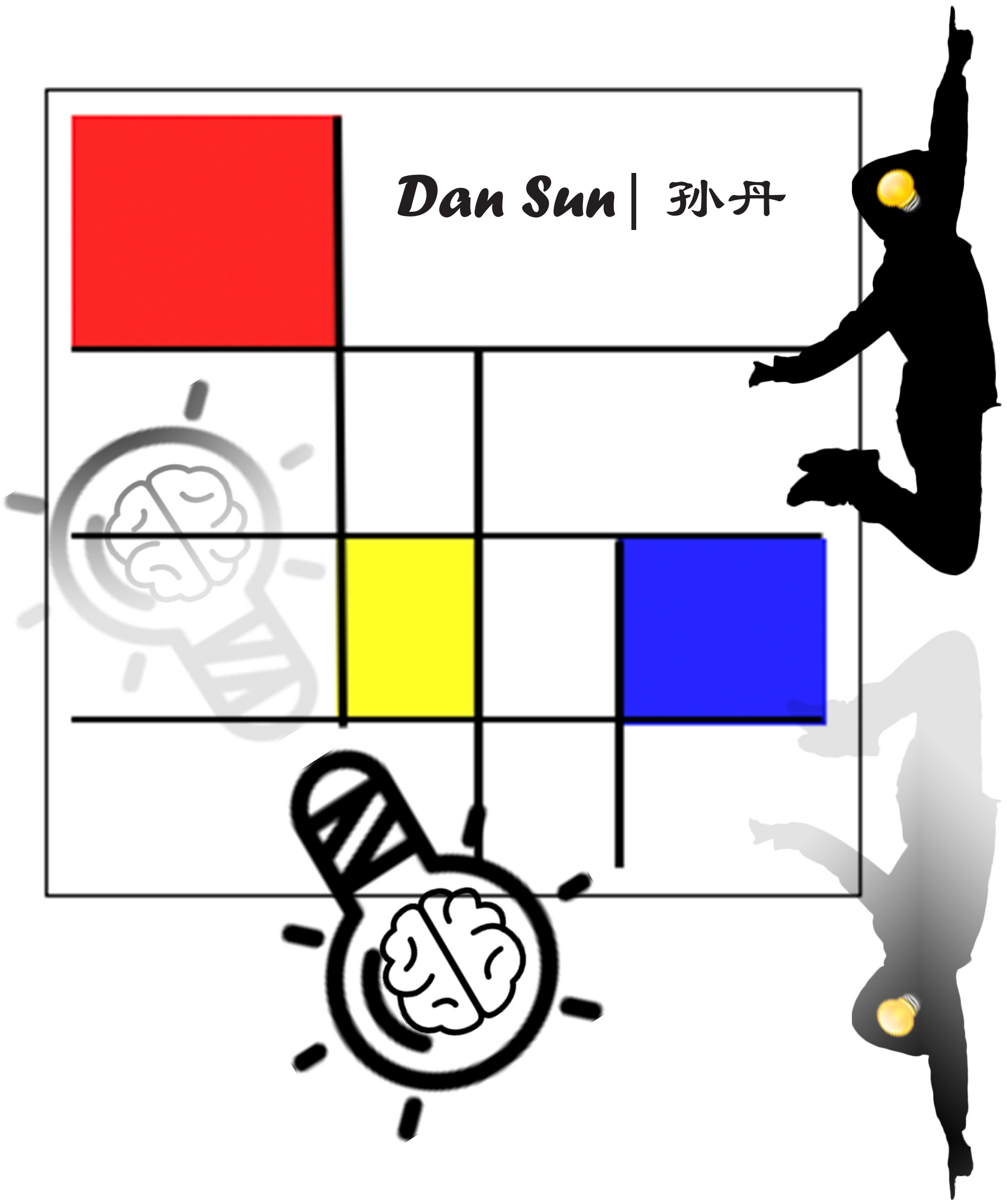
kurtle
winins
tituut

How People Learn to Act on Goals: A New Examination of the Mechanistic Ideomotor Action Account

Dan Sun

How People Learn to Act on Goals:
A New Examination of the Mechanistic Ideomotor Action Account



Dan Sun | 孙丹

How People Learn to Act on Goals: A New Examination of the Mechanistic Ideomotor Action Account

Dan Sun

The research described in this thesis was financially supported by PhD scholarship funding from the Chinese Scholarship Council.

ISBN: 978-90-393-7482-5
KLI Series Number: 2022-11
Cover design: Dan Sun, Chujun Zhang
Original image: https://www.iconninja.com/, self-portrait
Lay-out: Dan Sun
Printing: www.proefschriftmaken.nl
DOI:https://doi.org/10.33540/1274

How People Learn to Act on Goals: A New Examination of the Mechanistic Ideomotor Action Account

Hoe mensen leren om te handelen naar doelen: Een nieuw onderzoek naar de mechanistische ideomotorische actie-account

(met een samenvatting in het Nederlands)

Proefschrift

ter verkrijging van de graad van doctor aan de
Universiteit Utrecht
op gezag van de
rector magnificus, prof.dr. H.R.B.M. Kummeling,
ingevolge het besluit van het college voor promoties
in het openbaar te verdedigen op

dinsdag 21 juni 2022 des ochtends te 10.15 uur

door

Dan Sun

geboren op 28 januari 1991
te Jiangsu, China

Promotor:
Prof. dr. H. Aarts

Copromotor:
Dr. R. Custers

Contents

Chapter 1: Introduction and Overview 7

Chapter 2: Examining the Support for Ideomotor Theory from the Two-Stage Paradigm 15

Chapter 3: Ideomotor Action: Evidence for Automaticity in Learning, but not Execution 33

Chapter 4: Automaticity in Ideomotor Action? Moderators in the Two-Stage Paradigm 65

Chapter 5: Examining Mechanistic Explanations for Ideomotor Effects 91

Appendices 113

Summary(Nederlandse Samenvatting) 127

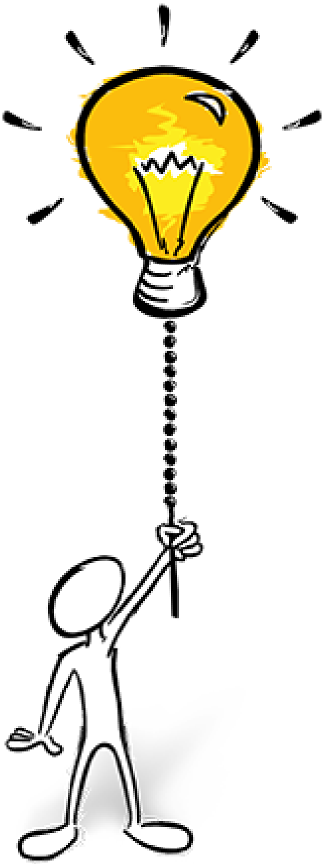
References 133

Curriculum Vitae 151

KLI Distertation Series 155

CHAPTER

1



Chapter 1:
Introduction and Overview

“*Sow a thought, and you reap an action; sow an act, and you reap a habit; sow a habit, and you reap a character; sow a character, and you reap a destiny.*" [1]

Human behavior consists of more than reflexes. People do not merely respond to their environment. Most of the time, they behave in a purposeful manner, with a particular goal in mind. They stretch their arm to pick up a glass, buy groceries to make dinner later tonight, or work hard to obtain a Ph.D. These examples have in common that behavior is directed at producing a particular state or event that can often not be directly perceived, but at that moment of action only exists in people’s minds. Goal-directed behavior, then, seems to depend mainly on people’s cognitive abilities, which allow them to act on future events they envision in the mind’s eye.

Apart from the ability to conjure up visions of events that are not present in the here and now (Gilbert & Wilson, 2007), there is another ingredient that is a prerequisite for goal-directed behavior: To realize a particular goal, one must be able to select the proper actions that produce the goal-state. People relax and flex specific muscles to make their arm grab a glass, take a particular route to the nearby grocery shop, buy particular ingredients based on the dinner recipe tonight, and write all the manuscripts based on the research question to finish one’s Ph.D. Thus, to engage in goal-pursuit, people must somehow be able to select the actions that produce the envisioned goal state.

The current dissertation aims to investigate how people formulate the cognitive structures that enable them to behave in such a goal-directed way, and how outcomes are produced based on such cognitive structure. Specifically, it examines the support for the ideomotor (IM) theory, which provides an elegant mechanistic account for how these cognitive structures arise and produce behavior. In the following, the thesis first explains how this mechanistic account followed from theorizing on goal-directed behavior. Then, it critically examines the existing evidence supporting this mechanistic account and points out several shortcomings. Subsequently, three empirical chapters are presented to explore how people acquire cognitive representations about the relations between actions and their outcomes and how those representations affect behavior later.

Goal-directed behavior

Theories on goal-directed behavior see goal pursuit as a mostly deliberate affair. Most notably, according to Gollwitzer’s action phases model (Gollwitzer, 1990), this endeavor starts out as a decision-making process, where one has to decide which goals to select for pursuit based on (amongst others) their desirability. Once a goal is set, one has to select the proper means to implement goal-directed action based on which actions are effective and efficient in producing the goal-state. Therefore, the implementation of goal-directed behavior can be seen as a multiple-layer decision-making process. People select goals and means based on expectancy (e.g., will a means produce the goal state) and value (how desirable is the goal).

Clearly, this process relies on cognitive knowledge about goals and actions. First of all, people need to *mentally represent* a goal state of being able to select it. Second, it seems that choosing the proper means requires knowledge about the causal relations between actions and their outcomes. That is, they have to know which actions will cause the goal state to materialize. While such deliberation and planning may characterize the pursuit of some goals, such as deciding on a route to take to a new destination, or planning for a dinner party, it does not seem to capture the fluency of goal-pursuit in everyday behavior, where we often seem to act virtually without deliberation in a fairly automatic manner.

In an attempt to explain such seemingly automatic behavior, researchers have proposed that goal-directed behavior does not have to be constructed by deliberative processes all the time. For well-rehearsed goal-directed behaviors, goals, and the actions that produce them could simply be retrieved from memory (Bargh, 2006; Custers & Aarts, 2005, Kruglanski et al., 2002). These cognitive structures would, on the one hand, consist of a mental representation of the goal state that would contain information about the goal state itself, but also about its desirability (Custers & Aarts, 2005). This goal representation would also be connected to one or more means in a hierarchical structure, in which the means most strongly associated with the goal would be selected and retrieved when a goal state would come to mind (Kruglanski et al., 2002). Together, this would explain how goals that were brought to mind by stimuli in the environment (primes), could evoke goal-pursuit: With the goal being selected by the environment, the associated means could be readily activated, leading goal-directed behavior to run its course (Bargh, 2005, 2006).

Ideomotor Theory

This notion that thinking of actions and their outcomes can produce behavior can be traced back to the German philosophers (Herbart,1825; Lotze, 1852; Harless, 1861) and British psychologists (Laycock,1845; Carpenter, 1852; see for a historical overview, Stock & Stock, 2004). However, it was William James (2007) who firmly established this theory in his *Principles of Psychology*, saying, “Whenever movement follows unhesitatingly and immediately the notion of it in mind, we have ideo-motor action” (p. 522). According to James, ideas evoking action was the default, although this process could be disrupted by flashed ideas that might inhibit the intended behaviors.

It was Greenwald (1970) who revived the notion of ideomotor action, linking it firmly to goal-directed behavior, by pointing out that thinking of an action is not possible without thinking of the outcomes it produces. In a paradigm with two stages, he demonstrated that after learning that actions produce certain outcomes in an initial acquisition phase, stimuli related to these outcomes automatically influence behavior in a subsequent test phase.

While this approach quickly gave way to the more popular deliberative theories on goal-directed behavior, this approach quickly gained traction in the late 90’s of the 20th century when more mechanistic models of goal-directed behavior became more popular. The work eventually culminated in the theory of event coding (Prinz, 1997; Hommel et al., 2001; Hommel, 2019). According to that, learning and execution of goal-directed behavior are not in terms of causal knowledge and deliberate decisions, but rather a mechanistic process that could explain goal-directed behavior based on merely the formation of associations. In short, it assumed that cooccurrence of actions and resulting events could lead to the formation of bidirectional associations, by which activation of the representation of the event or outcome could readily activate the representation of the action that produced it, without any deliberation, reflection, or decision.

Although there are similarities with the notion of automatized goal-directed behavior (Bargh, 2006; Custers & Aarts, 2005; Kruglanski et al., 2002), this approach not only removed deliberate conscious processes from the execution of goal-directed behavior, it also removed the need for complex cognitive processes such as inferences of causal relations from the learning process. This account, therefore, became highly popular as an explanation for goal-directed behavior in agents that seemed to lack these capacities.

A great example is a notion that the development of goal-directed behavior in infants starts with exploratory/random movements (“motor babbling”) (e.g., Meltzoff & Moore, 1997; Demiris & Dearden 2005). It is assumed that this behavior starts as random activation of motor commands (activation patterns in the brain that lead to overt behaviors of limbs) that produce certain behavioral outcomes (e.g., an arm shooting up, a leg kicking). After enough repetition, the specific motor programs would become associated with their outcomes, which could lead to activation of motor programs in the opposite direction: Thinking of the behavioral outcomes could then lead to the activation of the relevant motor programs, rendering goal-directed behavior possible just based on associations that developed over times. Indeed, spontaneous bidirectional associations between actions and outcomes have been demonstrated in in 9-month-old infants (Verschoor et al., 2010).

Such a link between the representations of perceived outcomes and motor programs has become a common explanation for motor mimicry (Iacoboni, 2009). It has even been suggested to provide an explanation of the origin of mirror neurons: single neurons fire both when a specific action is executed and when the result of this action is perceived. It was suggested that such neurons are not innate, but rather the result of this rudimentary learning process that would create representations of goal-directed actions. Motor programs and outcomes share a common code during such learning stage, which could be measured even at the single neuron level (Heyes, 2010).

The most likely candidate for such a rudimentary process would be Hebbian learning. Based on Donald Hebb’s (2002) notion: “neurons that fire together, wire together.”, mere coactivation of representations would be enough to support this form of learning. From this perspective, the only features necessary for goal-directed action would be the ability form bidirectional associations between actions (or motor programs) and the outcomes (or sensory effects) they produce, simply based on the fact that the two always occur in temporal contiguity.

In sum, then, the ideomotor theory provides a mechanistic account for how people and other organisms can acquire the (rudimentary) capability to act in a goal-directed manner. This account does not assume the acquisition of rather sophisticated causal knowledge, nor a decision-maker who deliberates on selecting goals and means. Goal-directed action is simply believed to result from the ability to form bidirectional associations between actions and outcomes, which capture a history of cooccurrences of acting and perceiving the resulting events. Similarly, the initiation of goal-directed behavior would not require any deliberation or even intentions. As the case of mimicry suggests, behaviors could be activated by merely perceiving their associated outcomes or events.

Outline of the Dissertation

The current dissertation critically examines the evidence for this mechanistic account for goal-directed behavior. The addressed questions are basically two-fold: First, is ideomotor learning indeed the result of bidirectional associations formed as a result of repeated cooccurrence of actions and outcomes? And second, are ideomotor effects on action indeed the result of mere activation of the outcome representation.

Chapter 2 examines the evidence for ideomotor action obtained in the literature featuring the two-stage paradigm inspired by Greenwald's work (1970). The argument starts that while the learning phase in this paradigm was designed with the mechanistic account and Hebbian learning in mind (i.e., hundreds of trials in which usually two finger presses each produce a specific sensory outcome), this learning phase does not rule out other forms of learning. That is, every box is ticked to facilitate the formation of bidirectional associations through mere repetition. However, people can also easily make inferences about the causal structure of the task (e.g., pressing the left finger causes a high-pitched tone), based on which the hypotheses or propositions about the relations between actions and outcomes can be formed. Likewise, although the mechanistic account would indeed predict that stimuli related to the outcomes featured in the learning phase would bias behavior in the direction of the associated action, other explanations for these effects can be offered. Putting together, these arguments provide support for the assumption that the actual evidence for the mechanistic account of ideomotor actions is relatively thin, to say the least, and that more work is needed to put this account to the test.

Chapter 3 investigates in line with this reasoning to which extent learning in the two-stage paradigm is spontaneous (Sun et al., 2020). Even though learning should be the result of mere cooccurrence of actions and outcomes, instructions almost always emphasize the causal nature of the relation between actions and the events that follow them. Furthermore, two different forms of testing for ideomotor effects are used: the Free Choice Task, in which people can freely choose which action to perform, and the Forced Choice Task, in which people have to react to an imperative stimulus with an instructed response.

While both tasks are used frequently in the literature, only Experiment 1 featuring the Free Choice Task produced the ideomotor effect. That is, when stimuli that previously served as outcomes in the acquisition phase were presented together with the imperative stimuli in the Free Choice Task, it could bias action towards the corresponding responses. This effect occurred regardless of whether the instructions emphasized the causal nature of the learning phase or not, demonstrating that learning was indeed spontaneous. However, results also proved that people could easily report the causal structure of the learning phase, suggesting that people could very likely have relied on this explicit knowledge while choosing what to do in the Free Choice Task. However, Experiments 2, 3a and its high-powered replication Experiment 3b used a Forced choice task that didn't allow for the choice of explicit knowledge, did not produce an ideomotor effect. Together, these findings suggest that while action-outcome relations can be learned spontaneously, ideomotor effects only occur under conditions in the test phase that allow for the application of explicit causal knowledge.

Chapter 4 further addresses this issue by exploring potential moderators of the mechanistic ideomotor effect in the two-stage paradigm (Sun et al., under revision). While we failed to observe such an effect in the forced-choice paradigm (Sun et al., 2020), which is the best test for the mechanistic account, three conditions are investigated that may be prerequisites for the ideomotor effect to occur. Experiment 1 addresses whether reward in the learning phase may facilitate the formation of associations. Experiment 2 tests whether presenting the imperative cues in the same modality helps bring out the ideomotor effect. And finally, Experiment 3 looks at whether – unlike the standard two stages paradigm – presenting the outcomes as a result of responses in the test phase makes a difference.

Results show that, out of these three plausible candidates, only the addition of effects in the test phase gave rise to an ideomotor effect using a forced-choice test phase. That is, when a required response was preceded by the perception of its previous outcome, responses were faster than when they were preceded by the opposite outcome. Although this could be seen as support for the mechanistic account, it also poses a problem: If outcomes are presented in the test phase, so that testing doesn't occur under extinction, how can one be sure that learning has occurred during the learning phase and not during the test phase? It could even be the case that the effect occurs because people make inferences about the causal relations in the test phase, rather than that they rely on associations formed in the learning phase.

Chapter 5 presents a novel paradigm in which it is explicitly tested whether ideomotor effects can arise purely in test phase (Sun et al., 2022). In contrast with the two-stage paradigm, participants are exposed to only short testing bocks featuring a forced-choice task in which the outcomes of the responses are presented. In line with earlier findings (Wolfensteller & Ruge, 2011), it was found in Experiment 1 that ideomotor effects appear almost instantly. When the learned mapping changes from one block to the next, there are no spill-over effects learning from one block to the next. This finding was replicated in Experiment 2, and additionally, it was demonstrated that the ideomotor effect did not change in strength when the contingency between actions and outcomes moved from 100% to 80%. Together, these findings indicate that ideomotor effects can arise in a task that doesn't allow for deliberation. These effects are most likely the result of causal inference made during the task, rather than bidirectional associations formed in memory.

Reading Guide

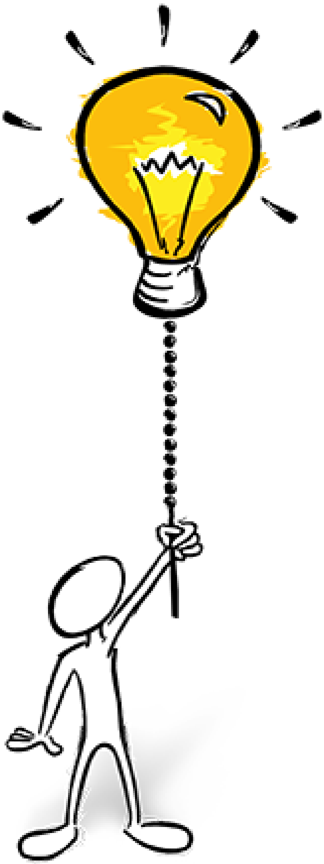
The preceding mentioned research outlines are discussed in detail in the remaining chapters of this dissertation. It should be noted in advance, that all chapters were written in such a way that they can be read independently. As noted earlier, Chapter 2 provides a broader overview of the literature on ideomotor action and its shortcomings, setting the stage for the three empirical chapters. One may choose to read specific chapters, or read them in a different order. As a consequence, however, some overlap exists between parts of the chapters.

Footnote

1: According to the Wikiquote (https://en.wikiquote.org/wiki/Charles_Reade), the original source has never been isolated. Its structure strongly reflects that of a "classical Chinese" set of aphorism, but it (as an anoumous poem) has also been cited by Samuel Smiles in Life and Labor (1887), or attributed to philosophers like Ralph Waldo Emerson (1803-1882), William James (1842-1910)

CHAPTER

2



Chapter 2:

Examining the Support for Ideomotor Theory from the Two-Stage Paradigm

This chapter is based on: Sun, D., Custers, R, & Aarts, H. (2022). Examining the support for ideomotor theory from the two-stage paradigm. *Manuscript in preparation.*

Credit author statement:
DS: Conceptualization, Writing-Original-Draft, Writing-Review & Editing, Visualization, Project administration. **RC:** Conceptualization, Writing-Review & Editing, Supervision, Project administration. **HA:** Writing-Review & Editing, Supervision.

Abstract

A classic question in psychology is how people learn to act in a goal-directed manner. Ideomotor theory takes a mechanistic approach, assuming that after repetition, actions and outcomes are linked by bi-directional associations, so that the thought or perception of the outcome can activate the action that causes it. We argue here that the empirical evidence generated by the dominant two-stage paradigm is open to alternative interpretations. First, learning in the acquisition phase can give rise to explicit knowledge about causal relations. Second, effects in the test phase may not be driven by response activation by the outcome information, but by strategic processes that are driven by explicit causal knowledge. This chapter first reviews the standard two-stage paradigm, and then critically looks at learning, response activation, and the transition of knowledge from the learning phase to the activation phase. Potential implications of ideomotor theory are discussed.

Keywords: action control, automaticity, mechanistic learning, causal reference

Introduction

Humans are agents that engage in intentional action based on the goals or desired outcomes they have in mind. For goal achievement to materialize one needs to know what action causes the desired outcome and how to act on it (Gollwitzer, 1990). A classic question in psychology is how such intentions translate into action. This link between thought and behavior proved notoriously problematic for dualist accounts (Damasio, 1994), and still a challenge for scholars who rejected it. Building on German philosophy (e.g., Harless, 1861; Herbart,1825; Lotze, 1852) and British psychologists (e.g., Laycock,1845) (see in-depth historical beginning in Stock & Stock, 2004), William James argued that thinking of the experience of conducting an action would be enough to produce the actual motor behavior leading to this action and that control was mainly required to inhibit, not initiate action. As such, thinking of an action and executing it are considered to be intertwined.

This ideomotor (IM) principle regained popularity in the 90's, in line with the emerging perspective of embodied cognition. According to this perspective, people do not (only) store internal representations about the world in the form of abstract knowledge, but often in the format of perceptual experiences we have when interacting with the world (Barsalou, 1999, 2008). Capitalizing on this assumption, Prinz and colleagues raised the idea of "common coding" of perception and action (Prinz, 1990; 1997; Prinz et al., 2009), claiming that they share *the distal reference*. Perception and action were considered to rely on the same representations. This perspective eventually gave rise to the Theory of Event Coding (TEC; Hommel et al., 2001; Hommel, 2019). This seemingly all-encompassing theory has strong appeal and offers a very promising candidate for understand and examining the basic mechanisms underlying human goal-directed behavior. TEC introduced the term *Event Files*, referring to the shared and homogenous representations of actions and perceptions in a cognitive (or neural) system. It assumed that features of the stimulus in a specific environment (S; stimulus; context), a related response (R; decision, effector), and its following effects (E; perceptual and affective) are integrated into a distributed event file whenever actions are executed in a given context and its effects perceived. In other words, activating an effect code (or any features of E) will automatically initiate some motor activation by the linked distributive codes, thereby affecting action selection (R).

Crucially, TEC claims to offer a mechanistic explanation for goal-directed action. In contrast to theories of deliberate goal-setting (e.g., Gollwitzer, 1990; Locke & Latham, 1984, 1990), it does not assume any deliberation in the actor. Its goal is to be minimalist and parsimonious, providing a mechanistic model of goal-directed behavior and goal representations, without assuming "… any overhead, as common in many philosophically colored approaches, where representing a goal might imply having some understanding of the goal or some conscious awareness of it …" (Hommel, 2021, p.6.). Because of its mechanistic nature, this model of goal-directed behavior, linking together actions and outcomes combined representations, has been used to explain various instances of automatic or nonconscious behavior, such as imitation (Iacoboni, 2009), behavior priming (Dijksterhuis & Bargh, 2001), goal priming (Custers & Aarts, 2010), and have even been suggested as an explanation for the existence of mirror neurons (Heyes, 2010). Clearly, the idea of actions and outcomes sharing common codes has left its mark on psychology.

Empirical support was obtained using the two-stage paradigm, based on the work by Greenwald (1970). In this two-stage paradigm, behavior representations are first shaped in an acquisition task in which actions (e.g., pressing a left key) are repeatedly followed by effects (e.g., playing a high pitch tone). Subsequently, a test task investigates whether exposing people to the earlier effect information would evoke the corresponding actions. There is a substantial amount of literature featuring this paradigm, providing evidence in support of the IM principle in numerous studies (for an overview, see Shin et al., 2010; Stock & Stock, 2004).

Our main argument in the current chapter is that this dominant paradigm for testing IM effects is developed and biased to seek confirmatory evidence for the Theory of Event Coding (Hommel et al., 2001; Hommel, 2019), but not to rule out alternative explanations. Although the obtained results in the literature are indeed in line with the theory's predictions, we argue that the paradigm is open to – and may rely on – the deliberative processes the theory aims to do without. In the remainder of this chapter, we will first briefly review the evidence built on the two-stage paradigm. After that, we will take a critical look at the acquisition phase, examining alternatives to the mechanistic approach to learning, and then we will do the same for the test phase. Finally, we look at the processes by which the acquisition phase affects the test phase. We will present findings from our own and other labs, arriving at an alternative interpretation for IM effects.

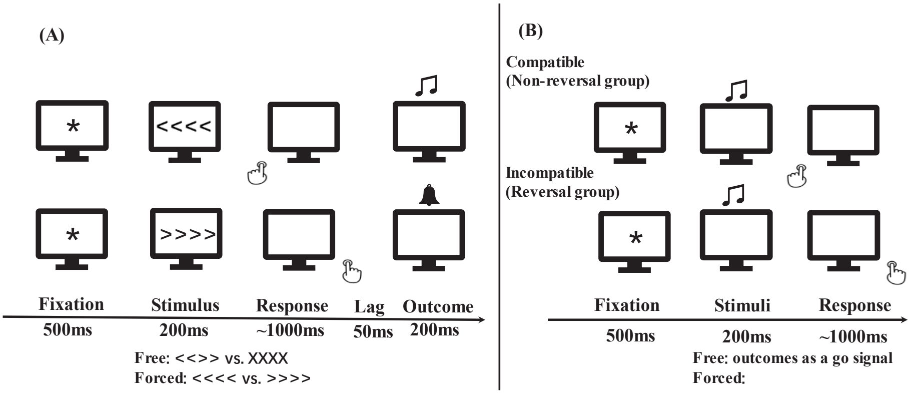
Evidence for Ideomotor Effects from the Two-stage Paradigm

Following the work of Greenwald (1970), tests of ideomotor learning typically contain two phases: An acquisition phase in which action-outcome associations are acquired and a test phase that tests whether stimuli that previously served as outcomes facilitate the activation of associated responses.

This two-stage paradigm became the dominant paradigm in IM research (see Pfister, 2019; Shin et al., 2010) for investigations of the formation of action-effect representations (e.g., Elsner & Hommel, 2001) and has been applied to many complicated but socially meaningful stimuli as action effects, such as faces (e.g., Herwig & Horstmann, 2011), letters in languages (e.g., Ziessler & Nattkemper, 2002), and the intensity of responses (e.g., Kunde et al., 2004). Although other paradigms are used as well, they mainly investigate the embodied nature, not the acquisition of action-outcome representations. For instance, Kunde and colleagues (2001, 2003) demonstrated that actions of which the outcomes share perceptual features facilitate each other. Taking a different approach, the work on motor resonance (e.g., Rizzolatti & Craighero, 2004, Zwaan & Taylor, 2006) from the field of neuroscience shows that observing outcomes seems to prepare associated motor patterns. However, the two-stage paradigm is the only one that looks at how the action-outcome representations that drive IM effects develop. As this paradigm is the primary source for theory testing and development, we critically examine the claims that are related to the acquisition and testing of action-outcome representations in the paradigm.

Figure 2.1

The standard setup of the two-stage IM paradigm, including an acquisition phase and the test phase.



Note. (A) The acquisition phase consists of two learning types, the free choice and forced-choice setting, including nogo trials for the free choice group as indicated by "XXXX" as a stimulus. (B) The test phase also has two types: free-choice and forced-choice tests. Here, the non-reversal and reversal group are only limited to the context of the forced-choice tests.

Table 2.1

Examples per Sub-types of the standard two-stage Ideomotor paradigm

acquisition phase test phase	free-choice	Forced-choice (i.e.,stimulus-based)
free-choice	e.g., Chapter 3 (Experiment 1); Elsner & Hommel (2001)	e.g., Pfister, Kiesel & Hoffmann (2011)
Forced-choice (i.e., stimulus-based)	e.g., Chapter 3 (Experiment 2/3); Chapter 4; Elsner & Hommel (2001)	e.g., Herwig, Prinz, & Waszak(2007); Wolfensteller & Ruge (2011)

Note. The table is the adapted version of the Fig1 of Pfister, Kiesel and Hoffmann (2011)

In general, for the acquisition phase, participants are consecutively exposed to a pair of contingent links between their responses and outcomes (e.g., R1-O1), where hypothetically, the response (R1) is conditioned to the anticipated image of distinctive sensory consequences (O1). Two sub-types of the learning phase are employed, depending on the type of action modes (i.e., Figure 2.1, Table 2.1). That is, the responses in the task can be cued by imperative stimuli (i.e., the stimulus-driven or forced-choice learning task) or be the result of one's own intentions (i.e., the free-choice learning task). Figure 2.1A depicts the basic experimental setup of both free choice and forced-choice learning trials. Each trial starts with a fixation, followed by a response cue. In the free-choice task, participants are instructed to choose to press either a left or right key in response to a go signal (i.e., << >>) and not do anything for a nogo signal (i.e., XXXX). In the forced-choice task, however, participants are asked to press the related key depending on the direction of the stimulus (i.e., here, pressing the left key when seeing a <<<< signal, and pressing the right key when seeing a >>>> signal.). Each correct response generates a specific outcome (i.e., tone) in both conditions, and the outcomes are explained as irrelevant to the current task.

