressed in the subsection on sample size. Specifically, number of errors should be enough for each stimulus category in order to estimate this random effect.

7. Conclusion

The present thesis makes several contributions to the existing research on action–effect learning. Whereas action–effect learning has been studied primarily through the lens of behavioral research, this thesis approaches that topic from a language perspective. The results of its three constituent articles show that action–effect instructions exert a direct priming effect on action selection, and furthermore, this thesis has established—and consistently replicated—a central effect of verbal (effect–response) instructions on subsequent actions. Although additional aspects (e.g., order of the perception–action information and alternative explanations for associative learning) could not be resolved conclusively in the present thesis, they raise important questions that future research may seek to answer, and in doing so lead us toward a better understanding of the underlying mechanism that translates verbal information into action.

Interpreting these findings from an ideomotor perspective, I suggest that language serves as an important mechanism in action control; that is, once a preverbal sensorimotor experience becomes verbalized during the early stages of life, language becomes an important self-regulatory tool that brings flexibility and adaptivity to action control. This, in turn, constitutes an important contribution to the ideomotor perspective, extending its core principle and suggesting that language serves as an additional learning mechanism for establishing new action–effect associations.

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Paper 1

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ORIGINAL ARTICLE

Unintentional response priming from verbal action–effect instructions

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Abstract

Action–effect learning is based on a theoretical concept that actions are associated with their perceivable consequences through bidirectional associations. Past research has mostly investigated how these bidirectional associations are formed through actual behavior and perception of the consequences. The present research expands this idea by investigating how verbally formulated action–effect instructions contribute to action–effect learning. In two online experiments (Exp. 1, $N=41$, student sample; Exp. 2, $N=349$, non-student sample), participants memorized a specific action–effect instruction before completing a speeded categorization task. We assessed the consequences of the instructions by presenting the instructed effect as an irrelevant stimulus in the classification task and compared response errors and response times for instruction-compatible and instruction-incompatible responses. Overall, we found evidence that verbal action–effect instructions led to associations between an action and perception (effect) that are automatically activated upon encountering the previously verbally presented effect. In addition, we discuss preliminary evidence suggesting that the order of the action–effect components plays a role; only instructions in a perception–action order showed the expected effect. The present research contributes evidence to the idea that action–effect learning is not exclusively related to actual behavior but also achievable through verbally formulated instructions, thereby providing a flexible learning mechanism that does not rely on specific actual experiences.

Many of our daily activities are aimed at achieving specific desired outcomes. However, how are specific actions selected to produce a desired outcome? The concept of action–effect learning, based on the principles of ideomotor theory, provides a basic idea for how intended outcomes can control our actions. The general idea is that specific actions trigger perceivable changes in one's surroundings (i.e., effects), and the temporal proximity of these events results in the formation of associative links between actions and their perceivable consequences (action–effect associations, e.g., Elsner & Hommel, 2001). Because of these associative links, thinking about the effect will activate the linked behavior that previously produced the effect.

Empirical testing of the action–effect concept typically involves two stages (Elsner & Hommel, 2001; Greenwald, 1970). In the first stage (*learning phase*), participants experience the co-occurrence of specific actions and their effects (action–effect contingencies). The second stage (*test phase*) tests whether associations have been formed. In line with the assumption that such associations are bidirectional, exposing participants to previously encountered effects has been found to facilitate the respective associated actions (e.g., Elsner & Hommel, 2001; Paulus et al., 2011; Pfister, 2019; Shin et al., 2010). In the present research, we tested the idea that the acquisition of action–effect associations is not limited to actual behavior but can be acquired through verbal instructions.

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Verbally induced action control

In this section we summarize two research areas (i.e., implementation intentions and instruction implementation) that provide evidence that verbal information can influence subsequent action, potentially mediated by stimulus–response learning. Based on this evidence, we will then argue that verbal information about an action and an effect might also lead to action–effect learning.

The theory of implementation intentions suggests that behavior can be strategically controlled by forming a verbal plan in an *if–then* format (Gollwitzer, 1999). According to this theory, if–then planning creates direct perception–action links between the anticipated situation (critical cue) and the intended behavior (action). For instance, after forming the plan "If I pass a supermarket, then I will buy fruit," the situation (supermarket) serves as a critical cue that triggers the planned action (buying fruit). Empirical laboratory tests of this idea are similar to the previously described action–effect learning procedure, except that during the learning phase, participants form specific verbal if–then action plans instead of actually enacting responses. In one example of such a test (Cohen et al., 2008, Exp. 2), participants memorized "If I hear the low tone on the left side, then I will press the right button especially fast." In the test phase, participants were asked to perform a two-alternative forced-choice task (i.e., if the tone was high, they pressed the left button; if the tone was low, they pressed the right button). The results showed a response/verbal plan compatibility effect: required responses to the critical stimulus (if-part) were facilitated if they overlapped with the responses specified in the then-part of the plan. These and other similar results demonstrate that verbal (stimulus–response) planning influences subsequent behavioral responses (Cohen et al., 2008; Martiny-Huenger et al., 2017; Miles & Proctor, 2008).

Research from an instruction-based perspective provides additional evidence that instructions in the form of stimulus–response mappings can influence performance. The basic design of this type of research also involves a learning phase (verbal instructions) and a test phase. However, in many studies, the test phase is split into a *diagnostic task* and an *inducer task* (e.g., Liefooghe et al., 2012). The given instructions are relevant for the inducer task but irrelevant for the diagnostic task. For instance, the instructions for the inducer task might read, "if you see 'cat', press left; if you see 'dog', press right." However, before completing the inducer task, a preceding diagnostic task is introduced that shares both the stimuli (i.e., words 'cat' and 'dog') and responses (left/right button press) with the inducer task, but has different task instructions (e.g., to press the right or left button if the words are italicized or upright, respectively). Using this design, studies have demonstrated the presence of an instruction-based compatibility effect in the diagnostic task when the required response and the stimulus match the instructions given for the inducer task (e.g., when "cat" was italicized and required the left key response; for a review see Brass et al., 2017).

One of the fundamental differences between implementation intentions and instruction implementation research is that critical if–then sentences in implementation intention research are strongly highlighted and repeated as a central, important sentence to encode and remember (reviewed by Gollwitzer & Sheeran, 2006). Instruction-based research does not include such emphasis on a single sentence. The critical "if–then" instructions are just a part of the typical task instructions (Liefooghe & De Houwer, 2018; Liefooghe et al., 2012). Another central difference in these approaches is in the delay between reading the verbal plans/instructions and tests of their effects. If–then plans' effects are tested minutes (in laboratory settings, e.g., Cohen et al., 2008) or even days or weeks later (e.g., Conner & Higgins, 2010; Papies et al., 2009). The effects of "instructions" are tested only seconds later (Brass et al., 2017). These time differences are relevant to the present research, and we will continue to discuss them later. In general, however, the two approaches share many similarities. For example, the verbal information in both cases typically includes a stimulus–response contingency. Both imply that verbally presented stimulus–response (perception–action) contingencies influence subsequent behavior.

Verbal instructions within the action–effect paradigm

Theeuwes et. al. (2015) used a similar learning-test design in the context of action–effect learning. In three experiments, the authors provided instructions in an action–effect format in a learning phase and tested whether presenting the "effect" in a subsequent test phase would trigger the associated action. An example of an action–effect instruction from this research was: "If you press left, 'P' appears." These instructions made sense to the participants as there was a part of the test phase in which participants produced the letter 'P' by pressing the left key (similar to the previously described inducer task in stimulus–response instruction-based research). Importantly, for testing action–effect learning, the letter 'P' also appeared as a target for a classification task (related to whether the letters were presented upright or italicized in the diagnostic task). The left and right key presses in the classification task established compatible and incompatible response trials with the action–effect instructions. The authors found that compatible responses (e.g., having to press the left key for the upright/italicized 'P') were facilitated compared to incompatible responses (e.g., having to press the right key in response to the upright/italicized 'P'). Consequently, Theeuwes et. al. (2015) provide evidence that instructions that link an action to an effect can influence performance in an immediately followed (separated only by a few seconds) ostensibly irrelevant task.

The present experiments

In the present research, we tested whether verbal action–effect instructions lead to associations between an action and an effect that are automatically activated upon perceiving the effect even if instructions and test are separated by more than a few seconds. We asked participants to memorize a specific verbal instruction that contained information about an action–effect relation ("To make the screen blue, I have to press the left key"). Afterward, participants performed a vowel-consonant categorization task. Although the task was unrelated to the action–effect instructions, responses in the categorization task overlapped with the responses specified in the action–effect sentence (i.e., left/right key). Importantly, on some trials, the screen background color turned blue (i.e., effect). This aspect was irrelevant for the categorization task and participants were instructed to ignore it. However, the presented blue background visually primed the effect from the action–effect instructions. We hypothesized that the priming of the effect would result in facilitated action–effect-compatible responses (i.e., categorization responses that align with the action–effect instructions) and/or in impaired incompatible responses (i.e., categorization responses that are different from those specified in the action–effect instructions).

While conceptually related to Theeuwes et. al. (2015), our present studies go beyond their evidence that action–effect instructions influence subsequent actions. We separated the processing of the instructions from the performance in the diagnostic task. To do this, we presented one action–effect instruction at the beginning of the experiment instead of continuously updating the instructions every 4, 6, or 16 trials. Thus, whereas Theeuwes et al. observed effects of instructions that participants read a few seconds earlier, we tested the effects of a single action–effect instruction presented to the participants a few minutes earlier (before reading other information like the categorization task instructions). Second, in the case of Theeuwes et. al. (2015), participants continuously performed inducer-task trials in the test phase, where the action–effect instructions were relevant after every 4, 6, or 16 trials. In the present work, participants were also told that the verbal action–effect instructions would be relevant at some point during the experiment. However, this information served only as a cover story and the participants never actually had to implement the instructions.

In sum, the effects of instructions on subsequent responses in Theeuwes et. al. (2015) were observed with instructions processed only seconds prior to testing their effects and in a context, where the participants were aware that the instructions were relevant just a few seconds later. In contrast, we tested effects with a longer time interval and in a context, where the instructions never had to be implemented and thus there were no explicit reminders of the action–effect instructions during the test phase. We conducted two online experiments. In the first experiment, we tested verbal action–effect instructions in an *effect–action* order. The central focus of the second experiment was to provide a direct replication of Experiment 1 with an increased sample size. In addition, we added an exploratory part in which we reversed the order of the instructions (*action–effect* order).

Experiment 1

Participants memorized the action–effect instructions "To make the screen blue, I have to press the [left/right] key". They then received additional instructions on how to perform the subsequent categorization task (press left/right for vowels/consonants). During this categorization task, the effect from the action–effect sentence (i.e., the blue screen background) was presented on a fourth of the trials. We hypothesized that perceiving the effect from the instructions should activate the verbally associated action and thus facilitate compatible responses (e.g., for blue-left instructions, perceiving blue and the left key is the required response) and/or interfere with incompatible responses (e.g., for blue-left instructions, perceiving blue and the right key is the required response).

In contrast to typical action–effect learning, where the action comes first, we presented the instructions in an effect–action format. This decision was driven by if–then planning research, where verbal information is given in a perception (if-part)–action (then part) order. Furthermore, in typical action–effect learning (and testing), a bi-directional link is required for an effect to trigger an associated response. As bi-directionality is an additional assumption that was not the central focus of our experiment, we decided to formulate the action–effect instructions in an effect–action format to align it with the to-be-encountered order in the test phase (i.e., perceiving the effect and executing the associated action; see "Experiment 2" for more information on the action–effect instruction order).

Method

Participants

A total of 43 Norwegian-speaking adults participated in the study. Following data cleaning described in "Data analysis and data preparation approach" section below, the analyzed sample included the data of 41 participants (20 females, 20 males, and one missing gender response). The ages ranged from 19 to 51 ($M = 24.14$, SD = 5.04). The participants were compensated by participating in a drawing for one of two gift cards for a local shopping mall with a value of 500 NOK each. The study was approved by the local ethics committee, and all participants provided informed consent.

Design

Our design included two within-participant factors: *required response* (left key vs. right key) and *effect prime* (present vs. absent), and one between-participant factor: *instructed response* (press left key vs. press right key). *Required* *response* specified what response was required from participants in a given trial according to the categorization task instructions. *Effect prime* specified whether the blue screen was present (critical) or absent (neutral) in a given trial. *Instructed response* was a between-participant factor indicating the instructed action in the action–effect sentence (left key vs. right key; "To make the screen blue, I will press the left/right key"). Key assignment to vowel/consonant was counterbalanced between participants.

Procedure

The design and the procedure of this experiment originates from an unpublished experiment in a laboratory setting with various adjustments (see Appendix 1). The present experiment was conducted online and was programmed using PsychoPy v. 2020.1.3 and uploaded to the Pavlovia server (Pavlovia, 2021; Peirce et al., 2019). Each participant received a link to the experiment allowing them to open it in the browser of their choice. Participants were required to use a physical keyboard.

Learning phase Participants were presented with an action–effect sentence: e.g., "*To make the screen blue, I will press the left key*" (in Norwegian: "Å gjøre skjermfargen blå, skal jeg trykke på venstretasten"). We presented an example of the critical stimulus (the color blue to be used in the study) prior to the action–effect instructions and told the participants that this would be the color that is referred to later in the instructions. To consolidate the instruction in memory, the participants were told to repeat the action–effect sentence silently to themselves a few times. We informed participants that this instruction would become relevant in a later task. Participants then received instructions for the test phase.

Test phase The presented stimulus was either a vowel (A, Ø, or E) or a consonant (K, M, or T), and each appeared an equal number of times in random order. During this part, the participants judged whether a presented stimulus was a vowel by pressing the left key (A) or a consonant by pressing the right key (L). Along with each presented letter, the background color was either blue (effect prime present; 25% of the trials) or gray (effect prime absent; 75% of the trials). All stimulus and response combinations were equally distributed between the effect-present and effect-absent trials. We implemented a short response deadline. If a response was incorrect or longer than 1500 ms, an error feedback message was displayed for 1500 ms. Participants performed eight practice trials and 96 testing trials. The practice trials did not include any critical trials (i.e., the background was always gray).

Data analysis and data preparation approach

We used the R software package to prepare and analyze the data (R core Team, 2021). Response errors and reaction times were analyzed with a mixed ANOVA (*stats* package). Confidence intervals adjusted for the within-participant design were calculated by using *Rmisc* package (Hope, 2013). In addition, the reaction time variable was log-transformed (Judd et al., 1995). Responses other than A and L were removed prior to analyses (5.01% responses). No participant made "other" responses more than 50% of the time. Visual inspection of the data indicated one participant made an excessive number of fast responses. Therefore, we applied a criterion used in other online response-time studies (Greenwald et al., 2003; remove participant data with more than 10% responses faster than 300 ms). This resulted in the removal of the data from one participant. The boxplot method (Tukey, 1977) applied to mean error responses identified one participant as an extreme outlier (± 3 times the interquartile range) with a mean error rate of 20% (compared to the full sample's mean error rate of 5.3%), so the data of this participant was also removed resulting in an analyzed sample size of 41 participants.

Individual trials were removed when the response deadline of 1500 ms was missed (0.51%). Prior to the response time analysis, we removed all error responses (5.3%). No responses were faster than 150 ms. We further removed trials with response times beyond the mean ± 3 times the standard deviation calculated by participant and within-participant conditions (1.07%).

Results and discussion

Response errors

All results of the ANOVA analysis with response errors as the dependent variable are presented in Table 1. In the following, we focus only on the hypothesis-relevant effects. The expected three-way interaction effect between required response, effect prime, and instructed response was marginally significant $F(1, 39) = 3.50$, $p = 0.069$, $\eta_p^2 = 0.08$. To explore this interaction effect, we analyzed response errors for prime present (critical) and prime absent (control) trials separately. For trials with the prime present, we found a significant two-way interaction effect between required response and instructed response, $F(1, 39) = 6.61$,

$p = 0.014$, $\eta_p^2 = 0.15$. In contrast, for the control trials with the prime absent, the interaction effect was not significant, $F(1, 39) = 0.62$, $p = 0.434$, $\eta_p^2 = 0.02$.

Despite the marginally significant result, the response error analysis showed that the pattern of results is in line with our predictions (see Fig. 1a). Presenting the action effect in a trial that required an incompatible response to the action–effect instructions (i.e., the action–effect instructions involved the right key and the required response was left or the action–effect instructions involved the left key and the required response was right) resulted in more errors than when the required response was compatible with the action–effect instructions (Fig. 1a, left pane). These differences were not observed in the control trials with the effect prime absent (Fig. 1a, right pane).

Reaction time

All results of the ANOVA analysis with reaction time as the dependent variable are presented in Table 2. The analysis of reaction times revealed a main effect of prime $F(1, 39) = 22.07$ $p = < 0.001$, $\eta_p^2 = 0.36$, indicating that participants responded more slowly on critical trials than neutral trials. The three-way interaction effect between required response, effect prime and instructed response was marginally significant $F(1, 39) = 3.17$, $p = 0.083$, $\eta_p^2 = 0.08$ (see Fig. 1b). We evaluated the descriptive pattern separately for the prime present and prime absent trials, to test whether the response-error pattern described in the previous section is further substantiated by a similar pattern in response times or whether it can instead be explained by a speed–accuracy

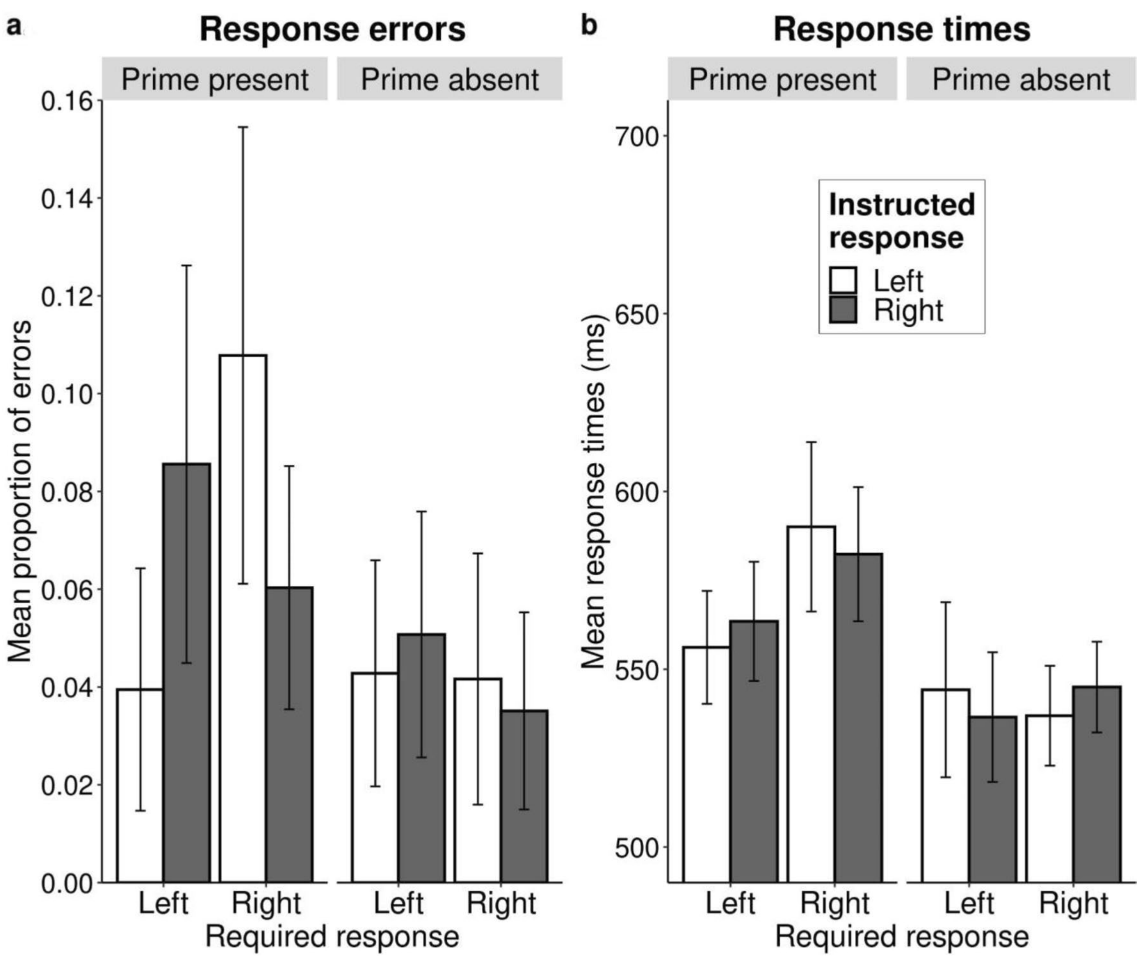

Fig. 1 Mean response errors (**a**) and reaction time (**b**) as a function of required response, effect prime and instructed response. Bars represent descriptive means with the confidence intervals adjusted for the within-participant design according to the method of Morey-Cousineau (2008). The left pane **a** represents mean proportion of errors and the right pane **b** mean reaction times. *Required response* specifies what response was required from participants in a given trial according to the categorization task instructions. *Effect prime* specifies whether the blue screen was present (critical) or absent (neutral) in a given trial. *Instructed response* indicates the instructed action in the action–effect sentence ("To make the screen blue, I will press the left/right key")

trade-off (i.e., an effect in the opposite direction of the response errors). For trials with the effect prime present, the two-way interaction effect between required response and instructed response was not significant $F(1, 39) = 0.71$, $p = 0.406$, $\eta_p^2 = 0.02$. Similarly there was no significant two-way interaction effect for trials with the effect prime absent $F(1, 39) = 1.01$, $p = 0.321$, $\eta_p^2 = 0.03$. In sum, the response-time pattern (see Fig. 1b) indicates that the response-error pattern is not compromised by a speed–accuracy trade-off.