For the test phase, there are also two kinds of measurements depending on the action's modes. In the stimulus-driven test task (i.e., the instructed forced-choice task), the previous outcome (e.g., O1) is presented in combination with (or without) a stimulus (S1). When the required response to the target stimulus S1 (i.e., the to-be-produced response) is consistent with the one that was previously paired with the presented O1, it will be executed faster and more accurately than in the opposite condition. Similarly, the test phase can also be measured in a free-choice mode, where the previous outcome O1 works as a go signal. According to the IM theory, participants should tend to choose the response related to previous experiences. The procedure of the test phase is depicted in Figure 2.1B, where the outcomes appear as imperative cues. Specifically, depending on the action-effect mapping acquired in the acquisition phase, the trials in the test phase can be labeled as compatible or incompatible in the free-choice task. Ideally, the response proportion will be over 50% in the compatible trials, that is, a more biased selection of the response related to the outcome. Participants are asked to respond to the effect directly according to specific instructions in the forced-choice version. The reaction times and error rates are measured as variables of interest. Usually, there are two conditions, participants in the *non-reversal group* are asked to respond to the outcomes (i.e., tones) with the same key that previously produced it, whereas for the other group, the instructions were reversed (i.e., *reversal group*). Better performance (with faster reaction times and lower error rates) is usually observed in the non-reversal group compared to the reversal group.

Acquisition phase: Is Learning Mechanistic?

In humans, the ability to learn and master systematic relations between actions and the following sensory consequences is expected to start right after (or perhaps even before) birth. That is, newborns produce responses (motor output), and receive sensory information (sensory input) from their sensory systems (e.g., vision, proprioception, hearing). The baby acquires action-outcomes representations during the dynamic interaction with the world through trial-and-error (e.g., O'Regan & Noë, 2001). For instance, after activating the motor program for lifting their hand, they may learn how to reach for something.

Although TEC and later adaptations are not explicit about how they are formed, these theories assume that bidirectional relations between actions and outcomes develop due to such repeated execution of motor programs and perception of the subsequent effect

(Hommel et al., 2001; Hommel, 2019). Analogously, in the two stage-paradigm, participants are only exposed to actions and outcomes in the learning phase in one direction (actions precede outcomes). Hence, IM effects in the test phase would result from the association being used in the opposite direction (perceived outcomes activating associated responses). The substantial number of trials in the learning phase of the two-stage paradigm suggests that repeated coactivation of motor programs and the related perceptual codes of the outcomes is required to forge integrated action-outcome representations. Such an associative mechanism underlying common coding dates back to the concept of "Hebbian learning." Hebb's (2002) influential theory of the role of synapses in learning and memory was that if a neuron repeatedly fired together with another near neuron, a connection would develop in the synaptic structure. According to this notion, a Hebbian-like learning mechanism would explain how bidirectional associations between the sensory information and motor event could develop as a result of mere coactivation of the two (e.g., Gallistel & Matzel, 2013; Heyes, 2010). In other words, the activation of perceptual codes in reaction to an outcome, in close temporal contiguity to the activation of the motor programs that produced it, would in principle be enough for bidirectional associations to develop. As a result, activating the representation of the outcome would also be expected to activate the associated motor programs.

Commentary

A central assumption in TEC is that bidirectional associations are formed in a mechanistic manner, which can eventually lead to activation of the motor program when an outcome is observed. In the last decade, however, it has been pointed out that knowledge about the relations between events can also develop through propositional learning. Propositions do not merely reflect patterns of coactivation, but specify relations between events or features (e.g., R produced O) in the world (Mitchell et al., 2009). As such, propositions have a truth value and can be proven right or wrong. Importantly, these propositions are not formed in a mechanistic way, but have to be generated by an individual that can reflect on the regularities in the environment. In the case of learning about action-outcome relations, that actor has to formulate a proposition about the relation between conducting the action and observing the outcome.

Although propositional learning is fundamentally different from Hebbian learning, the resulting knowledge structures can have the same effects on cognition and behavior (Gawronski & Bodenhausen, 2011; Mitchel et al., 2009). That is, forming the proposition that R causes O, could also lead to R being activated when O is perceived. Relevant for the current argument is that propositions about the relation between actions and outcomes that are formed in the acquisition phase of the two-stage paradigm, could in principle produce IM effects in the later test phase.

It is clear that participants form such propositions during learning. In their seminal study, Elsner and Hommel (2001) already reported that most participants could explicitly report the relations between actions and outcomes after the task. One could argue that those propositions may result from the instructions, emphasizing that there are actions and related outcomes in the task (see Eder and Dignath, 2017). Recent evidence by Sun and colleagues (2020) demonstrated that participants could report the causal relation between actions and outcomes and displayed an IM effect in the free-choice task, regardless of whether the instructions explicitly mentioned outcomes or not during the lengthy acquisition phase. This suggests that people could form causal inferences (e.g., Waldmann et al., 1995) spontaneously in the acquisition phase, either by mere observation of the outcomes or their actions, or by actively testing their self-generated hypotheses. While such propositions about causal relations are formed in the two-stage paradigm, it is unclear, though, whether they drive the IM effect in the test phase.

Evidence

Direct evidence for the idea that propositions can in principle be responsible for IM effects, comes from Theeuwes and colleagues (2015). In an upcoming task, they gave people instructions about response-outcome relations (i.e., the R-O contingencies). Then, before this task took place, they tested participants in a second task. It turned out that when participants perceived the stimuli referred to as outcomes in the instructions of the upcoming task, related responses were activated. That is, responses were faster and more accurate when participants had to react with the response that matched the instructed outcome. This happened despite the fact that the instructed action-outcome relations had at that point never been experienced as such (for similar findings and reasoning, see Tibboel & Liefooghe, 2020).

Indirect evidence for propositional learning comes from research demonstrating very fast learning. Several studies show that a lengthy learning phase is unnecessary for the IM effect to emerge. Instead, action-outcome associations can be built almost instantly (e.g., Wolfensteller & Ruge, 2011; Sun et al., 2022). According to work by Wolfensteller and Ruge (2011), ideomotor effects on reaction times (faster responses on compatible vs. incompatible trials) were found even when learning phases were as short as 16 trials (8 pairings for each action and its outcome). In line with this observation, we found a fast-learning effect in a task where learning and testing occurred on the same trials (Sun et al., 2022, Experiment 1). This effect was successfully replicated later (Sun et al., 2022, Experiment 2a), and even emerged when the contingency between actions and effects decreased to 80% (Sun et al., 2022, Experiment 2b). Such fast learning would fit a propositional account, but would be hard to explain by Hebbian learning.

Conclusion: Is Ideomotor Learning Mechanistic?

While the two-stage paradigm is optimized for Hebbian learning, it does not rule out the possibility of propositional learning. Intuitively, it seems evident that one does not need 200 trials to figure out the relation between, for instance, two different key presses and two different tones. This can be done in a couple of trials. Or even in one, if one assumes that each response has its own unique effect. Evidence suggests that people make these causal inferences spontaneously, and that they can explicitly report the propositions about causal relations they have formed. Research on instruction effects demonstrates that such propositions can cause stimuli that were communicated to be the outcomes of specific responses to activate those very responses, even in the absence of any previous experience of those actions causing the outcomes. Moreover, the fast learning observed in the literature and our lab is hard to explain by Hebbian learning, but easy to explain in terms of propositional processes.

Test phase: Do Outcome Primes Activate Responses?

In the standard two-stage ideomotor paradigm, examining how an idea of an outcome initiates an action is mainly done in choice-reaction tasks, including the forced-choice and the free-choice testing task. In these tasks, the outcome stimuli that followed responses in the acquisition are used as a prime. According to the ideomotor theory, outcome primes should activate the associated motor response if participants have formed bidirectional associations between each action and outcome in the acquisition phase. In support of this mechanistic effect, the literature almost exclusively relies on the free-choice task and a specific type of forced-choice task in which participants receive instructions that are either consistent or inconsistent with the earlier acquired action-outcome mappings (see similar arguments in Sun et al., 2020). We argue here that these two tasks do not rule out the use of explicit propositional knowledge. Put differently, we argue that the IM effects obtained in those tasks do not necessarily follow from associations and mere priming effects on behavior. It is plausible that the propositions people form in the acquisition phase influence the choice process in the free-choice task or may interfere with remembering and implementing task instructions in the forced-choice task.

Commentary

While, in theory, the mere perception of an outcome prime could activate responses and thereby biases people's choices, this task does not rule out alternative, less mechanistic processes. In fact, as participants are invited to make free choices, a host of alternative processes could play a role here. Because people acquire explicit propositional knowledge in the acquisition phase, strategic processes cannot be excluded. For instance, participants may guess the hypothesis or believe they should act in line with the earlier acquired mapping. Moreover, as responding freely or randomly may be quite hard, participants may base their choices on the outcome primes that appear in seemingly random order as well. Although many other processes are possible, we believe it is evident that the IM effect in the free-choice task does not necessarily have to be the result of response activation by the primes if people have explicit knowledge of the action-outcome mappings. It can also result from people's strategic choices based on propositional knowledge.

In the same way, the dominant forced-choice task, in which participants are instructed to respond to stimuli that served as outcomes in the acquisition phase (e.g., Elsner & Hommel, 2001, Herwig et al., 2007; Herwig & Waszak, 2009; Hommel, et al., 2003), may be open to alternative processes. While response activation by the outcome primes should indeed produce an IM effect (faster and more accurate responses on compatible trials), we argue that such an effect could also arise because explicit propositions about the relation between actions and outcomes may interfere with remembering or implementing the task instructions. This suggestion is based on the literature on task switching, in which it is demonstrated that there is a switch cost (more errors and slower responses) when participants are instructed to switch rules from one block of trials to the next (Monsell, 2003). In the same way, a proposition about relations inferred in the acquisition phase (e.g., R1 causes O1 and R2 causes O2) could make it easier to implement compatible instructions that rely on the same (although reversed) mapping (e.g., respond to O1 with R1 and O2 with R2) rather than the instruction that requires a switch to the opposite mapping (e.g., respond to O1 with R2 and O2 with R1).

Evidence

Concerning the free-choice task, it might be the case that explicit propositional knowledge about the action-outcome relations drives the behavioral effects (c.f., Seabrooke et al., 2016). In Experiment 1 of Sun and colleagues (2020), we indeed obtained an IM effect using the free-choice task. While this effect was not affected by our instruction manipulation (i.e., emphasizing outcomes or not), a closer inspection of the responses revealed a telling bimodal distribution: There was a small number of participants who showed a very large bias, with some close to 100% in line with the previously acquired mapping, while the majority of participants was at chance level. Such a distribution would be very unlikely if the responses are merely activated by associated outcomes In that case, all participants should, on average, be slightly above chance level, biased in line with the mapping of the acquisition phase. Although we are not aware of other papers using the free-choice task that present the data points of individuals, the means and standard errors of our results are virtually identical to those of Experiments 3a and 3b by Elsner and Hommel (2001), after which the experiment was modeled. As an even distribution of correct proportions would lead to a considerably lower standard error, we believe it is not unlikely that the IM effects reported in these studies also result from a bimodal distribution. Together, this suggests that effects in the free-choice task may be caused by strategic decisions based on explicit propositional knowledge. Although we agree with Elsner and Hommel (2001) that quick reaction times would argue against strategic decision being made based on the identity of the outcome prime, we believe strategies in the free choice task (e.g., press left for high tones and right for low tones) could still easily be implemented for the entire test phase leading to fast and effortless responses in line with explicit knowledge.

A way to rule out such strategic processes would be to present imperative cues to which the participant has to react together with separate outcome primes. Although Sato and Itakura (2013) used this method in a study in which participants responded to facial properties that were manipulated independently of the features of the facial expression that served as outcomes in the earlier acquisition phase, a conceptual replication study by Riechelmann and colleagues (2019) failed to reproduce this effect. In line with this finding, in Experiment 3a of Sun and colleagues (2020), we found no evidence for IM effects in a similar trial-based forced-choice task. In addition, a high-powered replication (Sun et al., 2020, Experiment 3b) replicated this null effect.

Conclusion: In testing Ideomotor learning, are outcome-response prime effects mechanistic?

While substantial literature reports evidence in line with the idea that outcome primes activate responses in the test phase, the results almost exclusively rely on test phases that allow other processes to drive the effects. That is, based on the finding that people do have access to explicit propositional knowledge about the relation between actions and outcomes, deliberative strategies could drive effects in the free-choice, and the instructed forced-choice task as well. Importantly, studies using an adapted trial-based forced-choice task intended to rule out the possibility of such strategic processes, did not produce an IM effect. Thus, it seems possible that response activation by outcome primes does not drive the IM effects reported in the literature.

Processes by which the Acquisition phase Affects the Test phase

In the two-stage paradigm, the IM effect would not only depend on associations being formed in the acquisition phase. They would also depend on whether the associations are strong enough to activate responses in the test phase and whether the outcome primes successfully activate the representations in the test phase. It is especially important given the fact that IM effects in the test phase are typically tested under extinction. That is, to test response activation as a result of associations formed in the acquisition phase, responses are not followed by outcomes anymore in the test phase. The observed lack of IM effects in paradigms that rule out the influence of strategic processes could, in principle, be due to a lack of sufficient strength of the associations formed in the acquisition phase, or weak activation of representations in the test phase (see similar discussion in Sun et al., 2020).

Commentary

While insufficient strength of associations may explain the lack of results in test phases that aim to rule out strategic responding, other factors could determine whether learning transfers to the test phase. Even though propositional knowledge could produce IM effects through a genuine IM process (i.e., activation or responses in reaction to outcome primes), such processes could be conditional on whether or not the propositions are deemed to be applicable in the test phase. That is, the typical practice of testing IM effects under extinction could lead participants to immediately decide that the learned rules are not applicable anymore, causing them to abandon or update such propositions. Below, we present evidence that aims to answer two questions: First, do factors that increase the strength of associations moderate the IM effect in a test phase that does not allow for strategic processing? To answer this question, we manipulated rewards during the acquisition phase. Second, does the strength of priming in the test phase moderate the IM effect? To answer this second question, we changed the modality of the imperative cues in the test phase to match that of the outcome primes and added outcomes in the test phase. This latter change was made to keep action-outcome knowledge active and valid during the test phase (c.f., Dogge, Custers, & Aarts, 2019).

Evidence

Rewards.

Eder and colleagues (2020) investigated the effects of rewards in an experiment that featured a sequence of short two-stage blocks, comprised of an acquisition phase and a test phase. Using four instead of two pairs of actions outcome relations, they found IM effects only for the two relations for which remembering them was rewarded later in the task. Although this effect demonstrates that people selectively learn relations that are relevant to them (c.f., Eitam, et al., 2013), this effect could be the result of strategically using this knowledge in the test phase, so such a forced compatibility paradigm may also reflect propositional knowledge as discussed in the preceding section. Moreover, the results conflict with those of Muhle-Karbe and Krebs (2012), who found no evidence of rewards' positive effect on IM learning. Similarly, we reported a study in which we used a cued version of the forced-choice task (i.e., the cue-based compatibility task) to test the effect of rewards (Sun et al., 2022, under revision). A significant IM effect failed to emerge, suggesting that rewards that should strengthen bidirectional associations, do not promote the IM effect. Besides, Bayesian analyses corroborated these null effects.

Modality.

Another factor that may influence the transfer of learning to the test phase in the cue-based compatibility paradigm, is the match between the modality of imperative cues in the test phase and the primes that served as outcomes in the acquisition phase. We tested this notion by conducting an experiment where participants had to respond to the location of the presented tones instead of relying on a visual stimulus in the test phase (Sun et al., 2022, under revision). Specifically, the tones presented in the acquisition phase as outcomes in both ears, were now presented either in the left or right ear. Because the outcome primes doubled as imperative cues now (i.e., participants had to respond to the location of the tone, while its pitch served as outcome prime), participants could no longer ignore outcome primes by shielding a task-irrelevant modality, as was possible in previous studies (see in Sun et al., 2020). In other words, as participants had to attend to the tones to complete the central task in the test phase, attention would have had to be focused on the relevant modality (i.e., auditory instead of visual), which should increase the chance of the IM effect emerging in the test phase. Results did not reveal a significant IM effect. Even though the study could be regarded as underpowered or affected by the variance between individuals, we took an in-depth look at the distribution via quantifying the data into deciles (Rousselet et al., 2017). These robust graphical methods suggests that increased power would be unlikely to produce a significant effect (see supplementary results in Experiment 2 of Sun et al., 2022, under revision, p81). Similar modalities for imperative cues and outcome primes are (at least in the context of our experiments) apparently not what it takes to produce IM effects in a testing phase that aims to minimize the influence of strategic processes.

Post-response Outcomes in the test phase.

The IM effect does not seem to transfer from the acquisition phase to the cue-based compatibility test phase even when conditions are optimal (i.e., rewards during learning, equimodal cues and outcome primes). There may be another moderator to consider. According to the reasoning above, the propositional processes that drive learning of action-outcome associations would suggest that applying the learned rules may not be unconditional. That is, even though a proposition is formed (e.g., outcome Y is caused by action X), this does not mean that perceiving Y always activates X. As propositions have a truth value and can be considered to apply or not in the current setting, the absence of IM effects observed in the studies discussed could also be explained by people no longer relying on the propositions formed in the acquisition phase.

This would not be unlikely, as the testing phases in the IM paradigm typically test under extinction: The actions that produced specific outcomes in the acquisition phase, no longer do so in the testing phase. Therefore, someone who inferred the causal relation between actions and outcomes in the acquisition phase (“Hey, Y appears every time I hit X!”), is not unlikely to also realize that the proposition doesn’t apply anymore in the test phase (“Hey, X doesn’t cause Y anymore!”). If propositions are quickly abandoned or updated, this may explain why IM effects are no longer present in the test phase.

Indeed, the presentation of post-response outcomes in the test phase has been found to have a compelling impact on the IM effect not only in the instructed compatibility test task (e.g., Elsner & Hommel, 2001), but also in the cue-based compatibility test task (e.g., Vogel et al., 2020). And as argued, there is evidence suggesting that participants can quickly acquire new propositional rules. Wolfensteller and Ruge (2011) demonstrated that people could learn various action-outcome relations quickly across different short learning and test phases. In line with this evidence, Experiment 3 of Sun and colleagues (Sun et al., 2022, under revision) demonstrates that presenting outcomes during the test phase does bring out the ideomotor effect. This suggests that propositions may have to be applicable in the test phase for IM effects to occur in a test phase that rules out strategic processes.

Such experiments are problematic from a learning perspective as they open up the possibility that learning (also) takes place during testing in addition to the acquisition phase. Indeed, from a propositional perspective, a rule may be inferred in a couple of trials in the test phase (“Hey, X causes Y again here!”) to drive the IM effect. In that case, test phases with post-response outcomes might even be enough to produce an IM effect without an acquisition phase at all.

This is exactly what we found in the two experiments before (Sun et al., 2022). In this study, participants engaged in 10 blocks of 20 trials that featured one of the two possible mappings of two outcomes on two responses. First of all, IM effects were assumed to arise based on propositional processes that would allow for a quick inference of the causal relations in the block, so that the IM effect could reveal itself on the rest of the block. This process (demonstrated to occur spontaneously in Sun et al., 2020) was even encouraged by rewarding people to report back the mapping at the end of the block. Results provided evidence for IM effects arising on a block level, but no evidence that learning spilled over from one block to the next. That is, while new mappings were learned instantly to produce IM effects, they also seemed to be instantly abandoned by the beginning of the next block. Hence, IM effects seem to be produced by acquiring propositional knowledge about the rules that apply at the moment.

Conclusion: Is the Transition from Learning Effects to the Test Phase Mechanistic?

Action-outcome learning in the acquisition phase has been demonstrated to transfer to the testing phase mainly in paradigms that do not exclude the possibility that formed propositions about action-outcome relations are used in strategic responding or interfere with memorizing and implementing novel task instructions. With the cued-based compatibility test task, which more strictly tests the processes underlying the mechanistic account, recent studies (Sun et al., 2020) obtained null effects. Of the potential moderators, rewards that should strengthen the action-outcome associations do not seem to bring about the IM effect, nor does equimodality of the primes and the imperative cues. However, what does seem to matter is whether post-response effects are presented in the testing phase. A novel paradigm studying continuous learning of changing action-outcome relations reveals that inferred causal relations create instant IM effects. These effects can flip immediately without switching costs once the relations change. Together, this suggests that perceived outcomes may indeed activate related actions, but only if the inferred causal relation between actions and outcomes is active and valid in the task at hand.

General Discussion and Conclusions

Contemporary approaches to Ideomotor (IM) theory, most likely represented by TEC, offer an elegant mechanistic account for understanding human goal-directed behavior. They assume that actions become associated with their perceived outcomes and that perceiving or activating the representation of an outcome can activate the associated action. The research discussed here suggests that the mechanistic account does not fit well with the recent empirical evidence. Below we discuss the implications for Ideomotor Theory in general, and goal-directed behavior in particular.

First of all, it is hard to explain the recent empirical observations without propositional learning as a key process. IM effects in the test phase of the two-stage paradigm seem only to arise if there is room for propositions to affect strategies or interfere with novel instructions. In a paradigm designed to rule out these processes, the effect is only observed when learned relations between actions and outcomes are valid in the test phase. This suggests that genuine ideomotor actions (i.e., actions activated by perceived outcomes) can indeed occur in the two-stage paradigm, but that propositions and judgments about their validity play a role.

There is a remarkable parallel here with current research on predictive coding attesting to the flexibility of action-outcome representations. Based on the same assumption that motor programs and perceptual outcomes are integrated into one representation, it has been demonstrated that activating motor codes activates the perceptual codes about the outcome as well, which biased the perception of ambiguous visual stimuli (e.g., Wiese & Metzinger, 2017; Wolpert et al., 1995). While these effects prove stable for actions and outcomes that have their basis in bodily sensorimotor processes (e.g., trying to tickle your arm; Blakemore et al., 1998, 2000), it has been demonstrated that these effects are flexible when interacting with objects in the outside world. For instance, recent work (Dogge, Custers, Gayet, et al., 2019) showed that the perception of bistable spheres could be biased by motor movements (e.g., rotating a knob that controls the sphere), that is, participants tended to perceive the rotation of the sphere in line with how they rotated the knob. However, participants were able to eliminate such motion-based perceived bias if they had acquired an opposite action effect either though instructions or experiencing a short exposure period. By contrast, such motion bias won’t change if it was as consistent as preceding action effect. In another words, the perceived bias effect depends on the relation between action and outcome in the setting at hand. Thus, the perception of action effects that were considered to rely on stable action-effect representations, seem to depend on propositions about the rules that link actions to outcomes in the here and now (see more discussion in Dogge, Custers, & Aarts, 2019). This notion fits perfectly with the research on the IM effect reported here.

A crucial assumption in the mechanistic approach on ideomotor effects is that when outcome representations are activated, they activate action or motor representations directly, without mediating for deliberative processes. De facto, a test of ideomotor effects, should fulfill the criteria of unintentionality and uncontrollability (Bargh, 1994). The often-used free-choice test phase does not necessarily fulfill those criteria. One could argue that instructing people to freely choose between response options and perhaps instructing them to ignore the outcome primes would unintentionally render any responses to the primes. If one assumes this is the case, one could even argue that any resulting IM effect would have to be uncontrollable. Of course, this is only the case if participants truly and fully act in line with the experimental instructions. However, it does not make controlled responding impossible. As it is a free choice task, participants could respond as they see fit. While the instruction-based compatibility paradigm is already better, there is still the possibility that interference effects originate at the instruction level, rather than the response level. That is, explicit representations formed in the acquisition phase may interfere with memorizing and implementing the task instructions. Hence, the dominant two-stage paradigms in IM research do not satisfy the automaticity criterion for the activation of responses by outcome primes.

By contrast, the cue-based compatibility paradigm used in our recent research does, in our opinion, satisfy these criteria. As participants have to react to imperative cues separate from outcome primes, any intentional or controlled behavior would have to occur at the moment of perception of the prime. As the IM effect would be mainly reflected by faster responses on compatible than incompatible trials, intentional and controlled responses and highly unlikely. While in theory, participants could create an artificial IM effect by slowing down on incompatible trials (e.g., Fiedler et al., 2006), this would require a thorough understanding of the task.

Contemporary views on automaticity recognize that automaticity is not all or nothing, but rather a graded variable. Processes can be considered automatic on some criteria but not on others. Automaticity is now assumed to depend on goals, situations, and other variables (see, e.g., Hommel & Wiers, 2017; Melnikov & Bargh, 2018; Moors, 2016). While this conditional automatic may strengthen IM theory at first glance because it could help explain when IM effect occurs and when they do not (Frings et al., 2020; Hommel, 2021), we argue here that this conditional automaticity also provides an existential threat to the mechanistic approach itself.

Indeed, the idea of a mechanistic process that is conditional on variables that actually are excluded from the mechanistic model of human agents (including motivation, mindsets, etc.), also allows for a radically different view: IM effects are the result of propositions, motivation, mindsets, etc., and, under some conditions, these largely deliberative processes produce automatic effects of certain kinds (e.g., activation of a response by a stimulus). Although the two are mirror images and just assume different processes as the default, we believe that the central role of propositions argued for in this thesis makes the perspective of a reflective agent that in specific situations acts in an automatic manner far more plausible than the alternative view of a mechanistically driven agent.

Indeed, such interactive effects of deliberation and automatic effects can be found in research on implementation intentions, in which plans that take a propositional structure (e.g., if I see stimulus X, I will respond with response Y) lead to automatic responses to the stimuli in question (Aarts & Dijksterhuis, 2000; Gollwitzer, 1999). Interestingly, though, the current studies suggest that just knowing that your actions will produce specific outcomes in the current situation is enough to create IM effects. This could explain fairly automatic goal-directed behaviors in all sorts of settings. For instance, when playing tennis, just focusing on the desirable outcome (e.g., landing the ball in the far corner) could automatically trigger the associated action pattern that produces the effect. While tennis arguably takes years to learn, the fact that one can seemingly update these automatic responses quite easily under different conditions (e.g., wind direction, a new racket, balls that are heavier when wet), may suggest that these action-outcome representations that help to react in a goal-directed manner in a split second are still quite malleable and easily updated under different conditions.

One area in which the mechanistic model should perhaps still be the primary explanation is acquiring the first links between motor programs and their perceptual effects created in infancy. That is, the direct bodily sensations of motor programs (e.g., perceiving your hand making a grasping movement as a result of activating the related motor program), may be the result of bi-directional associations that may have been formed through Hebbian learning (e.g., Heyes, 2010). However, once these fundamental action-outcome relations have been acquired, learning new relations between actions and outcomes in the outside world does not have to take place in the same way. Rather than associating the raw code of the basic motor program with the perceptual experience of a well-known outcome (e.g., a flashing light bulb), learning can happen much more efficiently, by linking motor programs that are already represented in terms of meaningful outcomes (e.g., pressing a button with your finger) to an existing representation of the outcome (e.g., a flashing light). In other words, if motor programs are associated with their proximal bodily effects, these representations can as building blocks be used to represent new relations through propositional processes.

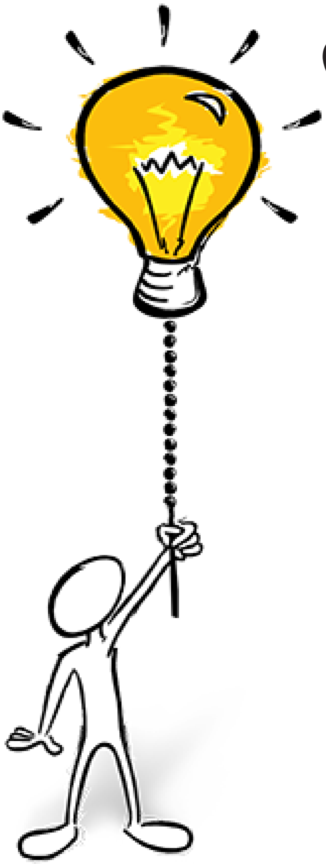
Such a combination of processes may explain the observed flexibility in IM action (Hommel, 2021) and related fields like mimicry (Iacoboni, 2009) and research on mirror neurons (Heyes, 2010). For instance, while research on mirror neurons demonstrates that a single neuron plays a role in the perception and execution of actions, it has also been demonstrated that these relations are dependent on context and can be altered if people or animals focus on the distal, rather than the proximal outcomes of their actions (Umiltà et al., 2001, 2008). That is, once a motor program can be selected intentionally, by thinking of its proximal effects, this proximal effect can (e.g., pressing a button) may be linked to all kinds of effects using propositional processes. While learning these new links may happen much faster in humans than in animals, evidence for propositional learning in animals suggests that such learning could emerge across species (e.g., Mitchel et al., 2009).

If propositional learning is indeed responsible for most IM effects (i.e., these with outcomes outside the body), this would have huge implications for IM action. Instead of emerging only after hundreds or thousands of repetitions, IM action may emerge almost instantly in situations in which people have hypotheses or expectations about the consequences of their actions. This may force us to reconsider mechanistic effects in psychology. For instance, smooth social interactions may require fast actions at the right moment. And these actions are often conducted with expected outcomes in mind. Hence, people may respond with a quick joke to diffuse a situation not because thinking of a laugh triggered an associated action (e.g., making a particular joke), but maybe a fairly automatic response based on the proposition that this particular type of joke would elicit a laugh in the present audience. Although such flexible, contextualized goal-direct responding would be in line with recent adaptations of the mechanistic ideomotor theory (Frings et al., 2020; Hommel, 2021), the reliance on propositional processes would open up a whole new category of IM actions that could help to further theories of conditional automatic ideomotor actions in human behavior.

Together, the analysis outlined in the present chapter sheds more light on how thinking of outcomes can produce action. While a mechanistic account may help understand effects within the body where action-outcome relations are fixed (Pfister, 2019), it may be less successful in explaining learning of novel action-outcome relations in the environment. As propositions can instantly link novel outcomes to well-rehearsed actions, such learning seems far more adaptive. Intriguingly, though, the findings presented here suggest that such learning could facilitate instant ideomotor effects. That is, realizing that your actions produce a particular outcome in the setting at hand, may be enough for thinking of outcomes to trigger actions. Thus, goal-directed behaviors could be activated by thoughts or stimuli related to outcomes in a fairly automatic manner (see Custers & Aarts, 2010), once a causal rule between action and outcome has been inferred through experience, or communicated (e.g., Theeuwes et al., 2015). This novel perspective on ideomotor action may shed new light on how automatic goal-directed behaviors such as habits, impulses, and imitation develop. Understanding the interplay between deliberation and automatic responses could be the key to tackling these issues.

CHAPTER

3



Chapter 3:
Ideomotor Action: Evidence for Automaticity in Learning, but not Execution

This chapter is based on: Sun, D., Custers, R., Marien, H., & Aarts, H. (2020). Ideomotor action: Evidence for automaticity in learning, but not execution. *Frontiers in Psychology, 11*, 185.

(The paper is a part of a special issue on Habits: Melnikoff, D. E., Bargh, J. A., & Wood, W. (2021). On the Nature and Scope of Habits and Model-Free Control. *Frontiers in psychology*, 4530)

Credit author statement:
DS: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data Curation, Writing-Original-Draft, Writing-Review & Editing, Visualization, Project administration. **RC:** Conceptualization, Data Curation, Writing-Review & Editing, Supervision, Project administration. **HM:** Writing-Review & Editing. **HA:** Conceptualization, Data Curation, Writing-Review & Editing, Supervision.

Abstract

Human habits are widely assumed to result from stimulus-response (S-R) associations that are formed if one frequently and consistently does the same thing in the same situation. According to Ideomotor Theory, a distinct but similar process could lead to response-outcome (R-O) associations if responses frequently and consistently produce the same outcomes. This process is assumed to occur spontaneously, and because these associations can operate in a bidirectional manner, merely perceiving or thinking of an outcome should automatically activate the associated action. In the current paper we test this automaticity feature of ideomotor learning. In four experiments, participants completed the same learning phase in which they could acquire associations, and were either explicitly informed about the contingency between actions and outcomes, or not. Automatic action selection and initiation were investigated using a free-choice task in Experiment 1 and forced-choice tasks in Experiment 2, 3a and 3b. An ideomotor effect was only obtained in the free-choice, but not convincingly in the forced-choice tasks. Together, this suggests that action-outcome relations can be learned spontaneously, but that there may be limits to the automaticity of the ideomotor effect.

Keywords: action control, automaticity, goal-directed behavior, ideomotor,

implicit learning.

Introduction

Habits are often regarded to be the result of stimulus-response (S-R) associations that are assumed to be formed if people repeatedly and consistently perform the same behavior in the same situation, often because there is an incentive to do so (Wood & Rünger, 2016). As a consequence, the situation may trigger the associated response in an automatic fashion, leading to habitual behavior that is no longer guided by deliberative processes (Aarts & Dijksterhuis, 2000), but controlled by the environment. A relevant but distinct line of research proposes a similar mechanism in which behaviors can become associated with the situations or events that follow actions: Ideomotor theory proposes that if a behavioral response is repeatedly and consistently followed by the same perceptual outcome, thinking about or activating the mental representation of that outcome can to a certain extent prepare or trigger the behavior through bi-directional response-outcome (R-O) associations. This mechanism of ideomotor action has been used to explain various instances in which the environment triggers behaviors in an automatic fashion, such as mimicry, or behavior from affordances (Custers & Aarts, 2010; Iacoboni, 2009).

Ideomotor-action could be relevant to the understanding of habitual behavior in at least two ways. First, it may help to understand how the environment could trigger behaviors that look like habits, but may not be the result of classic habit formation processes (i.e., not resulting from S-R associations). Second, it may help to understand the implementation of seemingly abstract S-R associations. That is, many behaviors that are regarded as habits (reading the newspaper on Saturday morning, having coffee after dinner, reading a book before going to sleep) are not directly represented at the motor level, but representations include a rich collection of experiences of the consequences of executing the behavior and allow for an abstract representation of the behavior. Research indeed suggests that people represent behaviors in a hierarchical way, in which more abstract representations of the behavior are often the outcomes of the lower-level actions that produced them (Cooper & Shallice, 2006; Kruglanski et al., 2002; Vallacher & Wegner, 1987). Representing behaviors in terms of their outcomes may therefore help to produce the same behavioral outcome (e.g., reading the newspaper) under slightly different conditions (e.g., picking up the paper from a slightly different location on the doormat each time and finding an empty chair to read it; Custers and Aarts 2010; Powers, 1973).

Although action-outcome representations may be indispensable for human behaviors, and especially goal-directed actions, it is less clear how these associations are acquired. Moreover, although contemporary approaches to ideomotor action (Hommel, 2013) assume that bi-directional R-O associations could trigger responses in an automatic fashion, there are few rigorous tests that demonstrate this. In the present paper we put the automaticity in the formation and execution of ideomotor action within the classic ideomotor paradigm to the test. We first review current evidence for the automatic nature of ideomotor action and evidence for spontaneous ideomotor learning. We then investigate whether or not learning relations between actions and outcomes can occur spontaneously, by merely executing actions and observing following events, and without specific instructions. Three different ideomotor tests are used to gain insight in the degree to which potentially resulting ideomotor actions are automatic.

Ideomotor theory

The notion of ideomotor action dates back to the 19th century (Carpenter, 1852; Lotze, 1852), aiming to explain how thought can trigger action (for reviews see, Stock and Stock 2004; Shin, Proctor, and Capaldi 2010). The central idea of early ideomotor theory was that merely envisioning an action triggers that action to a certain extent (James, 1890), even in the absence of a conscious intention to act (Ansfield & Wegner, 1996). Embracing the idea that thinking of an action includes envisioning its anticipated outcomes, Greenwald (1970) proposed that ideomotor action relies on bi-directional R-O associations. That is, thinking about an actions involves thinking about the perceptual experiences that have become associated with particular motor programs (see also., Zwaan & Taylor, 2006). While such associations enable response selection based on outcomes of actions (i.e., goal-directed behavior), the strong version of ideomotor theory (see Shin et al., 2010) holds that once the association is formed, thinking (ideation) of an outcome, or merely perceiving a related stimulus, is enough to trigger the associated action. This backward activation appears to be a robust and general phenomenon which has been observed for many different action and stimuli, such as auditory stimuli (e.g., Elsner & Hommel, 2001), faces (Herwig & Horstmann, 2011), locations (Hommel, 1993), and letters (Hommel et al., 2003; Ziessler & Nattkemper, 2002).

In the last two decades, the Theory of Event Coding (TEC) (Hommel et al., 2001; Hommel, 2019) has revived interest in ideomotor action, by providing a cognitive-perceptual framework for understanding these effects. This framework holds that both actions and their perceived sensory effects are cognitively represented in a similar distributed fashion and that their feature codes become intricately linked in action-stimulus representations that contain information about both. As these representations can be used bi-directionally, observing or thinking of an outcome activates the representation of the corresponding action, explaining phenomena such as mimicry (Iacoboni, 2009), action priming (Dijksterhuis & Bargh, 2001), and goal priming (Custers & Aarts, 2010). According to TEC, representations of effects and basic motor movement already become intertwined in early infancy (Heyes, 2010; Hommel et al., 2001). It appears, then, that R-O associations emerge spontaneously as a result of acting and observing, giving rise to representations that can drive behavior in an automatic, habit-like fashion.

Ideomotor Research

Following Greenwald (1970), tests of ideomotor learning typically contain two-phases: An acquisition phase in which action-outcome associations are acquired, and a test phase that tests whether these stimuli (i.e., outcomes) facilitate associated actions. In a classic study, Elsner and Hommel (2001) had participants freely choose in the first phase (i.e., free-choice acquisition phase) between left and right key presses that were each consistently followed by a specific tone (high or low pitch). Importantly, participants were explicitly informed that the tones were irrelevant to the task. In the second phase (forced-choice test phase, Experiments 1a, 1b), participants had to press left or right keys preceded by the tones that mapped on the earlier learned responses (non-reversal group), whereas for the other group the Response-Outcome mapping was reversed (reversal group). Results showed that actions were performed faster when the mapping was consistent with that in the acquisition phase, rather than reversed. Follow-up experiments (Experiments 2-4) revealed a similar consistency effect in a free-choice test phase that required subjects to press left and right keys randomly: Actions that were consistent with the Response-Outcome mapping were more frequently selected after the tones, showing a response bias in free choice as a result of outcome priming.

Later studies have systematically compared the effects of free- and forced-choice learning phases. Herwig, Prinz, and Waszak (2007) used a forced choice test-phase in which participants were allocated to a non-reversal or reversal group. They found that effects of ideomotor learning between actions and resulting outcomes only occurred when participants voluntarily selected actions in the learning phase (free-choice learning), and not when the required responses were forced by cues (forced-choice learning). These findings suggest that participants more readily represented the stimuli (tones) as outcomes of their actions when they engaged in free-choice learning, whereas merely responding to cues did not produce such a psychological process. Hence, even though actions were followed by stimuli in exactly the same way in free- and forced-choice learning phases, the stimulus information appears to have been encoded differently during learning.

Subsequent work by Pfister, Kiesel, and Hoffmann (2011) suggested that it may not be the encoding in the acquisition phase, though, that makes the difference, but rather the mode in which people control their behaviors in the test phase. Using a free-choice test phase, they found evidence for ideomotor effects, regardless of whether learning took place in a free- or a forced choice phase. They concluded that ideomotor learning takes place whenever actions are followed by events, regardless of the acquisition task, but that participants need to be engaged in “intention-based control” in the test phase (that is, selecting outcome-related actions), for ideomotor effects to arise. This would suggest that while learning of habitual action-outcome relations may be spontaneous, it may be conditional on a certain mind set or task set (i.e., conditional automaticity; see Aarts and Dijksterhuis, 2000)

Instruction effects

Although the research discussed above suggests that ideomotor learning occurs spontaneously whenever events follow actions, this “spontaneous learning” always occurs within the experimental setting. As it happens, though, task instructions in the acquisition phase often explicitly mention the presence of outcomes in the task, stating that they are irrelevant and should be ignore (e.g., Elsner and Hommel 2001). Whilst it is not always clear which exact instructions are provided in the acquisition phase in ideomotor research, Eder & Dignath (2017) have recently demonstrated in a task in which learning and testing of ideomotor action are intertwined, that such task instructions matter a lot. Based on recent insights in the power of instruction effects (see Liefooghe et al., 2018), Eder and Dignath (2017) provided instructions to ignore, attend, learn, or intentionally produce action outcomes in one combined learning / test phase. Results showed that instructions affect the task set with which action-stimulus relations are learned (Custers & Aarts, 2011), but that unlike the learning and intention instructions, instructions to ignore or attend to outcomes did not lead to ideomotor learning, at least not in this experimental setting.

In the present paper, we investigate whether ideomotor learning occurs spontaneously in the standard two-phase paradigm with auditory stimuli. In four studies, we manipulated instructions in a free-choice learning phase, either saying nothing at all about tones that followed actions, or emphasizing their relationship in terms of actions and outcomes. All experiments used a free-choice acquisition phase, as previous research suggests that action-outcome relations are more strongly acquired and subsequently used (Herwig et al., 2007; Pfister et al., 2011). Given the complexity of obtaining clear and reliable ideomotor effects, and in order to gain more insight in what is learned in the acquisition phase, we employed three different ideomotor tests in four separate experiments. In Experiment 1, we used a free-choice test phase, as earlier work has suggested that ideomotor effects are most likely to occur under such conditions (e.g., Pfister et al., 2011). However, as the free-choice ideomotor test is - by definition - open to influences of conscious deliberation and choice, we follow up in Experiment 2, 3a, and 3b with a forced-choice ideomotor test. While Experiment 2 used a 2-block design where participants received opposite instructions on the different blocks that forced them to react to outcome stimuli either in line with the acquired action-outcome mapping, or the opposite mapping, Experiment 3a and 3b used an interference paradigm with imperative cues (presented together with outcome stimuli) to force people's choice on trial level. These forced-choice ideomotor tests would provide stronger evidence for the automatic initiation of actions than the free-choice test, with Experiment 3a and 3b being the least susceptible to alternative explanations. As such, the current line of experiments not only tests, but also aims to verify the automatic nature, of potential ideomotor actions arising from spontaneous ideomotor learning.

Experiment 1: Free-Choice Ideomotor Test

Method

Participants and design

Sample sizes on previously published ideomotor learning studies which varied from 12 (e.g., Kühn et al. 2009, Experiment 1) to 20 participants per condition (e.g., Herwig and Waszak 2009, Experiment 1-3), and given the fact that small sample sizes can counterintuitively inflate effect size, we decided prior to data collection to test at least 20 participants per condition in each experiment.

Fifty participants took part in the experiment in exchange for a small monetary payment or extra course credits. Participants with attention-related disorders or those who were on related medication were excluded beforehand. The experimental design consisted of one between-subjects factor: Instructions (No-Instructions vs. Instructions). After signing the informed consent, participants were randomly assigned to either the No-Instructions condition or the Instructions condition.

Data of one participant were lost because of a technical issue, and two participants were excluded due to the unbalanced proportion of key presses during the acquisition phase (outside of the range of a left-to-right ratio of 40% to 60%), which was defined before data collection. Data of the remaining 47 participants (No-Instructions condition: n = 23, Instructions condition: n = 24) were included in the analyses (37 females, mean age: 24 years [18 – 30 years], no left-handed and 2 ambidextrous participants).

Procedure

Participants were told that they would perform two tasks on a computer and were asked to read the instructions carefully. The present study used the same design as the third experiment of Elsner and Hommel (2001), consisting of an acquisition phase and a test phase. Both phases featured a Go – No-Go paradigm, and the auditory stimuli following responses in the acquisition phase (i.e., a low tone (400Hz) and a high tone (800Hz)) were presented again in the test phase upon which participants were to freely choose a left or a right response. After the acquisition phase, they continued with the second task (i.e., the test phase).

After the two main phases, participants filled out a short questionnaire that tested their knowledge about Response-Outcome mappings acquired in the learning phase and measured the representation levels on four hierarchically different levels of self-causation (i.e., association, prediction, causality, and agency level of Response-Outcome relations, see below) to check whether the instructions induced the desired processing goals differently. Response-Outcome mappings were counterbalanced among the participants. That is, for half of the participants, the left key was followed by the high tone and the right key by the low tone (Response-Outcome mapping A), whereas for the other half, the opposite key-tone mappings (Response-Outcome mapping B) were used.

Acquisition Phase

After general task instructions, all participants read the following specific instruction for the acquisition phase:

“In this part you have to press a key with your left or right index finger, depending on the instructions on the screen: If you see "<<>>", you can choose yourself to press the left key ("z"), or the right key ("/"). You can choose freely, but try, on average, to press left and right equally often. If you see "xxxx", however, you should not press any key.”

Participants in the Instructions condition were then given detailed additional information about the R-O mappings – which depended on the counterbalancing of the mapping – and were provided with processing goals through descriptions of the relationship between the responses and their outcomes in ascending levels of self-causation (i.e., from associative, predictive, to causal) in the acquisition phase:

“Pressing your left key is associated with a High [Low] tone and pressing your right key with a Low [High] tone. This means that upon pressing your left key you can predict a High [Low]

tone and upon pressing your right key a Low [High] tone. In other words: pressing your left key causes a High [Low] tone and pressing your right key causes a Low [High] tone."

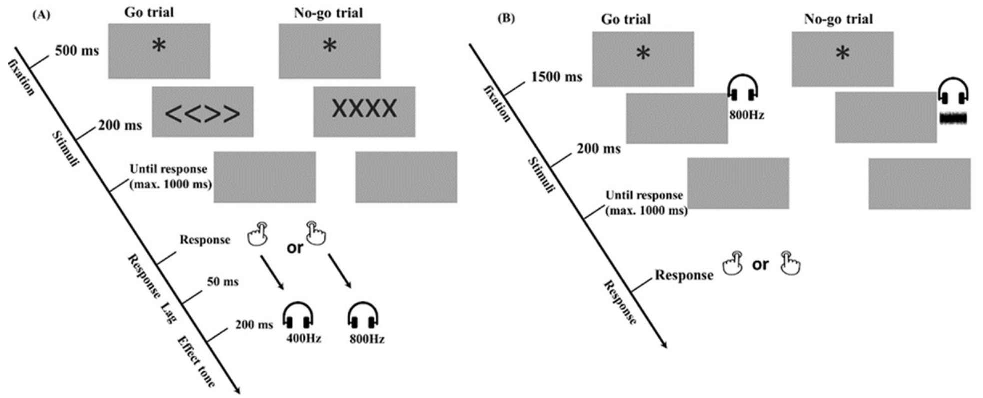
It is important to keep in mind that in the No-Instructions condition the tones are just stimuli that consistently followed key-presses, without any related mention about the occurrence of the tones, and that in the Instructions condition the stage was set for processing the tones as outcomes of self-chosen actions.

The trial procedures of the acquisition phase are depicted in Figure 1(A). Each trial of the acquisition phase started with a fixation asterisk (*) for 500 milliseconds (ms) on the middle of the screen, followed by a 200-ms Go (i.e., "<<>>") or No-Go (i.e., "xxxx") signal. Participants were asked to press the left or right key freely as soon as they saw the Go signal and were asked not to respond in No-Go trials. The program waited up to 1,000 ms for a response. On Go trials, reaction times over 1,000 ms were treated as omissions and responses faster than 100 ms as anticipations. Only reaction times in the valid range (100 – 1000 ms) triggered the contingent tone, which started after a 50-ms lag from the onset of the key-press and was presented for 200 ms. Incorrect trials (i.e., omissions, anticipations, and responses to No-Go trials) were recorded, and were signaled to the participant by a 1000-ms warning messages on the screen saying: "too slow", "too fast", or "No-Go trial, respectively. All incorrect trials were repeated in random order by the end of the first task. Participants had to redo all the incorrect trials until all required responses were valid.

The acquisition phase consisted of 3 practice trials and 300 valid trials, divided into 10 blocks. Every two blocks, there was a 10 seconds break, during which participants were informed about how often they had pressed the left and right keys. In the Instructions condition, the extra processing information about the Response-Outcome mappings was also repeated (e.g., "Each specific key causes a specific tone. The left key causes a High tone and the right key causes a Low tone").

Figure 3.1

Procedure of Experiment 1.



Note. (A) acquisition phase for all the experiments. (B) free choice ideomotor test phase of Experiment 1. Note: in the present example of the acquisition phase, the left response is always followed by a low tone (i.e., 400 Hz); whereas the right response is always followed by a high tone (i.e., 800 Hz), but these mappings were counterbalanced.

Test Phase

The test phase was similar to the acquisition phase, also using the Go – No-Go paradigm. This time, however, two tones that previously served as outcomes were presented as cueing stimuli (see Figure 3.1 B). Participants were instructed to press the left or right key randomly in response to the tone. In addition, as suggested by Elsner and Hommel (2001), to add response uncertainty and prevent participants from responding before the tone appeared, a novel sound (i.e., a 200-ms white noise signal) was presented in one third of the test trials, serving as a No-Go signal after which participants were to withhold their response. Each test trial started after an inter-trial interval of 1,500 ms with an asterisk on the center of the screen, followed by a 200-ms sound (i.e., a high tone, a low tone or a white noise signal), which were presented in a random order. Then the program waited up to 1,000 ms for an appropriate response. Response omissions and anticipations were defined in the same way as in the acquisition phase. However, this time no error message was presented and participants worked through 6 practice trials and 288 valid trials, divided into 8 blocks, including 96 No-Go trials in total. Again, every two blocks, there was a 10 seconds break. This time no extra information about the Response - Outcome mappings was provided during the break.

Manipulation Check of R-O Mappings

After the test phase, participants answered two questions that tested their knowledge about the relationship between the responses (i.e., left / right key presses) and the corresponding outcomes (i.e., low / high tones) in the acquisition phase, to check whether participants were able to report which tone followed which response. There were four answer options to each mapping question. For instance, when asked: “Which tone did the left key press produce?”, response option were: 1) the left key press produced the High tone, 2) the left key press produced the Low tone, 3) the left key press produced both tones, 4) the left key press was irrelevant to the tones” (see Appendix in Supplementary materials for more details).

Manipulation Check of Instructions

Subsequently, participants filled out a questionnaire designed to measure changes in the representation of the response-outcome relations as a result of the instructions manipulation. The questionnaire probed the four levels of the hierarchical representation used in the Instructions condition (i.e., association, prediction, causality, and agency level of Response-Outcome relations). Specifically, for each level, three items probed representations using a 9-point scale. The complete questionnaire can be found in the Appendix a (see questionnaires, p.117). A difference between instruction conditions on these measures would indicate that the manipulation changed the way in which participants represented the response-outcome relations.

Data Analysis Plan

Data were analyzed using R 3.5 (R Core Team, 2020). Visualizations of raw data points were built with the raincloud plots (Allen, Poggiali, Whitaker, Marshall, & Kievit, 2018). ANOVA's were calculated using the aov_ez function and Type III sums of squares (afex package Version 0.22–1 in R) (Singmann, Bolker, & Westfall, 2015). When assumptions of sphericity were violated Greenhouse-Geisser (GG) correction was utilized in the ANOVA model. In this case, we reported uncorrected degrees of freedom and corrected p-values. To further draw conclusions about the support of null effects, we also calculated Bayesian factors (BFs) with the default prior setting in JASP (version 0.9, JASP Team 2020) (van Doorn et al., 2021). The advantage of BFs is that it quantifies evidence in favor of one (e.g., null) hypothesis compared to another (e.g., alternative) hypothesis given the observed data.

Results

Acquisition Phase

First, we excluded all acquisition trials with anticipations (No-Instructions: 0.01%, Instructions: 0.01%) and omissions (No-Instructions : 0.04%, Instructions: 0.09%). Failures to withhold responses on the No-Go trials were calculated and all participants fell below the pre-set criteria of less than 20% (No-Instructions: 2.89%, Instructions: 2.55%). After that, response proportions (left/right keypress) were calculated. To make sure the participants had followed the general instruction to press the left and right key randomly but equally often, participants with proportions outside the 40% to 60% range were excluded (see section 2.1.1.). The mean left/right response proportions were equal in each condition – No-Instructions condition: 49.9% vs. 50.1%; Instructions condition: 49.6% vs. 50.4 %.

The mean RTs of the participants did not differ between the No-Instructions, M = 362.94 ms, SD = 60.24 ms, and Instructions condition, M = 362.41 ms, SD = 39.01, F(1, 45) = 0.00, p = .97. The mean RTs of right responses, M = 360.75 ms, SD = 52.40 ms, were marginally faster than the mean RTs of left responses M = 364.59 ms, SD = 48.49 ms, F(1, 45) = 2.87, p = .10. This difference was not qualified by an interaction with the between-subjects factor Instructions, F(1, 45) = 0.75, p = .39.

Test Phase

Test trials with response anticipations (No-Instructions: 0.05%, Instructions: 0.06%) and omissions (No-Instructions: 1.31%, Instructions: 0.91%) were excluded from data analysis and the percentage of responses that were consistent with the previously acquired Response-Outcome mapping was calculated for each participant.

As expected, in the No-Instructions condition the mean proportion of consistent responses was significantly larger than chance (i.e., 50%), M = 61.49%, SD = 22.61% , t(22) =

2.44, p = .012 (one-tailed), Cohen's dz = .508, and the Bayesian one sample T-Test resulted in BF+0 = 4.80, which means that the data are approximately 4.8 times more likely to occur under H+ (i.e., proportion in consistent condition is higher than chance level, that is, larger than 50%), than under H0 (i.e., proportion in consistent condition is at chance level). This result indicates moderate evidence in favor of H+. The same effect was observed for the Instructions condition: M = 69.98%, SD = 25.42%, t(23) = 3.85, p = .0004 (one-tailed), Cohen's dz = .786, and the Bayesian one sample T-Test result is BF+0 = 83.90, which indicates strong evidence in favor of H+. Finally, we tested whether instructions affected the proportion of consistent responses, but the direct comparison between the two conditions did not reveal any significant difference, t(45) = -1.21, p = .23 (two-tailed), and the Bayesian Independent samples T-Test result equals (BF01) 1.91, which only slightly favors the null hypothesis (H0: The Instructions condition has no effect on response preference) over the alternative hypothesis (H1: the Instructions condition biases response selection). In sum, while there was very strong support for an ideomotor effect in the Instructions condition and substantial evidence in the No-Instructions condition, evidence for no difference between Instructions conditions was only anecdotal (see Figure 3.2 for distribution).

Furthermore, we compared RTs for consistent and inconsistent trials in both Instructions conditions. There was no difference between consistent (M = 502.42 ms, SD = 95.89 ms) and inconsistent trials (M = 503.64 ms, SD = 99.22 ms) in the No-Instructions condition, t(22) = -0.14, p = .894; nor between consistent (M = 510.68 ms, SD = 70.11 ms) and inconsistent trials (M = 506.64 ms, SD = 69.98 ms) in the Instructions condition, t(21) = 0.77, p = .453. The corresponding BF also indicates moderate evidence for the null hypothesis (H0: The reaction times are not different between consistent and inconsistent trials) over the alternative hypothesis (H1: The reaction times are different between consistent and inconsistent trials) in No-Instruction condition (BF01 = 4.535) and Instruction condition (BF01 = 3.448), respectively.

Manipulation Check of R-O Mappings

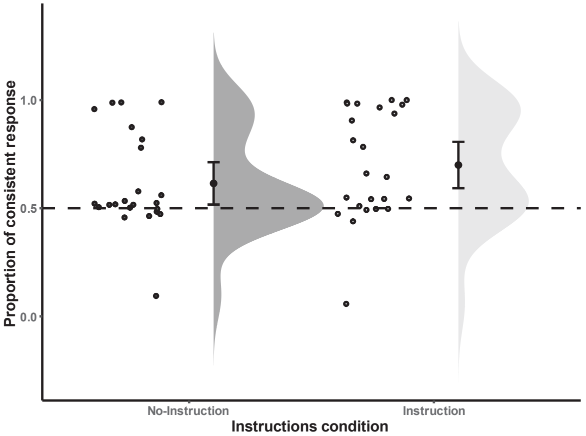
Most participants (85% in total) were able to explicitly report the correct mapping of responses and subsequent stimuli they were exposed to in the acquisition phase. Six people missed the response-stimulus mapping in the No-Instructions condition (6 out of 23), and only one participant failed in the Instructions condition (1 out of 24).

Manipulation Check of Instructions

In order to assess whether there were differences in how people represented the relation between responses and outcomes in the acquisition phase, the average of each of the three questions measuring association, prediction, causality, and agency was calculated. The mean scale ratings were analyzed as a function of Instructions conditions and as a function of representation level (i.e., the hierarchical levels explained before). Only a main effect of representation level was found, F(3, 135) = 7.97, p[GG] < .001, η_p^2 = .15, which merely showed that collapsed over Instructions conditions, there were significant differences in ratings between the four level of representation constructs (Table 3.1 and see Appendix a presents more details of the responses to the scales).

Figure 3.2.

Distribution of the proportion of consistent responses of all the individual data points in Experiment 1.



Note. Error bars represent the 95% confidence intervals.

Table 3.1.

Means and Standard deviations of the four different representation levels collapsed over Instructions conditions for all three experiments.

Representation level	Exp1: free	Exp2:block-based	Exp3: trial-based
Association	7.45 ± 2.18	6.90 ± 2.63	7.55 ± 2.20
Prediction	6.67 ± 2.86	6.99 ± 2.73	7.32 ± 2.53
Causality	6.99 ± 2.78	7.03 ± 2.75	7.60 ± 2.23
Agency	6.26 ± 2.92	6.16 ± 3.10	6.83 ± 2.75

Note. That the means of each scale is relatively high, indicating that in both the Instructions and No-instructions condition participants processed the learning task in line with the Instructions manipulation.

Discussion

These results provide support for an ideomotor effect, in the sense that tones followed responses in the acquisition phase were more likely to evoke these responses in the test phase. Moreover, this effect occurred regardless of instructions about the relation between responses and tones, which demonstrates that ideomotor learning – at least in the current paradigm – unfolded spontaneously.

Although the ideomotor effect was observed within both instruction conditions, it appeared more pronounced in the instructions condition. Bayesian tests, however, revealed slightly more support for the absence of difference between the two conditions. While it cannot be ruled out that instructions can strengthen ideomotor learning, it is clear that instructions were not necessary for learning to occur in the acquisition phase. This finding is further corroborated by an absence of a difference in the representation-level checks.

While the observed ideomotor effect obtained in the test phase seems comparable in size with other ideomotor studies (c.f., Elsner & Hommel, 2001), the free choice test phase does not provide strong evidence for the automatic nature of the effect (i.e., that the responses are triggered automatically by the stimuli that served as outcomes in the acquisition phase), as this task allows for deliberate responses in the test phase as well. On closer inspection, the response data show a bimodal distribution, with the majority of people responding at chance level and a considerable amount of people demonstrating a very large bias, with some participants showing near perfect consistence with the mapping acquired in the acquisition phase. This could suggest that the observed effect was not so much produced by the tones triggering the corresponding actions in the test phase, but by some people deliberately responding in line with the mapping learned in the acquisition phase. We return to this issue in the general discussion.

To rule out these more deliberate sources of the compatibility effect and to investigate whether spontaneously learned action-outcome associations can cause outcome stimuli to trigger ideomotor action directly, Experiments 2, 3a, and 3b used a forced-choice task, in which responses required by imperative cues or instructions were accompanied by tones that – according to the mapping learned in the acquisition phase – should trigger either compatible or incompatible responses. While compatible and incompatible trials were presented in separate blocks in Experiment 2, they were intermixed in Experiments 3a and 3b.

Experiment 2: Block-Based Interference Ideomotor Test

In Experiment 2, we used a block-based interference ideomotor test in which participants completed two test blocks. In the compatible block, participants received instructions to respond to tones that were compatible with the earlier acquired mapping. In the incompatible block, the instructions were reversed. The order of the two test blocks was counterbalanced across participants. We expected to observe significantly reduced RTs and lower error rates in compatible blocks compared to incompatible blocks.

Method

Participants and Design

Fifty participants took part in the experiment in exchange for a small monetary payment or extra course credits. Participants with attention related disorders or those who were on related medication were excluded beforehand. Participants were randomly assigned to a cell of the 2 (Instructions: No-Instructions vs. Instructions) * 2 (Compatibility: Compatible vs. Incompatible) mixed factorial design, with Compatibility as a within-participants variable. The order of the compatible and incompatible blocks was counterbalanced across participants.

Three participants were excluded due to the unbalanced proportion of key pressing during the learning phase, that is, the balanced left-to-right key ratio (i.e., 40% - 60%). Data of the remaining 47 participants (No-Instructions condition: n = 24 vs. Instruction condition: n = 23) were analyzed in the test phase (23 Females, mean age: 22 years [18 – 31 years], no left-handed and 2 ambidextrous participants).

Procedure

The procedure was similar to Experiment 1. After finishing the unchanged acquisition phase, participants came to the interference ideomotor task with the compatibility manipulated on the block level. With regard to the acquired R-O mapping, the response rule participants received on one block was compatible, whereas on the other block it was incompatible. For example, if the participant got the R-O mapping A (left key – high tone, right key – low tone), the compatible block meant that participants were asked to press left key when hearing a high tone, and right key for a low tone; while the response rule in the incompatible block was reversed, that is, pressing left key for a low tone, and right key for a high tone.

Acquisition Phase

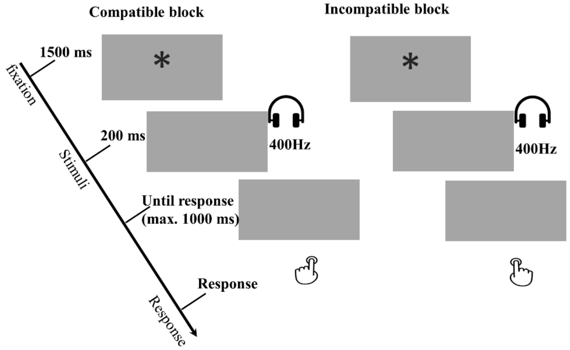
The acquisition phase was as identical as the first task of Experiment 1 (see Figure 3.1 panel (A)).

Test phase

Both the compatible and the incompatible block, consisted of 4 sub-blocks of 24 trials (see Figure 3). The order of the blocks was counterbalanced between participants. Each trial began with a 1500-ms fixation with an asterisk (“ * ”) centered in the screen, and then one of the two effect tones (i.e., the one learned in acquisition phase) was presented for 200 ms. The program would wait up to 1,000 ms to accept a response. On the first block, participants were instructed to respond according to either the compatible or incompatible response rule. Before switching to the second block with the opposite rule of responding, participants had to perform two example trials in which the responding requirements where explained as well as four practice trials without any clues.

Figure 3.3.