Experiment 2

The first experiment provides initial evidence that the action–effect sentence influenced subsequent performance in response to the priming of the effect. However, we found this evidence only in response errors. Furthermore, although the separate analyses of the critical and control trials provided a clear picture, the overall three-way interaction effect was only marginally significant. A sensitivity analysis of the Experiment 1 data suggests that a mixed-design ANOVA with 41 participants across four within-conditions within two groups would be sensitive to an effect of $\eta_p^2 = 0.21$ with 80% power ($\alpha = 0.05$). Given that the observed effect size was $\eta_p^2 = 0.08$, we conclude that the first experiment was underpowered. Therefore, we conducted a second experiment with the central focus of providing a higher powered exact replication of Experiment 1. If the result pattern found in Experiment 1 was due to chance, it is unlikely that a second, higher powered, independent replication would produce such a specific pattern again.

In addition to the central aim of replicating Experiment 1, we added an exploratory examination of an action–effect sentence formulated in a more typical *action–effect order* ("*I will press the left key to make the screen blue*"). This formulation of the instruction reflects the theoretical assumption of the action–effect principle that the associations resulting from action–effect learning are bidirectional; even if learning occurs in an action–then–effect order, encountering the effect first should trigger the response (e.g., Elsner & Hommel, 2001). Thus, Experiment 2 includes one part that is an exact replication of Experiment 1 with the effect (stimulus)–action (response) order format. Our hypotheses for this replication were the same as in Experiment 1: required responses that are incompatible (compatible) with the verbally linked, primed effect should be impaired (facilitated). The exploratory second part differed only in the order of the components (i.e., action [response]–effect [stimulus]). We had no specific hypotheses for this exploratory analysis. Whereas verbal if (stimulus)–then (response) planning research represents the order of presenting the verbal information as relevant, prior verbal action–effect studies have also found significant effects with an action (response)–effect (stimulus) order (Theeuwes et al., 2015). Whereas Experiment 1 participants consisted mainly of students from Norway (mean age 24.1), Experiment 2 participants were recruited from the general population of the United Kingdom (mean age 41.4).

Method

Participants

A total of 400 English speaking participants participated in the second experiment. Following data cleaning described in "Data analysis and data preparation approach" section below, the analyzed sample included the data of 173 participants in the replication study and 176 participants in the exploratory addition (199 females, 148 males, 2 missing responses). Their age ranged from 18 to 60 years ($M = 41.9$, SD = 12.1). Each participant was recruited by the recruiting agency Toluna (2021) and received a small monetary payment for taking part in the study. The study was approved by the local ethics committee, and all participants provided informed consent. A power analysis using the effect size from the first experiment showed that with $N = 170$ and $\alpha = 0.05$, our mixed-design ANOVA had a power of $\beta = 90\%$ to detect the effect size reported in Exp. 1 ($\eta_p^2 = 0.08$).

Design

The design was identical to the first experiment with two within-participant factors: *required response* (left vs. right), *effect prime* (present vs. absent) and one between-participant factor *instructed response* (press left key vs. press right key). In addition, we introduced a separated condition: action–effect order. The additional condition allowed us to test both whether the effect–action order findings from Experiment 1 would replicate and whether we find an effect for the exploratory reversal of the component order (action–effect).

Procedure

All materials were identical to the first experiment. In addition to the effect-order format ("*To make the screen blue, I will press the left key*") presented in the first experiment and Part 1 of this second experiment, the additional instruction sentence was formulated in an action–effect format (e.g., "*I will press the left key to make the screen blue*"). As in the previous experiment, key assignment to vowel/consonant was counterbalanced between participants.

Data analysis and data preparation approach

The data preparation procedure and outlier detection were identical to the first experiment. Prior to analysis, we removed the data of 3 participants who used different response keys than instructed more than 50% of the time (neither left “A” nor right “L”). Then we removed all individual responses that were neither ‘A’ or ‘L’ (1.5% of the total sample; accounted for by the same programming error as in Exp. 1). As in Experiment 1, we removed the data from 15 participants who made more than 10% of their responses below 300 ms (Greenwald et al., 2003). The response deadline of 1500 ms was missed in only 0.26% of trials. Using boxplot with interquartile range of ±3 (Tukey, 1977), we removed the data of 26 participants with more than 22% response error. The full analyzed sample size was 349 participants.

Prior to the reaction time analysis, we removed all error responses (3.63%). We also excluded responses below 150 ms (i.e., fast guesses; 0.03% of the data) and trials with response times beyond the mean ±3 times the standard deviation calculated by participant and within-participant conditions (1.15%).

Results and discussion

Effect–action order (replication)

Response error

All results of the ANOVA analysis with response errors as the dependent variable for the effect–action order are presented in Table 3. As in the first experiment, we focus only

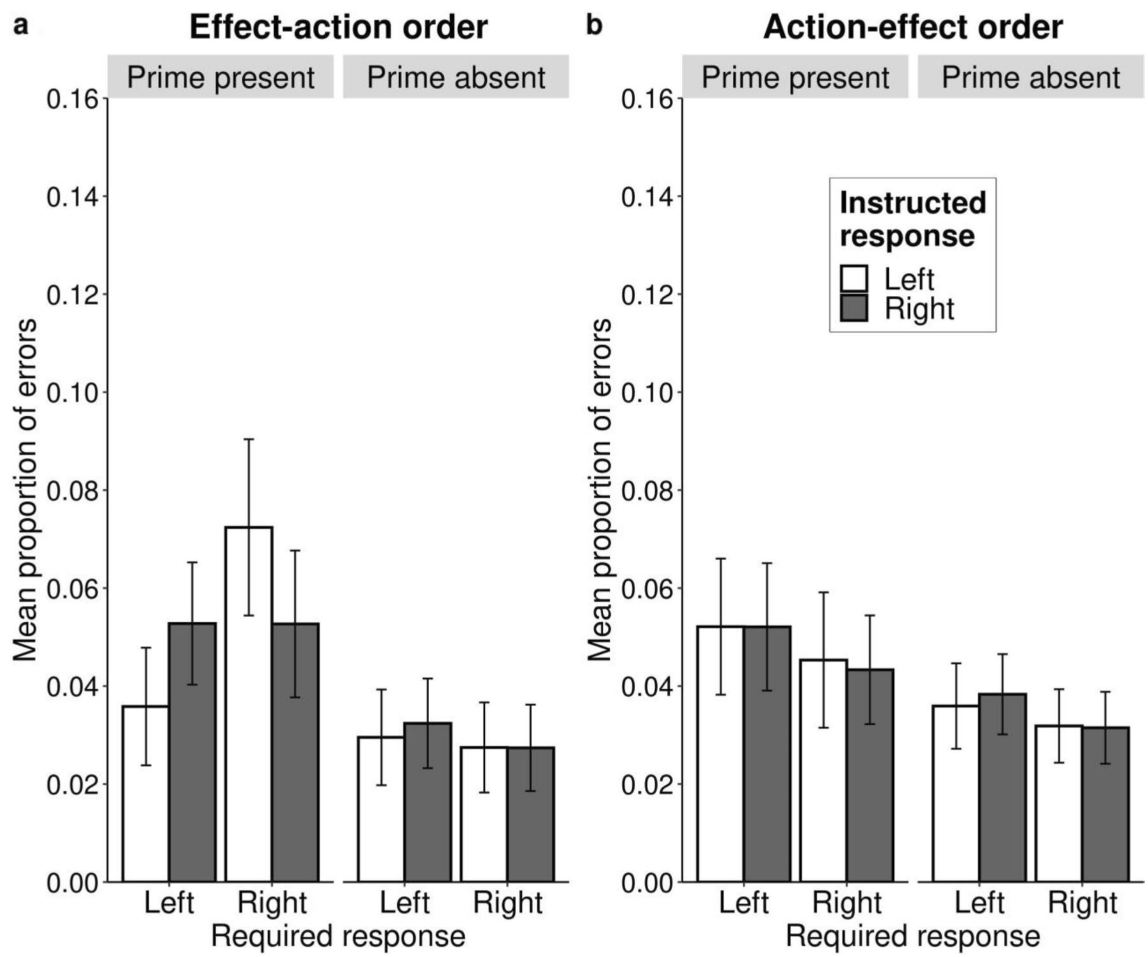

Fig. 2 Mean response errors for effect–action sentence condition (**a**) and action–effect condition (**b**) as a function of required response, effect prime and instructed response (Replication of Experiment 1). Bars represent descriptive means with the confidence intervals adjusted for the within-participant design according to the method of Morey-Cousineau (2008). *Required response* specifies what response was required from participants in a given trial according to the categorization task instructions. *Effect prime* specifies whether the blue screen was present (critical) or absent (neutral) in a given trial. *Instructed response* indicates the instructed action in the action–effect sentence (“To make the screen blue, I will press the left/right key”)

on the hypothesis-relevant effects. The expected three-way interaction effect between required response, effect prime, and instructed response is marginally significant $F(1, 171) = 3.64$, $p = 0.058$, $\eta_p^2 = 0.02$. As in Experiment 1, we evaluated the experimental effect further within the effect prime present (critical) and effect prime absent (control) trials separately. We found a significant two-way interaction effect in the effect prime present condition between required response and instructed response $F(1, 171) = 5.41$, $p = 0.021$, $\eta_p^2 = 0.03$. Whereas the same interaction effect was not significant within the effect prime absent trials $F(1, 171) = 0.17$, $p = 0.680$, $\eta_p^2 < 0.01$. In sum, in line with Experiment 1, when the effect prime was present, trials that required a response that was incompatible with the action–effect instructions resulted in more errors than responses that were compatible with the action–effect instructions (Fig. 2a, left pane). There was no such effect in the control trials with the effect prime absent (Fig. 2a, right pane). Thus, the results replicated the response error findings from Experiment 1.

Reaction time

All results of the ANOVA analysis with reaction time as the dependent variable are presented in Table 4. The analysis revealed a significant main effect of prime $F(1, 171) = 116.53$ $p < 0.001$, $\eta_p^2 < 0.41$, indicating that participants responded slower on critical trials than on neutral trials. The three-way interaction effect between required response, effect prime, and instructed response was not significant $F(1, 171) = 1.24$, $p = 0.268$, $\eta_p^2 < 0.01$. As in the first experiment, we evaluated whether there was a

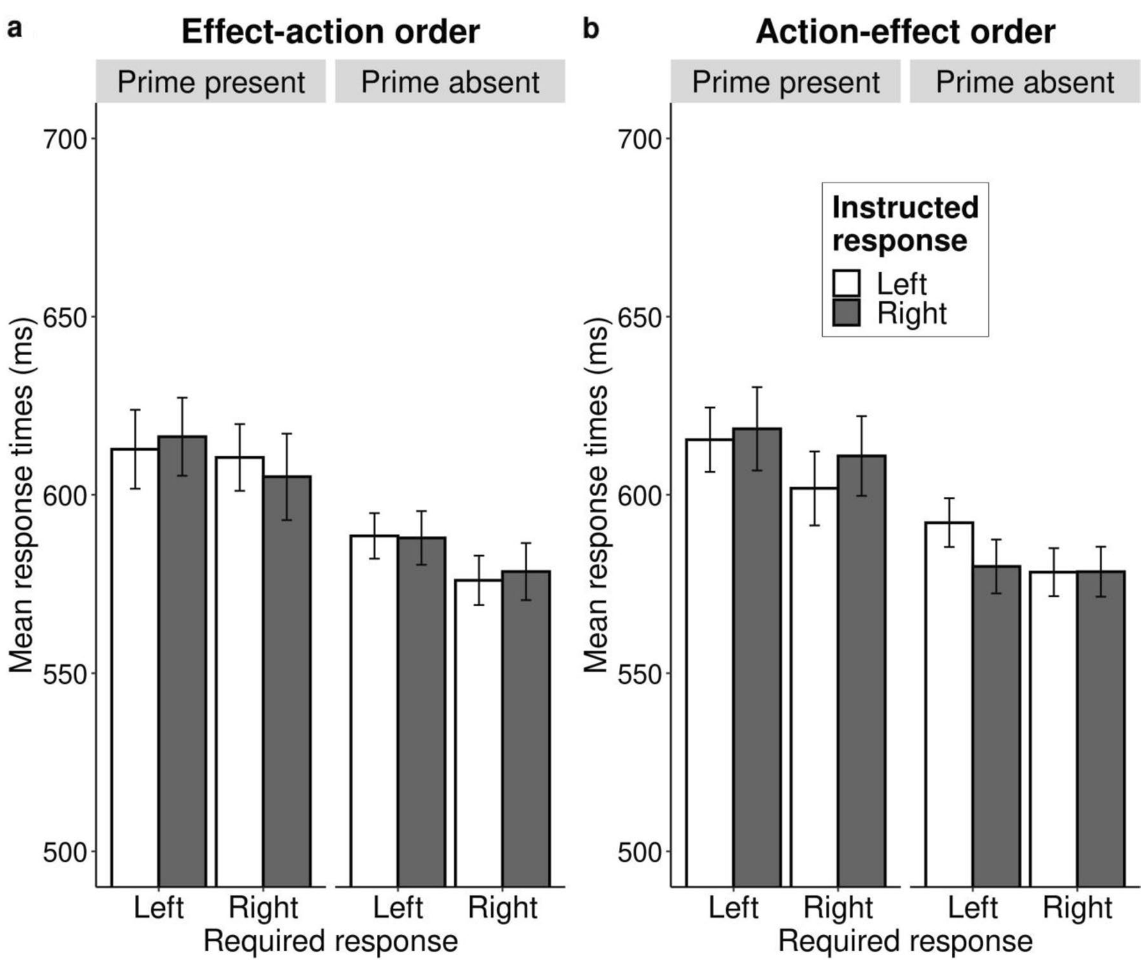

Fig. 3 Mean response times for effect-action sentence condition (**a**) and action–sentence condition (**b**) as a function of required response, effect prime and instructed response. Bars represent descriptive means with the confidence intervals adjusted for the within-participant design according to the method of Morey-Cousineau (2008). *Required response* specifies what response was required from participants in a given trial according to the categorization task instructions. *Effect prime* specifies whether the blue screen was present (critical) or absent (neutral) in a given trial. *Instructed response* indicates the instructed action in the action–effect sentence (“To make the screen blue, I will press the left/right key”)

speed–accuracy trade-off. The two-way interaction effect between required response and instructed response was not significant in trials with the effect prime present $F(1, 171) = 0.52$, $p = 0.470$, $\eta_p^2 \leq 0.01$. The same analysis also did not show an effect in the control trials with the effect prime absent $F(1, 171) = 0.18$, $p = 0.672$, $\eta_p^2 \leq 0.01$. As in the first experiment, these results indicate that the pattern of response errors were not affected by a speed–accuracy trade-off (Fig. 3a).

Action–effect order

Response errors

All results of the ANOVA analysis with response errors as the dependent variable for the action–effect order are presented in Table 5. The interaction effect between required response, effect prime, and instructed response was not significant $F(1, 174) < 0.01$, $p = 0.960$, $\eta_p^2 < 0.01$ (see Fig. 2b). Thus, we have no evidence that the instructions in the action–effect order influenced the responses.

Reaction time

Table 6 presents the results of the ANOVA analysis with reaction time as the dependent variable. Similar to the effect–action order, the analysis of the action–effect order showed a significant main effect of prime $F(1, 174) = 79.65$, $p < 0.001$, $\eta_p^2 = 0.31$, indicating that participants responded more slowly on critical trials than on neutral trials. We did not find a significant three-way interaction effect between required response, effect prime, and instructed response $F(1, 174) = 0.30$, $p = 0.582$, $\eta_p^2 < 0.01$, indicating that reaction times (Fig. 3b) were not influenced when the sentence was formulated in the action–effect order.

In sum, in Experiment 2 we replicated the effect observed in Experiment 1 by finding an effect of the action–effect instructions if the sentence was formulated in an effect (situation)–action (response) order. However, we found no effect of priming the effect when the instructions were formulated in an action (response)–effect (situation) order. It should be noted that the four-way interaction effect (required response × instructed response × effect prime × sentence-component order) did not reach significance, $F(1, 345) = 2.16$, $p = 0.142$, $\eta_p^2 < 0.01$. The decision to analyze the two parts of the experiment separately was guided by the aim to test whether the results of Experiment 1 were replicated. However, any conclusions based on the exploratory investigation of the order of the components can only be considered preliminary and should be interpreted with caution considering the non-significant four-way interaction effect.

General discussion

In the present experiments we examined whether verbal action–effect instructions led to associations between perception (effect) and action that are automatically (i.e., unintentionally) activated upon encountering the effect. We tested this activation in behavioral responses in a speeded categorization task, where the effect was included as a task-irrelevant prime. Although some of the main findings were only marginally significant, the two experiments in combination revealed consistent evidence that the action–effect instructions (in an effect–action order) in combination with the effect prime influenced the accuracy of participants' responses (with no evidence of a speed–accuracy trade-off). If the action effect prime was present, required responses that were incompatible with the instructed response showed more errors than when the responses were compatible with the previous action–effect instructions. Whereas Experiment 1 was underpowered, the replication in Experiment 2 (with four times the sample size) supported the results from Experiment 1. Why this increased sample size did not result in a clearer effect may be explained by the sample characteristics. There may have been increased random error variance from the significantly older, non-student sample in Experiment 2.

The result patterns could be interpreted as showing an interference effect in the effect-prime trials in which the previously verbally linked response was incompatible with the required response in the respective trial. However, facilitation from compatible response activation or interference from incompatible response activation can only be evaluated in comparison to an adequately similar control condition. The control condition in the present studies differed in terms of the critical priming factor (i.e., it did not include distracting sudden background-color changes). Assuming that the background color change negatively influenced responses in the prime/color-change trials, the absolute differences between critical and neutral trials are not comparable as we cannot estimate the size of that negative influence of the prime (i.e., prime main effect). Depending on the size of the prime/color-change induced interference, all combinations—only facilitation, facilitation and interference, or only interference—are possible. Investigating this would require a control condition that includes the same background-color change without including any (verbal) links of that color to a response. In such a condition, we could observe the consequences for responses induced merely by the sudden background-color change. Importantly, however, this limitation of not knowing whether facilitation, interference, or both caused the effect, does not reduce the informative value of the observed interaction effects, indicating that the verbal information systematically influenced the responses.

The absence of the hypothesized interaction effect in reaction times maybe explained by the response deadline. Response deadlines (i.e., forcing participants to emphasize speed over accuracy) typically leads to a reduced variability in response times and diminished power to detect reaction time effects (for a similar argument and findings in accuracy vs. reaction time measures, see Mekawi & Bresin, 2015). In sum, for the effect–action order formulation, we provide evidence that the verbally formulated perception–action relation—that was never directly experienced or executed—resulted in an association that was automatically reactivated upon perceiving the effect.

Our results align with previous research showing that imagining an effect while actually performing a response can lead to action–effect bindings (Cochrane & Milliken, 2019; Pfister et al., 2014). However, in the present research, participants did not previously experience the effect or response, but processed them merely as verbal action–effect instructions.

Eder and Dignath (2017) also showed action–effect learning from verbal instructions. However, in their test phase, participants experienced the previously instructed action–effect associations with each response. Therefore, it is not clear whether the observed effects were the direct effect of the instructions or some conflict between the instructions and the instruction-incompatible experiences. In our present experiments, participants never directly experienced the previously instructed action–effect contingency in the test phase. Thus, our study focused more narrowly on response priming from an instructed, verbal action–effect contingency. Finally, in contrast to the previously introduced research by Theeuwes et. al. (2015) in which instructions were likely to be kept in working memory (i.e., with responses given within a short interval after instructions were given), the present results indicate that the impact of instructions can have a longer lasting effect (beyond seconds and with processing other information in between), in line with the findings from implementation intention studies (Gollwitzer, 2014; Webb & Sheeran, 2008).

Martiny-Huenger et. al. () suggested a possible mechanism for this effect. According to their theoretical framework, verbal instructions that include a perceivable effect and executable action may work similarly to associative learning from direct processing and execution of the perception and action. This idea is based on theories of simulation and embodied cognition (Barsalou, 1999, 2010; Hesslow, 2012) and past findings that language comprehension of concrete concepts overlaps with sensorimotor areas activity of the brain (e.g., Arbib, 2008; Gallese & Lakoff, 2005; Pulvermüller & Fadiga, 2010). From this perspective, comprehension of verbal information activates the same sensorimotor brain areas that are involved during actual perception and behavior. Verbally processing a stimulus–response or action–effect contingency can thus result in the formation of specific associations between them—associations that are unintentionally activated upon encountering the perception (e.g., visual action effect) as suggested by our present experiments.

Studies on action–effect learning from direct experiences usually appear to form bi-directional links between action and effect, because the learning order (action, then effect) is reversed in the test phase (effect presentation, then action, e.g., Elsner & Hommel, 2001) However, the results from the second experiment indicated that the effect of the instruction sentence was only observed in the condition when the action–effect sentence was formulated in an effect–action direction (i.e., perception, then action: "*To make the screen blue, I will press the left key*"). In the action–effect order (action, then perception: "*I will press the left key to make the screen blue*"), the effect of the instructed sentence was not observed. These findings are not in line with the results of Theeuwes et. al. (2015), who only used the action–effect order and found effects of these instructions. If the present results prove to be robust in subsequent replications, a potential explanation could be in the differences of the procedure. Participants in the studies by Theeuwes et. al. (2015) were more likely to have kept the action–effect relation active in working memory. Thus, the order of the relation may be less important when the components are active in working memory. However, with the delay between processing the verbal instructions and executing the responses, whatever memory processes mediated the effects (e.g., associative learning), they may be sensitive to the order in which the components were processed before.

The statistically weak evidence for a difference between the two instruction component orders prohibits us from drawing strong conclusions about potential differences between the order of processing the action–effect components. However, our results are in line with a previous study by McCrea et. al. (2014), who investigated the consequences of differently formulated self-regulation instructions before doing a prospective memory task. Although the authors modeled instructions to fit different theoretical concepts, one of the instructions included a stimulus–response order that was similar to our effect–action order ("Whenever I see the red circle, then I will immediately press the spacebar"). The other two formulations included a response–stimulus order (e.g., "I will immediately press the spacebar when I see the red circle!") similar to our action–effect order. Like our findings, only the stimulus–response order (i.e., perception–action) was effective in their study (McCrea et al., 2014). More anecdotally, in the initial publications of if–then planning research, the strategy was sometimes presented in a response–stimulus format (e.g., "I intend to do y when situation z is encountered"; Gollwitzer, 1993; Gollwitzer & Brandstätter, 1997). However, at some point, this changed, and subsequent publications almost exclusively used the if (stimulus)–then (response) order (e.g., "When situation x arises, I will perform response y!"; Gollwitzer, 1999). This could have been the result of a mere refinement of the concept, or as a result of practical experience that the reversed order (response–situation) is less effective.

Why might the perception–then–action order be more effective than the action–then–perception order (at least in measures after a few seconds)? Disregarding the rich subjective experiences that we associate with language in general and discussing it from the perspective of simulation accounts of cognition alone might provide an interesting answer. As argued previously, repeating the presented instructions in the presented form may act as a placeholder for the actual experiences. From this actual-experience perspective and the fact that reading is sequential—the order of the components in the instructions results in differences in whether the perception (e.g., effect) is predictive of a response or not. In our effect–action order and McCrea et. al.'s (2014) stimulus–response order, the perception (effect/stimulus) is followed by the response; the perception part is thus predictive of the action part. During the test phase, the perception is there first (effect prime, blue screen) and the perception, therefore, biased actual responses in line with the prior learning. In contrast, in the action–effect order and McCrea et. al.'s (2014) response–stimulus order, the perception of the effect/stimulus was not predictive for the action, because in this case, the action preceded the perception. Thus, when the perception occurred in the test phase, it did not have any systematic predictive value and thus did not bias the subsequent responses.

Whereas the evidence we present for such an order effect in the present research is weak, it lines up with other prior evidence (e.g., McCrea et al., 2014; if–then planning research in general). Furthermore, where it conflicts with prior evidence (e.g., Theeuwes et al., 2015), it can easily be reconciled with differences in the procedures (i.e., instructions kept in working memory for a few seconds vs. effects that could not have been kept in working memory). More research is needed to support the reliability of a systematic difference between the component order. In addition to the new theoretical questions about action–perception learning raised by these findings, the present study contributes to the idea that language is intertwined with action control (Perlovsky & Sakai, 2014) and can be strategically used to control our behavior (Gollwitzer, 1999; Martiny-Huenger et al., 2017).

Conclusions

In the present work, our findings showed that action–effect associations can be formed through verbal instructions. Although the perception–action relation presented as action–effect instructions was never executed by the participants before, it still had unintentional consequences when the perception component (effect) was encountered in the instruction-irrelevant classification task. We interpret these findings as evidence that verbal instruction can serve as a learning process in addition to learning from actual behavior. The complexity of human behavior would be hard to imagine if learning was limited to learning from actual behavior. The unrestricted combinatory potential of language allows us to learn relations that we have never actually experienced before in such a combination. Importantly, our present research suggests that such learning from language does not necessarily happen only at the declarative knowledge level (Anderson, 1982), but that encountering verbal perception–action contingencies might directly influence procedural knowledge.

Appendix 1

For transparency reasons, we want to disclose that prior to conducting the two online experiments presented in this manuscript, we conducted one more experiment in a laboratory setting ($N = 50$) aimed to test the same hypothesis (this first experiment included a pre-registration on Open Science Framework (https://osf.io/w2j53) of the hypothesis tested in the present manuscript. The results did not reveal any compatibility effects. We decided not to include this study in the main manuscript because of a technical issue that resulted in half of the Norwegian participants receiving instructions in English. There was no way to know which participants received the incorrect instructions. Thus, considering the importance of language in our design (the experimental manipulation is contained in a single sentence), it is hard to evaluate the meaningfulness of the experiment. In addition, there were significant procedural differences as we improved the procedure and simplified our approach: originally, responses were done with two joysticks by making left or right push movements (as compared to pressing a left or right key in the present experiments). In addition, we used several colors (red, green, yellow, blue) as the background colors. Practically, this resulted in a rather distracting background color change on each trial (as compared to the more stable grey background color with occasional switches to the critical blue color in the present experiments). Furthermore, participants performed 194 trials in the testing phase, which could have potentially diminished an experimental effect due to learning during the test-task execution (see Schmidt et al., 2016); the present studies contained only 96 trials. Finally, we realized that participants may imagine different variations of the color "blue" when reading the critical action–effect sentence. To establish a single color that would be more consistently imagined between participants and more consistent with what they would see in the test phase, we showed the critical color to participants in the present study (before they learned the action–effect instructions) and told them that this would be the color that later instructions would refer to.

Appendix 2

See Tables 1, 2, 3, 4, 5 and 6.

Table 1 Anova results for experiment 1 (response errors)

Predictors	df	Sum of squares	F	p	η_p^2
Instructed response	1, 39	0.01	0.87	0.352	0.02
Required response	1, 39	< 0.01	0.45	0.506	0.01
Required response × instructed response	1, 39	0.03	7.63	0.009	0.16
Effect prime	1, 39	0.04	8.17	0.007	0.17
Effect prime × instructed response	1, 39	< 0.01	< 0.01	0.948	< 0.01
Required response × effect prime	1, 39	< 0.01	2.00	0.165	0.05
Required response × effect prime × instructed response	1, 39	0.02	3.50	0.069	0.08

Table 2 Anova results for experiment 1 (reaction times)

Predictors	df	Sum of squares	F	p	η_p^2
Instructed response	1	11,438	0.33	0.570	< 0.01
Required response	1	7236	3.50	0.069	0.08
Required response × instructed response	1	1	< 0.01	0.980	< 0.01
Effect prime	1	41,628	22.07	< 0.001	0.36
Effect prime × instructed response	1	1	< 0.01	0.979	< 0.01
Required response × effect prime	1	6634	8.93	0.005	0.19
Required response × effect prime × instructed response	1	2357	3.17	0.082	0.08

Table 3 Anova results for experiment 2 (effect–action sentence; response errors)

Predictors	df	Sum of squares	F	p	η_p^2
Instructed response	1	0.007	0.88	0.350	< 0.01
Required response	1	0.009	3.03	0.084	0.02
Required response × instructed response	1	0.017	5.48	0.020	0.03
Effect prime	1	0.101	29.09	< 0.001	0.15
Effect prime × instructed response	1	< 0.001	0.10	0.758	< 0.01
Required response × effect prime	1	0.020	6.07	0.015	0.03
Required response × effect prime × instructed response	1	0.012	3.64	0.058	0.02

Table 4 Anova results for experiment 2 (effect–action sentence; reaction time)

Predictors	df	Sum of squares	F	p	η_p^2
Instructed response	1	35,739	1.14	0.286	< 0.01
Required response	1	13,527	4.33	0.039	0.02
Required response × instructed response	1	375	0.12	0.729	< 0.01
Effect prime	1	138,723	116.53	< 0.001	0.41
Effect prime × instructed response	1	153	0.13	0.721	< 0.01
Required response × effect prime	1	757	0.61	0.437	< 0.01
Required response × effect prime × instructed response	1	1540	1.24	0.268	< 0.01

Table 5 Anova results for experiment 2 (action–effect sentence; response errors)

Predictors	df	Sum of squares	F	p	η_p^2
Instructed response	1	0.004	0.67	0.410	< 0.01
Required response	1	0.008	2.59	0.110	0.01
Required response × instructed response	1	< 0.001	0.08	0.780	< 0.01
Effect prime	1	0.033	15.90	< 0.001	0.08
Effect prime × instructed response	1	< 0.001	0.09	0.770	< 0.01
Required response × effect prime	1	< 0.001	0.09	0.760	< 0.01
Required response × effect prime × instructed response	1	< 0.001	< 0.01	0.960	< 0.01

Table 6 Anova results for experiment 2 (action–effect sentence; reaction time)

Predictors	df	Sum of squares	F	p	η_p^2
Instructed response	1	17,928	0.43	0.511	< 0.01
Required response	1	14,603	6.49	0.012	0.04
Required response × instructed response	1	3697	1.64	0.202	< 0.01
Effect prime	1	150,868	79.65	< 0.001	0.31
Effect prime × instructed response	1	6425	3.39	0.067	0.02
Required response × effect prime	1	381	0.26	0.609	< 0.01
Required response × effect prime × instructed response	1	443	0.30	0.582	< 0.01

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Code availability The codes used to analyze the data are available via the Open Science Framework (OSF): https://osf.io/w72mx/?view_only=85d511d72bd646138a00ef71f107abd7.

Declarations

Conflict of interest The authors have no conflicts of interest to declare that are relevant to the content of this article.

Ethics approval This study was performed in line with the principles of the Declaration of Helsinki. Approval was granted by the Ethics Committee of the Arctic University of Norway (Date: October 2019/No: 2017/1912).

Consent to participate Informed consent was obtained from all individual participants included in the study.

Consent to publish The participants have consented to the submission of the case report to the journal.

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Paper 2

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Associative learning from verbal action–effect instructions: A replication and investigation of underlying mechanisms

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Associative Learning from Verbal Action-Effect Instructions: A Replication and Investigation of Underlying Mechanisms

RESEARCH ARTICLE

ubiquity press

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ABSTRACT

According to the ideomotor principle, repeated experience with an action and its perceivable consequences (effects) establish action-effect associations. Research on verbal instructions indicates that such associations are also acquired from verbal information. In the present experiment (N = 651), first, we aimed to replicate unintentional response-priming effects from verbal action-effect instructions (direct replication; Condition 1). Second, we investigated the involvement of perceptual processes in the verbally induced response-priming effect by perceptually presenting (Condition 1) versus not presenting (Condition 2) the color that was subsequently named as an effect in the instructions. Third, we tested a saliency-based explanation of the verbally induced response-priming effect by highlighting all components (action and effect) without an association between them (Condition 3). Overall, we found the predicted response-priming effect following verbal action-effect instructions (overall conditions and in the replication Condition 1). Condition 2, which did not include perceptual information in the instructions, still showed a significant response-priming effect but was descriptively weaker compared to the effect of the replication Condition 1. Condition 3, which merely highlighted the action and effect component without endorsing an association, did not show a significant effect. In sum, our study provides further solid evidence that verbal instructions lead to unintentional response-priming effects. Other conclusions must be considered preliminary: The between-condition comparisons were descriptively in the predicted direction—perceptual aspects are relevant, and a saliency-based account can be excluded—but the differences in accuracy between conditions were not statistically significant.

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According to the ideomotor principle, goal-directed actions are driven by anticipatory representations of their effects (i.e., action-effect; Elsner & Hommel, 2001; James & Hutchins, 1952). Our actions produce perceivable changes in the surroundings. The temporal overlap between actions and their perceptual effects results in the formation of associations between them. When a person mentally activates a particular perceptual effect (e.g., when forming an intention to achieve a certain outcome), it also activates the associated action that has led to the effect previously. This mechanism is postulated to enable goal-directed behavior.

Experimental procedures to test action-effect learning typically include two phases: learning and testing. In the learning phase, participants experience the co-occurrence of specific responses and their perceptual effects. The test phase is designed to evaluate the relations between those actions and their effects in choice-reaction tasks in which the previously-learned effect is encountered (Elsner & Hommel, 2001). The idea is that exposure to a learned effect should automatically activate the corresponding action. From a measurement perspective, this activation is inferred from an observed response bias in the choice-reaction task (compatibility effect).

We use the term "compatibility effects" (Kornblum et al., 1990) when responses in the test phase are facilitated or impeded by the presence of an effect stimulus from the learning phase. Perceiving the effect from the learning phase (as a target or a task-irrelevant stimulus) leads to a retrieval of a response that has become associated with that effect stimulus in the learning phase. When an associated response *matches* the required response in the test phase (compatible trials), responses are facilitated (i.e., shorter response times and/or fewer response errors). When the associated response is *different* from the required response (incompatible trials), responses are impeded (i.e., longer response times and more response errors).

There is ample evidence for compatibility effects resulting from action-effect learning based on direct experiences (Elsner & Hommel, 2001; Hommel, 2004; for a review see Shin et al., 2010; Waszak et al., 2012;). However, prior research also indicates that action-effect learning is not limited to learning from actual experience with an action-effect contingency. For example, Pfister et al. (2014) demonstrated that action-effect learning occurred in the absence of direct experiences with the action-effect pairing. While participants executed a response, they only imagined the anticipated outcome of their action. This was sufficient to produce response-compatibility effects that are indicative of action-effect learning.

Another study (Eder & Dignath, 2017) showed action-effect learning when both components were instructed before the test phase. However, participants directly experienced the action-effect contingencies in the test phase. While this may be interpreted as evidence for readily observable action-effect learning (i.e., early in the test phase) following instructed action-effect contingencies, the contribution of the instructions in Eder and Dignath (2017) are not clearly separable from the effect of learning from the first direct experiences in the test phase or from instruction/direct-experience interactions. Most relevant to our present focus are two recent publications that report studies with a clearer separation of instructions and direct experiences (Damanskyy et al., 2022; Theeuwes et al., 2015).

The experimental procedure in these two recent publications on verbal action-effect instructions (Damanskyy et al., 2022; Theeuwes et al., 2015) is similar to those that induce learning based on direct experiences. However, in the learning phase, instead of performing an action and perceiving the effect, participants see verbal instructions for specific action-effect relationships. The test phase is the same as in research from direct experiences. The influence of the verbal instructions on participants' responses is tested in a categorization task where instruction-relevant features are visually presented to create response-instruction compatible and incompatible trials.

For example, Theeuwes et al. (2015) provided evidence that verbal action-effect instructions produce a compatibility effect that would be expected from learning based on direct experience. In three experiments, participants were provided with action-effect instructions (e.g., pressing the left key will remove the letter A from the grid filled with letters; "learning phase"). Before starting the task where these instructions should be applied (inducer task), participants completed a separate task that was unrelated to the instructions but contained features from them (diagnostic task/test phase). Participants were asked to judge whether the previously-encountered letters – including the letter from the action-effect instructions – were presented *upright* or *italic* by pressing the left or right key. Thus, instruction combinations for the diagnostic task were either compatible or incompatible with the action-effect instructions presented for the inducer task. The results showed a compatibility effect in the diagnostic task, pointing to an effect of the instructions on the subsequent performance.