Examples of compatible and incompatible conditions in the Block-based interference test phase of Experiment 2.



Note. In these examples a low tone of 400Hz was mapped to a left response. All other combinations were possible, but are not presented in this figure. During the task, participants were asked to respond to tones directly based on the response rule, and the orders between compatible and incompatible are counterbalanced between participants. The compatible and incompatible blocks are defined depending on the Response-Outcome mapping in the acquisition phase.

Manipulation Check of R-O mappings

The questions were the same as in Experiment 1.

Manipulation Check of Instructions

The questionnaire was the same as in Experiment 1.

Results

Acquisition Phase

Trials with response omissions (No-Instructions condition: 0.05 %, Instruction condition: 0.11%) or anticipations (No-Instructions condition: 0.05%, Instructions condition: 0.05%) were excluded. After that, response proportions (left vs. right keypress) were calculated for each group. The mean left/right response proportions were equal in each condition (No-Instructions condition, 50.2% vs. 49.8%; Instruction condition: 49.6 % vs. 50.4 %).

The mean RTs of the participants did not differ between the No-Instructions, M = 374.38 ms, SD = 33.98 ms, and Instructions condition, M =376.57 ms, SD = 37.73 ms, F(1, 45) = 0.04, p = .83. The mean RTs of right responses M = 375.82 ms, SD = 33.81 ms, were not faster than the mean RTs of left responses M = 375.09 ms, SD = 37.83 ms, F(1, 45) = 0.13, p = .72. There was also no interaction with the between-subjects factor Instructions, F(1, 45) = 0.92, p = .34.

Test Phase

Participants who failed to meet the response criteria in the acquisition phase were excluded (3 participants), Furthermore, this time there were no trials with response anticipations (No-Instructions condition: 0%, Instructions condition: 0%), and trials with omissions (No-Instructions condition: 1.60 %, Instructions condition: 1.22%) were excluded from data analysis.

Error Rates.

A 3-way mixed 2 (Instructions: No-Instructions vs. Instructions) * 2 (Order: Compatible First vs. Incompatible First) * 2 (Compatibility: Compatible vs. Incompatible) ANOVA yielded a main effect of Order, F(1,43) = 5.51, p = 0.02, ηp2 = 0.11. Neither the main effect of Instructions, F(1,43) = 0.04, p = 0.85, nor that of Compatibility, F(1,43) = 0.00, p = .97, was significant. No significant interaction effects between Instructions * Order F(1,43) = 0.99, p = 0.32, between Instructions * Compatibility, F(1,43) = 0.25, p = .62, or between Instructions * Order * Compatibility, F(1,43) = 0.02, p = 0.89, were found. Only a 2-way interaction between Order and Compatibility, F(1,43) = 4.36, p = 0.04, η_p^2 = 0.09, was found showing that the direction of the Compatibility effect was different for the two Order conditions. However, the Compatibility effect was not significant in the Compatible first condition, t(43) = 1.46, p = 0.15, nor was the Compatibility effect significant in the Incompatible first condition, t(43) = -1.49, p = 0.14.

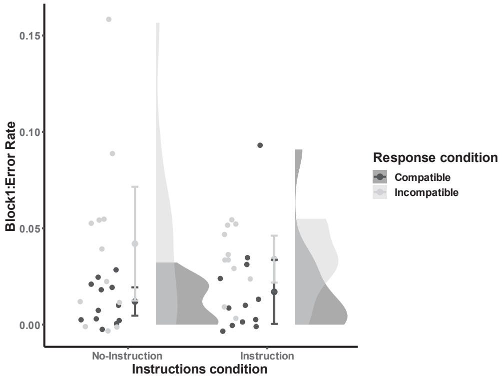
To further evaluate the evidence for the absence of a compatibility effect, the compatibility effect on error rates was calculated for all participants regardless of Instructions. If anything, errors showed a reversed compatibility effect (M_{CE} = -0.07324 , SD_{CE} = 0.042) and the independent T-Test results, t(46) = -0.012, p = 0.51(one-tailed), BF0+ = 6.373, indicated moderate evidence for the null hypothesis (i.e., there is no difference between compatible and incompatible condition, namely, CE = 0) against the one-sided alternative hypothesis (i.e., the incompatible condition has more error rates than the compatible condition, namely, CE > 0).

Previous research tested the compatibility effect in a between-subjects design with a non-reversal and reversal group (e.g., Elsner & Hommel, 2001, Experiment 1a, 1b). In such a design, there is only one test block and participants just receive a compatible or incompatible response rule. To perform a comparable analysis on our date we zoomed in on the first block only, with Compatibility as a between-subjects factor.

For the first block, we conducted a 2-way between-subjects ANOVA ($M_{\text{no-instruction\_compatible}}$ = 0.012, SD = 0.012; $M_{\text{no-instruction\_incompatible}}$ = 0.042, SD = 0.046; $M_{\text{instruction\_compatible}}$ = 0.017, SD = 0.026; $M_{instruction\_incompatible}$ = 0.034, SD = 0.018). The results found a significant effect of compatibility, F(1,43) = 7.47, p = 0.009, ηp2 = 0.15, but no main effect of Instructions, F(1,43) = 0.02, p = 0.90, nor an interaction, F(1,43) = 0.66, p = 0.42.

Figure 3.4.

Distribution of error rates as a function of Compatibility and Instructions of block 1 in Experiment 2.



Note. The compatible and incompatible trials are defined depending on the Response-Outcome mapping in the acquisition phase. Error bar represent the 95% confidence interval.

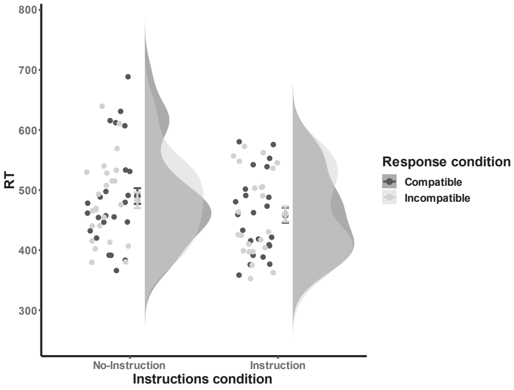
Reaction Times.

Mean RTs for correct trials were subjected to a 3-way 2 (Instructions: No-Instructions vs. Instructions) * 2 (Order: Compatible First vs. Incompatible First) * 2 (Compatibility: Compatible vs. Incompatible) mixed measure ANOVA, that along with the between-participants factor Instructions and the within-participants factor Compatibility also included the counterbalancing between-participants factor Order. No main effects of Instructions, F(1,43) = 1.67, p = 0.20, Order, F(1,43) = 0.03, p = 0.88, and Compatibility, F(1,43) = 0.18, p = .67, were found. Furthermore, the Instruction * Order, F(1,43) = 0.10, p = 0.75, Instruction * Compatibility, F(1,43) = 0.46, p = 0.50, and Order * Compatibility, F(1,43) = 0.65, p = 0.42, interactions were not significant, neither was the 3-way interaction, F(1,43) = 0.00, p = 0.99 (see Figure 3.5 for visualized distribution).

To further evaluate the evidence for the absence of a compatibility effect, the compatibility effect was calculated for all participants regardless of Instructions and Order. If anything, the compatibility effect was reversed, M_{CE} = -2.65 ms, SD_{CE} = 42.34 ms, and the independent T-Test results, t(46) = -0.43, p = 0.665, BF0+ = 8.54, provided relevant moderate evidence for the null hypothesis (i.e., there is no difference between compatible and incompatible condition, namely, CE = 0) against the one-sided alternative hypothesis (i.e., the reaction time in the incompatible condition is longer than the compatible condition, namely, CE > 0).

Figure 3.5

The response performance distribution of all the individual data points for each condition in Experiment 2.



Note. The compatible and incompatible trials are defined depending on the Response-Outcome mapping in the acquisition phase. Error bars represent the 95% confidence intervals.

To further explore the data we zoomed in on the first block only, with Compatibility as a between-subjects factor, comparable to earlier ideomotor research. The RTs were subjected to a 2-way between-subjects ANOVA ($M_{\text{no-instruction\_compatible}}$ = 492.46 ms, SD = 81.76 ms; $M_{\text{no-instruction\_incompatible}}$ = 475.46 ms, SD = 73.38 ms; $M_{\text{instruction\_compatible}}$ = 454.18 ms, SD = 57.94 ms; $M_{\text{instruction\_incompatible}}$ = 459.18 ms, SD = 72.06 ms). Again, no significant results were found, Instructions, F(1,43) = 1.69, p = 0.20; Compatibility: F(1,43) = 0.08, p = 0.78; Interaction: F(1,43) = 0.28, p = 0.60.

Manipulation check of R-O mappings

Not all participants (only 60% correct, 28 out of 47) were able to explicitly report the correct mapping of actions and outcomes they were exposed to in the acquisition phase. In the No-Instructions condition, 10 out of 24 participants failed, either forming a reversed R-O mapping, or randomly guessing the R-O mapping. The Instructions condition has similar pattern, 9 out of 23 participants missed the correct R-O mapping rule. This number may be lower than in Experiment 1, though, as the test phase also featured the opposite mapping, which may have confused participants.

Manipulation Check of Instructions

In order to assess whether there were differences in how people represented the relation between responses and outcomes in the acquisition phase, the average of each three questions measuring association, prediction, causality, and agency was calculated. The 2(Instructions condition: No-Instruction vs. Instructions) * 4 Representation level ANOVA only found a main effect of Representation level, $F(3, 135) = 4.25$, $p[GG] = 0.01$, $\eta p2 = .09$, which merely showed that collapsed over Instructions conditions, there were significant differences in ratings between the four level of representation constructs (Table 3.1 presents more details of the responses to the scales).

Discussion

The block-based compatibility paradigm only provided limited support for an ideomotor effect. While no effects on RTs were found, participants made more errors on incompatible than compatible trials, though only on the first block. With no difference between instructions, this effect on errors at first glance seems to replicate the finding of Experiment 1, that ideomotor learning occurs spontaneously, also in the absence of instructions.

This compatibility effect – especially in the first block – could, however, also emerge as a result of a task switch (Monsell, 2003) that required participants who started with the incompatible block to use a new mapping, whereas participants in the compatible condition could still rely on the mapping that was learned in the acquisition phase. This effect should be less pronounced – or non-existing – in the second block, as participants in both order conditions would have to switch mappings. Note that an ideomotor effect based on an R-O association forged in the acquisition phase would predict a compatibility effect on the second block as well, as participants who entered the compatible after the incompatible block would benefit from the automatic responses triggered by the primes.

Evidence for a within-participants compatibility effect, however, was not obtained. A closer inspection of the pattern revealed that while participants who moved from a compatible to an incompatible block made more errors on the second block, showing a classic compatibility effect, participants who moved from the incompatible to the compatible block also made more errors on the second block. This suggests that the switch in instructions from block 1 to block 2 created more errors, regardless of whether the new rule was compatible or incompatible with the acquisition phase. This may indicate that people simply struggled to switch to a new response rule.

In order to rule out this possibility Experiment 3a and 3b were conducted, in which the compatibility effect was tested at trial level. This time, participants were instructed to react to imperative cues, but were at the same time presented with stimuli that had followed responses in the acquisition phase. These stimuli should interfere with participants' responses if they are associated responses that are incompatible with the imperative cues. Such a trial-based interference ideomotor test would be the most rigorous test and cannot be regarded as a task-switch effect.

Experiment 3: Trial-Based Interference Ideomotor Test

Experiment 3a Trial-Based Interference Ideomotor Test

Method

Participants and Design.

Sixty participants took part in the experiment in exchange for a small monetary payment or extra course credits. Participants with attention-related disorders or those who were on related medication were excluded beforehand. The experimental design consisted of one between-subjects factor: Instructions (No-Instructions vs. Instructions), and one within-subjects factor: Compatibility (Compatible vs. Incompatible). After signing the informed consent, participants were randomly assigned to either the Instructions condition or the No-Instructions condition.

Data of one participant were lost because of a technical issue, and five participants were excluded due to the unbalanced proportion of key presses during the learning phase (outside of the range of a left-to-right ratio of 40% to 60%), which was defined before data collection. Data of the remaining 54 participants (No-Instructions condition: n = 25 vs. Instructions condition = 29) were analyzed in the test phase (35 females, mean age: 23 years, [18 – 37 years], 7 left-handed and 3 ambidextrous participants).

Stimuli and Procedure.

We used the same sounds as in Experiment 1, plus a standard Landolt “C” ring and its mirror image, as the target for the interference ideomotor task in the test phase. We selected these stimuli because they are clearly different form the arrow stimuli in the acquisition phase, making sure that imperative cues were not associated with responses (Muhle-Karbe & Krebs, 2012). Procedures were similar to Experiment 1, including an acquisition phase and a test phase.

Acquisition Phase.

The acquisition phase was as identical to the one used in Experiment 1 (see Figure 3.1 panel (A)).

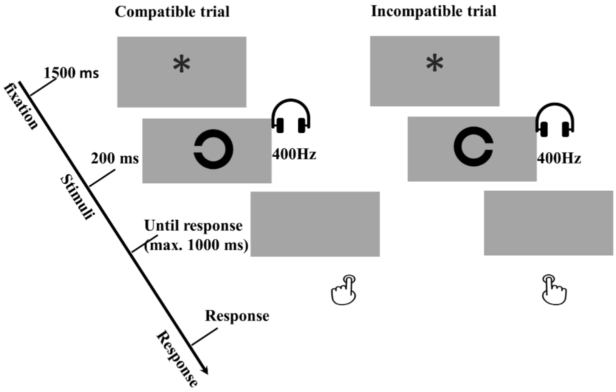
Test Phase.

In the test phase participants were asked to perform an interference task, namely, the compatibility task, consisting of eight main blocks of 24 trials. Each trial started with a 1500-ms fixation (“ * ”), and then one of the former effect sounds was simultaneously presented with the Landolt “ C ” (see Figure 6). The duration of the prime and the target were 200 ms and 250 ms, respectively. Participants were told to detect and respond to the opening direction of Landolt “ C ” ring as fast and accurately as possible. Pressing the left key (“ z ”) for a left opening, and the right key (“ / ”) for a right. The program waited up to 1,000 ms for a response. Response omissions and anticipations were defined in the same way as in the acquisition phase. There was no response feedback in the test phase.

Based on the R-O mapping in the acquisition phase, the test trials were categorized as a compatible trial when the to-be-executed response was the same as the response that was followed by the primed tone in the acquisition phase and incompatible trials when the to-be-executed response was the opposite of the response that was followed by the primed tone in the acquisition phase. For instance, if one had received the response – outcome mapping “left key – low tone, right key – high tone”, a trial was compatible when a left opening “ C ” ring was presented together with a low tone, and when a right opening “ C ” ring was presented with a high tone. A trial was incompatible when a left opening “ C ” ring accompanied by a high tone, and a right opening “ C ” ring with a low tone.

Figure 3.6.

Examples of compatible and incompatible conditions in the trial-based interference test phase of Experiment 3a and 3b.



Note. In these examples a low tone of 400Hz was mapped to a left response, which depending on the R-O mapping in the acquisition phase. All other combinations were possible, but are not presented in this figure. The main task is the orientation discrimination task with the tones as primes, and the compatible and incompatible are intermixed in trials level.

Manipulation Check of R-O Mappings.

The questions were the same as in Experiment 1.

Manipulation Check of Instructions.

The questionnaire was the same as in Experiment 1.

Data Analysis Plan.

Analyses were similar to Experiment 1, RTs and error rates in the test phase were analyzed as a function of Instructions and Compatibility conditions.

Results

Acquisition Phase.

First, we excluded all acquisition trials with anticipations (No-Instructions: 0.09%, Instructions: 0.09%) and omissions (No-Instructions: 0.05%, Instructions: 0.08%). The remaining mean error rate for the No-Instructions condition was 4.78%, whereas for the Instructions condition it was 3.63%. After that, response proportions (left vs. right keypress) were calculated for each group. The mean left/right response proportions were equal in each condition (No-Instructions condition: 49.6% vs. 50.4%; Instructions condition: 49.8% vs. 50.2%).

The mean RTs of the participants did not differ between the No-Instructions, M = 344.61 ms, SD = 51.34 ms, and the Instructions condition, M = 358.07 ms, SD = 43.48 ms, F(1, 52) = 1.10, p = 0.30. The mean RTs of right responses M = 349.00 ms, SD = 46.91 ms, were significantly faster than the mean RTs of left responses M = 354.67 ms, SD = 48.43 ms, F(1, 52) = 7.03, p = 0.01, ηp2 = 0.12. This effect was not qualified by an interaction with the between-subjects factor Instructions, F(1, 52) = 0.60, p = 0.44.

Test Phase.

Participants who failed to meet the response criteria in the acquisition phase were excluded (5 participants). Furthermore, trials with response anticipations (No-Instructions condition: 0.02%, Instructions condition: 0.036%) and omissions (No-Instructions condition: 0.0%, Instructions: 0.018%) were excluded from data analysis.

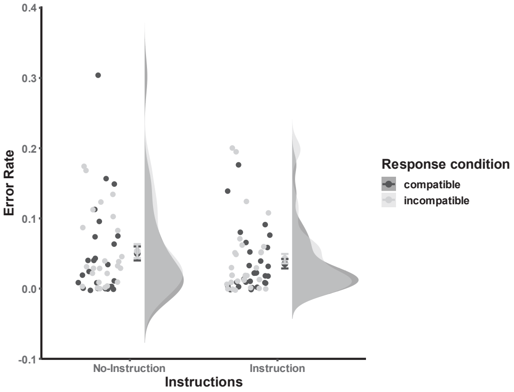
Error rates.

Error rates were analyzed based on all trials. As Figure 3.7 shows, participants were relatively accurate, and most of the error rates per condition were less than 10% ($M_{\text{no-instruction\_compatible}}$ = 0.050, SD = 0.070; $M_{\text{no-instruction\_incompatible}}$ = 0.053, SD = 0.054; $M_{\text{instruction\_compatible}}$ = 0.035, SD = 0.043; $M_{\text{instruction\_incompatible}}$ = 0.043, SD = 0.053). The 2(Instructions: No-Instructions vs. Instructions) * 2(Compatibility: Compatible vs. Incompatible) mixed ANOVA did not reveal any significant effects [Instructions effect: F(1,52) = 0.74, p = 0.39; Compatibility: F(1,52) = 1.67, p = 0.20; Interaction: F(1,52) = 0.23, p = 0.63].

Thereafter, in further exploratory analyses, we calculated the compatibility effect on error rates by collapsing over the Instructions factor (M_{CE} = 0.0054, SD_{CE} = 0.029). An independent T-Test, t(53) = 1.34, p = 0.09, BF0+ = 1.61, provided relevant moderate evidence for the null hypothesis (i.e., there is no difference between compatible and incompatible condition, namely, CE = 0) against the one-sided alternative hypothesis (i.e., the incompatible condition has more error rates than the compatible condition, namely, CE > 0).

Figure 3.7.

Distribution of error rates of Experiment 3a.



Note. The compatible and incompatible trials are defined depending on the Response-Outcome mapping in the acquisition phase. Error bars represent the 95% confidence intervals.

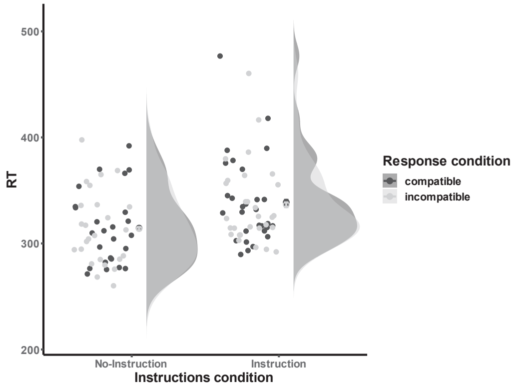
Reaction times

Reaction times (RTs) for remaining correct trials were aggregated over compatible and incompatible trials for each participant (see Figure 3.8, for visual distribution). Subsequently, the mean RTs and error rates were subjected to a 2 (Instructions: No-Instructions vs. Instructions) * 2 (Compatibility: Compatible vs. Incompatible) ANOVA, with Instruction as between and Compatibility as within-subjects factor. RTs analysis did not reveal a significant compatibility effect, F(1,52) = 2.26, p = 0.14. Neither the effect of interaction reached significance F(1,52) = 0.91, p = 0.34, but we found a main effect of Instruction F(1,52) = 5.23, p = 0.03, η_p^2 = 0.09, indicating that participants in the Instructions condition were overall slower to respond ($M_{\text{No-Instrucitons}}$ = 314.22 ms, SD = 34.57 ms; $M_{\text{Instruction}}$ = 337.66 ms, SD = 39.65 ms). If anything, RTs in the compatible condition, M = 327.57 ms, SD = 40.05 ms, were higher than the incompatible condition, M = 326.05 ms, SD = 38.33 ms, t(53) = 1.58, p = 0.06, BF01= 2.10.

To further evaluate the evidence for the absence of a compatibility effect, the compatibility effect (CE) was calculated for all participants regardless of Instructions, M_{CE} = -1.515 ms, SD_{CE} = 7.05 ms. A directional T-Test, t(53) = - 1.58, p = 0.94, BF0+ =16.31, provided strong evidence for the null hypothesis (i.e., there is no difference between compatible and incompatible condition, namely, CE=0) against the one-sided alternative hypothesis (i.e., the reaction time in the incompatible condition is longer than the compatible condition, namely, CE>0).

Figure 3.8.

The response performance distribution of all the individual data points for each condition in Experiment 3a.



Note. The compatible and incompatible trials are defined depending on the Response-Outcome mapping in the acquisition phase. Error bars represent the 95% confidence intervals.

Manipulation Check of R-O Mappings.

Most participants (nearly 91% correct, 49 out of 54) were able to explicitly report the correct mapping of actions and outcomes they were exposed to in the acquisition phase. Participants could still recall previous learned R-O mapping rule. In the No-Instruction condition, 4 out of 25 failed, and only one missed in the Instructions condition (n=29). Collectively, this suggests that participants indeed acquired R-O knowledge spontaneously, although this did not translate into automatic response priming in the test phase.

Manipulation Check of Instructions.

In order to assess whether there were differences in how people represented the relation between responses and outcomes in the acquisition phase, the average of each three questions measuring association, prediction, causality, and agency was calculated. The 2(Instructions condition: No-Instructions vs. Instructions) * 4 Representation levels ANOVA only found a main effect of Representation level, F(3,156) = 3.69, p[GG] = 0.03, ηp2 = .07, which showed that collapsed over Instructions conditions, there were significant differences between the questions of the four levels. among these four levels (See Table 3.1 for more details).

Discussion

The results in the present experiment did not reveal a compatibility effect in any of the two groups, suggesting that the presented outcomes did not trigger associated actions. The paradigm used, though, was designed as the strongest test for automatic action selection, with compatibility being manipulated at the trial level. In such a paradigm, compatibility effects have to arise at the trial level itself, if two stimuli evoke either the same or two conflicting responses. As far as we known, only two articles reported compatibility effects on trial level when using the classical two-phases paradigm (Kühn et al., 2009; Sato & Itakura, 2013). However, we were not able to replicate these effects regardless of whether we provided participants with instructions to pay attention to R-O mappings in the acquisition phase or not.

Experiment 3b: Replication

To make sure that the null findings in the rigorous test of Experiment 3a were not a false negative, we conducted a high-powered replication of the core part of Experiment 3b. Because of practical constraints we could not include the manipulation checks, but we assume based on the previous three experiments that most participants were aware of the correct mapping and that the instructions had no effect on the way participants represented the R-O relations.

Method

Participants and Design.

Two hundred and two participants (N= 202) took part in the experiment in exchange for a small monetary payment or extra course credits. Participants with attention-related disorders or those who were on related medication were excluded beforehand. The experimental design consisted of one between-subjects factor: Instructions (No-Instructions vs. Instructions), and one within-subjects factor: Compatibility (Compatible vs. Incompatible). After signing the informed consent, participants were randomly assigned to either the Instructions condition or the No-Instructions condition.

Data of ten participants were lost because of a technical issue, and six participants were excluded due to the unbalanced proportion of key presses during the learning phase (outside of the range of a left-to-right ratio of 40% to 60%), which was defined before data collection. Data of the remaining 186 participants were analyzed in the test phase (No-Instructions condition: a total of 90 participants, 63 female, age: M = 23 years, SD = 5; Instructions condition: a total of 96 participants, 70 female, age: M = 23 years, SD = 4).

Stimuli and Procedure.

We used the same stimuli and procedure as mentioned in Experiment 3a, except that the procedure only had an acquisition phase and a test phase. In this experiment the experimenter was also blind to the real research goals, and waited outside the testing room.

Acquisition Phase.

The acquisition phase was as identical to the one used in Experiment 3a (see Figure 3.1 panel (A)).

Test Phase.

The acquisition phase was as identical to the one used in Experiment 3a (see Figure 3.6)

Data Analysis Plan.

Analyses were the same as Experiment 3a, RTs and error rates in the test phase were analyzed as a function of Instructions and Compatibility conditions.

Results

Acquisition Phase.

First, we exclude all acquisition trials with omissions (No-Instructions:0.10%, Instructions: 0.08%) and anticipations (No-Instructions:0.15%, Instructions: 0.12%). The remaining mean error rates for the No-Instructions condition was 5.29%, whereas for the Instructions condition it was 5.26%. After that, response proportion (left vs. right keypress) were calculated for each group. The mean left/right response proportions were equal in each condition (No-Instructions condition: 49.8% vs. 50.2%; Instructions condition: 49.5% vs. 50.5%).

The mean RTs of the participants did not differ between the No-Instructions, M = 360.76 ms, SD = 52.70 ms, and Instructions condition, M = 356.10 ms, SD = 44.84 ms, F(1,

184) = 0.43, p = 0.51. The mean RTs of left responses M = 356.95 ms, SD = 49.96 ms, were significantly faster than the mean RTs of right responses M = 359.76 ms, SD = 47.69 ms, F(1, 184) = 4.91, p = 0.03, ηp2 = 0.03. This effect was not qualified by an interaction with the between-subjects factor Instructions, F(1, 184) = 0.05, p = 0.83.

Test Phase.

Participants who failed to meet the response criteria in the acquisition phase were excluded (6 participants). Furthermore, trials with response anticipations (No-Instructions condition: 0.0%, Instructions condition: 0.02%) and omissions (No-Instructions condition: 0.08%, Instructions: 0.11%) were excluded from data analysis.

Error rates

Error rates were analyzed based on all valid trials. Similar to the results in Experiment 2b, participants were relatively accurate ($M_{\text{no-instruction\_compatible}}$ = 0.0578, SD = 0.073; $M_{\text{no-instruction\_incompatible}}$ = 0.059, SD = 0.073; $M_{\text{instruction\_compatible}}$ = 0.0512, SD = 0.062; $M_{\text{instruction_incompatibl}}$e = 0.0588, SD = 0.090). We employed the same 2-way mixed ANOVA with Instructions as between-subjects factor (No-Instruction vs. Instruction) and Compatibility as within-subjects-factor (Compatible vs. Incompatible). Again, the results were not significant: Instructions: F(1,184) = 0.11, p = 0.74; Compatibility: F(1,184) = 2.20, p = 0.14; Interaction: F(1,184) = 1, p = 0.32.

Following the analyses in Experiment 3a, we also conduct the same Bayesian one sample T-test for the compatibility effect on error rates by collapsing over the Instructions factor ($M_{\text{error rates\_CE}}$ = 0.0046, $SD_{\text{error rates\_CE}}$ = 0.042). The corresponding BF indicates more support to the null hypothesis (i.e., there is no difference between compatible and incompatible condition, namely, CE = 0), t(185) = 1.516, p = 0.07, BF0+ = 2.13).

Reaction times

The mean RTs on each condition ($M_{\text{no-instruction\_compatible}}$ = 341.29 ms, SD = 56.13; $M_{\text{no-instruction\_incompatible}}$ = 341.34 ms, SD = 54.61; $M_{\text{instruction\_compatible}}$ = 339.36 ms, SD = 52.15; $M_{\text{instruction\_incompatible}}$ = 338.97 ms, SD = 53.62) were also subjected to the same 2 (Instructions: No-Instructions vs. Instructions) * 2(Compatibility: Compatible vs. Incompatible) ANOVA, with Instruction as between-subjects and Compatibility as within-subjects factors. No effects approached significance [Instructions: F(1,184) = 0.07, p = 0.79; Compatibility: F(1,184) = 0.04, p = 0.84; Interaction: F(1,184) = 0.07, p = 0.79].

To further evaluate the evidence for the absence of a compatible effect, the CE was calculated for all participants regardless of Instructions (M_{CE} = -0.179 ms, SD_{CE} = 11.49), and the Bayesian one sample T-test still give strong evidence for the null hypothesis (H0: CE = 0), t(185) = -0.213, p = 0.584, BF0+ = 14.34).

Discussion

The results in present experiment provide a powerful replication of the effects obtained in Experiment 3a, namely strong evidence for the absence of a compatibility effect and no effects of the Instruction manipulation.

General Discussion

Habits are often understood as actions that are automatically triggered by stimuli or situations through S-R associations resulting from repeated and consisted coactivation. In the present paper, we explored whether repeated and consistent coactivation of actions and effects can result in similar structures (R-O associations) by which mere perception of stimuli can then elicit the associated response (i.e., ideomotor action). Specifically, we investigated whether learning of R-O associations can occur spontaneously and whether as a result, these stimuli can automatically trigger associated responses. Accordingly, in four experiments, we tested automaticity in ideomotor learning in the standard two-phases paradigm that required participants to perform actions (pressing keys) that lead to specific outcomes (tones). In each experiment, we manipulated instructions in a free-choice learning phase, either making no mention in any way of the tones that followed actions, or induced a processing goal that explicitly emphasizing the relation between responses and the subsequent stimulus. In Experiment 1, evidence for ideomotor action was observed in a free-choice test phase, regardless of instructions. Experiment 2, 3a, and 3b, however, which employed forced-choice tasks to test for automaticity, provided little evidence for ideomotor effects. Together, these results don’t support the strong version of ideomotor theory. That is, they suggest that ideomotor learning can occur spontaneously, but that there are limits to the automatic effect on behavior.

Mixed evidence for Automatic Ideomotor Effects

The findings of Experiment 1 demonstrate that ideomotor learning can take place in the absence of explicit instructions that emphasize the relation between actions and outcomes. Although this finding matches with the literature on implicit learning (e.g., Cleeremans, et al., 1998) and may indicate that associations have been formed as a result of coactivation of response and resulting stimulus representations, this does not necessarily mean that learning occurred outside of awareness (Melnikoff & Bargh, 2018). Indeed, given the fact that the large majority of participants could indicate which outcome was produced by which action in the acquisition phase, and the relatively high scores on these R-O mapping checks in the no-instruction and instruction conditions, it seems to be the case that although learning was spontaneous and may have resulted in associations, the acquired knowledge was clearly propositional in nature (Mitchell et al., 2009). This effect on the R-O mapping checks was consistent across all experiments, although reports were understandably less accurate when the mapping was changed during the test phase in Experiment 2. In sum, while learning occurred spontaneously, it seems that participants had explicit knowledge about which action caused which outcome in the learning phase.