Damanskyy et al. (2022) also provided evidence that action-effect instructions produce a compatibility effect, but with some conceptual changes. While the results of Theeuwes et al. (2015) can be explained by participants holding the action-effect instructions in working memory, the procedure of Damanskyy et al. made it less likely that the verbal instructions were held in working memory by creating a stronger separation between the action-effect instructions (learning phase) and the test phase. This was mainly achieved by testing the effects a few minutes after the instructions were presented (Damanskyy et al., 2022) rather than after a few seconds (Theeuwes et al., 2015). In the learning phase, participants memorized action-effect instructions (e.g., "To make the screen blue, I have to press the left key"). In the test phase, participants performed an ostensibly unrelated vowel-consonant categorization task. However, in some trials the background of the screen turned blue, creating response-instruction compatible and incompatible trials. The results showed a compatibility effect for the action-effect instructions on the participants' response accuracy. This effect was observed despite the delay between the learning and test phase and the action-effect instructions never becoming relevant in the test phase. Thus, the authors concluded that in comparison to the study procedure by Theeuwes et al., it was less likely that participants held the action-effect instructions actively in working memory. This provides support for the idea that associations were formed while memorizing the action-effect instructions.

Two questions arise from this previous research. First, Damanskyy et al. (2022) involved not only verbal information in the instructions, but also a visual sample of the color blue that was referenced in the verbal instructions. This leads to questions about the contribution of visual perception in verbal action-effect learning. Does the mechanism underlying this verbally induced learning involve perceptual aspects? Second, an alternative explanation of the findings could be that the mere familiarity with the stimuli (effect and response), and not associative learning between them, could account for the findings (i.e., a saliency-based explanation). The present study was designed to address these two questions.

PRESENT RESEARCH

In the present research, we investigated three central aspects related to the effect of verbally induced action-effect instructions. First, we sought to provide a high-powered replication of an unintentional response-priming effect from a verbally processed – but never directly executed – action-effect contingency. Second, we investigated the relevance of perceptual processes for verbal action-effect learning. Third, we tested the idea that verbal action-effect instructions establish associative links against a saliency-based alternative explanation. The experimental procedure of this study was similar to Damanskyy et al. (2022). In the learning phase, participants read action-effect instructions formulated in an effect-action order ("To make the screen blue, I will press the [left/right] key"). In the test phase, participants categorized letters as a vowel or consonant by pressing the left or right key. On 1/4 of the trials, the screen background turned blue (i.e., action-effect; critical trials). Thus, the participants encountered the effect that was previously verbally linked to either a left or right response. Thus, the required categorization response (left or right) was either compatible or incompatible with the response specified in action-effect instructions.

Our study consisted of three between-participant conditions. The first condition (visual-verbal link) served as a standard for comparing the remaining two conditions and is an exact replication of Damanskyy et al. (2022; effect-action order). In this condition the presented verbal information included perception and action components that were combined to form an action-effect contingency ('To make the screen blue, I will press the [left/right] key'). Before processing the verbal action-effect instruction (on a separate instruction page), participants were presented with the perceptual component (blue color) and told that this was the color referred to in later instructions.

The second condition (verbal link only) was identical to the first, except the blue color was not presented before the verbal instructions. This was designed to address the role of perceptual aspects in the link-formation process. If the presentation of the actual color before the verbal instructions influences the compatibility effect, this suggests that the learning mechanism that mediates the effect between reading the verbal instruction and observing a response-compatibility effect does not rely solely on language-like symbolic processes. Instead, perceptual processes would contribute to that mechanism.

The third condition (no verbal link) was designed to test the associative-learning account against a saliency-based alternative explanation. To that aim, the instruction presentation did not include the verbal action-effect contingency, but instead presented the effect and the action components independently from each other. On one page of the task instructions participants were informed that pressing a specific key (either right or left, counterbalanced by participant) was important and they should thus memorize the statement 'I will press the [right/left] key.' Later, on a different instruction page, participants were presented with the blue color sample and were informed that this color was relevant and will appear during the categorization task. In other words, we highlighted both components (perceptual and action aspects) but did not facilitate an association between them. If previous findings (i.e., Damanskyy et al., 2022) and those in the first two conditions are a result of associative learning and not merely a result of increased salience of the perceptual and action component, then we should observe a weaker effect in this third no verbal link (saliency-only) condition.

In sum, to provide further information about the mechanism involved in verbal action-effect learning, we compared three conditions. The "visual-verbal link" condition facilitated associative learning and provided exact information about the perceptual properties of the perceptual component. The "verbal link only" condition also facilitated associative learning between the instructed perceptual and action component but did not include the exact perceptual properties. The "no verbal link" condition highlighted the perceptual and action components but did not facilitate associative learning between them. We expected the strongest response-compatibility effect in the visual-verbal link condition, replicating Damanskyy et al. (2022). We expected a comparatively weaker response-compatibility effect in the verbal link only condition, and no effect in the no verbal link condition.

METHOD

PARTICIPANTS

A total of 655 English-speaking adults participated in the study (228 males, 417 females, and 10 missing responses). Participants' age ranged from 18 to 50 (M = 32.1, SD = 8.8). The participants were recruited by the online participant recruitment platform Prolific and received monetary compensation. We removed four participants who participated in the study twice due to technical errors. The study was approved by the local ethics committee, and all participants provided informed consent. The required sample size to find the central response-compatibility effect was not calculated prior to the analysis but was instead based on prior experiences with a similar design (~200 participants; effect-action condition in Damanskyy et al., 2022, Exp. 2).

DESIGN

The study included three main conditions: *visual-verbal link, verbal link only, no verbal link.* In the instructions for the visual-verbal link condition participants saw an example of the critical stimulus (color blue) followed by the action-effect instructions. In the verbal link only condition, participants saw only the action-effect instructions. In the no verbal link condition, participants were presented separately with instructions for a specific response and an example of the to-be-presented color. The data collection of the no verbal link condition reported in this manuscript was done after the data collection of the other two conditions, once we realized that a design error (missing verbal-response factor) in the originally-collected third condition made it impossible to calculate a comparable response-compatibility effect. Because of this error and the subsequent changes, the relationship between the present research and the initial pre-registration (https://osf.io/qfmc6) is complicated. However, the overall hypotheses and technical details of the analyses (e.g., outlier exclusion) remain the same. Analyses were conducted only after data collection was completed for all conditions.

All three conditions included two within-subject factors (*required response* and *effect prime*) and one between-subject factor (*instructed response*). Required response represented a factor that specified what response was required from participants according to the categorization task instructions (i.e., left vs. right key). Effect prime specified whether the blue screen was present (critical) or absent (control) in a given trial. Instructed response was a between-participant factor indicating the instructed response in the action-effect instructions (i.e., "To make the screen blue, I will press the [left vs. right] key") or response instructions (i.e., "I will press [left vs. right] key"). Key assignment to vowels/consonants was counterbalanced between participants.

PROCEDURE

The experiment was programmed using PsychoPy v. 2020.1.3 and uploaded to the Pavlovia server (Peirce et al., 2019; Pavlovia, 2021). Each participant received a link to the experiment allowing them to open it in the browser of their choice. Participants could not participate using devices other than a PC with a physical keyboard.

Learning phase

In the visual-verbal link condition, we presented an example of the critical stimulus (the color blue) and told them that this would be the color referred to later in the instructions. Afterwards, participants were presented with the action-effect instructions: e.g., "*To make the screen blue, I will press the right key.*" To consolidate the instruction in memory, the participants were told to repeat the action-effect sentence silently to themselves a few times. We informed participants that this instruction would become relevant in a later task. In the verbal link only condition participants saw only action-effect instructions; the critical color was not presented to them. They were also asked to repeat these instructions silently to themselves a few times. In the no verbal link condition participants saw an instruction that was not formulated in an action-effect manner and was not associated with the color blue (i.e., 'I will press the right key'). They were also informed that this instruction was important and they were instructed to memorize the sentence. After some intermediate instructions, the participants were presented with the color blue and told that this color will appear in the subsequent categorization task. A few minutes passed between memorizing the critical action-effect instructions and starting the test-phase task. These minutes where filled with action-effect unrelated instructions (e.g., instructions how to perform the categorization task).

Test phase

The categorization task was identical for all conditions. The presented stimulus was either a vowel (A, O, or E) or a consonant (K, M, or T), and each appeared an equal number of times in random order. During this part, the participants judged whether a presented stimulus was a vowel by pressing the left key (A) or a consonant by pressing the right key (L). Along with each presented letter, the background color was either blue (effect prime present; 25% of the trials) or gray (effect prime absent; 75% of the trials). All stimulus-response combinations were equally distributed between the effect-prime present and effect-prime absent trials. We implemented a response deadline of 1500 ms. If a response was incorrect or longer than 1500 ms, an error feedback message was displayed for 1500 ms. Participants performed eight practice trials and 96 test trials. The practice trials did not include any critical trials (i.e., the background was always gray). Instructions for the test phase included information that the background color may change during the task and they were explicitly instructed to ignore these color changes and focus on the vowel-consonant categorization task.

DATA PREPARATION AND DATA ANALYSIS

We used the R software package to prepare and analyze the data (R core Team, 2021). Response errors and reaction times were analyzed with a mixed ANOVA (*ez* package; Lawrence, 2016). In addition, the reaction time variable was log-transformed (Judd et al., 1995). No participant made excessively fast responses (i.e., more than 10% responses faster than 300 ms). Based on a boxplot outlier analysis (+/–3× interquartile range; Tukey 1977), we removed the data of 13 participants (>17% response errors). The final analyzed sample included 638 participants.

Responses with missed deadline were omitted (0.83%). Prior to the response time analysis, we removed all error responses (3.43%). No response was faster than 150 ms (i.e., fast guesses). In addition, we removed trials with response times beyond the mean +/− 3 times the standard deviation calculated by participant and within-participant conditions (1.11%).

We applied ANOVA with Type II Sums of Squares for statistical analysis as recommended for unbalanced groups and for models in which an interaction effect is of interest (Langsrud, 2003). Furthermore, we coded the three main conditions visual-verbal link, verbal link only, and no verbal link as ordered factors (i.e., 1, 2, 3 respectively) as we expected a successively weaker effect in each condition.

RESULTS

RESPONSE ERROR

The 3-way interaction effect between required response, effect prime, and instructed response for response errors was significant $F(1, 633) = 14.1$, $p < .001$, $\eta_p^2 = .02$. The 4-way interaction effect including the three main conditions did not reach the conventional significance level, $F(1, 633) = 2.08$, $p = .149$, $\eta_p^2 < .01$. The non-significant trend could indicate an effect in the opposite direction than predicted: for example, showing no replication of Damanskyy et al. (2022) in the direct replication (visual-verbal link) condition and the strongest effect in the alternative, saliency-based (no verbal link) condition. To investigate this possibility, we performed further analysis of each condition separately to test whether we replicate the previous results and whether the trend is in the predicted direction. All results of the ANOVA analysis with response errors as the dependent variable are presented in Appendix B. In the following subsections we report only the hypothesis-relevant effects. To simplify comparisons between the three conditions, we present the size of the compatibility effect for each condition at the end of this section (Figure 2).

Visual-verbal link (Replication of Damanskyy et al., 2022)

The expected 3-way interaction effect between required response, effect prime, and instructed response on response errors was significant $F(1, 241) = 10.08$, $p = .002$, $\eta_p^2 = .04$. We analyzed the experimental effect within prime present and prime absent trials separately. The expected 2-way interaction effect between required response and effect prime was significant for the trials with the effect prime present $F(1, 241) = 7.36$, $p = .007$, $\eta_p^2 = .02$. The same 2-way interaction effect was marginally significant for the trials with the effect prime absent $F(1, 241) = 3.31$, $p = .070$, $\eta_p^2 = .01$. The visual inspection of both 2-way interactions (Figure 1a) illustrate that the experimental effect within prime present trials was in the expected direction (compatible instructed and required responses are facilitated), whereas the effect in prime absent trials contained a tendency of the reverse pattern.

Verbal link only

The 3-way interaction effect between required response, effect prime, and instructed response was significant $F(1, 201) = 3.98$, $p = .047$, $\eta_p^2 = .01$. We analyzed the data further separately for the prime present and prime absent trials. For the critical prime-present trials, the 2-way interaction effect was not significant $F(1, 201) = 1.89$, $p = .171$, $\eta_p^2 = .00$. Similarly, the same interaction effect was also not significant for prime-absent control trials $F(1, 201) = 2.41$, $p = .122$, $\eta_p^2 = .01$. Visual inspection of the result pattern (Figure 1b) nonetheless indicates a pattern in the expected direction. Whereas the prime absent trials (left pane) indicate facilitation of responses in which instructed and required response are compatible, this pattern is reversed in the prime-absent trials (right pane).

No verbal link

In contrast to the previous two conditions, the 3-way interaction effect between required response, effect prime and instructed response was not significant $F(1, 189) = 1.13$, $p = .289$, $\eta_p^2 = .00$. The separate analyses for the prime present $F(1, 189) = 2.07$, $p = .152$, $\eta_p^2 = .00$ and prime absent trials $F(1, 189) = 0.24$, $p = .622$, $\eta_p^2 = .00$ also did not show significant effects. Figure 1c illustrates the visual presentation of this analysis.

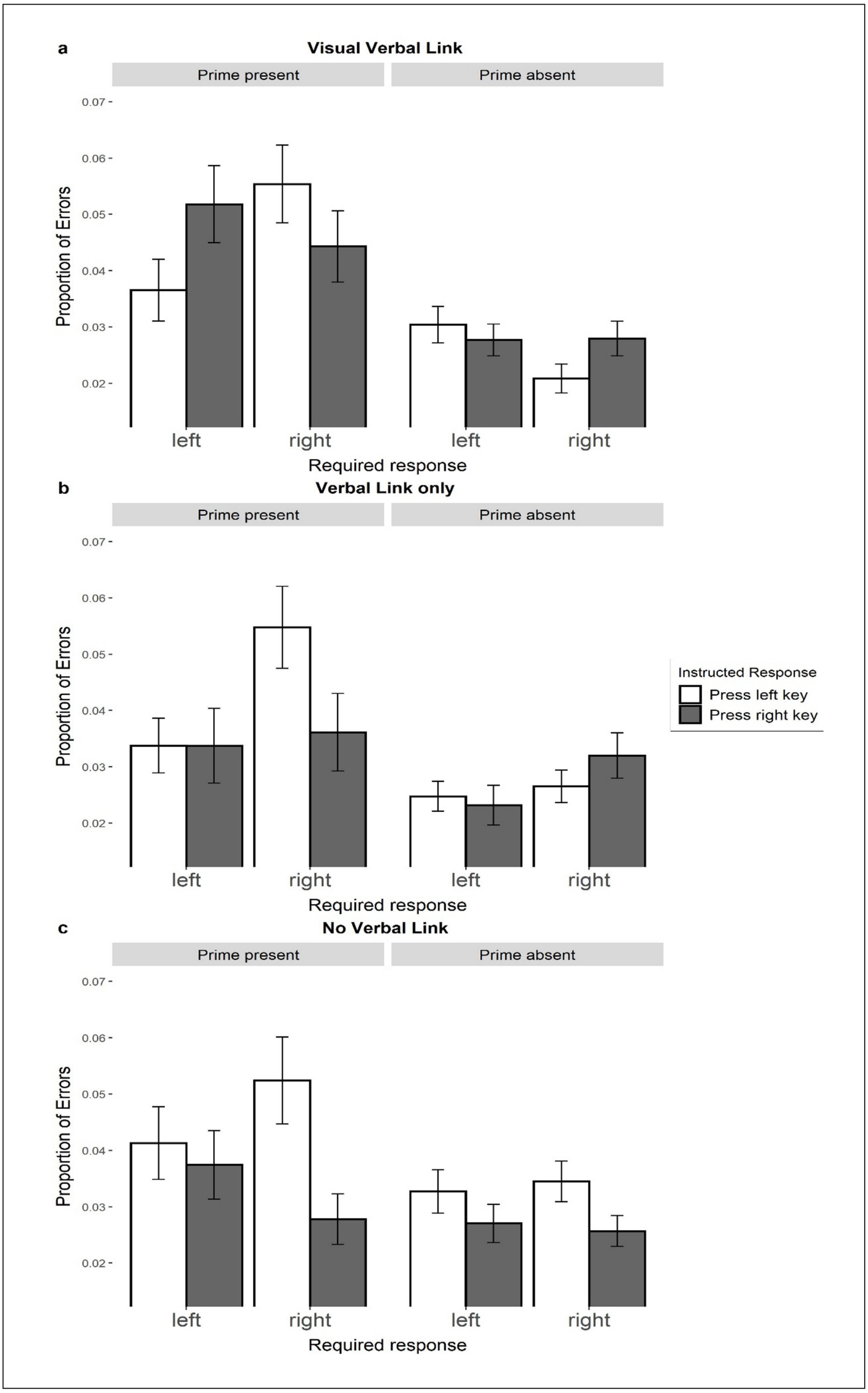

Figure 1 Mean response errors as a function of required response, effect prime, and instructed response for three conditions separately. The graph represents three different parts for three conditions separately: visual-verbal link **(a)**, verbal link only **(b)** no verbal link **(c)**.

Note: Bars represent descriptive means with the standard errors for three main conditions. *Required response* specifies what response was required from participants in a given trial according to the categorization task instructions. *Effect prime* specifies whether the blue screen was present (critical) or absent (neutral) in a given trial. In the visual-verbal link (a) and verbal link only (b) conditions, *instructed response* indicates the instructed action formulated in action-effect manner ("To make the screen blue, I will press the left/right key"). In the no verbal link (c) condition the instructed action was a simple sentence ("I will press the left/right key").

Response compatibility score

As the magnitude of each compatibility effect is not easily visible from the three 3-way interaction effects illustrated in Figure 1, we calculated a response compatibility score for each experimental condition. The score is calculated by the sum of both compatibility effects in the critical prime present condition, minus the same sum calculated for the control condition (prime absent). The subtraction of the control condition "compatibility" effect results in an overall zero score (i.e., no effect) if the control condition shows the same result pattern as the critical experimental condition. The more positive the compatibility score, the larger the observed facilitation effect of compatible configurations and/or interference from incompatible configurations in the critical (prime present) condition as compared to the control (no prime) condition. Appendix A presents an example of this calculation procedure.

As illustrated in Figure 2, although the overall 4-way interaction effect is not significant (p = .149), the result pattern is descriptively in the predicted direction. The visual-verbal link condition shows the strongest compatibility effect, and the effect in the verbal link only condition is weaker. The no verbal link condition shows the weakest compatibility effect.

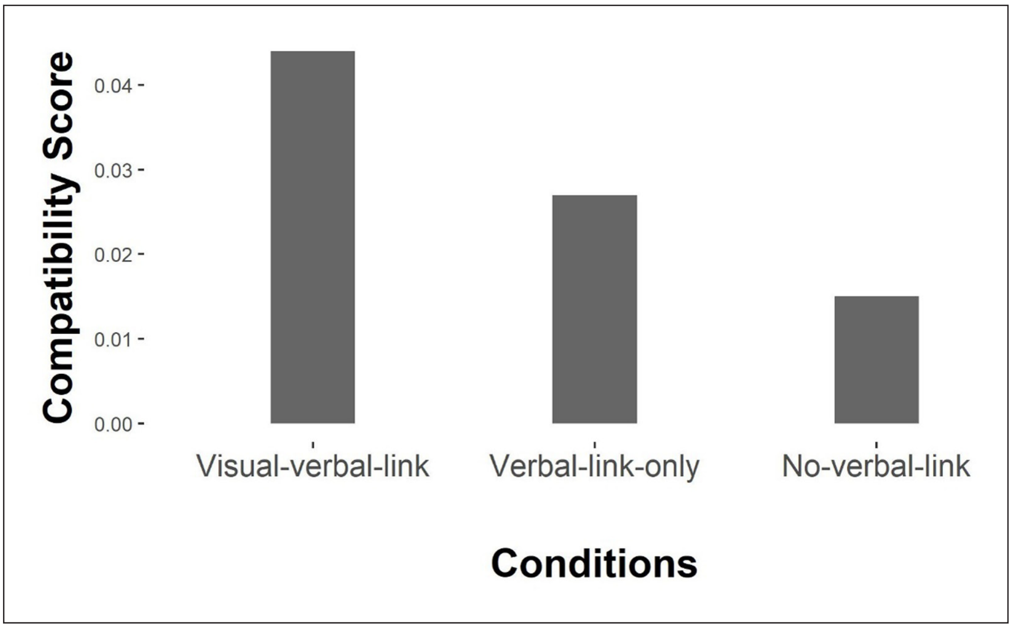

Figure 2 Compatibility gain scores of response errors.

Note: Bar represents descriptive mean compatibility gain scores of response errors for each condition.

REACTION TIME

The 3-way interaction effect between required response, effect prime, and instructed response was not significant $F(1, 633) = 1.24$, $p = .266$, $\eta_p^2 = .00$. The 4-way interaction effect including the three main conditions was also not significant $F(1, 633) = 0.23$, $p = .635$, $\eta_p^2 = .00$. To stay consistent with the response errors analysis and to evaluate potential speed-accuracy trade-offs, we analyzed each condition separately. None of the 3-way or 2-way interaction effects are significant (all ps > .201). Appendix C presents the full ANOVA result tables for the reaction times analysis.

Visual-verbal link

The 3-way interaction between required response, prime, and instruction response was not significant $F(1, 241) = 1.03$, $p = .310$, $\eta_p^2 < .01$. The analysis revealed a significant main effect of prime, indicating that participants responded slower on critical trials than on control trials. We analyzed further the experimental effect within effect prime present (critical) and effect prime absent (control) trials separately to evaluate potential influence of speed accuracy trade-off. The 2-way interaction between required response and effect prime in critical trials was not significant $F(1, 241) = 0.05$, $p = .831$, $\eta_p^2 < .01$. The same 2-way interaction was also not significant within control trials $F(1, 241) = 1.61$, $p = .206$, $\eta_p^2 < .01$. These results indicate the speed-accuracy trade-off did not affect participants' response errors.

Verbal link only

The 3-way interaction between required response, prime, and instructed response was not significant $F(1, 201) = 0.49$, $p = .483$, $\eta_p^2 < .01$. We analyzed further the experimental effect within effect prime present (critical) and effect prime absent (control) trials separately to evaluate the potential influence of a speed-accuracy trade-off. The 2-way interaction was not significant in both critical trials $F(1, 201) = 0.09$, $p = .765$, $\eta_p^2 < .01$ and control trials $F(1, 201) = 0.48$, $p = .487$, $\eta_p^2 < .01$.

No verbal link

The 3-way interaction between required response, prime, and instructed response was not significant $F(1, 189) = 0.12$, $p = .735$, $\eta_p^2 < .01$. We analyzed further the experimental effect within effect prime present (critical) and effect prime absent (control) trials separately to evaluate the potential influence of a speed-accuracy trade-off. The 2-way interaction was not significant in both critical trials $F(1, 189) = 1.88$, $p = .172$, $\eta_p^2 < .01$ and control trials $F(1, 189) = 2.30$, $p = .131$, $\eta_p^2 < .01$.

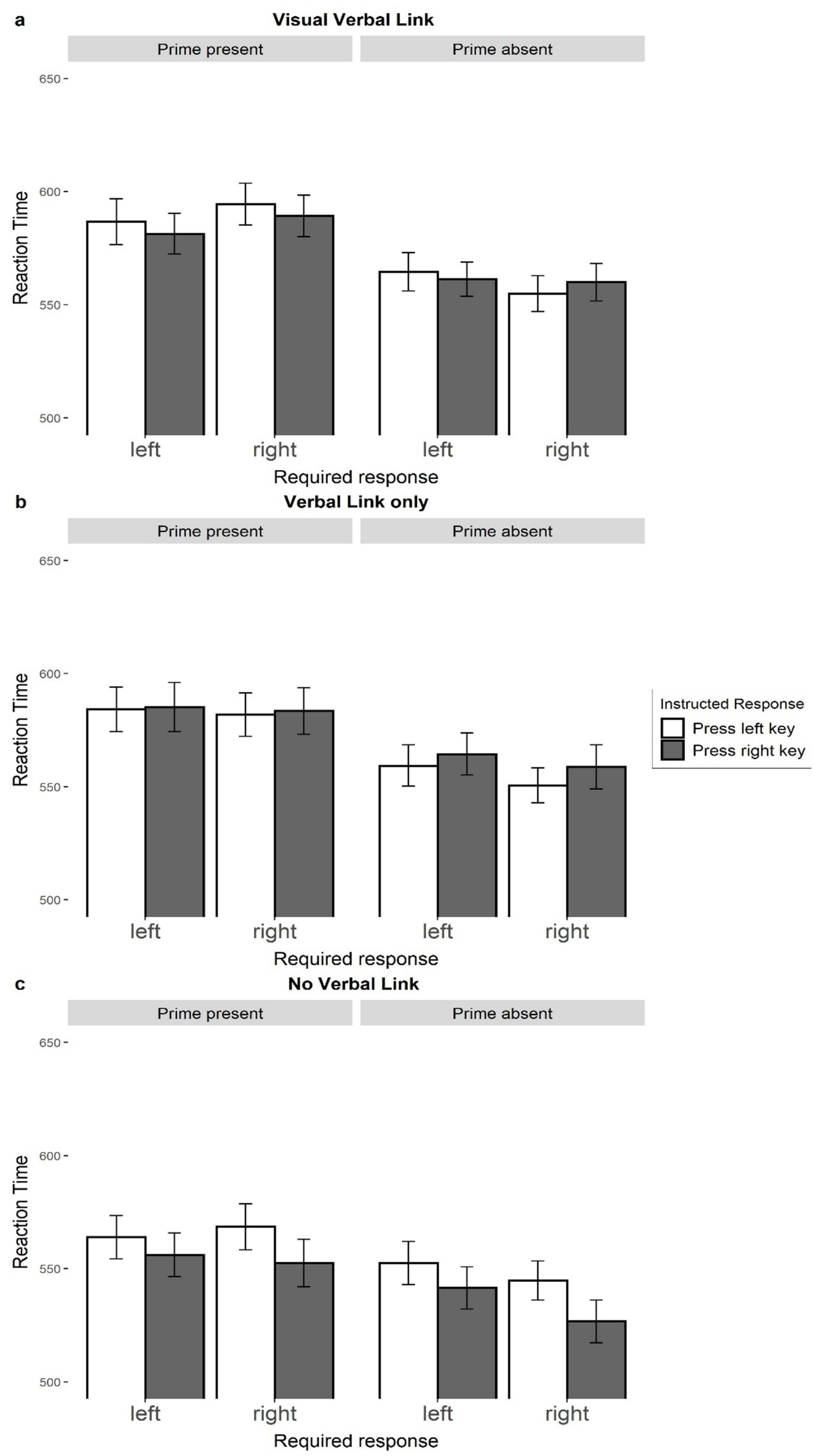

Figure 3 Mean response times as a function of required response, effect prime, and instructed response for three conditions separately. The graph represents three different parts for three conditions separately: visual-verbal link **(a)**, verbal link only **(b)** no verbal link **(c)**.

Note: Bars represent descriptive means with the standard errors for three main conditions. *Required response* specifies what response was required from participants in a given trial according to the categorization task instructions. *Effect prime* specifies whether the blue screen was present (critical) or absent (neutral) in a given trial. In the visual-verbal link (a) and verbal link only (b) conditions, *instructed response* indicates the instructed action formulated in action-effect manner ("To make the screen blue, I will press the left/right key"). In the no verbal link (c) condition the instructed action was a simple sentence ("I will press the left/right key").

Response compatibility score

The compatibility scores of response times were calculated in the same ways as the compatibility scores of response errors.

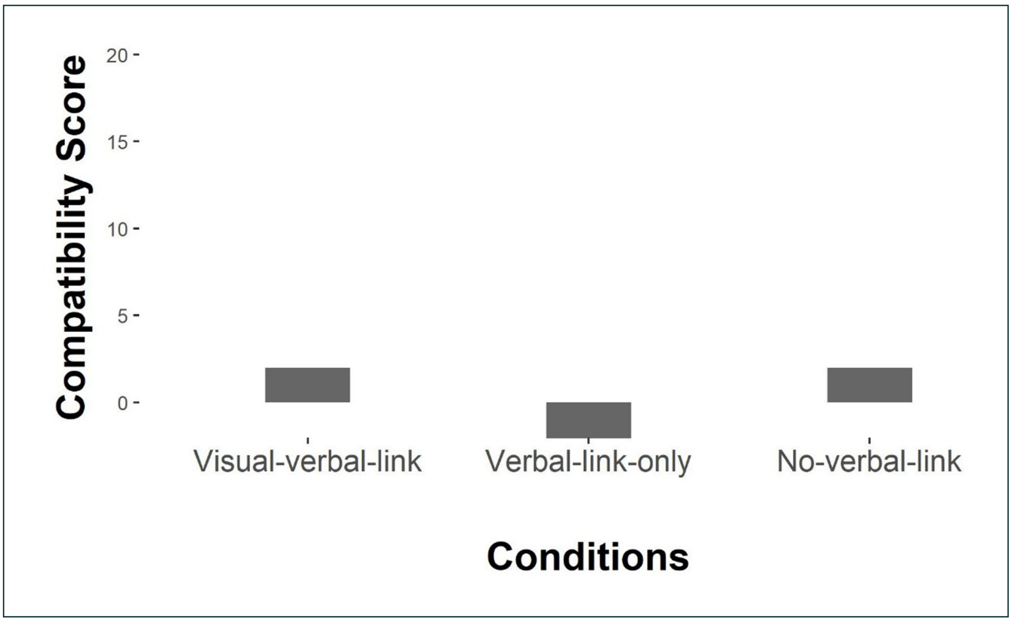

Figure 4 Compatibility gain scores of response times.

Note: Bar represents descriptive mean compatibility gain scores of response errors for each condition.

DISCUSSION

The aim of the present study was to provide a further test of verbally induced response-compatibility effects and to investigate some of the underlying conditions. We will start by discussing the visual-verbal ink condition as it is an exact replication of previous results that we included as a baseline against which to compare the outcomes of the other conditions.

VERBAL ASSOCIATION AND COLOR SPECIFICATION (VISUAL-VERBAL LINK)

The results from the visual-verbal link condition replicated findings to the study of Damanskyy et al. (2022; effect-action order). Based on the response error analysis, the action-effect instructions influenced participants' accuracy in the prime-present trials compared to the prime-absent control trials. Specifically, participants made fewer errors in compatible trials (i.e., when the required response matched the instructed response from the unrelated action-effect instructions), and more errors in incompatible trials (i.e., a compatibility effect). In the prime-absent trials, this compatibility effect was not observed. The response time analysis of this condition did not indicate a speed-accuracy trade-off. As an intermediate conclusion, the visual-verbal link condition provides a high-powered replication of previous findings (Damanskyy et al., 2022). They indicate that verbal action-effect instructions lead to unintentional response priming effects even when the instruction phase and the test phase are separated by a few minutes and the action-effect instructions are never used during the test phase. These results parallel research on the unintentional influences of stimulus-response instructions (e.g., Liefooghe & De Houwer, 2018).

VERBAL ASSOCIATION WITHOUT AN ASSOCIATED VISUAL CUE (VERBAL LINK ONLY)

The verbal link only condition aimed to evaluate the role of a perceptual component in the effect of verbal instructions on action. In this condition participants did not see the example of the blue color patch before processing the action-effect instructions. The analysis still revealed a significant compatibility effect in the expected direction. As in the visual-verbal link condition, participants made fewer errors in prime-present trials when the required response matched the instructed response from the action-effect instructions, and the response time analyses did not indicate a speed-accuracy trade-off. By itself, this condition that includes only a small design change provides another replication of unintentional response-priming effects from verbal instructions.

Descriptively, the compatibility effect in this condition was smaller than in the visual-verbal link condition (see Figures 2 & 3 for summaries of the compatibility effect scores). The only methodological difference was that participants in the visual-verbal link condition were exposed to a more exact specification of the color represented the action-effect instructions than those in the verbal link only condition. If the mechanism from verbal instructions to responses was based only on abstract, symbolic representations of the information, visually perceiving the color during the instruction phase would not have made a difference for the resulting compatibility effect. While we only provide descriptive evidence that the color presentation made a difference, similar results have also been previously reported by Schmidt and Zelinsky (2009) and Wolfe et al. (2004), using a visual search paradigm. This research demonstrated that visual search for a stimulus is not as effective when it is only presented in an abstract form (compared to conditions in which the priming stimulus was visually presented along with a verbal specification).

NO VERBAL ASSOCIATION (NO VERBAL LINK)

The no verbal link condition was included to evaluate whether the effect of action-effect instructions is based on associative learning, or if the results could be explained by a saliency-based alternative account. If the compatibility effect indeed stems from an associative link between the stimulus and response components and not merely familiarity with these components and an independent temporary activation of both components, then presenting them separately from each other (i.e., without linking them) should result in a weaker (or absent) compatibility effect. For the first time in a series of five tests (Damanskyy et al., 2022, Exp. 1 & 2 and the first two conditions of the present study), we predicted no effect and found no significant compatibility effect—the pattern of response errors did not differ between the prime-present and prime-absent trials. Although we certainly hoped for a clearer outcome regarding the statistical analyses between the different conditions, we want to emphasize the importance of such control conditions. Independent of whether effects of verbal instructions are attributed to associative learning or alternative proposals about the components' relationships (e.g., propositional learning; Sun et al., 2020), there is always the possibility that "mere exposure" to the instruction components, independent of their instructed relationships, can influence subsequent responses. Thus, experimental designs should account for such possibilities as we did in our design, even if it increases the likelihood of less clear-cut statistical outcomes.

LIMITATIONS

The central limitation of the present research is in the non-significant 4-way interaction effect. Thus, while we have evidence that the instructions affected subsequent responses (3-way interaction effect), our conclusions related to differences between the conditions should be considered in relation to related research and as a starting point to continuously putting them to the test. We nonetheless presented the descriptive condition differences as they corresponded to our hypotheses. We expected the strongest compatibility effect for the visual-verbal link condition, and a comparatively weaker effect in the verbal link only condition. Furthermore, we expected and found the weakest compatibility effect in the no verbal link condition.

Overall, the present findings are based on the analysis of response errors. The analysis of response times serves to rule out a speed-accuracy trade-off as an explanation of the findings, mirroring the pattern found by Damanskyy et al. (2022). Mekawi and Bresin (2015) suggest that short deadlines (i.e., instructing participants to emphasize speed over accuracy) reduce variability within response times, which in turn increase error rates—leading to greater probability of finding an experimental effect within response errors. Researchers should consider using experimental designs without a response deadline to show more variability between conditions in response times. This, however, will create the potential for speed-accuracy trade-offs, which could complicate the analyses.

The descriptive pattern of response errors and response times shown in Figure 1 and Figure 4 suggests that participants responded more slowly and made more errors in the prime-present trials (blue background) compared to the prime-absent trials (gray background). However, since the sudden background color change may have negatively impacted participants' performance in the prime-present trials, the absolute difference between critical and control trials cannot be accurately estimated without an additional control condition. Since our prime-present trials involve both a verbal priming effect and the potential interference effect of a sudden background change, various combinations of interference are possible, including facilitation, facilitation and interference, or only interference. Therefore, to separate and differentiate the possible effect of the sudden background-color change, future studies should include an additional control condition that includes a background-color change without a verbal link between that color and response. Although our present data cannot differentiate between these possibilities, this limitation does not diminish the informative value of the observed interaction effect that suggests that verbal priming systematically influenced the responses.

CONCLUSION

The present study provides another illustration of a verbally-induced response-compatibility effect. Unintentional response-priming effects are well documented (e.g., Shin et al., 2010), but typically derive from associations that are well-learned from direct experiences. In the present study, we observed response-priming effects following the memorization of a verbal representation of the action-effect contingency without any prior direct experiences with that contingency.

Beyond this central effect, we provide some initial evidence that perceptual aspects play a role in the mechanism that mediates the effect of verbal information on subsequent responses, and we find additional support for an associative-learning mechanism from verbal information to action by providing evidence against an explanation based solely on familiarity (or salience) of the individual perception and action components. These findings are in line with previously-presented theoretical perspectives (Martiny-Huenger et al., 2015, 2017) that suggest verbal information induces experiential simulations (e.g., Barsalou, 1999) that can then lead to (associative) learning that is similar to learning from direct experiences.

DATA ACCESSIBILITY STATEMENT

The datasets generated during the current study are available via the Open Science Framework (OSF): https://osf.io/28m6u/?view_only=fb3e8821628e41f1bf688ae71118b873.

The R script and R output are also available vis OSF: https://osf.io/28m6u/?view_only=fb3e8821628e41f1bf688ae71118b873.

The pre-registered hypothesis is also available via OSF: https://osf.io/qfmc6.

ADDITIONAL FILE

The additional file for this article can be found as follows:

- **Appendices.** Appendice A, B to C. DOI: https://doi.org/10.5334/joc.284.s1

ETHICS AND CONSENT

This study was performed in line with the principles of the American Psychological Association. Approval was granted by the Ethics Committee of the Arctic University of Norway (March 2020 /No: 2017/1912).

Informed consent was obtained from all individual participants included in the study.

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COMPETING INTERESTS

The authors have no competing interests to declare.

AUTHOR CONTRIBUTIONS

Conceptualization: Yevhen Damanskyy, Torsten Martiny-Huenger, Elizabeth J. Parks-Stamm.

Methodology & data collection: Yevhen Damanskyy, Torsten Martiny-Huenger.

Formal analysis: Yevhen Damanskyy Torsten Martiny-Huenger.

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Verbal instructions as selection bias that modulates visual selection

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ABSTRACT

Research has shown that in addition to top-down and bottom-up processes, biases produced by the repetition priming effect and reward play a major role in visual selection. Action control research argues that bidirectional effect-response associations underlie the repetition priming effect and that such associations are also achievable through verbal instructions. This study evaluated whether verbally induced effect-response instructions bias visual selective attention in a visual search task in which these instructions were irrelevant. In two online experiments (Exp.1, $N = 100$; Exp. 2, $N = 100$), participants memorized specific verbal instructions before completing speeded visual-search classification tasks. In critical trials of the visual search task, a priming stimulus specified in the verbal instructions matched the target stimulus (positive priming). In addition, the design of Experiment 2 accounted for the repetition priming effect caused by frequent appearance of the target object. Reaction time analysis showed that verbal instructions inhibited visual search. Response error analysis showed that verbal effect-response formed an effect-response association between verbally specified stimulus and response. The results also showed that the target object's frequent appearance strongly affected visual search. The overall findings showed that verbal instructions extended the list of selection biases that modulate visual selective attention.