The results of the different test phases across our experiments at first seem liked a mixed bag. While Experiment 1 produced a healthy ideomotor effect consistent in size with the ideomotor literature (c.f., Elsner & Hommel, 2001), Experiments 2, 3 did not provide such evidence. An exception is the effect in Experiment 2 on error rates in the first block of trials of the test phase. Below, we entertain two possible explanations to reconcile these findings.

First, one could argue that on top of the reportable causal knowledge about the action outcome mappings, people did indeed form bi-directional associations, capable of producing ideomotor effects. This explanation is consistent with the findings of Experiment 1. In accordance with the strong version of ideomotor theory (Shin et al., 2010), merely hearing the tones during the free-choice task could have automatically triggered the associated responses, leading to more mapping-consistent responses. As the learning phases and explicit reports were quite similar in Experiments 2 and 3, one would have to assume, though, that the tones at least had the potential to trigger similar responses in the corresponding test phases. Maybe the null effects there could be explained by a lack of power. This seems unlikely, though, as the number of trials is comparable with other reports in the ideomotor literature and the failure to find an effect in the high-powered replication of Experiment 3 seems more in line with the absence of an effect. It could be the case that the test tasks in Experiments 2, as well as Experiment 3a and its replication, were somehow flawed and not able to pick up the ideomotor effect. This seems unlikely as well. The tasks were closely modelled after Elsner & Hommel (2001) and should theoretically have produced the ideomotor effect, at least according to the strong version of the theory.

A theoretical explanation for the null effects in Experiment 2 and 3, though, is that people were able to suppress or inhibit ideomotor responses in the test phase. It has recently been argued that automatic responding may emerge in some tasks, but be overruled in others in which people have the goal to inhibit such responses (Melnikoff & Bargh, 2018). Although the task instructions in the test phases of Experiments 2 and 3 did not explicitly ask people to ignore the tones, it may be the case that people tried to ignore them, or at least suppress responses in order to meet the task goal. That is, responding according to the dictated response rule (Experiment 2), or responding to the visual target (Experiment 3 and its replication). It could indeed be possible that people were able to inhibit ideomotor responses in the task and exactly cancel out the effect, without revealing an opposite inhibition effect, or were fully able to shut out the auditory stimuli in the compatibility tasks, but not the free choice task. However, we believe another explanation is more plausible.

This second explanation follows the opposite line of argument: that bi-directional associations were not formed, at least not strong enough for the tones to trigger responses in an automatic fashion. This would then require and explanation for the findings in Experiment 1. In this experiment, participants engaged in a free-choice task, which – by definition – allows for deliberate control of behavior. It may have been the case that explicit knowledge about the action-outcome relations drove the behavioral effects (Seabrooke et al., 2016). Loersch and Payne (2011) have noted that such biases can occur if primes affect the explicit knowledge that is retrieved and used as input for the decision-making process. Although this does not necessarily imply that participants were aware of this bias, it would entail an indirect priming effect that operates through biasing conscious decisions rather than by stimuli automatically triggering responses. Although this may suggest that people use knowledge of R-O mappings to freely select their actions, this would not be ideomotor action according to the strong version of the theory. Interestingly, though, such a process fits well with action control models that consider the preparation of human behavior to be rooted in sensorimotor processes that operate under radar of conscious awareness, while the ultimate execution of actions is under the control of a decision making process that selects actions associated with an act of conscious will (Aarts, 2012; Brass & Haggard 2008; Gold & Shadlen 2007; Zedelius et al. 2014).

Another explanation for the findings of Experiment 1 is that participants may have used the tones to fulfill the criteria of responding randomly and equally often with the two keys, or have chosen to respond with the keys suggested by the tones simply because it is easier. Random selection of responses is extremely hard and the tones may have provided an easy way out. Note that this explanation still assumes that people use the R-O knowledge that was spontaneously obtained in the acquisition phase. As a considerable number of participants responded consistent with the mapping of the acquisition phase on nearly 100% of the trials (and two individuals in close to 0% of the time, reflecting the use of a reversed mapping; see Figure 3.2), this seems a plausible explanation. Although papers in the ideomotor literature typically don’t provide information about the distribution of scores, the means and standard deviations in the present study are remarkably similar to earlier studies (e.g., Elsner & Hommel, 2001) suggesting that these studies may be open to the same explanation.

In Experiment 2, we found no within-participants compatibility effects, but did obtain a difference in error rates in the first block of the experiment. While this effect is consistent with the classic forced-choice effect (e.g., Elsner and Hommel, 2001, Experiments 1a, 1b), these effects could also be interpreted as a task-switching effect (Monsell, 2003). That is, in the light of the explicit knowledge about the R-O mapping in the acquisition phase, the instruction to use the opposite mapping to respond to the outcome stimuli in the test phase could have caused the increase in errors. Hence, the obtained compatibility effect may say more about the challenges of remembering and responding according to reversed task rules, than ideomotor effects. The complexity of obtaining ideomotor effects under forced-choice conditions (Herwig et al.,2007; Pfister et al., 2011), and the relative absence of ideomotor effects in the forced-choice task in the present study indicates that further inquiry is needed to specify when and how ideomotor learning effects emerge in the test paradigms employed so far.

Implications for Habits

Although ideomotor learning can create R-O associations, only weak evidence for the ideomotor effect was obtained. So based on the current data, it seems that S-R associations underlying habits function in a different way than the R-O learning that drove the ideomotor effect in our free-choice test phase in Experiment 1. This does not necessarily mean that ideomotor action should be discarded as a mechanism by which outcome stimuli can trigger responses, in a similar way as stimuli trigger habitual responses. As the ideomotor effect has been demonstrated across a large literature (although often with less strict tests than in the current experiments), it could be the case that the ideomotor effect holds, but that the learning phase in our experiments was too short for R-O associations to develop through co-activation, and that habit-like structures take longer to develop. Moreover, research on rewards in ideomotor learning has demonstrated that rewarding stimuli that follow responses produce much stronger ideomotor effects in free-choice or instructed compatibility tasks (Eder et al., 2020; Muhle-Karbe & Krebs, 2012). It may be the case that ideomotor learning is therefore more likely to occur in daily life, where stimuli following actions are rarely neutral. Interestingly, with this notion, the ideomotor effect becomes similar to the Pavlovian Instrumental Transfer (PIT) effect, which holds that stimuli associated with rewards are found to facilitate instrumental responses that have been followed by those rewards during learning (Watson & de Wit, 2018). Such a mechanism may reflect habitual responses that are still mediated by outcome representations at some level.

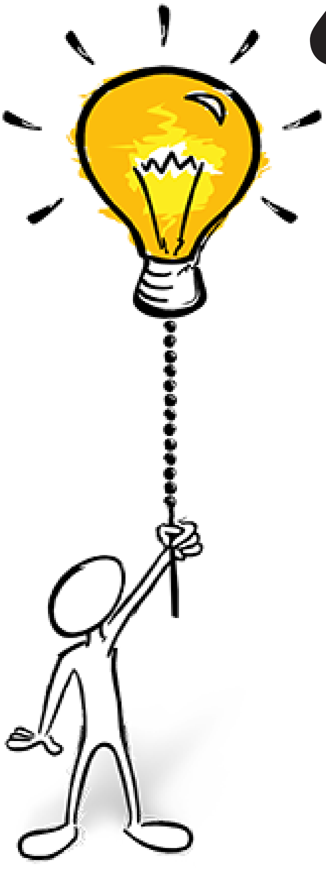
Further research is needed, though, to determine how rewards boost responses in the ideomotor and PIT paradigm. As ideomotor studies on this topic (Eder et al., 2020; Muhle-Karbe & Krebs, 2012) used a block-based compatibility paradigm, the enhanced effects could still be the results of explicit knowledge, as a result of propositional learning, interfering with conflicting task instructions. Relatedly, recent investigations into the nature of the PIT effect have demonstrated that the PIT effect itself is also dependent on propositional learning (Seabrooke et al., 2016; Seabrooke et al., 2017; Trick et al., 2011). As here it is also unclear whether rewards influence learning, response execution, or both, it is hard to predict whether the same results would emerge in a trial-based compatibility task, to provide strong evidence for the automaticity of ideomotor action.

Conclusion

Together, while the current findings do provide evidence for spontaneous ideomotor learning, it is less evident how resulting response-stimulus representations subsequently guide behavior. Rather than automatically facilitating responses, it may be the case that R-O knowledge affects behavior in a less automatic way. While primed outcomes may activate knowledge of associated actions (Bargh et al., 2001; Custers & Aarts, 2005, 2010), they may influence behavior indirectly by biasing conscious choice (see e.g., Custers et al., 2012). As such, responses following outcome primes may be more the result of biased choice than direct response priming. Given the parallels between ideomotor thinking and the study of habitual behavior, the current work suggests that research on habitual behaviors could benefit from more careful experimentation and theorizing (Marien et al., 2018, 2019) to help understand in which ways cues in the environment could elicit habitual behavior.

CHAPTER

4



Chapter 4:
Automaticity in Ideomotor Action? Moderators in the Two-Stage Paradigm

This chapter is based on: Sun, D., Custers, R, & Aarts, H. (2022). Automaticity in Ideomotor Action? Moderators in the Two-Stage Paradigm. *Manuscript under revision.*

Credit author statement:
DS: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data Curation, Writing-Original-Draft, Writing-Review & Editing, Visualization, Project administration. **RC:** Conceptualization, Data Curation, Writing-Review & Editing, Supervision, Project administration. **HA:** Conceptualization, Data Curation, Writing-Review & Editing, Supervision.

Abstract

Ideomotor theory assumes that merely activating the representation of the outcome of an action can trigger the action that produced it (i.e., ideomotor action). Supportive evidence comes mainly from the two-stage paradigm, in which actions are followed by specific outcomes in the acquisition phase, and are subsequently found to be facilitated by primes that share properties with those outcomes in a test phase. While previous research has shown that action-outcome relations can be acquired automatically (i.e., without any specific instructions) in the learning task, evidence for automatic ideomotor action in the test phase is less conclusive. Building on recent research by Sun and colleagues (2020) that provided evidence for ideomotor action, but not for automaticity, three moderators proposed in the literature were investigated to shed more light on this. While rewarding outcomes in the acquisition phase (Experiment 1) and presenting primes in a task-relevant modality in the test phase (Experiment 2) did not produce ideomotor action in a paradigm designed to test for automatic effects, presenting outcomes after responses in the test phase (Experiment 3) did produce an ideomotor effect. Implications for research and theorizing on ideomotor action are discussed.

Keywords: action control, automaticity, motivation, attention, memory

Introduction

Ideomotor theory proposes that merely activating the mental representation of an outcome can trigger the action that is associated with it (Carpenter, 1852; James, 1890). This mechanism is considered to explain various direct effects of perception on action such as mimicry (Iacoboni, 2009), behavior priming (Dijksterhuis & Bargh, 2001), goal-priming (Custers & Aarts, 2010), motor resonance during the dyadic interaction (Wilson & Knoblich, 2005) and even the existence of mirror neurons (Heyes, 2010). Evidence mainly comes from the two-stage paradigm in which participants first acquire action-outcome links by engaging in actions and observing their outcomes, and then engage in a test phase which measures whether actions are influenced by primes that share properties with their associated outcomes. While the literature provides numerous studies that present evidence *in line* with ideomotor theory (Shin et al., 2010), conclusive evidence for the automatic nature of this effect is lacking (see similar findings and discussion in Sun et al., 2020 and Vogel et al., 2020). Recently, Sun and colleagues (2020) have found that while learning of action-outcome relations can occur automatically (i.e., without instructions), strict tests for automatic ideomotor action in the test phase failed to provide evidence for automaticity.

In the present research, we address three possible moderators for automatic ideomotor effects. First, building on work suggesting that ideomotor effects are strengthened by rewards (Eder et al., 2019; Muhle-Karbe & Krebs, 2012), we explore whether outcomes may need to be rewarding for automatic ideomotor effects to occur. Second, we investigate whether the modality of the imperative (response) cues and the outcome primes in the test phase should be identical to render automatic ideomotor action more likely to occur. Finally, although ideomotor action is typically tested under extinction (i.e., without the responses producing the outcomes effects in the test phase), we examined whether adding outcomes in the test phase is a prerequisite for automatic ideomotor action.

Ideomotor Theory

The classic notion of ideomotor action regained popularity in the ‘90s when Prinz and colleagues raised the idea of “common coding” of perception and action (Prinz, 1990; 1997; Prinz et al., 2009), claiming that they share *the distal reference*. Perception and action were considered to rely on the same representations. This perspective eventually gave rise to the Theory of Event Coding (TEC; Hommel et al., 2001; Hommel, 2019). This seemingly all-encompassing theory has strong appeal and offers a very promising candidate for understand and examining the basic mechanisms underlying human goal-directed behavior. TEC introduced the term *Event Files*, referring to the shared and homogenous representations of actions and perceptions in a cognitive (or neural) system. It assumes that features of the stimulus in a specific environment (S; stimulus; context), a related response (R; decision, effector), and its following effects (E; perceptual and affective) are integrated into a distributed event file whenever actions are executed in a given context and its effects perceived. As a result, activating an effect code (or any features of E) would automatically initiate some motor activation though the bi-directional association (Elsner & Hommel, 2001) between the representations of actions and outcomes, thereby affecting action selection (R).

Crucially, TEC claims to offer a mechanistic explanation for goal-directed action. In contrast to theories of deliberate goal-setting (e.g., Gollwitzer, 1990; Locke & Latham, 1984, 1990), it does not assume any deliberation in the actor. Its goal is to be minimalist and parsimonious, providing a mechanistic model of goal-directed behavior and goal representations, without assuming "… any overhead, as common in many philosophically colored approaches,where representing a goal might imply having some understanding of the goal or some conscious awareness of it …" (Hommel, 2021, p.6.). According to this notion, a critical test for ideomotor action should provide evidence for automatic activation of the response upon activation of the outcome, in the sense that this activation is not the result of strategic or deliberate processes that rely on an explicit understanding of the relation between response and outcome.

Automaticity in Ideomotor Action and Alternative Explanations

Empirical evidence for ideomotor theory is mainly derived from the two-stage paradigm (for reviews, see Pfister, 2019; Shin et al., 2010; Stock & Stock, 2004), in which an IM effect is inferred from the observation that stimuli that previously served as outcomes in the acquisition phase facilitate the actions that preceded them in the test phase. Although numerous studies demonstrate such an IM effect, virtually none of them provide convincing evidence for the automaticity of IM responses in the test phase. That is, the vast majority of the studies using the two-stage paradigm either use a free-choice test or an instructed compatibility paradigm to test for ideomotor effects. In the first case, participants are asked to choose freely between two responses in the test phase, ignoring the primes that share properties with stimuli that previously served as outcomes in the acquisition phase. In the second case, participants react in the test phase to the stimuli that served as outcomes in the acquisition phase, according to between-subjects instructions that are either compatible or incompatible with the pairings of actions and outcomes in the learning phase. As Sun and colleagues (2020) have argued, there are alternative explanations for the IM effects obtained with these two test phases.

First, while the free choice test phase would indeed reveal an IM effect if primes in the test phase would automatically trigger the actions that produced similar outcomes in the leaning phase – as IM theory predicts – this task is also open to strategic effects. It has been demonstrated that participants are perfectly able to explicitly report the relations between actions and outcomes in the acquisition phase (Elsner & Hommel, 2001), even without instructions that encourage learning (Sun et al., 2020). Such explicit knowledge could be used as basis for response strategies in the test phase. For instance, participants could use this knowledge and respond in line with the learned relations if they think it is expected from them (i.e., demand characteristics), or just to make the task of "freely choosing" on each trial easier. In support of this explanation, Sun et al (2020) observed a clear and compelling bimodal distribution of the IM effect, where a small number of participants acted almost fully in line with the learned relations, while the majority of participants showed no IM effect at all. Thus, although the free choice testing phase would be able pick up genuine automatic IM effects, it does not rule out the possibility that the effect it reveals is the result of alternative mechanisms that are driven by explicit knowledge of action-outcome relations.

A similar case can be made for the instructed compatibility version of the test phase. In this version, one group of participants are instructed to respond to the outcome primes in the test phase either with compatible responses, that are in line with the pairings of actions and outcomes in the acquisition phase, and another group of participants are instructed to generate incompatible responses, that are exactly opposite in respect to the earlier pairings. Although in principle responding in the incompatible condition could be hampered by the outcome primes triggering the learned responses, it could also be the case that remembering or keeping the incompatible instructions in mind is more difficult compared to the compatible condition (Monsell et al., 2001). Thus, in this version of the acquisition phase, the automatic effects predicted by IM theory could be produced by well-known alternative mechanisms as well. As such, the instructed compatibility version is not a convincing test for automatic IM effects either.

In order to provide conclusive evidence for automatic IM effects Sun and colleagues (2020, Experiments 3a-b) recently conducted two high-powered experiments (N = 256). These experiments used the same acquisition phase that produced an IM effect in an experiment with a free-choice test phase (Sun et al., 2020; Experiment 1). In these Experiments 3a-b participants were given a cued-compatibility task as the test phase in which had to respond to imperative visual cues (indicating a left or right response), while they were at the same time exposed to outcome primes (low or high tones). Faster and more accurate responses on trials where cues and outcome primes were compatible (i.e., prompting the same response) versus incompatible would provide evidence for an automatic ideomotor effect. Contrary to a similar experiment by Elsner and Hommel (2001), which showed a significant IM effect with a limited number of participants (N = 24), these experiments failed to produce a significant IM effect. That is, despite the fact that the tones that served as primes in the test phase were learned to be produced by distinct responses in the acquisition phase, they did not elicit a significant response bias in the cued-compatibility test phase.

To conclude, we argue that the literature on ideomotor learning and ideomotor action using the two-stage paradigm, does not provide solid evidence for the idea that mere perception of outcomes triggers the actions that produced them before. More specifically, we maintain that the effects reported in the literature may not be the result of responses that are automatically triggered by the outcomes that are perceived on the trial level. We suggest that they could rather be produced by self-generated strategies at task level in the free choice task, or result from a difficulty to remember and implement instructions at task level in the instructed compatibility paradigm. Based on recent failures to find IM effects in a task designed to measure automatic response activation on a trial level, we report below a series of three experiments that aimed to examine three moderators suggested in the literature that may be required for automatic IM effect to occur.

Experiment 1: Reward as moderator?

The extent to which primes related to action-outcomes trigger associated actions has been demonstrated to be related to the rewarding properties of the outcome (Cartoni et al., 2016). Recent studies investigating ideomotor action in the two-stage paradigm have tested the idea that rewarding outcomes could promote the associations between actions and contingent sensory effects, which then may influence action selection in an automatic fashion (Eder et al., 2019; Muhle-Karbe & Krebs, 2012).

In the studies by Muhle-Karbe and Krebs (2012), participants responded to stimuli in the acquisition phase and observed the outcomes of their responses. For half of the learning trials, outcomes were followed by a reward. In the forced-choice test phase, a significant IM effect was obtained in the non-rewarding condition, but not in the rewarding conditions (Muhle-Karbe & Krebs, 2012). Eder and colleagues (2019) used a repeated two-stage paradigm in which four action-outcome relations could be acquired and tested within several two-stage blocks. In each block, participants were instructed that remembering two of the four action-outcome relations would be rewarded. Using a short instructed-compatibility test phase in each block, they found a significant IM effect only for the action-outcome relations that were rewarded. Although this demonstrates that instructions and rewards could lead to selective learning (cf. Eitam et al., 2009; Qin et al., 2021; Theeuwes et al., 2015; Tibboel & Liefooghe, 2020), the instructed compatibility paradigm does not provide evidence for a genuine automatic IM effect.

In the current experiment, we modified the paradigm as Sun and colleagues (2020), to match the designs by Muhle-Karbe and Krebs, (2012) and Eder et al. (2019). That is, during the acquisition phase, both responses were either followed by a distinct rewarding outcome or by a distinct non-rewarding outcome. Effects were then tested in the exact same cued-compatibility test phase used by Sun and colleagues (2020). To maximize the chance of obtaining an automatic IM effect, rewarded outcomes were presented 80% of the time compared to non-rewarding outcomes (20%). Thus, the experiment was not designed to establish an effect of reward over and above response frequency but rather to establish an automatic IM effect at all.

Method

Determining sample sizes of experiments

The original study (Sun et al., 2020, Experiment 3a-b) with a large sample size (N = 256) used a cued-compatibility testing phase in which participants were instructed to respond to two novel visual stimuli as fast and accurately as possible, while they were simultaneously exposed to the stimuli (tones) that served as outcomes in the acquisition phase. However, no IM effect was obtained. We identified two published studies (Eder et al., 2019; Muhle-Karbe & Krebs, 2012) that had similar design as our study, and only one of these two studies provided enough information to estimated effect sizes. More specifically, Eder and colleagues (2019) provided effect sizes values of the compatibility effect under different types of rewarding conditions. The effect size for the rewarding conditions was Cohen's d = 0.38 and we expected that our target sample sizes of 45 would achieve 80% power to detect a compatibility effect, assuming an α of 0.05. The final sample sizes for the experiments varied somewhat based on scheduling constraints and missed or cancelled testing sessions. The priori t test power calculation was performed with G*Power 3 (Faul et al., 2007).

Participants

Forty-nine healthy participants (mean age = 23.6 years, range18-30; 15 males, 46 right-handed) were recruited using campus flyers, and we only provided cash as incentives to motivate them that their final payment depending on their task performance. All participants reported normal or corrected-to-normal vision and were naïve to the purpose of the study. Written informed consent was obtained from all participants before the experiment. The protocol for these experiments was approved by the ethics committee of the Faculty of Social and Behavioral Sciences, Utrecht University (FETC19-097).

Prior to data-collection, target sample size was pre-registered together with the study design, data-analysis plans, and experimental hypotheses on the Open Science Framework (https://osf.io/9cr57).

Stimulus and Apparatus

The experiment was programmed using E-Prime 2.0 (Psychology Software Tools Inc., Sharpsburg, PA, USA), and stimuli were presented on a 23-in monitor (refresh rate: 60 Hz, spatial resolution: 1920*1080 pixels). Participants sit individually in a cubicle at a viewing distance of approximately 65cm. Four auditory outcomes [1] (i.e., O1- O4) were presented during the whole experiments. The response – outcome mapping depended on the counterbalanced condition.

Design and Procedure

All participants had to complete the two-stage ideomotor task. That is, a free-choice acquisition phase in which actions were followed by outcomes, and a cued-compatibility test phase to measure the automatic triggering of actions. Furthermore, in the acquisition phase participants were told that they had the chance to win extra money if they passed the interleaved memory checking tasks. For the sake of exploration, at the end of the two-stage ideomotor tasks a short questionnaire was administered to assess the BIS/BAS scale (Carver & White, 1994), which was used to examine potential associations between individual reward responsiveness and the response compatibility effect of rewarding outcomes (see Muhle-Karbe and Krebs, 2012; for such evidence) . We also tested for explicit knowledge about the response-outcome mapping rule at the end of the experiment.

---

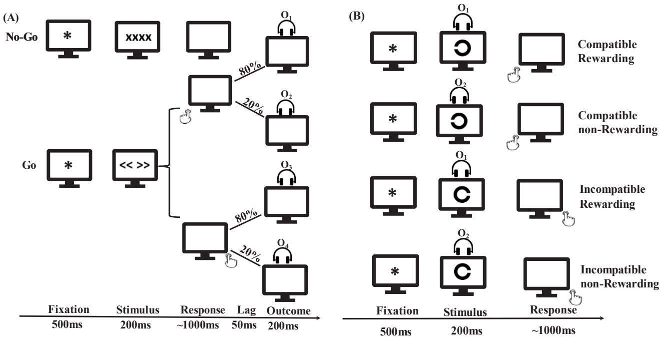
[1] We used the online generator (https://onlinetonegenerator.com/) to create the tones. Here, we picked two waveforms: since, sawtooth, underlying the 200 and 400 Hertz respectively. Then, we used Audacity to standardize the four tones in two steps: Firstly, we cut the duration to 250ms. Secondly, we multiple the envelop function on the sound wave to synthesis the stage.

Acquisition phase

Participants in the acquisition phase completed five blocks of 36 trials each. Figure 4.1A depicts the sequences of each trial. The procedure of the acquisition phase here closely matched the procedure of our previous lab setting, except the following modifications. Firstly, to familiarize participants with the rewarding sounds, they heard the two different sounds at least twice for each at the beginning of the experiment.

Figure 4.1

Procedure of the Rewarding Outcome Experiment 1



Note. (A) Procedure of the free-choice acquisition phase. A row of xxxx indicates a no-go trial, while a row of << >> indicates a free-choice trial that is followed by a tone (outcome). (B) Procedure of the forced-choice test phase. The Landolt “C” rings are the imperative response cues, accompanied by the outcome (tone) primes. Here, O1 means that it is a rewarded outcome, and appears with a high-weighted ratio in the acquisition phase (i.e., 80% of the go trials, the response will generate the O1 . O3 has the same meaning as O1.); whereas O2 represents that it is a pure non-valued outcome and appears with a low ratio in the acquisition phase (i.e., the remaining 20% of the go trials will generated O2. O4 has the same meaning as O2.). The mapping between response and outcome was counterbalanced among participants, and each response followed two kinds of outcomes with different proportion (R-2O).

Secondly, each valid response generated two types of the outcomes (i.e., R-2O). Specifically, 83% of the whole learning trials in the acquisition phase are “Go” trials, in which participant’s each correct response was linked to two types of the auditory outcomes (i.e., the rewarding sound or the non-rewarding sound). In these “Go” trials, followed by a 50ms time-lag of every correct response, one of the two mapped tones appeared for 200ms. For each response, the two mapped tones had unequalled weighted, with 80% of the go trials the rewarding tone was generated and 20% of the non-rewarding tone. Participants were instructed to press the left and right keys randomly and equally when seeing go signals and to figure out that which key was mapped to which types of the rewarding tone. Of the remaining 17% of the whole trials were “No-go” trials so that no responses were needed and no auditory outcomes. “No-go” trials were used to add the uncertainty and to avoid deliberate strategies in free-choice tasks, such as pressing the left / right keys in a predetermined order.

Thirdly, to keep participants more engaged and formulate the associations between correct responses and rewarding outcomes, every block was followed by a memory check task to strengthen the mapping between actions and rewarding tones. That is, after an inter-trial interval of 1,500ms, one of the two rewarding tones (O) appeared for 200ms, requesting a no-time-limit left or right choice (R). Participants would win 50 cents if they indicated the correct mapping. The mapping between responses and two outcomes (rewarding O and non-rewarding O) were counterbalanced among participants. To set the robust condition of rewarding effect, we also kept consistent memory check tasks during the acquisition phase (See the section of ‘the second- round memory check tasks’). Extra instructions were given before entering the acquisition phase. Specifically, the verbatim was:

“Sometimes, your response is rewarded, which will be indicated by a specific sound for each key. It is very important for you to remember the relation: which rewarding sound belongs to which key. The first task consists of five blocks, and we will check your memory for the relations between the responses and sounds after each block by playing the two rewarding sounds and asking you to press the corresponding button. You will WIN 50 cents for each correct answer in the first task. No money is lost if you are incorrect. Please work hard and keep the relations in mind. IMPORTANT: No money will be paid if you fail the memory check after each block.”

Test phase

The procedure of the test phase was the same as we did before. That is, as in the original study (Sun et al., 2020, Exp. 3a-b), an interference task was adopted per trial to measure the compatibility effect. Namely, participants were asked to respond to the direction of the Landolt “C” ring (thus serving as an imperative response cue presenting for 200ms), whereas the auditory events from the acquisition phase appeared simultaneously as outcome primes (See Figure 1B for the single trial procedure). Based on the response – outcome mapping in the acquisition phase, the test phase consisted of four practice trials and eight main blocks of 32 trials, yielding 256 trials, equally allocating two within-subjects factors included compatibility (compatible vs. incompatible) and rewarding condition (rewarding vs. non-rewarding).

Second round Memory Check Tasks

To increase the associations between responses and rewarding outcomes, participants were told that they would win more money (i.e., 100 cents) if they kept the relation in memory until they passed the second-round memory check after the second task (test phase). This time, the relationship between responses and non-rewarding tones of the acquisition phase were also measured.

Individual Reward Responsiveness

After the main task, a questionnaire was administered assessing the BIS/BAS scale to measure individual reward responsiveness (See Appendix for the full questionnaires). In brief, the BIS/BAS Scale consist of 20 items that can be allocated to two primary subscales: the Behavioral Inhibition System scale (BIS; 7 items) and the Behavioral Approach System scale (BAS; 13 items). Items are scored on a four-point-Likert-type scale (1= very true for me, 2 = somewhat true for me, 3 = somewhat false for me, 4 = very false for me). The BAS scale can be divided into three subscales: Fun Seeking (BAS-Fun; 4 items), Reward Responsiveness (BAS-Reward; 5 items), and Drive (BAS-Drive; 4 items).

Data Analysis Plan

We treated the data in the same way as Sun and colleagues (2020). In brief, we examined two dependent variables – reaction times (RTs) and percentage of error rates – as the function of two within-subject factors, namely compatibility (compatible vs. incompatible) and rewarding condition (rewarding vs. non-rewarding). The following exclusion criteria were applied: 1) To make sure the participants had followed the general instruction to press the left and right key randomly but equally often in the acquisition phase, participants with proportions outside the 40% to 60% range were excluded. 2) Participants whose error rates of over 20% in the acquisition phase were also excluded. 3) RTs over 1,000 ms were treated as omissions and responses faster than 100 ms as anticipations, which were excluded in the analysis of the acquisition phase and the test phase. That is, only correct and valid trials were included for RT analysis.

The remaining valid data were analyzed using R 3.5 (R Core Team, 2020). Basic statistical tests were calculated using the pipe-friendly package named rstatix (Kassambara, 2021). ANOVA's were calculated using the aov_ez function and Type III sums of squares (afex package Version 0.28 in R; Singmann et al., 2015). When assumptions of sphericity were violated Greenhouse-Geisser (GG) correction was utilized in the ANOVA model. In this case, we reported uncorrected degrees of freedom and corrected p-values. To further draw conclusions about the support of null effects, we also calculated Bayesian factors (BFs) with the default prior setting in JASP (JASP Team, 2020). The reporting of a BFs classification was based on work of van Doorn and colleagues (van Doorn et al., 2020). The advantage of BFs is that it quantifies evidence in favor of one (e.g., null) hypothesis compared to another (e.g., alternative) hypothesis given the observed data.

Results

Acquisition phase

First, all the participants followed the instructions and allocated their left / right pressing in the valid range, and they did well during the task with a low error-rates (M = 4.77%, SD = 3.08%). Trials with anticipation (0.014%) and omissions (0.48%) were excluded. The remaining valid trials were used to calculate the distribution of left- and right-hand presses. A paired-samples t test of proportion did not show a difference, t(48) = -1.126, p = 0.266, d = 0.16. There was also no difference between RTs of left press- and right-hand presses, as proved by the non-parametric paired two-samples Wilcoxon test, p = 0.790, d = 0.039.

Test phase

Participants that failed to meet the criteria of the acquisition phase were excluded (no one excluded). For RT analysis of the remaining participants, we excluded all trials with anticipation (0.024%) and omissions (0.39%), and only considered correct trials. For statistical analysis of RTs and error percentages, we conducted repeated-measures analyses of variances (ANOVAs), with two within-subject factors: compatibility (compatible vs. incompatible) and rewarding primes (rewarding vs. non-rewarding).

Error rates.