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Given that people can only process limited information at one time, they therefore need to selectively focus their attention on a behaviourally relevant scene or object. The predominant models of attentional control describe selective attention in terms of interplay between bottom-up and top-down processes (Carrasco, 2011). These models provide rich flexibility for exploring human attention from a variety of perspectives and explain how attention navigates actions and perception through the interplay of physical properties (colour, shape, location) and the behavioural relevance of various objects.

While such a theoretical split into different processes explains many aspects of selective attention, an alternative framework argues that selective attention is also controlled by section biases that might overcome the salience of either physical properties or the behavioural relevance of stimuli (Awh et al., 2012). For example, history-based selection or reward associated with specific stimuli bias selective attention, making it more sensitive to those stimuli (e.g., Anderson et al., 2011b). A large body of research provided evidence that past episodes of encountering and selecting specific objects bias attentional selection through the *repetition priming* mechanism (e.g., Logan, 1990; Theeuwes, 2018; Theeuwes & Failing, 2020), arguing that such an influence on attentional selection acts beyond top-down and bottom-up processes.

Furthermore, research on action control argues that associative learning underlies the repetition priming effect (Henson et al., 2014; Soldan et al., 2012). Specific actions with specific objects result in the formation of bidirectional associations between those actions and objects (stimulus-response and response-effect associations; Elsner & Hommel, 2001; Frings et al., 2020; Shin et al., 2010). This formation, in turn, can serve as a unified priming mechanism that navigates attentional focus toward previously encountered stimuli (Hommel, 2005; Memelink &

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Hommel, 2013; Müsseler & Hommel, 1997). Moreover, encountering that stimulus also triggers an associated response with that stimulus. Therefore, the repetition priming effect involves an interplay between perception and actions, and this interplay is based on the associative learning principle (Soldan et al., 2012).

Parallel to action-control research, the last decade has seen the rapid development of research on verbal information and verbal action planning, and their effect on behavioural control (Brass et al., 2017; Liefooghe & De Houwer, 2018; Martiny-Huenger et al., 2017; Meiran et al., 2015a; Meiran et al., 2015b). These lines of research investigate how verbal instructions formulated in a stimulus-response (Liefooghe et al., 2012; Liefooghe et al., 2018), response-effect (Theeuwes et al., 2015), or effect-response (Damanskyy et al., 2022) manner influence cognitive control. Despite growing evidence that verbal instructions, formulated in a stimulus-response manner, are highly important to action control, little is known about whether verbal instructions serve as a selection bias. Studying how verbal instructions modulate selective attention can provide valuable insights into the topic of selection biases. This study investigates whether verbal instructions formulated in an effect-response manner affect visual selective attention.

Selective attention

Selective attention is the ability to select specific stimuli, select behavioural responses, access particular memories, or navigate behaviourally relevant thoughts at a given moment (Maurizio, 1998). Human perception is continuously exposed to complex input from surroundings targeting the five perceptual domains of sight, hearing, smell, touch, and taste. Visual selective attention is one of the central topics in research on perception because it provides a rich experimental flexibility that allows scholars to investigate attention across multiple visual domains (e.g., colour, shape, location; Carrasco, 2011). On a conceptual level, visual selective attention operates by representations of a priority map (Awh et al., 2012; Theeuwes, 2013, 2018; Treisman & Gelade, 1980) and integrates three sources of influence: current goal (top-down), physical salience (bottom-up), and selection history.

Bottom-up attention is based on the basic salient visual features of a stimulus (e.g., orientation, colour, motion, size). Research from this perspective focuses mainly on a *feature singleton* (Yantis & Egeth, 1999), which implies that when a presented stimulus is locally unique in one visual dimension (colour, shape, orientation, or size), it attracts focus toward the self. Numerous studies (for a review see Carrasco, 2011) have demonstrated that a unique salient feature in the visual field can capture human focus independently of the task at hand (e.g., a red flower on a green background, a light point in the dark).

Whereas the bottom-up process is often called stimulus-driven, the top-down process entails task-driven factors that shape and navigate perception (Theeuwes, 2018). Yarbus' (1967) classic study demonstrated an example of the top-down guidance of selective attention. Participants viewed a family room scene and had to answer specific questions about that scene. Participants' attentional focus varied depending on the specific task they were asked to perform. For example, the eye saccades of participants whose task was to identify the people's ages differed from the saccades of both those whose task was to remember object locations and those who had no particular task. These differences indicated that task-relevant factors navigated selective attention.

While many studies have explained selective attention solely from bottom-up and top-down perspectives, Awh et al. (2012) proposed an integrative framework specifying a modified taxonomy of attentional control. According to this model, three sources of attentional control contribute to the priority map that guides selective attention: current goal, physical salience, and selection history. The concept of selection history adds two additional sources of influence on the priority map. The underlying notion is that attention is often driven by neither salient stimuli nor the goal of an observer. Indeed, in many cases, attention guidance is biased by a reward associated with specific stimuli (Anderson et al., 2011a; Anderson & Yantis, 2013; Bucker & Theeuwes, 2014; Failing & Theeuwes, 2015; Failing & Theeuwes, 2016; Libera & Chelazzi, 2006) or by previous history-based selection in terms of the repetition priming effect (Theeuwes & Van der Burg, 2011, 2013; for a review see, Kristjánsson & Campana, 2010; Lamy & Kristjansson, 2013).

Repetition priming from verbal instructions

Repetition priming is a change in the reaction time or response accuracy to a stimulus due to prior presentation of the same stimulus (Henson et al., 2014; Logan, 1990). Encountering the same stimulus and performing the same response is sufficient for automatic stimulus-response associations to emerge, meaning that the repetition priming effect involves associative learning (Henson et al., 2014; Soldan et al., 2012). In addition, a large body of research demonstrated that stimulus-response associations are bidirectional (Elsner & Hommel, 2001).

Bidirectionality implies that associative learning also emerges from response-effect associations in which a stimulus serves as an effect of a particular response. For example, participants may perceive that a particular response (left keypress) leads to a playback of a specific sound (high or low pitch). A temporal overlap between such a response and its effect results in the formation of bidirectional response-effect association. When the participants hear the same sound again, they effortlessly and automatically retrieve a previously formed response-effect association that provides a faster and more direct route of responding (e.g., Elsner & Hommel, 2001).

However, a growing interest in research on verbal instructions has demonstrated that verbally induced priming can also form stimulus-response associations linking perception and actions. The empirical evidence comes primarily from two different research directions: *instruction-based research* and *implementation intentions*. Within instructions-based research, verbal instructions are treated as a simple form of stimulus-response mappings (e.g., "if cat press left; if dog, press right") that have an immediate effect. Such mappings are translated into procedural representations in working memory, enabling their execution through reflexive behaviour (Brass et al., 2017; Liefooghe et al., 2012; Meiran et al., 2012; Meiran et al., 2015a; Van 't Wout et al., 2013). However, several studies emphasize that the effect of verbal instructions also relies on representations in long-term memory (Liefooghe & De Houwer, 2018; Pfeuffer et al., 2017).

In contrast, implementation intentions research emphasizes a specific verbal action plan (e.g., "If I pass a supermarket, I will buy bread") as a critical component of action planning, and participants are asked to repeat this plan several times to ensure encoding and remembering (Gollwitzer & Sheeran, 2006). Such planning creates the direct perception-action link between the anticipated situation (critical cue) and the intended behaviour (e.g., passing a supermarket serves as a critical cue that automatically triggers the planned action of buying bread). The execution of such a plan does not require conscious involvement. As soon as the individual encounters that cue, it triggers a specific behaviour linked to it. The theory of implementation intentions suggests that such verbal planning can serve as an alternative path to the strategic automaticity of action control (Gollwitzer, 1999; Gollwitzer, 2014) and that the effect of this action planning may be observed over days or even weeks (Conner & Higgins, 2010; Papies et al., 2009).

While both implementation intention and instruction-based research provided empirical evidence that verbally induced stimulus-response associations influence response selection and retrieval, an open question remains as to whether these associations influence selective attention. Several studies using visual search tasks found that while using verbal cues influences visual selective attention, this influence is not as effective as using specific visual cues (Knapp & Abrams, 2012; Schmidt & Zelinsky, 2009; Wolfe et al., 2004).

However, these studies within the visual search paradigm used verbal primes as simple textual cues, with participants aware that these textual cues were relevant for an upcoming task. Furthermore, these studies did not formulate textual cues in sentence instruction or action plans formulated in a stimulus-response format. In contrast, research on verbal instructions argues that verbal instructions can have an unintentional priming effect in tasks in which those instructions are irrelevant, especially when participants are asked to form an intention to execute given priming instructions (Sheeran et al., 2005).

The present experiments

This study investigated whether verbally induced stimulus-response associations affect visual selective attention as a selection bias in the facilitation paradigm. In Awh et al.'s (2012) framework, the effect of selection biases can either facilitate the top-down or bottom-up processes or work in opposition to them. Thus, if verbal instructions act as a selection bias, the effect of that bias should be observable through one of those characteristics.

The general design of these present experiments involved prime-probe phases similar to those of other studies of implementation intention and instruction-based research (Liefooghe & De Houwer, 2018; Martiny-Huenger et al., 2017). In the prime phase, participants formed a specific verbal action plan with specific stimulus-response associations. The probe phase involved probe trials to evaluate whether previously verbally induced associations influenced participants' selective attention and behavioural responses in a subsequent, two-alternative forced-choice task (2AFC). In this task, participants categorized a target stimulus as either a fruit or a vegetable. To evaluate whether verbally induced associations modulate visual selective attention, the 2AFC task was embodied in a visual search task with the additional objective of finding a target stimulus among distractors (Wolfe, 1994).

In the present experiments, a stimulus that was specified in the verbal instructions matched one of the target stimuli in the 2AFC visual search task (facilitation paradigm; Logan, 1990). If the verbal instructions prime visual selective attention, then the participants' performance – upon encountering a critical stimulus from the prime phase – would result in faster and more accurate responses than their responses to target stimuli. Moreover, according to the stimulus-response priming principle (Henson et al., 2014), encountering a stimulus associated with a specific response should also lead to unintentional retrieval of that response. Therefore, in a compatible condition in which required and retrieved responses matched (left-left; right-right), I expected faster and more accurate responses than from an incompatible condition containing a reversed pattern (left-right; right-left).

In the present study, all target and distractor stimuli represented different real objects. Although a common procedure within the visual search paradigm involves using a feature singleton in which target and distractor stimuli differ on one or a few dimensions (e.g., colour, shape, size; Yantis & Egeth, 1999), using real objects is not new in these types of tasks (Bravo & Farid, 2004; Ehinger et al., 2009; Fletcher-Watson et al., 2008; Schmidt & Zelinsky, 2009; Yang & Zelinsky, 2009). Moreover, Bravo and Farid (2004) pointed out that using different object categories from real life as targets or distractor stimuli can provide certain advantages for visual search tasks. First, real objects allow researchers to avoid high artificial stimuli; second, real objects have more practical applications and bring the situation closer to real life (i.e., ecological validity; Orne, 2002).

In addition, several types of verbal instruction formulations appear in the research on verbal instructions: stimulus-response (Martiny-Huenger et al., 2017), response-effect (Theeuwes et al., 2015), and effect-response (Damanskyy et al., 2022). The effect-response is an action-effect modification of the stimulus-response formulation. Damanskyy et al. (2022) found that the effect-response formulation does not diminish the effectiveness of stimulus-response associations. Conversely, such formulation provides more flexibility to formulate an instruction sentence. Therefore, in the present study, I formulated the verbal instructions in an effect-response manner (e.g., "to make an apple appear on the screen, I need to press the left key") in which a critical stimulus (i.e., apple) was formulated as the effect of a response (i.e., "I need to press the left key").

Experiment 1

Methods

Participants

To determine the minimum sample size given $d = 0.2$, I ran a simulation analysis in R (R Core Team, 2021) using the Simr package (Green et al., 2016).[1] The results yielded 100 English-speaking participants in the first experiment (64 females, 30 males, and six participants who did not specify their gender). After the data cleaning described in the following "Data Preparation" subsection, the analyzed sample included 88 participants. Overall, the participants' ages ranged from 30 to 45 years ($M = 35.8$, $SD = 4.46$). All participants were recruited through the recruiting portal Prolific and received monetary compensation for their participation. The local Ethics Committee of the Arctic University of Norway approved the study, and all participants provided informed consent prior to the experimental procedures.

Materials

The experiment was programmed with PsychoPy v.2020.2.1 (Peirce et al., 2019) and uploaded to Pavlovia.org, thereby allowing online participation. All participants received a link to the experiment via Prolific, allowing them to participate remotely. The recruiting portal only allowed participation with a desktop computer or laptop with a keyboard.

Design

The experiment followed a 2 (*prime*: critical vs. control) by 2 (*compatibility*: compatible vs. incompatible) mixed design. Prime was a within-subject factor that specified whether a target stimulus in the visual search task represented a priming stimulus from the verbal instructions sentence (apple; critical trials) or control stimuli (all other fruits and vegetables; control trials).

Figure 1 illustrates all target stimuli. Compatibility was a between-subject factor that specified whether the associated response with a priming stimulus from the verbal instructions sentence matched or mismatched the instructed response for the 2AFC visual search task ("apple"-left/right, fruits-left/right).

Procedure

Prime Phase. Participants saw a critical verbal instruction formulated in an effect-response manner: "To make an apple appear on the screen, I need to press the left/right key." No time frame restricted participants when memorizing the priming sentence. Prior to the prime phase, participants were clearly instructed that any reference to "left" or "right" meant the "A" or "L" key, respectively. Response specification (press left vs. press right) was randomly counterbalanced. Participants were asked to remember these instructions because they would have to apply them during the final part of the experiment. Appendix B presents all the instructions that the participants saw.

Probe Phase. All probe trials had the same between-trial interval ("+") of 500 ms. Afterward, a 6×4 grid showed the participants 24 figures for 3000 ms. The locations of all 24 stimuli changed randomly in each trial. In each trial, one of those 24 stimuli was a target stimulus. For each control stimulus, the critical stimulus appeared in a proportion of 3:1 (approximately 2000 critical trials versus 500 trials for each control stimulus). In total, participants worked through 96 probe trials. The participants' task was to find – as quickly and accurately as possible – a target stimulus and categorize it as a fruit or vegetable by pressing either the left ("A") or right ("L") key, respectively (keypress conditions were counterbalanced). Six different fruits and six different vegetables represented target stimuli, including the priming stimulus ("apple"). The other 23 objects belonged to different categories. Before the probe phase, participants performed 10 practice trials without the priming stimulus ("apple"). Figure 2 provides a schematic presentation of the prime and probe phases.

At the end of the experiment, after completing all trials in the visual search task, participants performed a simple one-trial task. They had to press a corresponding key that would lead to the appearance of an "apple" on the screen as specified in the priming instructions during the prime phase. I created this task solely to avoid participant deception in the prime phase. Therefore, this task was not included in statistical analyses.[2]

Data preparation

I used the R software package to prepare and analyze the data (R Core Team, 2021). I excluded three participants whose ages fell outside of the predetermined criteria (i.e., younger than 30 years or older than 45 years). I removed one outlier whose responses included greater than 25% incorrect keypresses (not "A" or "L") during the test phase. Eight-point-four percent of critical trials were removed to account for potential intra-trial priming effect (Lamy & Kristjansson, 2013). Other participants' incorrect keypresses were also excluded (3.45% data lost). Participants missed the response deadline in only 0.07% of trials. After I applied the boxplot method (Tukey, 1977), I excluded eight participants due to an excessive amount of response errors (more than 10%). Response error analysis included data with 6956 observations.

To evaluate whether a specific target control stimulus caused a deviation in participants' response times and response errors compared to their responses to the other control stimuli, I applied the boxplot method (Tukey, 1977). This method was applied separately for response times and response errors. The boxplot analysis of the response errors revealed that the "onion" (bottom-right stimulus in Figure 1) caused an excessive amount of response errors (9% compared to the upper whisker boundary of 4.44%). Therefore, I removed this stimulus from the analysis. The remainder of the response errors that the other control stimuli produced fell within the interquartile range (2.79% – 4.44%).

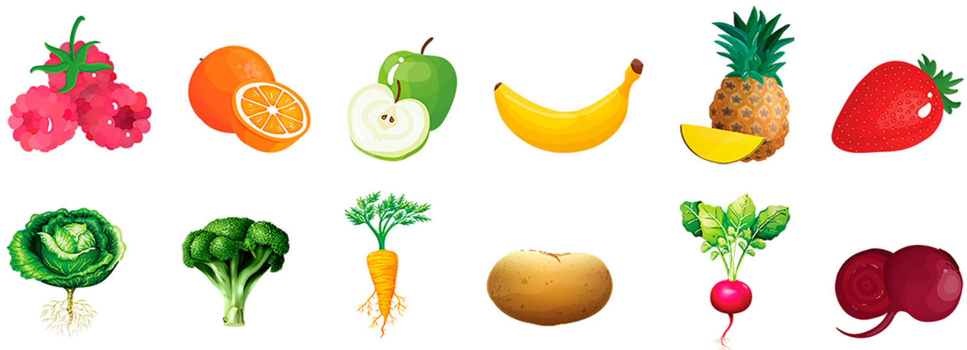

Figure 1. Target Stimuli. Note. An illustration of all target stimuli from Experiment 1 and 2.

Prior to the response time analysis, all response errors were excluded from the data (3.45% data lost). Individual response times beyond the mean ± 2.5 *SD,* calculated by participant and within-participant conditions, were also excluded (1.52% data lost). The response time variable was log-transformed to handle skewness (Judd et al., 1995). Boxplot analysis revealed that response times to the "onion" (1671 ms) and "potato"(1464 ms) deviated from the overall interquartile range (1242–1389 ms). Therefore, these two stimuli (Figure 1) were not included in the analysis. The final response time analysis included data with 6266 observations.

Data analysis

To apply and analyze the mixed models, I used the lme4, lmerTest, and Emmeans packages (Douglas et al., 2015; Kuznetsova et al., 2017). I calculated all confidence intervals in the result sections using bootstrap statistics with 1,000 simulations (Efron & Tibshirani, 1993). The statistical model also included an intercept of the participants as a random factor accounting for by-participant variability. To account for variability in the target stimuli, the model also included the intercept of the visual identity of the target stimuli as a random factor. The regression analysis treated two main factors as dummy variables. The final models for reaction times and response errors were specified as follows:

$$Outcome = prime{*}compatibility + (1\ participants) + (1\ stimulus\_identity)$$

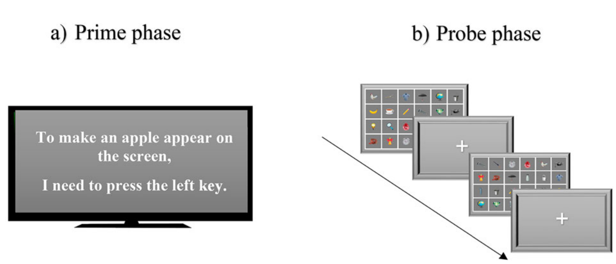

Figure 2. The Prime and Probe Phases Note. The prime phase included a single presentation of a critical verbal action-effect sentence without any deadline. After the prime phase, participants performed a visual search task (96 trials). During the probe phase, the participants' task was to find either a fruit or a vegetable and press the "A" or "L" key, respectively. In one-third of the trials, the target stimulus was an apple (priming stimulus from the prime phase). The location of all figures changed randomly in each trial, and there was only one target stimulus among the distractors.