Neither the main effects (Compatibility: F(1,48) = 0.52, p = 0.48, ηp2 = 0.01; Reward: F(1,48) = 0.00, p = 0.99, $\eta_p^2 < 0.0001$) nor the interaction (F(1,48) = 1.62, p = 0.21, $\eta_p^2 = 0.03$) were significant (see Table 4.1 for mean error rates under each condition).

To further evaluate the support for the absence of a compatibility effect, we calculated the compatibility effect ($M_{error\_CE} = M_{error\_inc} - M_{error\_comp}$), with positive values reflecting a positive compatibility effect, for both types of reward trials. For rewarding trials, the compatibility effect M_{error_CE} = 1.1%, SD = 7.9%, t(48) = 0.98, p = 0.166, d = 0.14, BF0+ = 2.48, provided anecdotal evidence for the null hypothesis (i.e., H0, $M_{error\_CE} = 0$) against the one-sided alternative hypothesis (i.e., H+, $M_{error\_CE} > 0$). For the non-rewarding trials, with M_{error_CE} = 0.4%, SD = 7.3%, t(48) = 0.391, p = 0.349, d = 0.06, BF0+ = 4.62, moderate evidence for the null hypothesis was found. No difference between the two reward conditions was found, t(48) = 1.27, p = 0.105, d = 0.18, and if anything, the Bayesian analysis provided anecdotal evidence for the null (BF0+ = 1.70).

Reaction times.

Mean RTs for correct trials were subjected to a two way 2(Compatibility: compatible vs. incompatible) *2(Reward: rewarding vs. non-rewarding) repeated ANOVA. No main of Compatibility, F(1,48) = 0.04, p = 0.85, ηp2 = 0.008, Reward, F(1,48) = 0.99, p = 0.32, ηp2 = 0.02, or their interaction, F(1,48) = 0.03, p = 0.85, $\eta_p^2 = 0.0007$, were found (see Table 4. 1 for mean RT per condition).

Again, we calculated the compatibility effect ($M_{RT\_CE} = M_{RT\_inc} - M_{RT\_comp}$), with positive values reflecting a positive compatibility effect, for both types of reward trials. That is, in rewarding condition, M_{RT_CE} = 0.316 ms, SD = 25.25, no significant compatibility effect was found, t(48) = 0.087, p = 0.465, d = 0.012, with moderate evidence for the null hypothesis (i.e., there is no difference between compatible and incompatible condition, namely, $M_{RT\_CE} = 0$) against the one-sided alternative hypothesis (i.e., the reaction time in the incompatible condition is longer than the compatible condition, namely, the positive compatibility effect, $M_{RT\_CE} > 0$). A similar pattern was found in the non-rewarding condition, M_{RT_CE} = 0.816 ms, SD_{RT_CE} = 20.18 and the independent t-test results, t(48) = 0.283, p = 0.389, d = 0.04, BF0+ = 5.096. Moreover, there was no significant difference between rewarding conditions, as the one-tailed paired sample t-test indicated, t(48) = -0.187, p = 0.574, d = -0.027, which goes against our hypothesis that the compatibility effect should be found (at least) in the rewarding condition.

Memory about the Action-Outcome Contingency Checks

There are two types of memory check tasks of Experiment 1. In the first-round action-outcome contingency check, which followed each block of the acquisition phase, the grand mean of the accuracy was very high (M = 97.3%, SD = 16.1%). In the second-round action-outcome contingency check, which was presented after the test phase, the grand mean of the accuracy decreased a bit (M = 88.3%, SD = 32.3%). One possible explanation was that we also asked participants to recall the mapping between responses and non-rewarding outcomes, which may have interfered with recalling the rewarding outcomes. In general, though, the accuracy was higher for the Rewarding outcome (M = 96.9%, SD = 12.3%) in contrast to the non-Rewarding outcome (M = 79.6%, SD = 12.3%).

Relationship between Performance and Individual Reward Responsiveness

In general, the average BIS score among the valid participants was 12.84 (SD = 3.57). The average BAS total score was 23.65 (SD = 4.08), including the following three subscales: Fun Seeking (M = 7.57; SD =2.15), Reward Responsiveness (M = 7.06; SD = 1.57), and Drive (M = 9.02; SD =1.96). Despite the absence of a compatibility effect in both reward conditions, we examined the relationship between the compatibility effect and individual reward responsiveness as preregistered. We employed a correlation between the individual compatibility effect (i.e., M_{RT_CE}) and the individual rating measurements, including BIS/BAS scores. If, as suggested by Muhle-Karbe and Krebs (2012), rewards would strengthen the compatibility effect, it should be most pronounced for participants with a high dispositional sensitivity to rewarding events. Perhaps not surprising given the absence of an overall compatibility effect, no significant correlation was found, neither between RT-compatibility effect and BIS/BAS total, nor between RT-compatibility and BAS subscales, all ps > 0.05.

Discussion

Our goal in Experiment 1 was to investigate whether and how outcome value affects reaction times (RTs) in the trial-based ideomotor interference test phase. That is, the cued compatibility effect was manipulated in the trial level by comparing the consistency between the to-be-produced response for a specific cue and the response associated to the outcome in previous acquisition phase. However, contrary to Eder and colleagues' work (2019), we did not observe any difference between compatible and incompatible trials, neither of the rewarding trials nor of the non-rewarding trials. Both the ANOVA and Bayesian analyses did not reveal any effects. Instead, we replicated the null effect obtained by Sun and colleagues (2020, Experiment 2-3), testing for an automatic IM effect.

In contrast to Eder and colleagues (2019, but see Muhle-Karbe and Krebs, 2012), we did not find that rewards strengthened or brought out this ideomotor effect. While we used a similar procedure in which participants were rewarded for remembering the rewarded outcomes, even presenting those outcomes more often than non-rewarding ones, no IM effect was obtained. The most likely explanation is that in contrast to Eder et al. (2019), we did not use an instructed compatibility paradigm – in which participants are instructed to respond to outcomes with the responses that produced them earlier, or with the opposite responses – but a cued compatibility paradigm. Although, as explained earlier, effects in the former instruction-guided paradigm could be the result of explicit knowledge affecting responses through non-automatic processes, this would not be the case in the latter cued paradigm. The current results, then, suggest that the IM effect, and its facilitation by rewards, is limited to paradigms that allow for explicit knowledge to affect responses in a non-automatic fashion.

Experiment 2: Modality as moderator?

In the experiment of Sun and colleagues (2020), tones were used as outcomes in the acquisition phase and also as primes in the testing phase, while participants needed to focus on visual cues in the test phase. While one would expect an IM effect to emerge from a full-automaticity point of view, this would not necessarily be the case if one assumes automaticity is conditional on attention (Hommel, 2019; Melnikoff & Bargh, 2018). Indeed, it is known that having to switch from one stimulus modality to another (e.g., from visual to auditory) is demanding and incurs a switch-cost (Marien et al., 2014; Rodway, 2005; Turatto et al., 2002). Hence, participants may exert top-down control on what stimuli will get attended to, shielding them from stimuli from task-irrelevant modalities (Dijksterhuis & Aarts, 2010; Koch & Tsuchiya, 2007). In Experiment 2, we therefore matched the modality of the imperative stimulus for responses in the test phase to the exact same modality of the outcome primes (both tones). This modality matching may increase the likelihood of bringing out the IM effect, as participants could no longer ignore outcome primes by shielding a task-irrelevant modality.

Method

Participants

However, given our goal of removing an unwanted modality effect that may have been present in our earlier work, we decided to approximate a sample size similar to our previous study (N = 30, Experiment 3a, Sun et al., 2020). Thirty-five participants took part in the experiment in exchange for a small monetary payment or extra course credits. Participants with auditory-related disorders were excluded beforehand. In the acquisition phase, two participants were excluded, one because of the over 20% grand mean error rates, and the other due to the unbalanced proportion of key presses during the acquisition phase (i.e., the valid range of a left-to-right ratio was from 40% to 60%). In the testing phase, another three more participants were excluded as their bad performance during the task. They reacted with the wrong response to the imperative stimuli on almost every trial. Data of the remaining 30 participants were used in the test phase (20 Female, mean age: 24 years, [19 – 38 years], 29 right-handed and 1 ambidextrous participants).

Stimulus and Procedure

In Experiment 2, we investigated the influence of the modality of action effects, so we made every effort to produce stimulus material that matched the stimuli used in original study (Sun et al., 2020, Experiment 3a-b). As in the original study, all participants completed the two stage paradigm, consisting of a free-choice acquisition task and a forced-choice speeded ideomotor test task, with the only difference that in the test phase, participants were not asked to respond to the imperative visual cues (i.e., the Landolt “C” ring), but to the location of auditory stimuli that for the rest were identical to the tones used as outcomes in the acquisition task.

Acquisition phase

The acquisition phase was as the same as our robust version of the ideomotor paradigm (See Sun et al., 2020, Experiment 3; or Figure 4.2, panel A). The responses – effects mappings were counterbalanced among participants. Participants completed five blocks of 60 trials each, including two-third “Go” trials.

Test phase

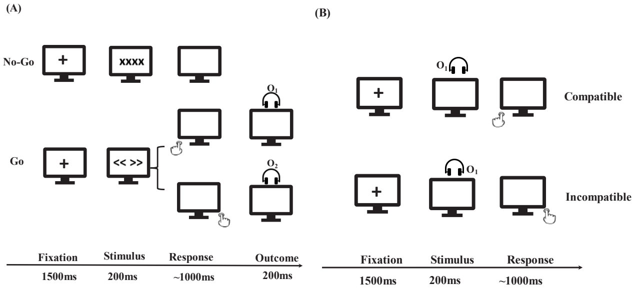
In the test phase participants were asked to perform a sound-location task serving to assess a compatibility effect, consisting of four blocks of 48 trials each. Again, the trial procedure had a similar setting as the one in Experiment 1 with the following exception: After a 1500-ms fixation, one of the previous auditory sounds was presented for 200ms as an imperative stimulus, either in the left or right ear, and the task was to press the spatial related button based on the location of the sounds (see Figure 4.2, panel B). Therefore, the action-effect association built in the acquisition phase categorized the trials of the test phase as compatible conditions when the to-be-executed response was the same as the response that was followed by the imperative tone in the acquisition phase, while incompatible conditions were those where an opposite response was followed by the imperative tone in the in the acquisition phase. The participants were told to respond to this stimulus as quickly and as correctly as possible.

Data Analysis plan

We treated the data in the same way as in Experiment 1. After applying the prescreening criterion, RTs and error rates in the test phase were analyzed as a function of Compatibility conditions.

Figure 4.2

Procedure of Experiment 2(Modality) and Experiment 3(Post-response effect factor)



Note. (A) Procedure of the free-choice acquisition phase in both Experiment 2 and 3, which was identical to the original study (Sun et al., 2020, Experiment 3). (B) Procedure of the forced-choice test phase of Experiment 2. In Experiment 2, the outcome (i.e., sound) would either be presented in the left or right ear, and participants were asked to press the related key based on the location of the sound. The mapping between response and outcome was counterbalanced among participants.

Results

Acquisition phase

First, we excluded two participants in terms of the mean error rates (> 20%) and uneven left-right proportion. Trials with omissions (0.05%) and anticipations (0.1%) were excluded. For the remaining data, a paired t-test neither yield deviation of the response ratio (t(32) = -0.99, p = 0.330; left hand mean: 49.39%, SD = 0.02), nor had different the hand pressing time (M_{left} = 363.00 ms, SD = 58.67 ms; M_{right} = 365.30 ms, SD = 53.60 ms, t(32) = -0.844, p = 0.41), confirming that the remaining participants experienced the two action-effect binding about equally often.

Test phase

First, two participants who failed to meet the response criteria in the acquisition phase were excluded, and three participants with extremely bad performance in the test task were also excluded (i.e., they seemed fully misunderstood the task with error rates above 80%). Furthermore, trials with omissions (0.23%) and anticipations (0) were also excluded, and only then RTs in correct trials were used. Error rates were analyzed based on valid data points (N = 30).

Error rates.

There was no difference between the mean error rates of compatible in the one-tailed paired t-test (M_{error_comp} = 1.5%, SD = 2.1%) and incompatible (M_{error_inc} = 1.9%, SD = 2.8%) trials, t(29) = -1.35, p = 0.093, d = -0.247, 95% CI = [-∞, 0.001]. Whereas the effect seems to be small in size, the corresponding Bayes Factor (BF0- = 1.256), if anything, indicates evidence for the null hypothesis (H0: there is no difference between compatible and incompatible condition, namely, error rates in compatible trials equal the ones in incompatible trials) over the one-sided alternative hypothesis (H-: the compatible condition has less error rates compared to the incompatible condition).

Reaction times.

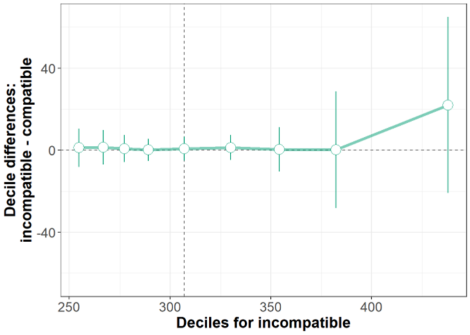
As in Experiment 1, the one-tailed paired t-test of RTs analysis did not reveal a significant difference between compatible and incompatible conditions (M_{RT_comp} = 322.29 ms, SD = 66.08, M_{RT_inc} = 326.38 ms, SD = 73.41, t(29) = -1.54, p = 0.068, d = -0.28, 95% CI = [-∞, 0.436]). A Bayes factor was calculated to determine whether the results support the null hypothesis, and it turned out that based on the current data, it was inconclusive (BF-0 = 1.03). Here the one-sided alternative hypothesis (H-) means that the compatible condition has faster RTs compared to the incompatible condition (as evidenced by the small effect size), while the null hypothesis (H0) represents that there is no difference between compatible and incompatible condition, namely, RTs in compatible trials equal the ones in incompatible trials.

Memory Checks

Most of participants (26 of 30) were able to recall the correct mapping between the response and outcomes that they learned in the acquisition phase.

Figure 4.3

Distributions that differ in spread



Note. Shift function. Compatibility effect (CE = Incompatible – Compatible) is plotted along the y-axis for each decile (white disks), as a function of incompatible deciles. For each decile difference, the vertical line indicates its 95% bootstrap confidence interval. When a confidence interval does not include zero, the difference is considered significant in a frequentist sense.

Discussion

In line with the previous experiment, the results of the ANOVA and Bayesian analyses showed no differences (i.e., neither in error rates nor in reaction times) between compatible and incompatible trials. Bases on the current data, it seems that even priming people with the outcome information in the same modality as the imperative stimulus in the test phase, does not produce an IM effect in the current paradigm. As the cue-based compatibility test phase was designed to test for automatic IM effects, we have to conclude that even with ipsimodal presentation of outcome primes and imperative stimuli, these primes do not evoke automatic ideomotor action.

On the other hand, even though the study could be regarded as underpowered or affected by the variance between individuals, we took an in-depth look at the distribution via quantifying the data into deciles (Rousselet et al., 2017). This robust graphical method suggests that increased power would be unlikely to produce a significant effect. That is, the traditional hypothesis test (here, the t-test) detects the difference by comparing two typical parameters (i.e., means) of each group / condition, but the effect is not necessarily homogeneous among participants, especially for the skewed RT distribution. Instead, the shift function, proposed by Doksum and colleagues (Doksum, 1974; Doksum & Sievers, 1976; Doksum, 1977) and developed by Wilcox (2012), offers a systematic way to characterize individual data point to study how RT distributions differ between conditions (or groups) and with what uncertainty. The logics are as follows: for each participant and each condition, the data was first ordered into deciles. Secondly, as the current experiment used the within-subject design, the decile difference was computed by subtracting the condition 1 (i.e., Compatible) from condition 2 (i.e., Incompatible). Thirdly, a one-sample test was run in each decile difference. And relying on the Harrell-Davis estimator and the bootstrap, each decile has its own confidence interval. It also adopts the multiple comparison of the p-value (0.05) to control the type I error remain around the same α level across the nine confidence interval. The shift function of the difference in Figure 4.3 was around 0 and almost flat, ex-cept that there was a growing difference towards the right tail (decile 9) of the distribution, potentially demonstrating that the CE difference (i.e., the slope) did not change too much when increasing the sample size. That is, individual variance plays an important role to affect the final conclusion.

Experiment 3: Outcomes in the test phase as moderator?

Another explanation for the absence of an ideomotor effect in the two-stage paradigm is that in the trial-based interference task the IM effects are typically tested under extinction. That is, even though actions consistently produced outcomes in the acquisition phase, they no longer did so in the test phase. Indeed, the presentation of the post-response effect does have an impact of an IM effect not only in the instructed compatibility test task (Elsner & Hommel, 2001), but also in the trial-based interference task (Vogel et al., 2020). Yet, it is not unlikely that participants update their representations, or conclude that learned action-outcome relations do no longer apply. Indeed, Wolfensteller and Ruge (2011) demonstrated that people can learn various action-outcome relations rapidly across different short learning and test phases. Assuming then that action-outcome relations may then also quickly be “unlearned” we conducted a preregistered Experiment 3 in which we tested whether the post-response effects could facilitate the automatic ideomotor action. The pre-registered plans and the experimental protocol are available at https://osf.io/jpt9m.

Method

Participants

All subjects were drawn from a pool of participants enrolled in the psychological studies at the campus. A different set of subjects participated in this experiment. The minimum sample size was between 25 and 29, based on previous experiments in our laboratory using tasks similar to those used here, in which samples of this size were adequate to detect small evidence using Bayesian analyses (BF ~2). Prior to data-collection, target sample size was pre-registered together with the study design, data-analytic plans, and experimental hypotheses.

The final sample included 40 participants for this experiment. Two participants were excluded due to the unbalanced proportion of key presses in the acquisition phase, as defined as a criterion in our earlier work. Data of test phase of the remaining 38 participants were analyzed (29 females; mean age: 26 years; four left-handed and one ambidextrous participants). Written informed consent was obtained from all participants before the experiment. Participants were debriefed after completing the study and received money in return for participation.

Stimulus and Procedure

Materials and procedure in the current experiment closely matched the procedure of the previous two, and only differed with respect to the test phase (see the following test-phase section for details).

Acquisition phase

The acquisition phase was identical to the acquisition phase of Experiment 2 (see Figure 4.2, panel A). In brief, participants practiced the task for five blocks of 60 trials, yielding 300 trials, 200 with, and 100 without a go signal. At the end of each block, there was a 10-second break, during which participants received the feedback on how often they had pressed the valid left and right keys.

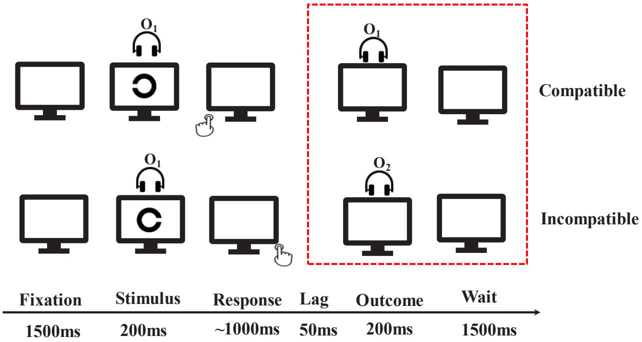
Test phase

As we explained earlier, the same forced-choice ideomotor test phase was used except for the following two changes: First, after responses in the test phase, we used the same outcomes as in the acquisition phase. Second, as we already established that participants acquired explicit knowledge of the action-outcome mappings in the previous two experiments (c.f. Sun et al., 2020) we choose to emphasize the presence of the outcomes in the instructions based on the finding from Vogel et al. (2020). They established that the ideomotor compatibility effect was strengthened when participant's attention was guided to the association between stimulus and effect (S-E) compared to stimulus and response (S-R). So, in the current experiment, we instructed participants in terms of outcomes, rather than responses, by saying: "When a left [right] Landolt C is presented, produce a high [low] tone".

The test phase consisted of eight blocks of 24 trials, yielding 192 trials in total. A signal trial comprised similar events as we did before (see Figure 4.4), except that each correct key press was now followed by the respective tone lasting 200ms that was paired with it during the earlier acquisition phase. As trials now both started and ended with the presentation of a tone, we also increased the inter-trial interval to 1500 ms to avoid confusion.

Figure 4.4

Procedure of the Test phase of Experiment 3(Post-response effect factor)



Note. The acquisition phase was identical to Experiment 2 (Figure 4.2A). In Experiment 3, after each correct response, participants would hear the effect tones again (from both ears). The response-post effect mapping was as the same as they acquired in the acquisition phase.

Data Analysis plan

The data were analyzed in the same fashion as the precedent experiments. Specifically, participants with 20% or more error rates were excluded. For RT analysis, RTs on error trials, as well as any latency slower than 1000 ms (i.e., omissions) and faster than 100 ms (i.e., anticipations) were removed. Of the remaining trials, after excluding the unbalanced response proportion of the acquisition phase, RTs and error rates in the test phase were analyzed as a function of the compatibility conditions. The results of each experiment with the same analysis were also summarized in Table 4.1.

Results

Acquisition phase

First, we excluded all acquisition trials with omissions (0.29%) and anticipations (0%). The left and right response proportions of the remaining trials were calculated, and the two-tailed paired t-test showed no difference between the allocation of the left and right hand, t(37) = 0.93, p = 0.357, left hand: Mean = 49.88%, SD = 0.024). Neither did the hand pressing time (M_{left} = 463.52 ms, SD = 58.40; M_{right} = 439.54 ms, SD = 63.21, t(37) = -0.888, p = 0.380).

Test phase

Two participants were excluded because they failed to meet the response criteria in the acquisition phase. Furthermore, trials with response omissions (0.27%) were excluded from the main data analysis, and there was no anticipation.

Table 4.1

Performance Summary for Each Type of the Response-Outcome Contingency

		Exp1: rewarding outcome	Exp1:non rewarding outcome	Exp2: task-relevant modality	Exp3: post-response outcome
RT (ms)	C	382.56 (64.06)	380.72 (68.26)	322.29 (66.08)	419.50 (55.17)
	IC	382.87 (79.09)	381.54 (82.31)	326.38 (73.41)	424.78 (56.60)
	D	t(48) = 0.087, p=0.465	t(48) = 0.283, p=0.389	t(29) = 1.536, p=0.068	t(37) = 2.638, p=0.006
error rate (%)	C	6.0 (9.1)	6.4 (9.5)	1.5 (2.1)	1.9 (2.4)
	IC	7.1 (15.2)	6.8 (15.2)	1.9 (2.8)	2.4 (3.2)
	D	t(48) = 0.980, p=0.166	t(48) = 0.391, p=0.349	t(29) = 1.352, p=0.093	t(37) = 1.117, p=0.136

Note. For comparing the difference between compatible and incompatible trials, we reported the one-tailed t-test result of RTs and Error Rates, respectively. That is, RTs (or error rates) in compatible trials were faster (or smaller) than those in incompatible trials. In Experiment 1, the rewarding condition was a within-subject variable, however, for better inspection, the results are presented separately. C: compatible; IC: Incompatible; D = IC-C

Error rates.

Error rates were analyzed based on all trials. In general, participants in each condition were relatively accurate, and most of the participants made less than 15% errors per condition (M_{error_comp} = 1.9%, SD = 2%; M_{error_inc} = 2.4%, SD = 3%). The one-tailed paired sample t-test showed no support for a compatibility effect (i.e., less errors in the compatible condition), t(37) = -1.12, p = 0.136, d = -0.181, 95% CI = $[-\infty, 0.003]$. The Bayesian paired sample T-Test resulted in BF-0 = 0.533, which did not give any clue based on current results.

Reaction times.

Individual reaction times (RTs) of the remaining correct trials were aggregated over compatible and incompatible. Subsequently, the mean RTs per condition (M_{RT_comp} = 419.50

ms, SD = 55.17; M_{RT_inc} = 424.78 ms, SD = 56.60) were compared with a one-tailed paired sample t-test [1] , yielding a significant compatibility effect with a moderate effect size, t(37) = -2.64, p = 0.006, d = -0.428, 95% CI = [-∞, -1.903]. The Bayesian paired sample T-Test resulted in BF-0 = 7.001, which means that the data are approximately 7 times more likely to occur under H- (i.e., RTs in compatible trials for faster than incompatible trials), than under H0 (i.e., same RTs in both conditions). This result indicates moderate evidence in favor of H-.

As trials in this experiment ended with the presentation of an outcome, we also investigated whether the standard compatibility effect was affected by the similarity of the outcomes on the previous trail and the prime on the current trial, as well as the similarity between the required response on the consecutive trials. To this end, we added two control variables, namely the outcome-primes similarity and the previous/present response similarity to check whether the outcome/response sequence interacted with the compatibility effects. That is, a three-way repeated ANOVA was conducted on RTs with three within-subjects factors: the IM compatibility (compatible vs. incompatible), the outcome-prime similarity (same vs. different), and the previous/present similarity (same vs. different). Results only found a significant main effect of the IM compatibility: F(1,37) = 7.92 **, p = 0.008, ηp2 = 0.176. The remaining main effects and interaction effects did not reach to significance, revealing that the compatibility effect on a given trial was not affected by events on the previous trial.

Memory Checks

Of the 38 participants, 35 recalled the correct mapping between the left (and right) key press and outcome that they exposed in the acquisition phase. Besides, we also measured their belief that whether particular key press generated particular tones with a 9-rated scale (i.e., 1: not at all; 9: very much). The subjective reports showed that 23 participants rated at least as level 7, of which 13 rated the highest level (i.e., 9).

Discussion

In the current experiment we found that presenting outcomes following responses during the test phase finally produced the IM effect, which was supported by the ANOVA and the Bayesian analyses. As the cued compatibility paradigm was designed to test for an automatic IM effect, we can conclude that outcome primes automatically trigger associated actions, but only when those actions are still producing outcomes.

Although this is not inconsistent with ideomotor theory – because one would expect actions associated with outcomes to produce those outcomes) – it raises a problem for the two-stage paradigm: If outcomes are presented in the test phase, how can we be sure that the ideomotor effect is based on associations formed in the acquisition phase? That is, associations could be acquired in the test-phase as well. On the one hand, our explicit

---

[1] Because the skew RT distribution, we repeated the paired sample t-test reported below using the reciprocal and logistic transformation of mean and median RTs, and found essentially the same pattern, so as the non-parametric Wilcoxon-Mann-Whitney test.

checks in Experiments 1 and 2 established that participants did learn the correct action-outcome mappings in the acquisition phase (c.f., Sun et al., 2020). On the other hand, action-outcome relations could also be (re-)learned in the test-phase, and were also explicitly communicated in the instructions of the test phase, which could further facilitate the IM effect (Theeuwes et al., 2015; Tibboel & Liefooghe, 2020). We return to this issue in the general discussion.

General discussion

The three experiments presented in the current paper investigated three distinct moderators (i.e., rewarding outcomes, stimulus modality of outcome primes, and presenting outcomes after responses in the test phase), that could have prevented automatic ideomotor effects from being detected in the two-stage paradigm (cf. Elsner & Hommel, 2001; Sun et al., 2020). While rewards in the acquisition phase and presenting outcome primes in the same modality in the test phase did not cause an IM effect in the current test phase that was specifically designed to pick up automatic effects, adding and repeating outcomes in the test phase did lead to a clear IM effect. That is, only when responses in the test phase were followed by the same results as in the acquisition phase, outcome primes (tones) that accompanied imperative visual cues speeded up compatible compared to incompatible responses.

Contrary to our expectations, rewards in the acquisition phase did not bring out an IM effect in the current task. While earlier experiments have failed to establish a facilitating effect of rewards before (Muhle-Karbe & Krebs, 2012), they seem to be at odds with the finding of Eder and colleagues (2019). As in the current experiment, they rewarded participants for remembering two specific action-outcome relations (but not for remembering another two relations). A crucial difference, though, is that they used an instructed compatibility paradigm that followed quickly after the acquisition phase. As participants were rewarded for explicitly remembering the relation between actions and outcomes, it may be no surprise that instructions that were compatible versus incompatible with this explicit knowledge were easier to implement. While our experiment revealed the presence of similar explicit knowledge, the more stringent test for automaticity (i.e., the cued compatibility paradigm) revealed no effect. Thus, while rewards may determine which action-outcome relations may be attended to in the acquisition phase, they may not lead to actions automatically being recruited in the test phase upon perceiving stimuli related to their previous outcomes.

We also have to conclude that presenting outcome information in the same modality as the imperative cues in the test phase does not produce an automatic IM effect. While, in principle, it should be less easy to ignore information coming in through the same modality (Marien et al., 2014; Rodway, 2005; Turatto et al., 2002), we did not obtain an IM effect in our test phase. It should be noted, though, that while the ANOVA and the Bayesian test of differences did not reveal a clear and convincing effect, the difference between compatible and incompatible trials was small, but in the right direction. Potentially, then, the obtained null effect may be the result of low power. Whereas high-powered studies in the area of ideomotor testing are very rare (but see Sun et al., 2021, Experiment 3b) further research with larger samples would have to determine whether presenting imperative cues and outcome primes in the same modality can lead to automatic IM effects.

In our third experiment, we found evidence for an IM effect when actions did produce the outcomes they produced earlier in the acquisition phase. While other studies have established the presence or absence of post-response outcomes in the test phase is able to affect action control (e.g., Elsner & Hommel, 2001), it should be noted that these experiments used an instructed compatibility paradigm that, as noted above, does not provide conclusive evidence for automatic IM effects. The current finding demonstrates, though, that stimuli that previously served as outcomes, and are in principle task-irrelevant, do nevertheless automatically trigger the actions that cause them, which is consistent with the finding of Vogel and colleagues (2020).

While the finding that adding outcomes in the test phase may add to the credibility of ideomotor effects (usually actions keep producing the same outcomes), this may be problematic for the two-stage paradigm. That is, it does not rule out the possibility that the effect is due action-outcome relations being acquired during the testing phase itself, rendering the acquisition phase irrelevant. So while the classic two stage paradigm assures that an IM effect is due to associations created in the acquisition phase, the effect obtained in the current paradigm could be due to representations of the relation between actions and outcomes that are included in the test phase and thus kept active in working memory in the moment In our opinion, such a process would be hard to explain by a mechanistic ideomotor account, as such a process would require causal inferences or propositions (Mitchell et al., 2009) regarding the relation between action and outcome.

These results are also in line with recent findings by Sun et al., (2022), who obtained significant automatic IM effects in a paradigm that only consisted of a series of short test phases in which action-outcomes were presented in blocks, without separate acquisition phases. As action-outcome relations could change from one test phase to the next, they established that these relations were acquired quickly enough to be learned and reveal themselves through a compatibility effect within merely 20 trials. Moreover, no spill-over effects were found from one block to the next, indicating that participants started with a blank slate on each new block and that compatibility effects were produced by action-outcome relations that applied in the moment.

Together, these findings suggest that automatic IM effect may not be produced by bi-directional associations between even files that are stored in (long-term) memory and automatically retrieved when outcome-related stimuli are encountered later, but that they are driven by hypotheses or positions participants form about of action-outcome relations in the task at hand. This interpretation dovetails nicely with the idea that relations between actions and outcomes that are communicated through instructions can also elicit automatic responses to outcome primes, even though the two have never been experienced together

(Theeuwes et al., 2015; Tibboel & Liefooghe, 2020).