Results and discussion

Reaction time

Random Factors. The effect of response times as a function of prime and compatibility varied in intercepts across participants ($SD = 0.09$, $\chi2(1) = 349.3$, $p < .001$), indicating significant by-participant variation around the average intercept. The effect of response times as a function of prime and compatibility varied in intercepts across stimulus identity ($SD = 0.02$, $\chi2(1) = 5.2$, $p = .002$), indicating significant by-stimulus identity variation around the average intercept.

Fixed Factors. The two-way interaction term between prime and compatibility was not significant

Table 1. Descriptive Statistics of the Response Time Test Phase and Response Errors.

Trial Type	Condition	Reaction Time (ms)		Response Errors (%)	
		M	SD	M	SD
Critical	Compatible	1239	456	2.73	16.3
Control	Compatible	1291	483	3.33	17.9
Critical	Incompatible	1309	513	2.40	15.3
Control	Incompatible	1316	479	3.84	19.2

($b = -0.01$, 95% CI [−0.06, 0.01], $p = .162$), indicating that the difference between critical and control trials did not differ between compatible and incompatible groups. In the compatible group, the difference between critical and control trials was not significant ($b = -0.04$, 95% CI [−0.00, 0.90], $p = .115$). In the incompatible group, the same difference between critical and control trials was also not significant ($b = -0.01$, 95% CI [−0.03, 0.06], $p = .495$). In contrast, a between-subject comparison revealed a significant difference between the critical trials in the compatible and incompatible groups ($b = 0.04$, 95% CI [−0.00, 0.10], $p = .073$), showing that participants responded to the critical priming stimulus faster in the compatible group than participants in the incompatible group.

The significant difference between the compatible and incompatible groups indicates the presence of the instruction-compatible effect caused not only by the priming stimulus but also by the priming response, as participants responded faster to the priming stimulus (apple) when the required response matched the priming response (left-left; right-right). In contrast, participants responded more slowly to the same priming stimulus when the required response in the visual search task did not match the priming response from the priming action-effect sentence (left-right; right-left). Figure 3A illustrates this regression model. Appendix A1 presents a table with all fixed factor results.

Response error

Random Factors. The effect of response errors as a function of prime and compatibility varied in intercepts across participants ($SD = 0.42$, $\chi2(1) = 7.4$, $p = .006$), indicating significant by-participant variation around the average intercept. As the effect of response errors as a function of the prime and compatibility did not vary in intercepts across stimulus identity ($SD = 0.00$, $\chi2(1) = 0.0$, $p = .1$), the final model did not include target object identity as a random factor.

Fixed Factors. The two-way interaction between prime and compatibility was also not significant ($b = 0.28$, 95% CI [−3.46, 0.98], $p = .365$), indicating that the difference between critical and control trials did not differ between compatible and incompatible groups. In the compatible group, participants' responses were not significantly more accurate in critical trials than in control trials ($b = 0.20$, 95% CI [−2.29, 0.66], $p = .368$). In the incompatible group, participants' responses were significantly more accurate in critical trials than in control trials ($b = 0.53$, 95% CI [0.06, 0.94], $p = .002$). These results indicate that participants responded more accurately when the priming stimulus from the verbal instructions matched the target stimulus in the visual search task. These results did not indicate that the associated response with the priming stimulus influenced participants' accuracy.

The between-subject comparison did not reveal any significant difference in the critical trials between the compatible and incompatible groups ($b = 0.13$, 95% CI [−7.68, 0.42], $p = .638$), showing that the associated response with the priming stimulus did not affect the participants' accuracy. Figure 3B illustrates this regression model, and Appendix A2 presents a table with all fixed factor results.

Discussion

Experiment 1 demonstrated that the verbal action-effect priming sentence influenced participants' performance in the visual search task. Although participants did not respond faster in critical trials than in control trials in either group, the between-subject comparison showed that participants in the compatible group responded significantly faster to the priming stimulus. Response error analysis provided no statistical evidence that the priming verbal action-effect sentence influenced participants' accuracy. Participant accuracy in the critical trials did not significantly differ between the two groups. The only significant difference in the incompatible group between the critical and control trials was that participants responded significantly more accurately in the critical trials. This pattern of findings is contrary to what this paper initially hypothesized (i.e., that participants' responses in the critical trials would be less accurate than their responses in the control trials).

One possible alternative explanation exists for the results for the repetition priming effect of the critical target stimulus. As this stimulus appeared more frequently than any other control target stimulus, participant familiarity with the target object may have influenced their responses. The repetition priming effect could have interfered with the effect from the verbal instructions and distorted the overall results. Therefore, Experiment 2 accounted for the potential

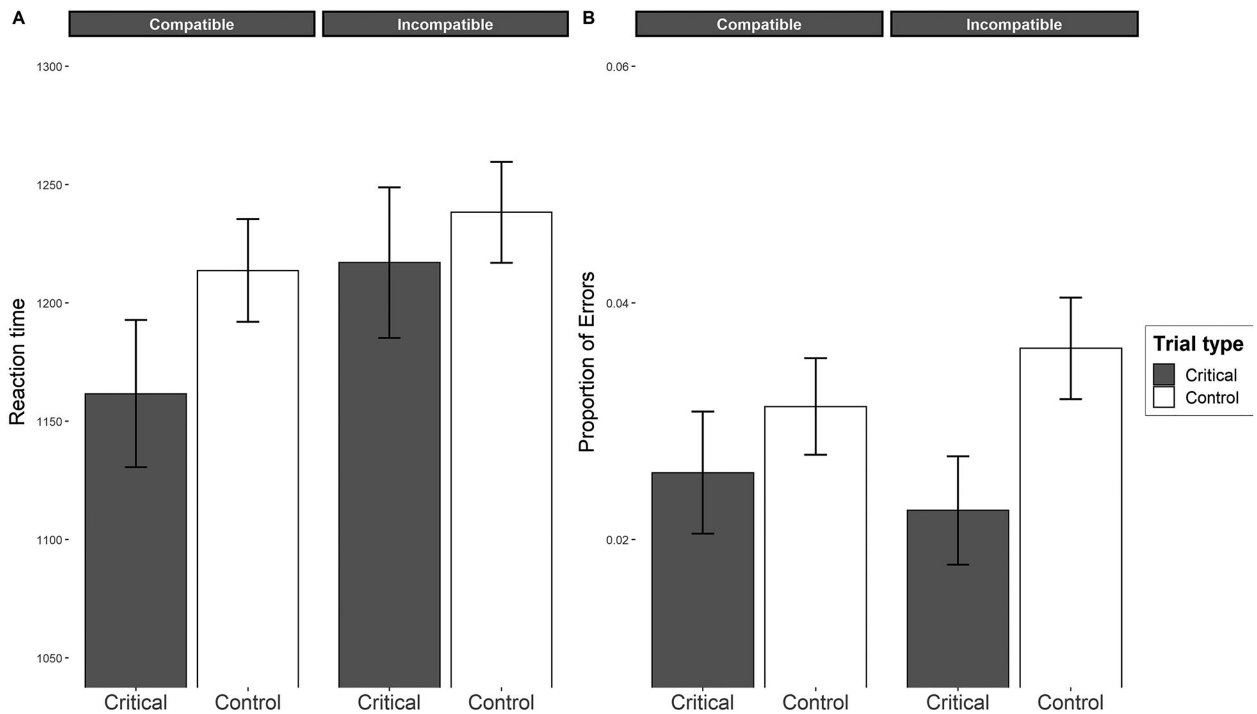

Figure 3. An Illustration of Mixed-Models Analysis Note. The plot illustrates the results from the linear mixed model (A) and generalized linear mixed model (B), with confidence intervals derived from these regression analyses. The mean values on both graphs represent marginally estimated means.

influence of the repetition priming effect by separating the effect of the verbal instructions from the repetition priming effect.

Experiment 2

Despite the significant findings in Experiment 1, the design of this experiment did not account for the potential influence of familiarity on the target object (Hout & Goldinger, 2010) that can appear during the probe phase. The familiarity effect implies that participants' response times and accuracy gradually improve due to multiple repetitions of the target stimuli. This concept shares the core idea of the repetition priming effect (Logan, 1990), which states that multiple repetitions of the same stimuli cause a priming effect.

In many behavioural studies evaluating the repetition priming effect, the priming occurs during a priming phase, and the successive evaluation of that effect occurs during a probe phase (e.g., Eder & Dignath, 2017; Hommel, 2009; Hommel & Hommel, 2004). However, an unequal number of stimuli appearing during the probe phase can cause the same repetition priming effect to emerge passively, thereby diminishing the overall results. Although in Experiment 1 the critical stimulus appeared in a proportion of 3:1 to the control stimuli, the appearance of an unequal proportion of stimuli could have caused the repetition priming effect during the probe phase and distorted the effect of the verbal instructions.

Therefore, Experiment 2 repeated Experiment 1 with an adjustment to account for the possible passive repetition priming effect. The critical stimulus from the verbal priming phase remained the same as in Experiment 1 (i.e., apple). However, it appeared an equal number of times as each of the other control stimuli. Furthermore, one of the control stimuli appeared three times more often than all of the other stimuli (i.e., repetition priming). This allowed me to evaluate specifically whether the repetition priming effect can occur solely during the probe phase.

Methods

Participants

I recruited the same sample size as in Experiment 1. A total of 100 English-speaking participants participated in the study (52 females, 42 males, and five participants who did not specify their gender). After the data cleaning described in the "Data Preparation" subsection, the analyzed sample included 90 participants. The participants' ages ranged from 30 to 45 years ($M = 36.5$, $SD = 4.5$). All participants were recruited through the recruiting portal Prolific and received monetary compensation for their participation. The local Ethics Committee of the Arctic University of Norway approved the study, and all participants provided informed consent prior to the experimental procedures.

Design

The experiment followed a 3 (*prime*: critical vs. control vs. frequency) by 2 (*compatibility*: compatible vs. incompatible) mixed design. Prime was a within-subject factor that specified whether a target stimulus in the visual search task represented a priming stimulus from the verbal instructions sentence (apple; critical trials) or control stimuli (all other fruits and vegetables; control trials), or repetition priming (carrot; frequency trials). Compatibility was a between-subject factor that specified whether the associated response with a priming stimulus from the verbal instructions sentence matched or did not match the instructed response for the visual search task ("apple"-left/right, fruits-left/right).

Prime and Probe Phases. The prime phase was identical to that in Experiment 1, and the probe phase was similar to that in Experiment 1 but with certain adjustments to evaluate the repetition priming effect. First, the number of target stimuli was reduced to eight. As the boxplot analysis showed in Experiment 1, two stimuli from the vegetable category deviated from the other stimuli in terms of reaction times and response errors. Therefore, I excluded these two stimuli from Experiment 2. I also removed two randomly chosen stimuli from the fruit category (pineapple and strawberry). As in Experiment 1, the apple was a critical target stimulus.

Second, I used the programme PsychoPy to choose one random stimulus from the vegetable category to be a frequency stimulus (carrot). The verbal priming stimulus appeared an equal number of times as each of the other control stimuli, and the frequency stimulus appeared at a 3:1 proportion. The visual search task was identical to the one in Experiment 1. The participants' task was to find a target stimulus and identify it as a fruit or vegetable as quickly as possible. Participants did not receive any information about frequency stimulus. They were therefore unaware that this stimulus would appear most often in the task.

Data preparation and data analysis

The second experiment involved the same data preparation and data analysis procedures as those in the Experiment 1. I excluded one participant from the analysis whose ages fell outside sampling criteria (younger than 30 or older than 45). I removed two outliers who made more than 25% incorrect key-presses (not "A" or "L") during the probe phase. Other participants' incorrect responses were also excluded (2.56% data lost). Participants missed the response deadline in only 0.30% of trials. Application of the boxplot method (Tukey, 1977) led to the exclusion of seven participants due to an excessive amount of response errors (more than 10%). Six-point-ninety two percent of trials were excluded to account for intra-trial priming (i.e., the same target stimulus appears two or more times in a row) Response error analysis included data with 8740 observations.

Before the response time analysis, all response errors were excluded from the data (3.05% data lost). Individual response times beyond the mean ± 2.5 *SD*, calculated by participant and within-participant conditions, were also excluded (1.92% data lost). Response time analysis included data with 7722 observations. The final statistical model was identical to that in Experiment 1:

$$Outcome = prime * compatibility + (1\, participants) + (1\, stimulus\_identity)$$

Table 2. Descriptive Statistics of the Response Time Test Phase and Response Errors.

Trial Type	Condition	Reaction Time (ms)		Response Errors (%)	
		M	SD	M	SD
Critical	Compatible	1418	498	3.86	19.2
Control	Compatible	1288	456	2.94	16.9
Frequency	Compatible	1133	379	2.40	15.3
Critical	Incompatible	1396	467	8.35	27.7
Control	Incompatible	1323	458	3.26	17.7
Frequency	Incompatible	1227	416	2.82	16.5

Results and discussion

Reaction time

The effect of reaction time as a function of prime and compatibility varied in intercepts across participants ($SD = 0.09$, $\chi2(1) = 393.3$, $p < .001$), indicating significant by-participant variation around the average intercept. The effect of response times as a function of prime and compatibility varied in intercepts across stimulus identity ($SD = 0.02$, $\chi 2(1) = 26.6$, $p < .001$), indicating significant by-stimulus identity variation around the average intercept.

The two-way interaction between prime and compatibility was significant ($b = 0.04$, 95% CI [0.01, 0.07], $p < .001$), indicating that the difference between critical, control, and frequency trials in the compatible group was different from the same difference in the incompatible group. In the compatible group, participants' responses were significantly slower in the critical trials than in the control trials ($b = 0.09$, 95% CI [−0.15, − 0.03], $p = .014$). Participants' responses were also significantly slower in the critical trials than in the frequency trials ($b = 0.21$, 95% CI [−0.29, − 0.13], $p < .001$). Participants' responses in the control trials were also significantly slower than their responses in the frequency trials ($b = 0.11$, 95% CI [0.06, 0.18], $p = .005$). These results indicated that the response times were fastest when the target stimulus was the most-repeated stimulus in the visual search task (i.e., repetition priming effect). The results also showed that the verbal priming stimulus had an inhibitory influence on response times.

In the incompatible group, the responses in the critical and control trials were not significantly different ($b = 0.05$, 95% CI [−0.12, − 0.01], $p = .123$). In contrast, the response times in the frequency trials were significantly faster than the response times in the critical trials ($b = 0.12$, 95% CI [−0.21, − 0.04], $p = .013$) and the response times in the control trials ($b = -0.07$, 95% CI [0.00, 0.13], $p = .050$). These findings indicated that the participants responded significantly faster to the most-repeated target stimulus than to the verbal priming stimulus or control stimuli.

In addition, the between-subject analysis revealed that participants' response times in the critical trials did not significantly differ between the critical trials in the compatible and incompatible groups ($b = -0.01$, 95% CI [−0.07, 0.04], $p = .689$). Response times in the control trials also did not differ between the compatible and incompatible groups ($b = -0.02$, 95% CI [−0.08, 0.05], $p = .213$). Finally, the analysis revealed a significant difference in the response times in the frequency trials between the compatible and incompatible groups ($b = -0.07$, 95% CI [−0.12, − 0.02], $p = .002$). Figure 4 illustrates this pattern of findings. Appendix A3 presents a table with all of the fixed factor results.

Response errors

The effect of response errors as a function of the prime and compatibility varied in intercepts across participants ($SD = 0.54$, $\chi 2(1) = 18.6$, $p < .001$), indicating significant by-participant variation around the average intercept. The effect of response errors as a function of the prime and compatibility also did not vary in intercepts across stimulus identity ($SD = 0.00$, $\chi 2(1) = 0$, $p = .99$). Therefore, the final model did not include stimulus identity as a random factor.

The overall two-way interaction between prime and compatibility was marginally significant ($b = -0.37$, 95% CI [−0.83, 0.06], $p = .089$), showing that the difference in the critical, control, and frequency trials was significant between the compatible and incompatible groups. In the compatible group, the results showed no significant difference between the critical and control trials ($b = 0.28$, 95% CI [−0.72, 0.33], $p = .258$). There was also no significant difference between the frequency and control trials ($b = 0.21$, 95% CI [−0.19, 0.68], $p = .315$). The participants' accuracy was only marginally significantly different between the critical and frequency trials ($b = 0.49$, 95% CI [−1.12, 0.14], $p = .090$).

In the incompatible group, the participants' accuracy was significantly different between the critical and control trials ($b = 1.06$, 95% CI [−1.54, − 0.60], $p < .001$). The participants' accuracy was also significantly different between the critical and frequency trials ($b = 1.15$, 95% CI [−1.75, − 0.54], $p < .001$). However, the participants' accuracy did not significantly differ between the control and frequency trials ($b = 0.15$, 95% CI [−0.41, 0.59], $p = .532$).

The between-subject comparison showed that participants' responses in the compatible group were significantly more accurate in the critical trials than participants' responses in the incompatible group ($b = 0.84$, 95% CI [0.20, 1.54], $p = .008$). In comparison, participants' accuracy did not significantly differ between the control trials ($b = -0.11$, 95% CI [−0.45, 0.10], $p = .561$) and frequency trials ($b = -0.17$, 95% CI [−0.45, 0.76], $p = .599$). Figure 4 illustrates this pattern of findings. Appendix A4 presents a table with all of the fixed factor results.

Discussion

In terms of reaction time analysis, the results showed that participants responded faster to the frequency stimulus

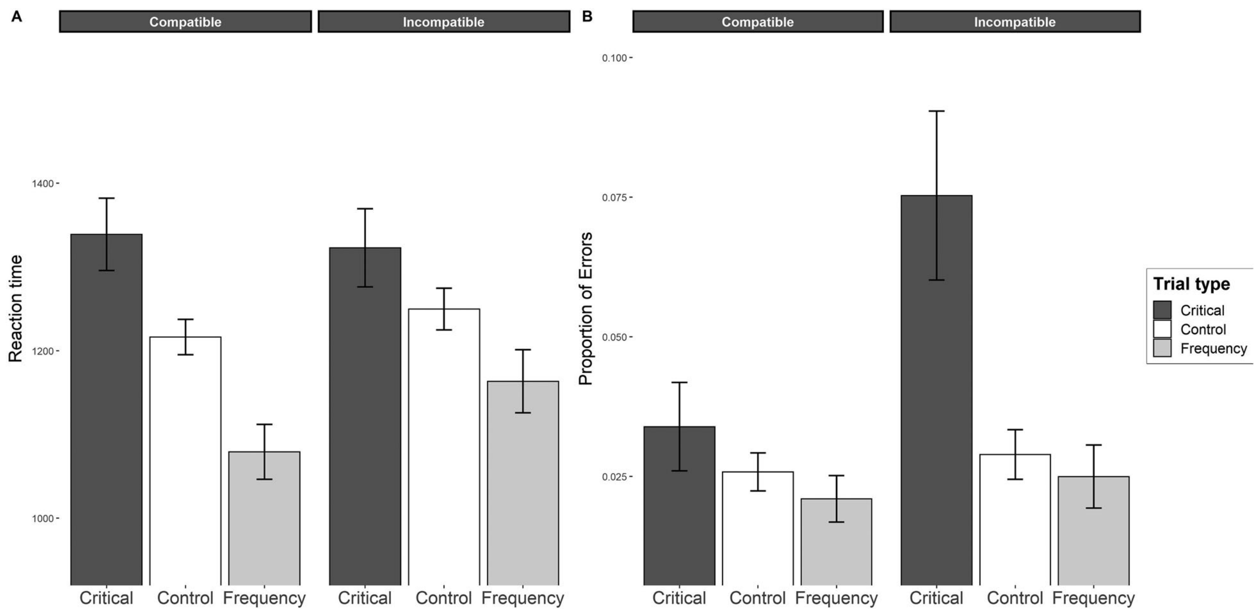

Figure 4. An Illustration of Mixed-Models Analysis Note. The plot illustrates the results from the linear mixed model (A) and generalized linear mixed model (B), with confidence intervals derived from these models. The mean values on both graphs represent marginally estimated means.

than to the critical and control stimuli. These findings indicate that the most-repeated stimulus (carrot) in the probe phase caused the repetition priming effect. Furthermore, the results showed that the participants' responses to the verbal priming stimulus (apple) were slowest in comparison to their responses to the control or frequency stimuli. These results contradict the findings in Experiment 1 and highlight that the results in Experiment 1 were caused by the repetition priming effect that overrode the effect of verbal priming.

In terms of response errors, the results from Experiment 2 indicate that the verbal action-effect sentence formed an association between the priming stimulus and the priming response, leading to improved response accuracy in the critical trials in the compatible group compared to the incompatible group. These results replicate previous findings on verbal instructions and indicate that verbal instructions formulated in an action-oriented manner (i.e., stimulus-response) function as a unified priming mechanism (Muhle - Karbe et al., 2017)

General discussion

The present study investigated whether a verbal priming action-effect sentence acts as a selection bias that influences visual search. The main idea behind the action-effect sentence was to evaluate not only the perceptual aspect of verbal priming but also the behavioural aspect through verbal effect-response associations. Specifically, a priming sentence formulated in an effect-response manner should not only prime perceptual areas, making them more sensitive to a specific stimulus; they should also prime a particular response selection associated with that stimulus, thereby acting as a unified priming mechanism.

Overall, the results show that the verbal action-effect sentence influenced visual search performance. In terms of reaction time, the verbal instructions decelerated participants' responses. Participants' responses were also slowed down in the compatible condition when the target stimulus and response direction required in the visual search task matched the stimulus and response direction specified in the priming action-effect sentence. In terms of response errors, the priming action-effect sentence influenced participants' accuracy as a unified (i.e., stimulus-response) priming mechanism, as the results in Experiment 2 showed. Their accuracy was also significantly improved in the compatible condition compared to the incompatible condition. That supports the previous findings of (Damanskyy et al., 2022; Theeuwes et al., 2015), which showed that a verbal priming action-effect sentence forms an association between verbally specified stimulus and response.

The repetition priming effect of the most frequent stimulus

In addition to the verbal priming effect, the present studies evaluated the repetition priming effect that could appear in the probe phase due to one of the target stimuli appearing most frequently. Repetition priming implies that when participants encounter a specific target object more often, their performance gradually improves in relation to that object throughout the visual search trials because the most-repeated object primes the participants' search template. Within the visual search paradigm, this effect has often been evaluated in studies on familiarity with the target object (Hout & Goldinger, 2010).

The findings from Experiment 2 demonstrated that repetition priming occurred when one of the control stimuli appeared at a 3:1 proportion with the other target stimuli. These results indicated that the repetition priming effect can occur passively during the probe phase, facilitating response times. Furthermore, the results of Experiment 2 showed that the effect found in Experiment 1 was caused by the repetition priming effect rather than by the verbal priming sentence. As participants' responses were facilitated in Experiment 1 and not inhibited as in Experiment 2, this facilitation indicates that the effect of verbal instructions (the inhibition of response times) was modified by the repetition priming effect (the facilitation of responses). This supports the previous arguments of (Huang et al., 2013) that proved that learning based on verbal instructions is more flexible to adaptation and changes than learning based on the active repetition of the same behaviour.

The results in Experiment 2 show that the frequency stimulus showed significant interaction with compatibility. When participants' responses to the most-frequent stimulus matched with the response specified in the verbal priming action-effect sentence, participants' response times were facilitated. However, this pattern of compatibility was not observed in the control condition in the two experiments. These findings potentially indicate that the part of a verbal priming sentence that specifies the response direction (i.e., "left" or "right") can have an independent priming effect, regardless of whether this part of the verbal priming sentence is syntactically connected to a specific stimulus. Consequentially, when responses to the most-frequent stimulus are habituated and become automatic, they might be more inducive of an additional priming effect. However, these findings require replication to validate this point.

The inhibition of visual searches

Previous studies within the visual search paradigm (Schmidt & Zelinsky, 2009; Wolfe et al., 2004) and the implementation intention paradigm (Wieber & Sassenberg, 2006) argued that verbal information affects visual selective attention. This effect has also been observed within the facilitation paradigm (i.e., the facilitation of visual searches; Knapp & Abrams, 2012; Schmidt & Zelinsky, 2009). However, my findings demonstrated that when verbal instructions are formulated in an action-oriented manner (i.e., action-effect) and are irrelevant to the visual search task, the effect of these instructions can slow down the visual search performance. Considering previous studies on verbal instructions (Hartstra et al., 2012; Muhle - Karbe et al., 2017; Van 't Wout et al., 2013), I suggest two possible explanations for this inhibitory effect.

First, this inhibitory effect was related to variability in the search templates that participants formed. When participants comprehended the verbal action-effect sentence, they retrieved the representation of a critical stimulus (apple) from a long-term memory that influenced their search template. Given that each participant could have had different representations of the critical stimulus, high variability in the search templates could have arisen among participants.

Consequently, when participants encountered a critical stimulus as a target object, their search template could have had a different representation of the target object, causing conflict between the search templates and inhibiting their responses. However, this idea does not explain the instruction-compatibility effect within the response errors. If participants formed different object representations of the critical stimulus, then the associated response should only have been associated with those specific object representations and not lead to better accuracy in the compatible condition than in the incompatible condition. However, as the response error analysis showed, participants' accuracy was significantly better in the compatible condition than in the incompatible one, highlighting the presence of the instruction-compatibility effect.

Second, the deceleration of response times was related to the relevance of the verbal priming sentence. the studies of (Knapp & Abrams, 2012; Schmidt & Zelinsky, 2009) participants were aware that the verbal cues they received were relevant to the upcoming visual search task. In contrast, in my experiments, I explained to participants that the verbal action-effect sentence was irrelevant to the visual search task, but they needed to remember it nonetheless, as they would have to apply it after the visual search task. Therefore, when they encountered the verbal priming stimulus as a target stimulus that could potentially cause a spontaneous memory retrieval of the primed behavioural intention coded in the verbal sentence, their responses were inhibited.

Research on implementation intention (for review, see Chen et al., 2015) has suggested that when participants form a verbal plan with the intention to execute it in the future, the execution of that plan will occur through a prospective memory mechanism as spontaneous memory retrieval. For example, Rummel et al. (2012) argued that when participants encountered stimuli that were specified in a previously learned verbal action plan, they spontaneously retrieved the previously learned action intention (i.e., prospective memory; McDaniel et al., 2008), which may have interfered with the current ongoing task. When participants encountered the verbally specified stimulus (i.e., apple) in Experiment 2, their responses could have been decelerated, not necessarily by the variability in the search templates but by the spontaneous memory retrieval of the action-effect sentence from the priming phase. This could have interfered with the ongoing task and caused delays in their response times.

Verbal instructions as selection bias

The results of Experiment 2 demonstrated how the repetition priming effect biases selective attention. When the same stimulus is encountered more often, it biases selective attention unintentionally and automatically, making participants' search templates being more sensitive to a specific stimulus and facilitating top-down attentional modulation (Grill-Spector et al., 2006). The results of Experiment 2 also demonstrate that the priming action-effect sentence inhibited visual search performance. This inhibition can also be considered an unintentional effect that worked in opposition to top-down attentional modulation.

Interpreting the present findings according to Awh et al.'s (2012) framework, I suggest that verbal instructions fall under the category of selection bias. Notably, however, verbal instructions can affect cognition in different ways (Braem et al., 2017). As the present study demonstrates, along with the findings of (Knapp & Abrams, 2012; Schmidt & Zelinsky, 2009), the effect of verbal primes is dependent on their relevance to the task in which they are evaluated. In addition, the underlying mechanisms of verbal instructions differ from the priming mechanisms of active behaviour in terms of flexibility to changes (Huang et al., 2013) and the practice effect (Pfeuffer et al., 2018). Therefore, exactly how verbal instructions influence cognitive control is debatable (Blache, 2017) because this question relates to how language influences cognition on a neural level (see Perlovsky & Sakai, 2014; and Poeppel, 2012, for an extended discussion).

Limitations and Future Directions

In the present study, the priming stimulus specified in the verbal instructions remained constant throughout the probe trials. Therefore, my findings do not answer the question of whether participants encoded specific features of the priming stimulus (apple) or an entire specific representation thereof that was potentially retrieved from long-term memory. Treisman and Gelade (1980) argued that the priority map of attentional focus does not necessarily encode an entire representation of a specific stimulus but only specific features thereof – that is, *feature-based attention* (for a review see Carrasco, 2011). Moreover, using real objects as target stimuli, Yang and Zelinsky (2009) demonstrated that visual searches are based on a category-defined principle. Future studies can therefore provide additional insight into this topic by manipulating specific features of a priming stimulus (e.g., colour, size, shape) that can explain how a priority map precisely encodes a priming stimulus that is only presented verbally.

Although Experiment 1, and 2 demonstrated that the modulation of visual selective attention was achieved in a spatially independent manner, these results do not exclude the possibility that spatial attention (Carrasco, 2011) does not affect visual search. To account for the possible effect of spatial attention, I randomly varied the location of the stimuli in each trial. Nevertheless, spatial sensitivity might be more relevant when a specific location is involved in purposive behaviour (Moore & Zirnsak, 2017). Therefore, the precise answer to whether selection bias can also overcome the spatial dimension requires an experimental procedure in which the spatial dimension is systematically manipulated.

Conclusion

In this study, my findings showed that verbal instructions modulated visual selective attention. Although the verbal instructions were irrelevant to the visual search task, they unintentionally affected visual search performance. I interpret these findings as evidence that verbal instructions extend the list of selection biases. While selection biases based on history-based selection have been studied primarily in behavioural research, biases based on active behaviour cannot explain all the flexibility that humans have within their cognitive control. In the present findings, I highlight the importance of language in cognitive control and show how verbal instructions interact with cognitive control.

Notes

1. The simulation power analysis was based on the following statistical model: Outcome = prime * compatibility + (1|participants). This model did not include stimulus identity as a random factor. In contrast, the main models in the present study include stimulus identity as a random factor.
2. For a post-hoc exploratory analysis on a subgroup of participants who answered correctly in the memory task, see the supplementary materials available at: https://osf.io/4w6k9/?view_only=b8b2208fd93b4d2299eeaefc60238929

Disclosure statement

No potential conflict of interest was reported by the author(s).

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Ethical approval

This study was performed in line with the principles of the American Psychological Association. The Ethics Committee of the Arctic University of Norway granted ethical approval for this study (March 2020/ No: 2017/1912).

Consent to participate

Informed consent was obtained from all individual participants included in the study prior to participation.

Availability of data and material

The datasets generated during the current study are available via the Open Science Framework (OSF): https://osf.io/4w6k9/?view_only = b8b2208fd93b4d2299eeaefc60238929.

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Appendices

Appendix A

Table A1. The Results from the Reaction Time Analysis of the Fixed Effects in Experiment 1.

	Condition	b	SE	*p*
Within-subject	Compatibility = Compatible Critical/Control	−0.04	0.02	.115
	Compatibility = Incompatible Critical/Control	−0.01	0.02	.495
Between-subject	Trials = Critical Compatible/Incompatible	−0.04	0.02	.073
	Trials = Control Compatible/Incompatible	−0.02	0.02	.386

Table A2. Results from the Response Error Analysis of the Fixed Effects in Experiment *1*.

	Condition	b	SE	p
Within-subject	Compatibility = Compatible Critical − Control	−0.20	0.22	.368
	Compatibility = Incompatible Critical − Control	−0.49	0.22	.027
Between-subject	Trials = Critical Compatible − Incompatible	−0.13	0.29	.638
	Trials = Control Compatible − Incompatible	−0.15	0.17	.389

Table A3. Results from the Reaction Time Analysis of the Fixed Effects in Experiment 2.

	Condition	b	SE	p
Within-subject	Compatibility = Compatible			
	Critical − Control	0.09	0.03	.014
	Critical − Frequency	0.21	0.04	.000
	Frequency − Control	0.11	0.03	.005
	Compatibility = Incompatible			
	Critical/Control	0.05	0.03	.123
	Critical − Frequency	0.12	0.04	.013
	Frequency − Control	0.07	0.03	.050
Between-subject	Trials = Critical			
	Compatible/Incompatible	−0.01	0.03	.689
	Trials = Control			
	Compatible/Incompatible	−0.02	0.02	.213
	Trials = Frequency			
	Compatible/Incompatible	−0.07	0.02	.002

Table A4. Results from the Response Error Analysis of the Fixed Effects in Experiment *2*.

	Condition	b	SE	p
Within-subject	Compatibility = Compatible			
	Critical − Control	0.32	0.25	.195
	Critical − Frequency	0.49	0.29	.089
	Frequency − Control	0.16	0.21	.449
	Compatibility = Incompatible			
	Critical/Control	1.08	0.23	.000
	Critical − Frequency	1.15	0.28	.000
	Frequency − Control	0.06	0.25	.782
Between-subject	Trials = Critical			
	Compatible/Incompatible	−0.83	0.31	.008
	Trials = Control			

(*Continued*)

Table A4. Continued.

Condition	b	SE	p
Compatible/Incompatible	−0.08	0.21	.703
Trials = Frequency			
Compatible/Incompatible	−0.17	0.30	.557

Appendix B

Slide 1

INFORMED CONSENT

Welcome to this study on concentration. We will ask you to perform a simple attention speed categorization task. You will be presented with a series of different figures; your task will be to find a specific figure among others and to press either the left (A) or right (L) key on your keyboard. More detailed instructions follow the informed consent information below.

VOLUNTARY PARTICIPATION

Your participation in this study is voluntary. You may leave the study at any time without needing to provide a reason. If you choose to leave before completing the study, your data will not be stored.

CONFIDENTIALITY

The recorded data will only be used for scientific purposes. Participation in this study is anonymous. The only personal information collected from you will be your age and gender. No additional participant identifiers will be recorded, and none of the information collected can be used to identify participants. As the data will be stored anonymously, individual data cannot be made available on request.

CONTACT INFORMATION

If you have questions about this study, you may contact the lead researcher Yevhen Damanskyy at yda002@uit.no.

Press the spacebar to continue (or close your browser if you do not want to participate now).

By pressing the spacebar, I acknowledge that I have read and understood these terms and conditions and agree to participate in the study.

Press the spacebar to continue (or close your browser if you do not want to participate now).

Slide 2

This study can only be completed using a traditional desktop PC or laptop with a physical keyboard. If you are reading this on a tablet or smartphone, you will need to switch to a personal computer and restart the study.

IMPORTANT! If you are colourblind, unfortunately you will not be able to complete the tasks correctly. In this case, we kindly ask you not to participate in this study and just close your browser page.

To prepare for the task, please place your chair in a comfortable position so that you can easily reach the keyboard with both hands. You will be using the A and L keys. Please place your left and right index fingers on the respective left (A) and right (L) keys and press each key a few times to get a sense of how they work.

For further instructions, press the spacebar.

Slide 3

At the end of the experiment, you will be presented with a single task with the following instructions.

Please read and memorize the following instructions:

TO MAKE AN APPLE APPEAR ON THE SCREEN,

I NEED TO PRESS THE LEFT (A) KEY.

It is important that you memorize the capitalized sentence above; please repeat the sentence in your head a few times to make sure that you have memorized it.

Press the spacebar to continue.

Slide 4

Please repeat the instruction sentence from the previous screen in your head a few times.

Please also remember that “left” refers to the “A” key, and “right” refers to the “L” key.

Press the spacebar when you are done.

Slide 5

Now check that you have memorized the instruction sentence correctly:

TO MAKE AN APPLE APPEAR ON THE SCREEN,

I NEED TO PRESS THE LEFT (A) KEY.

Press the spacebar for more information.

Slide 6

Before performing the memory task, you will be asked to perform a visual search task. You will be presented with a grid containing 24 different figures. (See the picture below for an example.) The locations of these figures will be different each time. Among the 24 figures, there will be either one fruit figure or one vegetable figure. Your task is to find either the animal or flower (as fast as you can) and respond as follows:

Vegetables – press the left key on your keyboard (A key)

Fruit – press the right key on your keyboard (L key)

Press the spacebar for more instructions.

Slide 7

The illustration below includes all of the possible types of fruit and vegetables that might be shown to you. Remember, only either one fruit or one vegetable from these lists will appear (never both). The locations of all figures will be different each time.

As a reminder:

If you find a vegetable, press the left key (A key).

If you find a fruit, press the right key (L key).

Press the spacebar for more instructions.

Slide 8

Next, you will complete 10 practice exercises to familiarize yourself with the procedure.

Please put your index fingers on the left (A) and right (L) keys on your keyboard.

Please make sure that you will not be interrupted for the next 10 min.

Please concentrate, then press the spacebar to begin.

Slide 9

Thank you. You have now completed the practice exercises. The main task will begin after these instructions.

Please note that you will now only have three seconds to respond to each figure. In general, we will ask you to respond as quickly and accurately as possible. If you make an error or respond too slowly, an error message will appear. Take this error message as a sign that you need to concentrate even harder to respond as quickly and accurately as possible.

This part of the study will take around 5–7 min.

When you are ready, please find a comfortable position at your computer and place your index fingers on the A and L keys.

Please concentrate, then press the spacebar to begin.

Slide 10

Thank you. You have completed the main categorization task. On the next slide, we will ask you to perform an action that was specified in the sentence at the beginning of the experiment.

Slide 11

Please press the corresponding key to make an apple appear on the screen.

Slide 12

(Feedback to the participant)

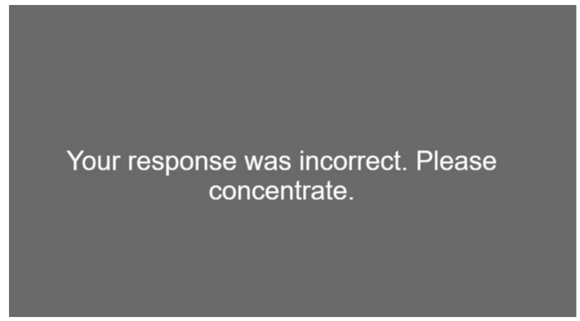
Press the spacebar to continue.

Final slide

You have now completed all tasks.

Feedback messages

If participants did not categorize the target object correctly.



If participants responded too slow.