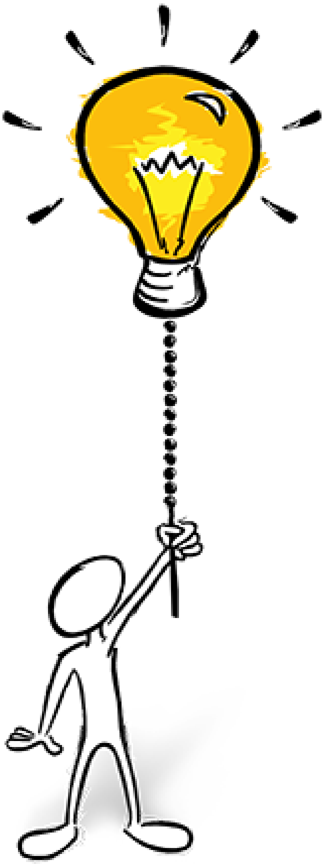
Although the notion that ideomotor effects are driven by in-the-moment representations would still allow for automatic IM effects to occur in real life, it may change our understanding of when and where ideomotor action may occur. For instance, hearing a guitar note in a restaurant may not immediately evoke a motor action in an experience guitarist, as the relevant action-outcome relation is not active in the task at hand. However, it may do just that in a student who just started to practice notes and is attempting to produce a tone with a specific pitch during a practice session.

This hypothesis maps nicely onto findings described in the literature on mirror neurons in monkeys, in which observing action outcomes in others is found to activate mirror neurons that are also activated when the same action is conducted by the monkey itself (Gallese et al., 1996; Rizzolatti et al., 1996; Rizzolatti & Craighero, 2004). While this was first regarded as a hard-wired link between perception and action, later studies demonstrated that these effects only occur when the action is relevant to the monkey (Umiltà et al., 2001) and that the mirror neurons that are activated are determined by the specific action-outcome relation in the task at hand (Umiltà et al., 2008).

Such flexibility has also been proposed in more recent adaptations of ideomotor theory, i.e. the binding and retrieval in action control framework (BRAC, Frings et al., 2020), in which ideomotor action is considered to be moderated by context and goals. While fully in line with the current findings, such concessions to the mechanistic approach to goal-direct action that ideomotor theory once offered, could be a slippery slope. While models like BRAC still claim to offer a mechanistic, although far more complicated, view on goal-directed action, it does still not put a conscious decision maker at the center-stage of the theory. An alternative to the mechanistic approach would be to assume that people infer causal relations between events in a task and that the resulting propositional knowledge (Mitchell et al., 2009) may, under some conditions automatically affect the task at hand. Although the two views may be two sides of the same coin, they invite a radically different view on ideomotor action that is in any case more dependent on subjective, in-the-moment hypotheses about action-outcome relation in the mind of the actor, than on bi-directional associations formed in memory by co-activation of event files. Whereas the examination of goal-directed behavior is an intriguing and important venture to understand the full potential of human functioning, which view fits best on the learning and emergence of goal-directed behavior remains open for further scrutiny.

CHAPTER

5



Chapter 5:

Examining Mechanistic Explanations for Ideomotor Effects

This chapter is based on: Sun, D., Custers, R, Marien, H, Liefooghe, B, & Aarts, H. (2021). Examining Mechanistic Explanations for Ideomotor Effects. *Journal of Experimental Psychology: Human Perception and Performance*

Credit author statement:
DS: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data Curation, Writing-Original-Draft, Writing-Review & Editing, Visualization, Project administration. **RC:** Conceptualization, Data Curation, Writing-Review & Editing, Supervision, Project administration. **HM:** Writing-Review & Editing. **BL:** Writing-Review & Editing. **HA:** Conceptualization, Data Curation, Writing-Review & Editing, Supervision.

Abstract

Ideomotor (IM) theory provides a widespread mechanistic account for understanding how goal-directed action can be learned and instigated. That is, when bidirectional associations between actions and outcomes have been established in memory, the perception or thought of the outcome could automatically activate the associated action. While a sizable literature provides evidence in line with this account, most findings can also be explained by propositions that are made based on causal inferences. In the present paper, we present an online IM paradigm that combines several features of propositional learning previously reported in the literature. That is, we aim to provide evidence for fast learning of action-outcome relations and flexible switching in between learned rules, in a single experiment in which learning and testing of action-outcome connections occur on the same trials. We demonstrate that IM effects emerge within a couple of trials, but also that people can update learned action-outcome associations immediately when an alternative mapping of outcomes on actions is introduced, without any switch costs. We argue that a propositional account, according to which people infer explicit causal rules, can explain these findings better than a mechanistic account relying on bidirectional associations.

Keywords: ideomotor, goals, automaticity, propositional learning

Introduction

Goal-directed behavior enables us to navigate our world and realize outcomes we have in mind. Such behavior would seem to require causal knowledge about the world: In order to produce a certain outcome, such as having a pizza delivered at our doorstep, one must have an understanding of the relations between calling a restaurant or placing an order on a website, and the delivery of the pizza at home (Kruglanski et al., 2002). And if one wants to pour a glass of water, one needs to know that lifting the handle of the faucet would make this possible.

While even the simplest action would therefore seem to require the use of fairly sophisticated mental models of causal relations (e.g.,Waldmann et al., 1995), Ideomotor (IM) theory (for reviews, see Pfister, 2019; Shin et al., 2010; Stock & Stock, 2004) provides an elegant mechanistic account of how organisms could acquire the ability to engage in goal-directed action. At least in its modern form (e.g., Hommel et al., 2001), it assumes that when an agent repeatedly performs an action (e.g., pressing the home key of a phone with the left thumb), bidirectional associations are established between the representation of the action and its perceived effect (e.g., the phone switching on), which may consist of visual, auditory, or proprioceptive information. As a result, simply thinking of, or merely activating the perceptual representation of the phone switching on, suffices to activate the representation of pressing the home key with your thumb, which would then prepare or trigger that action. Any organism, then, that is capable of storing associations between representations of its actions and representations of perceived outcomes in memory, can therefore – in principle – engage in (a rudimentary form of) goal-directed behavior.

This mechanistic account provided a compelling framework to understand automatic influences on behavior such as imitation (Iacoboni, 2009), behavior priming (Dijksterhuis & Bargh, 2001), goal priming (Custers & Aarts, 2010), and even the occurrence of mirror neurons (Heyes, 2010). A substantial literature on IM theory also provides evidence that is in line with this associative account (Shin et al., 2010). However, we argue here that most findings in the literature on ideomotor theory can also be explained by assuming that people explicitly realize which actions produce which outcomes and that hence, IM effects could be the result of propositional learning (Gawronski & Bodenhausen, 2011; Mitchell et al., 2009).

Recent research and theorizing suggest that such propositional learning is fundamentally different from Hebbian learning (Hebb, 2002), according to which associations arise from mere coactivation of mental representations (Gawronski & Bodenhausen, 2011; Mitchell et al., 2009). Instead, it involves the generation and testing of hypotheses. As such, propositional knowledge can be labeled as true or not true, as it represents a relation between variables in the world (e.g., X causes Y). Therefore, propositional learning can be faster and more flexible than Hebbian learning, as it does not require extensive repeated exposure to events for bidirectional associations to be formed through the coactivation of representations.

The current paper first reviews existing evidence in line with this alternative account. Based on that review, we then report two experiments featuring an IM paradigm that would be highly unlikely to produce IM effects according to the traditional account, but would be expected to have IM effects according to our causal inferences account. More specifically, the experiments feature a novel paradigm in which learning and testing of action-outcome relations are combined in one continuous task and the causal relations, or mappings, between actions and outcomes switch rapidly between blocks. While the short blocks and rapid switching between mappings would make IM learning unlikely under the traditional account, any learning would be expected to be accompanied by a spill-over effect across blocks, as an established association in memory should carry over to the next block. Under the alternative account, however, IM effects should emerge virtually instantly within blocks without any spill-over, as participants could infer the causal relation between actions and outcomes within a few trials on each block.

Evidence for Ideomotor Learning and Action

Empirical evidence for ideomotor theory mainly relies on the two-stage paradigm (for reviews, see Pfister, 2019; Shin et al., 2010; Stock & Stock, 2004), in which participants first engage in a lengthy acquisition phase where they execute actions that are followed by distinct outcomes. Then, in the test phase, people engage in these actions again, but these are preceded by the stimuli that served as outcomes earlier. An IM effect is inferred from the finding that stimuli that previously served as outcomes evoke the actions that preceded them.

For instance, in the classic study by Elsner and Hommel (2001), participants first executed two different responses (e.g., pressing a left or right key) that each consistently produced an arbitrary outcome (e.g., a high or low tone) for 200 trials. Then, in the test phase, participants were instructed to respond to the stimuli in a forced-choice task that previously served as outcomes either with the actions that produced them earlier, or the opposite actions, for 100 trials respectively.

It was found that when participants were instructed to react to an outcome with the response that produced it in the learning phase (the compatible response), responses were faster than when the participant had to react with the opposite (incompatible) response (Experiments 1A/B), and that the effect even occurred when responses no longer produced outcomes in the test phase (Experiment 1B).

In their subsequent experiments (Experiments 2-4), Elsner and Hommel (2001) used a free-choice task in the testing phase, in which participants were asked to respond on each trial with a response of their choice. The stimuli that served as outcomes in the learning phase were, however, presented just before the response with the instruction that they were irrelevant and should be ignored. Consistent with the results of the forced-choice task, though, participants' free choices were more often in line with the mapping that was presented in the learning phase. Using similar two-stage paradigms, this compatibility effect has been replicated over a hundred times (Watson et al., 2018), providing evidence that is consistent with ideomotor theory. However, we would like to point out that a similar effect could be predicted from an explicit causal inferences account. First, participants would only need a couple of trials in the lengthy acquisition phase to figure out the action-outcome mapping. Second, both the forced- and the free-choice task used as test phases could be influenced by such propositional knowledge. As we will argue below, incompatible instructions (according to which participants have to respond to a previous outcome, not with the response that produced it in the acquisition phase, but the opposite response) would be harder to remember and implement. And by definition, the free choice task would be open to any rule, strategy, or whim participants could come up with. Hence, while the traditional account indeed predicts the IM effect that are reported in an extensive IM literature, these paradigms do not rule out that the findings are the result of explicit causal inferences. Below, we report findings that are in line with this account.

Existing Findings in line with Explicit Causal Inferences

Explicit Knowledge from Acquisition Phases

There is evidence that participants indeed infer the causal relations that are featured in the lengthy IM acquisition phases. In fact, Elsner and Hommel (2001) reported that in their Experiments 2A and 2B, most participants were able to correctly recall the action-outcome mapping that was featured. A similar observation was made in recent work by Sun and colleagues (2020). Although one could argue that the questions they used to test causal knowledge (i.e., “which response produced this event in the acquisition phase?”) could prompt a correct answer due to bidirectional associations in memory in the absence of explicit causal knowledge, the near perfect accuracy suggests that people remember the causal relations. So, while an extensive learning task of hundreds of trials may create lasting bidirectional associations in memory, participants do also seem to acquire explicit knowledge about the causal relations between actions and outcomes in this task. Sun and colleagues (2020) demonstrated that these explicit causal inferences are also made spontaneously, without any instructions.

Ideomotor Effects from Mere Instructions

Recent research on instruction effects suggests that providing people with explicit knowledge about action-outcome relations through instructions can produce IM effects in the absence of any actual pairings of actions and outcomes. To demonstrate this, Theeuwes et al. (2015) gave people instructions about action-outcome relations in an upcoming task. They found that these instructions caused outcome cues to affect responses in a second task, even though the instructed action-outcome relations had at that point never been experienced (for similar findings and reasonings, see Tibboel & Liefooghe, 2020). Although explicit knowledge was manipulated through instructions, these studies suggest that inferences that are made spontaneously by participants in the acquisition phase could serve as self-generated instructions that produce IM effects in the test phase (c.f., Eder & Dignath, 2017).

Ideomotor Effects from Fast-Learning

While the use of lengthy training phases in the IM literature seems to reflect the assumption that feature repetition between stimuli and responses plays a key role in the binding and retrieval process in action control (Frings et al., 2020; Hommel et al., 2001), recent work demonstrates that action-outcome contingencies could be learned from even a couple of action-effect parings. For instance, Wolfensteller and Ruge (2011) obtained IM effects with a severely shortened two-stage paradigm. They used a cued forced-choice testing phase to test for automatic IM effects, but their paradigm used a short learning phase (8 to 32 trials) and a subsequent short test phase (20 trials). This shortened two-phase learning procedure was repeated over many blocks, each time with a novel action-outcome relation. Their findings revealed an IM effect on reaction times for learning phases as short as 16 trials (8 pairings for each action and its outcome).

Such rapid learning is more consistent with an explanation in terms of causal inferences, as it seems unlikely that bidirectional associations stable and strong enough to influence behavior would be formed nearly without repetition. Other findings demonstrating quick learning have also been obtained for stimulus-response bindings. In these studies, responses have also been facilitated by stimuli that preceded them after minimal learning (e.g., Dutzi & Hommel, 2009; Moeller et al., 2016; Moeller & Frings, 2019; Schwarz et al., 2018). Although not directly related, such research shows that learning the relation between events can happen so quickly, that the formation of bidirectional associations as a result of coactivation seems an unlikely candidate for the learning process driving these effects.

Flexible Ideomotor Effects

The findings discussed above also suggest that the application of action-outcome knowledge may be flexible (e.g., Eder et al., 2020; Wolfensteller & Ruge, 2011). While learning of novel relations on consecutive blocks could potentially be explained in terms of novel associations that are added to memory on each block, it also may suggest that people abandon a previously learned rule and replace it with a new one, which is more in line with propositional learning. This interpretation is supported by research showing that a specific action-outcome mapping that is cued at a trial level can prompt this mapping to be used in the initiation of their responses (e.g., Pfister et al., 2010; Zwosta et al., 2013). Specifically, when cues indicated that responses would be followed by a spatially compatible outcome, responses were faster than when cues indicated incompatible outcomes.

To conclude, although there is only indicative evidence for parts of our alternative account, the literature shows that people do make causal inferences during learning, and that these inferences could potentially produce IM action in the test phase. Moreover, studies demonstrating fast learning and flexible application of action-outcome knowledge suggest that learning may be propositional in nature.

Existing Evidence for Non-Automatic Ideomotor Responses

It is important to note that both the standard free-choice and the forced-choice test phases could be directly influenced by the causal knowledge or specific strategies people have in mind. For instance, in most forced-choice tests, response instructions are given that are either compatible or incompatible with the relations between action and outcome that were presented in the learning phase. While faster and more accurate responses in the group with compatible versus incompatible instructions could be the result of stimuli automatically triggering the response that produced them in the past, it could also be the case that instructions that are incompatible with the inferred causal relation from the learning phase are harder to remember or implement (Monsell et al., 2001). In a similar vein, in the free-choice testing phase participants could deliberately choose to act in line with the inferred causal relation, either because of demand characteristics, or because it makes it easier to follow the request to respond randomly. Note that in both cases, it is the explicit causal knowledge that either interferes with the instructions, or is used to produce a deliberate pattern of responding.

Recent evidence by Sun et al. (2020) indeed suggests that such explicit causal knowledge may drive IM effects. While their two-stage experiment with a free-choice test phase produced a solid IM effect, a closer inspection revealed a bimodal distribution of participants' IM effects. While the majority was clustered around the chance level (i.e., 50%), a small number of participants showed a very large bias. Some even responded 100% in line with the previously acquired mapping. As bidirectional associations influencing people's responses would be expected to result in a small but consistent IM effect across participants, the bimodal distribution suggests that the IM effect may have been driven by a small number of participants that used a deliberate strategy or response rule based on causal relations between actions and outcomes they inferred in the learning phase.

To recap, while the literature on the two-stage paradigm provides evidence in line with the idea that IM effects are produced by bidirectional associations between actions and outcomes, it does not rule out the possibility that these effects are produced by propositional knowledge, based on explicit causal inferences. Although such propositional knowledge could produce automatic responses in the test phase (Mitchell et al., 2009; Theeuwes et al., 2015), it could also produce such effects through more explicit, deliberate processes (interacting with task instructions, triggering strategic processes). While findings from different studies provide evidence for causal inferences during learning and the possibility of this knowledge driving ideomotor effects, more direct evidence for these processes driving ideomotor learning is lacking. While distinguishing between propositional and associative processes during learning is notoriously difficult (Mitchell et al., 2009) , we aim to provide more support for the role of explicit causal inferences in IM learning by creating a task that would be very unlikely to yield IM effects according to the traditional account, but not under the alternative explicit causal inferences account.

The Current Study

First of all, the current study features a continuous IM learning paradigm, in which relations between actions and outcomes can be acquired and tested at the same time. Participants perceived the outcomes (a colored light flicking on in the middle of the screen) of their actions (a left or right keypress) after responding to two imperative stimuli (a left light or a right light). These stimuli shared a key property (i.e., color) with one of the two outcomes. As such, stimuli that prompted a specific action could either be compatible or incompatible with its outcome. Crucially, the mapping between actions and outcomes changed over ten blocks, with half of the blocks using the opposite mapping between actions and outcomes.

In Experiment 1 we used a perfect contingency between actions and their specific outcomes on each block. An IM effect would demonstrate that learning can occur very fast, conceptually replicating the work by Wolfensteller and Ruge (2011). However, in contrast to Wolfensteller and Ruge, participants in the current experiments switch back and forth between mappings. As such, an IM effect in the absence of a spill-over effect from one block to the next would also indicate that people can abandon a causal rule when it no longer applies. Together, this would, in our opinion, be much easier to explain in terms of propositional knowledge about causal relations. Hence, the paradigm would pit in-the-moment IM effects that arise within the block against lasting IM effects, that would affect IM responses on later blocks. While Experiment 1 aims to test the basic effect, Experiment 2 aims to replicate it and investigate the effect of partial contingencies. Though less consistent pairings of actions and outcomes should lower the strength of the associations between the two (c.f., Elsner & Hommel, 2001), we reasoned that it would be less likely to affect the causal inferences people make. On the contrary, a partial contingency could promote inferences of causal relations by making the task more engaging or directing attention to the relations (Kunde, 2004). As such, this conceptual replication could provide additional evidence for the role of propositions based on causal inferences in IM learning.

Experiment 1: In the Moment versus Lasting Ideomotor Effects

Method

Participants and Design

To determine the number of participants, we employed a (pre-registered) Sequential Bayes Factor design with a maximum n (Schönbrodt et al., 2017). That is, after recruiting a minimum of 50 participants a Bayes factor (BF) for the compatibility effect was calculated across all experimental trials. Data collection was set to be terminated as soon as the Bayes factor for the overall compatibility effect, or the alternative hypothesis (i.e., no difference between compatible and incompatible) was equal or bigger than six. A Bayes factor of six is chosen as it is typically interpreted as reflecting ‘substantial evidence’ for a hypothesis (Jeffreys, 1961; Lee & Wagenmakers, 2013). If none of those criteria were met, an additional 10 participants would be recruited until the BF met the criteria, or the number of participants would reach Nmaximum = 100 participants. We used the Jeffreys-Zellner-Siow prior (JZS) default BF implemented in the software JASP (version 0.13, JASP Team 2020) (van Doorn et al., 2020) with a scale (width) parameter of ω = 0.707 as a conservative prior for the expected effect size. All participants received payment for their participation.

Procedure

Participants were recruited online via Prolific (https://www.prolific.co/). They engaged in an online experiment that was programmed in Gorilla (https://gorilla.sc/) and after providing consent they received general instructions. They learned that in the task they would repeatedly see three lights lined up horizontally at the center of the screen (see Figure 5.1A) and that it was their task to respond with a left (z) or right (/) keypress if the left or right light lit up respectively. They learned that after their response, the middle light would light up as a result of their action.

The experiment started with two practice blocks, in which participants could get acquainted with the task. After that, 10 experimental blocks followed, preceded by an induction block (see below). After the experiment, participants were thanked and debriefed. The procedure was pre-registered prior to data collection (https://osf.io/vfhjs/). Data and analysis scripts are available at the Open Science Framework.

Experimental Trials

A trial started with a fixation point for 250 ms followed by a row of grey lights for 250ms (see Figure 5.1A). Subsequently, the left or right light would light up in either red or green. The response – outcome mapping was fixed within a block. If the participants responded correctly to this imperative stimulus with a left keypress (z) for a left light or a right keypress (/) for a right light, the middle light would light up for 150 ms in either red or green, according to the response-outcome mapping that was applied within the block at hand (see below). If participants responded incorrectly, participants received the following message: “Sorry, you are wrong”. If participants did not respond within 3000 ms, they received the feedback: “Oops, you missed it”. These trials were recorded as errors or missing values respectively and then the next trial would commence. On compatible trials, the color of the imperative stimulus was the same as the color of the effect stimulus within the block, where the action-outcome mappings were fixed. On incompatible trials, the imperative stimulus was the opposite color to that of the required response within the block (See Figure 5.1A for examples). As learning and testing occur on the same trials, effects could emerge as soon as trial two on a block, after the first outcome of one of the actions was perceived.

Experimental Blocks

An experimental block consisted of 20 experimental trials. These consisted of five repetitions each of four possible trials with different imperative stimuli: a left light in green, a left light in red, a right light in green, or a right light in red (see Figure 5.1A). As the action-outcome mapping was perfectly consistent within the block, this always resulted in 10 compatible and 10 incompatible trials. These trials were presented in a random order.

At the start of each block participants were reminded that they would earn an extra point (that would convert to extra money at the end of the experiment) if they could correctly remember the action-outcome mapping of that block. Memory of the mapping was tested after all experimental trials in the block. Participants had to respond without time pressure to the two colors of the middle light (red and green), indicating which response had produced that color during that block (left or right keypress). The accuracy score was calculated by averaging the number of correct responses over the two questions. Subsequently, at the end of the block, participants received feedback on their mean response time (RTs) on that block, and received a rank-ordered overview of mean RTs on all completed blocks to keep them motivated and engaged. They also received feedback on their mean accuracy on the memory checks on all completed experimental blocks so far.

Action-Outcome Mapping

Of the 10 experimental blocks, 5 featured one action-outcome mapping (left-green / right red) and five the other (left-red / right-green). Pseudo-random sequences were created so that five blocks were preceded by the same mapping, and five by a different mapping. In order to do so, an additional induction block was added, so that the mapping of the first of the 10 experimental blocks could also be classified as the same, or different from, this induction block. Reaction times and error rates are reported for the 10 experimental blocks only.

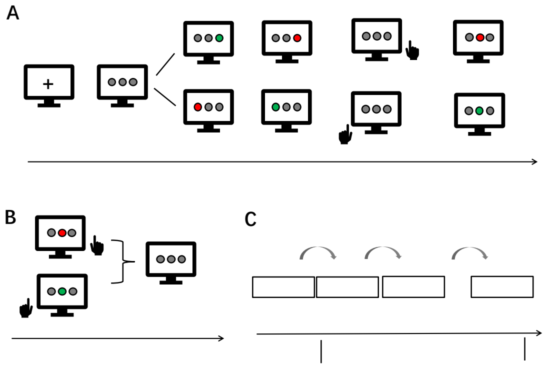
Data Analysis Plan

Data were analyzed using R 4.0 (R Core Team, 2020). Visualizations of raw data points were built with the raincloud plots (Allen et al., 2018). Before conducting the analyses, error rates (or accuracy) and reaction times (RTs) were both tested for normality by visually inspecting the Q-Q plots of the difference between conditions (e.g., compatible, and incompatible). Transformation was applied if the raw data violated the normality criteria, first checking whether log-transformation met, if not, using a reciprocal-transformation. If after both transformations the normality assumption was still violated, the Wilcoxon-Mann-Whitney test would be used to compare the difference between conditions. ANOVAs were calculated using the aov_ez function and Type III sums of squares (afex package Version 0.28 in R; Singmann et al., 2015). When assumptions of sphericity were violated Greenhouse-Geisser (GG) correction was utilized in the ANOVA model. In this case, we report uncorrected degrees of freedom and corrected p-values.

The Bayesian factors (BFs) were calculated with the default JZS prior setting in JASP (JASP Team, 2020). The reporting of a BFs classification was based on work of van Doorn and colleagues (van Doorn et al., 2020). The default prior is described by a Cauchy distribution centered around zero and with a width parameter (ω) of 0.707. For non-directional hypothesis assumption comparison, this corresponds to a probability of 50% that the effect size lies between -0.707 and 0.707. For the directional hypotheses, the Cauchy is simply cut in half. That is, if expecting a positive effect size, ‘we are 50% confident that the effect size lies somewhere between d=0 and d=0.707”. There is some literature to support that this is a reasonable expectation of the effect size and an interpretation can be found in a recent introduction paper by Schmalz and colleagues (2021).

Figure 5.1.

Basic design of Experiment 1



Note. Panel A depicts an experimental trial. Participants had to respond based on the location of the light on the cue screen (left for left and right for right). After their response, a middle light appeared as a result of their action according to the action-outcome mapping that remained unchanged throughout a block (in this case: left-green / right-red). The color of the light on the cue screen, then, could be compatible or incompatible with the action-outcome of the required action within each block. Panel B depicts the memory check task, in which participants had to recall the correct responses that generated the mapped color they learned on the previous block. Panel C depicts the whole pipeline of the main task. There were 11 blocks in total, including the first induction block. The R-O mapping was changed among blocks. Depending on the R-O mapping of the consecutive blocks, a block either had the same, or a different mapping as the previous block.

Results

Participants with an extremely high number of error rates (over 20%), were removed before analysis. For reaction times (RTs) analysis of the remaining participants, trials on which the RTs were faster than 100 ms (anticipation) and longer than 1500 ms (omission) [1]were also excluded. Error trials were removed prior to analyzing RTs. Using the Bayesian stopping rule to determine the final sample size [2], 100 participants were collected in total, as the BF for the compatibility effect, including the induction block, or the absence of it, did not reach 6.

Two participants were removed before analysis because their error rates exceeded 20%, so the final sample consisted of 98 participants (Mean age = 25.21 years, SD = 5.27; 37 females, 7 left-handed and 37 ambidextrous participants).

Memory Check

Memory scores were quite high with mean error rates around 0.112 (SD = 0.164), confirming that though the mapping between responses and outcomes changed across the 10 blocks (when including the induction block, the performance was also very high (M = 0.123, SD = 0.165), participants could easily learn the relation between actions and outcomes and then pass the contingency checks to win extra points.

Error Rates

As shown in Table 5.1, no difference in error rates was found between compatible and incompatible trials, t(97) = -0.095, p = 0.462, ($M_{compatible}$ = 0.031, SD = 0.030; $M_{incompatible}$ = 0.031, SD = 0.032).

Reaction Times

As explained above, for RT analysis of the remaining participants, trials with anticipation (0.938 % of trials) and omissions (0.227 % of trials) were removed. Figure 5.2 depicts the distribution of raw RTs of the remaining individuals between compatible and incompatible conditions. Consistent with our main hypothesis, RTs in compatible trials (M = 315.68 ms, SD = 50.10), in which the color of the imperative stimulus was the same as the result of the required response, were indeed significantly faster than those in incompatible trials (M = 318.05 ms, SD = 50.55), n which the imperative stimulus was the opposite color to that of the required response. The one-tailed t-test showed a significant difference, t(97) = -2.41, p = 0.009, d = -0.244. So did the Bayes Factor (BF-0 = 3.481), indicating that the data was approximately 3.481 times more likely to occur under the directional hypothesis

---

[1] The threshold of omission was a bit longer compared to our lab studies, which always used 1000 ms as a cut-off. We extended the value because the online data had more unpredictable concerns compared to the lab environment. We also checked the results when applying the strict lab boundary with omission (0.83% of trials) and anticipation (1.01% of trials) when Nmax = 100.

[2] See detailed reports when the sample size increased from the minimum to the maximum in Appendix c: Supplemental Material A

(i.e., H - : the RTs of compatible condition was faster than the incompatible condition, namely, RTcompatible < RTincompatible) than the null hypothesis (i.e., H0, there is no difference between compatible and incompatible condition, namely, $RT_{compatible} = RT_{incompatible}$).

Figure 5.2

Individual distributions of RTs between compatible and incompatible conditions.

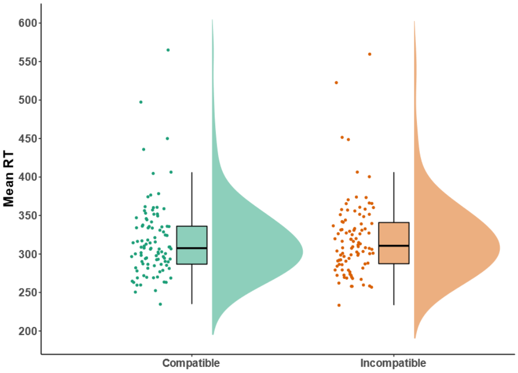


Table 5.1

Performance Summary for Each Type of the Response-Outcome Contingency

		Exp1: 100% contingency	Exp2: 100% contingency	Exp2: 80% contingency
RT (ms)	C	315.68 (50.10)	314.27 (37.16)	330.90 (70.86)
	IC	318.05 (50.55)	316.58 (40.45)	332.12 (71.15)
	D	t(97) = -2.41, p = 0.009, d = -0.244	t(109) = -1.91, p = 0.029, d = -0.182	t(109) = -1.22, p = 0.113 d = -0.116
error rate (%)	C	0.031 (0.030)	0.031 (0.031)	0.026 (0.027)
	IC	0.031 (0.032)	0.035 (0.035)	0.024 (0.027)
	D	t(97) = -0.095, p = 0.462 d = -0.010	t(109) = -1.66, p = 0.050, d = -0.159	t(109) = 0.762, p = 0.776 d = 0.073

Note. For RTs and Error rates, Mean and (standard deviation) were reported per condition. For comparing the difference between compatible and incompatible trials, we reported the one-tailed t-test result of RTs and Error Rates, respectively. The types of the Response-Outcome Contingency consisted of the 100% and the 80% occurrences. The former was set in two Experiments, the Base of Experiment 1 and the Full contingency of Experiment 2. C: Compatible; IC: Incompatible; D=IC-C

To investigate the IM effect over time, we employed a time (block 1 to block10) x compatibility (compatible vs. incompatible) ANOVA. This analysis revealed a main effect of compatibility (see also above), $F(1, 97) = 5.87$, $p = 0.017$, $\eta_p^2 = 0.057$, and a main effect of time, $F(9, 873) = 5.63$, $p < 0.001$, $\eta_p^2 = 0.055$, but no interaction effect, $F(9, 873) = 0.97$, $p = 0.429$. Furthermore, as the ideomotor effect proved independent of time, a second switch (no vs. yes) x compatibility (compatible vs. incompatible) ANOVA was conducted. Again, a significant compatibility effect was obtained $F(1, 97) = 5.87$, $p = 0.017$, $\eta_p^2 = 0.057$, but the effect of switch $F(1, 97) = 1.73$, $p = 0.192$ and the interaction $F(1, 97) = 1.10$, $p = 0.296$ were not significant, thus revealing evidence for in the moment, but not for lasting ideomotor effects.

Discussion

Experiment 1 created a novel action-outcome paradigm through combining response-outcome learning testing for ideomotor effects in one task. While the IM effect persisted over the 10 blocks, there was no evidence that the effect was affected by switches in action-outcome mappings. That is, participants did not show stronger IM effects when the mapping remained the same from one block to the next, indicating that the IM effect was only driven by the action-outcome mapping in the moment. Thus, whereas this conceptually replicates the general IM effect, as well as the finding by Wolfensteller and Ruge (2011) that IM learning can emerge very quickly, it also demonstrates that people can switch back and forth between opposite action-outcome mapping without any switch-costs. That is, participants learn within a couple of trials which mapping applies in the block at hand which produces IM effects on their responses, but are able to immediately abandon this knowledge and learn the opposing mapping on the next block.

In our opinion, this pattern is much easier to explain in terms participants inferring which of the two possible causal mappings applies in the block at hand, rather than the development of bidirectional associations in memory. Building on these initial findings, in Experiment 2 we set out to accomplish two things. First, we aimed to replicate the findings of Experiment 1. Second, we aimed to provide more evidence for the rule-based nature of these IM effects.

In line with the associative explanation of ideomotor effects, Elsner and Hommel (2004) found that stronger IM effects were obtained when a full (100%) versus partial (80%) contingency between actions and outcomes was employed, at least in the classical two-phases paradigm. We argue that if the effect in the current paradigm is driven by inferred rules, rather than long-lasting associations, it should be less hampered by these partial contingencies. In fact, a partial contingency could even result in people keeping the action-outcome representation more active during the task, as the hypothesis would have to be checked over more trials to extract the contingency information (Kunde, 2004). Of course, these forces would work in opposition and a low contingency would be harder to detect even when people are more engaged. We therefore hypothesize that a substantial, but partial contingency of 80% should not reduce, but perhaps even strengthen the IM effect.

Experiment 2: Replication and Extension of Rule-Based IM Effects

Method

Apparatus and Procedure

The current experiment used the same apparatus, stimuli, and procedures as Experiment 1 with one exception: to investigate whether the strength of location-color contingency (response-outcome contingency, R-O) affects the Ideomotor learning and interference effect, we added a between-subjects variable, that is, there was either a full contingency between response and outcome (i.e., 100% R-O, the full contingency group) or partial contingency (i.e., 80% R-O, the partial contingency group). In the partial contingency group, 80% of the trials within each block followed one mapping (e.g., the correct left response caused the middle light turned to green, and the correct right response caused the middle light turned to red). The remaining 20% of the trials used the reversed mapping (e.g., the correct left response caused the middle light turned to red, and the correct right response caused the middle light turned to green). The full contingency group was identical to the Experiment 1.

Participants and Design

Participants were randomly assigned to one of the two contingency conditions, either the full contingency group, or the partial contingency group. Furthermore, we opted for a slightly more conservative approach in calculating our sample size based on Experiment 1 (i.e., the base full contingency group). We used the effect size (d = -0.259, calculated from the induction block and the main ten blocks) after removing participants whose accuracy of the contingency check fell into the two standard deviations deviating away from the grand mean, and a power of 0.80 with significance at the 5% level to test the directional hypothesis. This resulted in a sample size of 94 participants using G*Power (Faul et al., 2007).

We recruited 220 new participants (ages 18-35 years) in total, 110 per contingency, that is, the partial contingency and replication group, from the Prolific website to ensure we would reach the approved sample size of 94 after applying the approved exclusion criteria and practical concerns (e.g., drop-outs; unpredictable cutoff threshold generating from the real data) within each group. However, no participants were excluded on this basis. None of them participated in Experiment 1. The experiment (including all stimuli and appendix) was pre-registered and available in the OSF project prior to data collection (https://osf.io/jdac3/).

The final sample consisted of 110 participants in the full contingency group (42 female, 1 preferred no to tell; mean ages = 23.93 years, SD = 4.68; 19 left-handed and 5 ambidextrous participants), and another new 110 participants in the partial contingency group (22 female, 1 preferred no to tell; mean ages = 25.26 years, SD = 4.65; 13 left-handed and 2 ambidextrous participants).

Data Analysis Plan

We performed the exact same analyses reported for Experiment 1, with the exception of adding a between-subject variable: Type of contingency groups (the full contingency group vs. partial contingency group) for each planned comparison.

Results

Memory Check

We found that overall, there was a significant difference between the full contingency group and partial contingency group with the one-tailed two-sample Wilcoxon test (i.e., the non-parametric test to the independent two-sample t test) (i.e., hypothetically, error rates from the full group should be smaller than the partial group), p = 0.005, effect size = 0.173, with the partial contingency yielding greater error rates (M = 0.164, SD = 0.191) than the full contingency group (M = 0.117, SD = 0.164).When excluding the performance of the memory check of the induction block, the visual pattern was consistent, with less error rates in the full contingency group (M = 0.107, SD = 0.165) compared the partial contingency group (M = 0.138, SD = 0.195), but the difference between the two group was not significant, p = 0.099 [1] .

Error Rates

No participants were removed as error rates did not exceed 20%. To test the robustness of the compatibility effects, we compared the mean error rates of Experiment 2 by conducting a two-way mixed ANOVA with one within-subjects factor: Compatibility (compatible vs. incompatible), and one between-subjects factor: Type of contingency groups (the full contingency group vs. partial contingency group) on error rates. We found that for individual mean error rates analysis, the main effect of Type of contingency groups was significant, F(1, 218) = 5.02, p = 0.026, η_p^2 = 0.003; but not the main effect of Compatibility, F(1, 218) = 0.61, p = 0.435, nor the interaction, F(1, 218) = 3.12, p = 0.079, η_p^2 = 0.014.

Reaction Times

RTs were trimmed in the same fashion as in Experiment 1. That is, RTs on error trials, as well as any latency faster than 100 ms (full-contingency group: anticipation =1.364%; partial contingency group: anticipation =1.018%) or slower than 1500 ms (full-contingency group: omission = 0.518%; partial contingency group: omission = 0.482%), were initially removed. For mean RTs analysis, the mixed ANOVA yielded a main effect of Compatibility, F(1, 218) = 5.07, p = 0.025 , η_p^2 = 0.023. Overall, RTs on compatible trials were faster (M = 322.58 ms, SD = 57.06), than on incompatible trials (M = 324.35 ms, SD = 58.26). Also, a significant effect of contingency emerged, F(1,218) = 4.39, p = 0.037, η_p^2 = 0.020.

---

[1] Because of the difference between conditions, we chose not to apply the preregistered criteria of excluding people with low memory checks in the main text. But the results of the high memory group did not change the pattern. Detailed results are in the Supplemental Material B of Appendix c).

Participants in the full contingency group (M = 315.43 ms, SD = 38.77) were faster than in the partial contingency group (M = 331.51 ms, SD = 70.85). No significant interaction effect was found, F(1, 218) = 0.48, p = 0.489.

To investigate whether the reaction times of compatibility was affected along with time, we calculated a three-way mixed ANOVA with the between-subjects factor: Contingency (full contingency vs. partial contingency) and the within-subjects factor: compatibility (compatible vs. incompatible) and blocks (1 to 10). The results yielded three significant main effects. That is, RTs in full contingency (M = 315.37 ms, SD = 51.75) were faster than the one in partial contingency (M = 331.49 ms, SD = 81.70), F(1, 217) = 4.34, p = 0.038, η_p^2 = 0.020. The main effect of compatibility proved that RTs were faster in compatible (M = 322.54 ms, SD = 68.31) than in incompatible trials (M = 324.30 ms, SD = 69.36), F(1, 217) = 5.41, p = 0.021, η_p^2 = 0.024. There was also a main effect of block, F(9, 1953) = 14.19, p < 0.001, η_p^2 = 0.061. Overall, RTs became faster over blocks. However, none of the other interactions reached to significance, with all ps > 0.5.

As in Experiment 1, we were also interested in whether the compatibility effect was affected by the switching of rules between blocks. Another mixed-effects ANOVA with the between-subjects factor: Contingency (full contingency vs. partial contingency) and the within-subjects factor: mapping switch (yes vs. no) and compatibility (compatible vs. incompatible) was run. Again, only the main effect of Contingency and compatibility reached to significance, with the same pattern as before (Contingency, F(1, 218) = 4.28, p = 0.04, η_p^2 = 0.019; Compatibility, F(1, 218) = 4.61, p = 0.033, η_p^2 = 0.021) .

Discussion

Experiment 2 aimed to replicate and extend the findings of Experiment 1, by manipulating the contingency between actions and outcomes. While the compatibility effect obtained in Experiment 1 was replicated, the R-O contingency did not – in contrast to the findings by Elsner and Hommel (2004) – affect the compatibility effect. Responses, in general, were found to be slower in the partial contingency condition, though. Although this effect could be due to the lowered contingency, caution would have to be taken to interpret this between-participants effect. Together, the results suggest that – at least in the current paradigm, causal relations in the learning phase were still inferred when contingencies are only partial (80%).

Although the IM effect in Experiment 1 was small but significant (~ 2 ms difference), it was replicated in Experiment 2. This difference is relatively small compared to the effects obtained in the classical IM paradigm by Elsner and Hommel (2001). In their Experiment 1A/B, the non-reversal group responded averagely around 100 ms faster than the reversal group, although the effect varied per block. As noted in the introduction, though, as well as in Sun et al. (2020) the effect of reversing the instructions may be driven by different processes than only the triggering of responses by the perceived outcomes. Indeed, the paradigm by Wolfensteller and Ruge (2011) produced differences of 4-8 ms, which is in the same ballpark as the current findings. It should be noted that in contrast to their work, our paradigm capitalized on the effects of inferred causal relations in the task at hand, by switching between the two available mappings, instead of creating novel mappings at each block. This, and the fact that the studies were conducted online, may explain the small, but meaningful effect obtained here.

General Discussion

In two experiments we report the results of a continuous IM paradigm in which learning and testing phases were combined in one task. In 10 short blocks, participants reacted to imperative cues and observed the result of their actions. Mappings between actions and outcomes either switched or remained the same from one block to the next. Experiment 1 revealed an IM effect: Actions to imperative cues that were compatible with the action's outcome yielded faster responses than imperative cues that were incompatible with the actions' outcomes. Experiment 2 replicated the finding and furthermore revealed that the IM effect was unaffected by a lower contingency between actions and outcomes.

The findings first of all conceptually replicate those by Wolfensteller and Ruge (2011), demonstrating that ideomotor effects can occur after very limited learning and be demonstrated within a very limited test phase. Importantly, however, in the current studies participants switched back and forth between the two opposite mappings. Hence, our findings do not only demonstrate fast learning, but also abandoning and retrieving of learned relations. The obtained ideomotor effect in the current studies, is therefore more likely to be the result of explicit causal inferences, or propositions about the relation between outcomes and actions, than the result of slow, associative learning (see Mitchell et al., 2009).

This explanation is consistent with the finding in Experiment 2 that moving from a 100% to an 80% contingency did not significantly affect the compatibility effect. Although a lower number of pairings could be expected to lead to weaker associations and weaker IM effects (Elsner & Hommel, 2004), inferring the causal relation should still be fairly easy. Therefore, in the current task, it seems that inferred causal relations were the driving force behind the compatibility effect.

Crucially, in none of the experiments, evidence was found for lasting bidirectional associations: No interaction between the compatibility effect and mapping switches was found. This reveals that responses on a particular block were unaffected by the mapping that featured in the block before. This demonstrates that propositions about causal relations in the task at hand could drive automatic IM effects, in the absence of lasting bidirectional associations in memory (see Dogge, Custers, Gayet, et al., 2019, for a similar approach).

It should be noted that the current paradigm used a novel continuous IM paradigm in which acquisition and testing were combined in the same continuous task. While this paradigm is less suitable for demonstrating IM effects based on lasting associations, it could have a number of advantages for studying IM learning. First of all, it does not create an artificial separation between the acquisition and test phase, which could be the reason that some IM studies do not find an IM effect (see Sun et al., 2022, under revision). Such a separation would be especially problematic from a propositional perspective, as the participant may wonder whether the relations learned in the acquisition phase still apply in the test phase. Second, it could dramatically shorten the duration of IM effects. But most of all, it does allow for studying IM learning in real time. Therefore, the paradigm reported here may provide a useful addition to the toolbox of IM researchers.

The current findings provide a compelling demonstration of an alternative account for which hints could already be found in existing findings in the literature. First of all, we already knew that explicit causal inferences are made by participants (Elsner & Hommel, 2001; Sun, et al., 2020). One could think of these as self-generated rules or instructions that have been demonstrated to produce ideomotor effects before (Theeuwes et al., 2015). Second, ideomotor effects have been demonstrated to arise very quickly after a few trials (Wolfensteller & Ruge, 2011). What the current paradigm adds is that it combines these insights in a paradigm in which bidirectional associations as an underlying mechanism is very unlikely. Especially because the paradigm requires abandoning and reinstating learned knowledge about relations between actions and outcomes on each block, the current findings favor an explanation in terms of propositional knowledge about causal relations.

Together, the results have important implications for our understanding of ideomotor effects. We feel it is safe to say that, by and large, the ideomotor literature features paradigms that are constructed with the mechanistic account in mind, but do not necessarily rule out the alternative account proposed here. Most strikingly, the vast majority of ideomotor studies use an extensive learning phase of hundreds of trials for participants to learn the relations between two keys and two outcomes (see Shin et al., 2010). It goes without saying that most of us would figure out this relation in one or two trials. Yet, experiments that question the assumption that a long learning phase is necessary emerged only recently. We argue that by putting the assumptions of ideomotor theory to the test, we will learn much more about what ideomotor effects are and what they are not.

While recent attempts have been made to salvage the mechanistic framework in the face of findings that challenge some of its core assumptions (Hommel & Wiers, 2017; Frings et al., 2020), such attempts also weaken the explanatory power of the mechanistic account: If ideomotor effects are moderated by goals, intentions, context, etc., the question arises whether they can still be best understood in terms of conditional automaticity (c.f., Monsell et al., 2001), or rather in terms of deliberate and explicit processes, that may occasionally produce an automatic effect on action. Such a switch in perspective, may have a large influence on the literature, as it departs from the exact opposite standpoint. Pitting ideomotor theory against alternative explanations would be the only way to solve this debate.

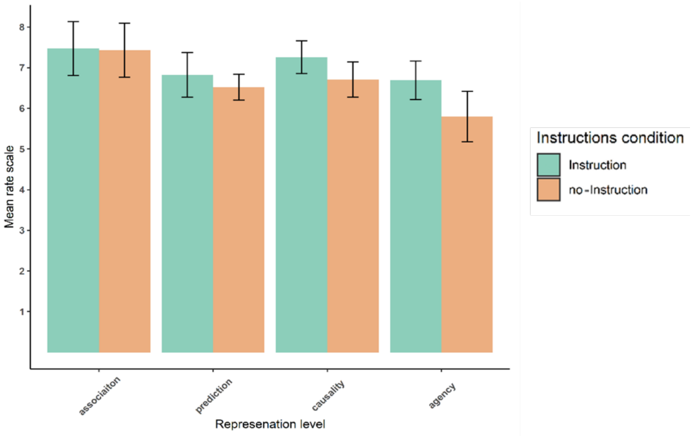
Appendices

Appendix a: Supplementary Section for Chapter 3

Supplementary Figures

Supplementary Figure 3.1.

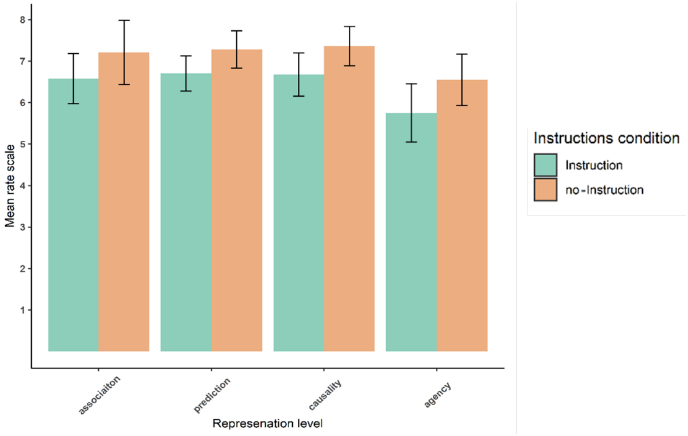
Results of the manipulation check of Instructions for Experiment 1.



Note. The bars represent the means of the four representation levels as a function of Instructions condition. Error bars represent 95% confidence intervals of the means.

Supplementary Figure 3.2

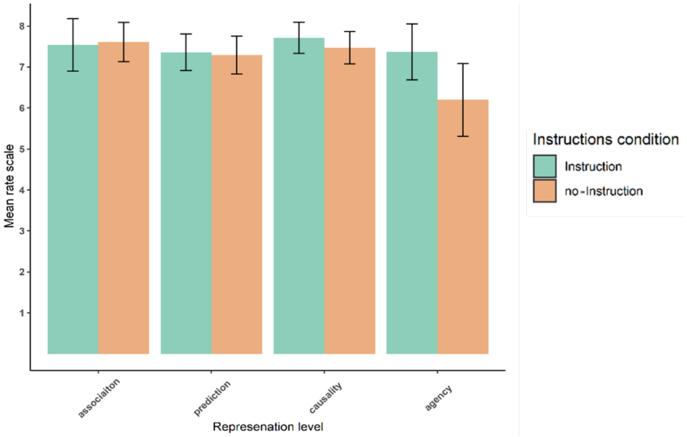
Results of the manipulation check of Instructions for Experiment 2.



Note. The bars represent the means of the four representation levels as a function of Instructions condition. Error bars represent 95% confidence intervals of the means.

Supplementary Figure 3.3

Results of the manipulation check of Instructions for Experiment 3a



Note. The bars represent the means of the four representation levels as a function of Instructions condition. Error bars represent 95% confidence intervals of the means.

Questionnaire : Manipulation check of R-O mappings

Dear participant,

Congrats! You have almost reached the end of the experiment. We will ask you to answer a few more questions only about the first part.

Please Remember:

The following questions are based on first part(P1).

1. Which tone did the left key press produce?

① the left key press produced the High tone

② the left key press produced the Low tone

③ the left key press produced both tones

④ the left key press was irrelevant to the tones

2. Which tone did the right key press produce?

① the right key press produced the High tone

② the right key press produced the Low tone

③ the right key press produced both tones

④ the right key press was irrelevant to the tones

Questionnaire : Manipulation check of Instructions

(1) Do you think particular tones were associated with particular key presses?

Not at all 1 2 3 4 5 6 7 8 9 very much

(2) Do you think particular tones went together with particular key presses?

Not at all 1 2 3 4 5 6 7 8 9 very much

(3) Do you think particular tones occurred with particular key presses?

Not at all 1 2 3 4 5 6 7 8 9 very much

(4) Could you predict the particular tones based on your key presses?

Not at all 1 2 3 4 5 6 7 8 9 very much

(5) Could you anticipate the particular tones based on your key presses?

Not at all 1 2 3 4 5 6 7 8 9 very much

(6) Did particular tones follow particular key presses?

Not at all 1 2 3 4 5 6 7 8 9 very much

(7) Do you think particular key presses caused particular tones?

Not at all 1 2 3 4 5 6 7 8 9 very much

(8) Do you think particular key presses produced particular tones?

Not at all 1 2 3 4 5 6 7 8 9 very much

(9) Do you think particular key presses generated particular tones?

Not at all 1 2 3 4 5 6 7 8 9 very much

(10) Did you feel you could influence which tone would occur?

Not at all 1 2 3 4 5 6 7 8 9 very much

(11) Did you feel you could control which tone would occur?

Not at all 1 2 3 4 5 6 7 8 9 very much

(12) Did you feel you could determine which tone would occur?

Not at all 1 2 3 4 5 6 7 8 9 very much

Appendix b: Supplementary Section for Chapter 4

Questionnaire: BIS/BAS Scale

Each item of this questionnaire is a statement that a person may either agree with or disagree with. For each item, indicate how much you agree or disagree with what the item says. Please respond to all the items; do not leave any blank. Choose only one response to each statement. Please be as accurate and honest as you can be. Respond to each item as if it were the only item. That is, don't worry about being "consistent" in your responses. Choose from the following four response options: 1 = very true for me; 2 = somewhat true for me; 3 = somewhat false for me; 4 = very false for me

1. A person's family is the most important thing in life.

2. Even if something bad is about to happen to me, I rarely experience fear or nervousness.

3. I go out of my way to get things I want.

4. When I'm doing well at something I love to keep at it.

5. I'm always willing to try something new if I think it will be fun.

6. How I dress is important to me.

7. When I get something I want, I feel excited and energized.

8. Criticism or scolding hurts me quite a bit.

9. When I want something I usually go all-out to get it.

10. I will often do things for no other reason than that they might be fun.

11. It's hard for me to find the time to do things such as get a haircut.

12. If I see a chance to get something I want I move on it right away.

13. I feel pretty worried or upset when I think or know somebody is angry at me.

14. When I see an opportunity for something I like I get excited right away.

15. I often act on the spur of the moment.

16. If I think something unpleasant is going to happen I usually get pretty "worked up."

17. I often wonder why people act the way they do.

18. When good things happen to me, it affects me strongly.

19. I feel worried when I think I have done poorly at something important.

20. I crave excitement and new sensations.

21. When I go after something I use a "no holds barred" approach.

22. I have very few fears compared to my friends.

23. It would excite me to win a contest.

24. I worry about making mistakes.

SCORCES

Items other than 2 and 22 are reverse-scored.

BAS Drive: 3, 9, 12, 21

BAS Fun Seeking: 5, 10, 15, 20

BAS Reward Responsiveness: 4, 7, 14, 18, 23

BIS: 2, 8, 13, 16, 19, 22, 24

Items 1, 6, 11, 17, are fillers.

The fact that there are three BAS-related scales and only one BIS-related scales was not planned or theoretically motivated. The factors emerged empirically, from an item set that was intended to capture diverse manifestations of the BAS, according to various theoretical statements. It is likely that a broader sampling of items on the BIS side would also have resulted in more than one scale. I do not encourage combining the BAS scales, however, because they do turn out to focus on different aspects of incentive sensitivity. In particular, Fun Seeking is known to have elements of impulsiveness that are not contained in the other scales.

Appendix c: Supplementary Section for Chapter 5

Supplemental Material A. Results of Experiment 1

Table 1.1

RTs in different group participants of Experiment 1 (with the induction block)

N (inclusion)	Compatible Mean (SD)	Incompatible Mean (SD)	t-test (C < IC)	BF_{-0}	BF_{0}
50(49)	319.308 (41.731)	322.242 (42.335)	$t(48) = -2.13$, $p = 0.019$, $d = -0.304$	2.393	0.41
60(59)	316.307 (42.654)	318.994 (42.615)	$t(58) = -2.07$, $p = 0.021$, $d = -0.240$	2.035	0.49
70(69)	317.198 (42.988)	319.753 (43.694)	$t(68) = -2.13$, $p = 0.019$, $d = -0.256$	2.138	0.46
80(79)	317.165 (46.224)	319.761 (47.953)	$t(78) = -2.23$, $p = 0.014$, $d = -0.251$	2.526	0.39
90(89)	316.007 (44.946)	318.722 (46.577)	$t(88) = -2.51$, $p = 0.007$, $d = -0.266$	4.421	0.22
100(98)	317.622 (50.072)	320.141 (51.016)	$t(97) = -2.46$, $p = 0.008$, $d = -0.248$	3.828	0.26

Note. (1) The Table 1.1 contains the comparison between compatible and incompatible conditions including the induction block as we preregistered to determine the sample size based on the general performance between compatible and incompatible trials. (2) We used the default JZS prior implemented in JASP (version 0.13), and BFs were calculated by the one-tailed Bayesian Paired Sample T-Test. For instance, given the selected data sets, BF-0 = 2.13 indicates that the result has 2.13 times in evidence of supporting the one-sided negative hypothesis (i.e., H-, RTs of compatible condition were faster than the incompatible condition, namely, $RT_{compatible} < RT_{incompatible}$) than the null hypothesis (i.e., H0, means that there were no difference between compatible and incompatible condition, namely, $RT_{compatible} = RT_{incompatible}$).

Table 1.2

Error Rates in different group participants of Experiment 1 (with the induction block)

N (inclusion)	Compatible Mean (SD)	Incompatible Mean (SD)	Wilcoxon-Test (C < IC)	BF_{-0}	BF_{0-}
50(49)	0.028 (0.024)	0.026 (0.025)	$p = 0.848$	0.107	9.348
60(59)	0.027 (0.025)	0.028 (0.029)	$p = 0.562$	0.188	5.316
70(69)	0.029 (0.027)	0.030 (0.031)	$p = 0.647$	0.153	6.521
80(79)	0.028 (0.028)	0.029 (0.031)	$p = 0.462$	0.196	5.108
90(89)	0.029 (0.028)	0.028 (0.031)	$p = 0.395$	0.178	5.623
100(98)	0.029 (0.028)	0.030 (0.031)	$p = 0.358$	0.162	6.186

Note. The one-sided negative hypothesis (i.e., H-) means that error rates of compatible condition were smaller than the incompatible condition, namely, $ER_{compatible} < ER_{incompatible}$); and the null hypothesis (i.e., H0) means that there were no difference between compatible and incompatible condition, namely, $ER_{compatible} = ER_{incompatible}$). Here, we used the default JZS prior implemented in JASP (version 0.13), and BFs were calculated by the one-tailed Bayesian Paired Sample T-Test of mean error rates for better interpretation of the results.

Supplemental Material B. Results of High memory group in Experiment 2

Error rates of the Primary task.

To verify that our results were consistent either in the whole data sets or in sub data of high memory group, of which individual memory check accuracy was higher than the lower threshold (0.55, above chance level), we treated the sub datasets in the same way. That is, a two-way mixed 2 (Compatibility: compatible vs incompatible) * 2 (Type of contingency groups: the full contingency vs. partial contingency group) ANOVA was applied to error rates of the high memory group. The results yielded a main effect of Type of contingency groups, F(1,198) = 5.08*, p = 0.025, η_p^2 = 0.025, and a week interaction, F(1,198) = 3.74, p = 0.055, η_p^2 = 0.019. No difference showed statistically significant of the effect of Compatibility, F(1,198) = 0.11, p = 0.0743. Follow-up t-test about the compatibility effect within each type of contingency groups did not approach to significance, all ps > 0.1.

RTs of the Primary task.

For RTs analysis, neither the main effect of Compatibility, F(1,198) = 2.11, p = 0.148, nor that of interaction effect, F(1,198) = 0, p = 0.148. Only a week main effect of Type of contingency groups, F(1,198) = 3.65, p = 0.058, η_p^2 = 0.018. Though the results in high memory group did not yield statistically significant, one of the possible reasons was because the preset cut-off ticket for picking high memory group was not universal to all experimental conditions. At least in the non-full contingency conditions, theoretically, individual mean accuracy was lower with larger variance compared to the full contingency condition. As it proved in the memory check, error rates of full contingency condition (M = 0.117, SD = 0.164) were smaller than the partial contingency condition (M = 0.164, SD = 0.191).

Summary (Nederlandse Samenvatting)

Summary (English)

Practice makes perfect. The more you practice playing the piano, or learning a new language, the more expertise you will have. While a similar perspective in neuroscience assumed that “Neurons that fire together wire together” (aka, the Hebbian learning rule, Hebb, 2002). That is, through exhaustive repetition, the neural sequence/link in your brain will also get more robust. Ideomotor (IM) theory offers an elegant and parsimonious account for understanding how the mind controls the body (see historical development in Shin et al., 2010). In brief, building on German philosophy and British psychologies, William James argued that thinking of an action would be enough to produce the actual motor behavior leading to this action and that control was mainly required to inhibit, not initiation action. As such, thinking of an action and executing it were considered to be intertwined. Capitalizing on this assumption, modern theorists raised the idea of “common coding” of perception and action (Prinz, 1990; Prinz, Aschersleben, & Koch, 2009) and introduced the term event files, referring to the shared and homogenous representations of actions and perceptions (aka, the Theory of Event Coding, TEC) (Hommel et al., 2001; Hommel, 2019).

Contemporary approaches to ideomotor effects are built on a standard two-stage paradigm, including the primary acquisition phase and the second test phase (Elsner & Hommel, 2001). That is, associative relations between responses (Rs) and outcomes (Os) are first trained through repetitions, after which the selection of the response by an outcome will be facilitated if the outcome is linked to the same response as before, or be interfered if the outcome connected to the opposite. The ideomotor principle has been invoked in domains as diverse as action control (e.g., Pfiter, 2019), imitation and empathy (e.g., Iacoboni, 2009), imitative learning (e.g., Paulus 2014), language processing and tool use (e.g., Badets, 2016, Badet & Osiurak, 2017). The associative link of action representations and action understanding could also be applied in social robotics contexts, that observing a robot movement elicited a similar action representation as one observed a human’s movement (e.g., Wykowska, Chellali, Al-Amin, Müller, 2014).

However, looking in-depth analysis of the growing body of the mechanistic ideomotor effect, it documents almost common on the free-choice task and a forced-choice task in which participants receive instructions that are either consistent or inconsistent with the earlier acquired mapping. Also, failures to observe IM activation when switching the testing phase from between-subject design to within-subject design have been haunting the idea that associations provide a simple account of the automatic nature of ideomotor activation. At least, it is not the only possible account. We argue here, that these two tasks do not rule out the use of explicit propositional knowledge.

The dissertation focuses on the question whether the automatic association is really the only crucial mechanism for acquiring the knowledge between actions and following outcomes, or that other factors usually contribute to activating the acquired knowledge. We first reviewed the recent literature to get an overview of alternative explanations and evidence that could also play a role in this compared to the traditional associative learning mechanism (Chapter 2). Then, in the remaining part of the dissertation, three empirical chapters systematically test the possibility of alternative explanations when acquiring the action-outcome associations. Specifically, we (Chapter 3 and 4) investigated verbal instructions, rewarding outcomes, equimodality of the outcomes and primes, and extinction effect based on the two-stage paradigm. Our results showed that people could spontaneously get the “gut” feeling about the contingency between every action and its following outcome even without direct instructions (Chapter 3). However, though such contingent links could be stored in memory, it is hard to retrieve and transfer into overt behaviors, even with the help of rewarding and equimodality (Chapter 4). Instead, only seeing the same contingent link again (e.g., R-->O), participants could automatically activate primary action-outcome knowledge in mind thereafter affect the secondary response, which means that awareness of the action-outcome knowledge is one of the crucial factors to ideomotor activation (Chapter 4). In line with the assumption of causal inference from the preceding chapters, we also came up with a novel paradigm by featuring the acquisition phase and the test phase in one trial to test whether ideomotor effects can arise instantly (Chapter 5). Conceptually replicated the fast learning by Wolfensteller and Ruge (2011), we did find instant IM effects, even the contingent link between actions and outcomes moved from 100% to 80%. We also found that these effects could flip immediately without switching costs once the relations change.

These results allow us to move beyond the mechanistic account of ideomotor effects. According to the findings from our own and other labs, it provides new insights into the role of causal inference during the ideomotor activation, rather than merely bidirectional associations formed in memory. So it seems that research on action control could benefit from more careful experimentation and theorizing to help understand in which ways cues could elicit goal-directed behavior given a situation. It definitely remains worthwhile to look further robust quality of the evidence for causal inference in ideomotor literature. The automatic spreading of activation via mere imagination should be cautioned; at least, future research should consider the joint impacts of propositions and associations on the ideomotor effects. Upon the broad implication, exploring the factors related to causal inferences allow us to investigate the mechanism of information processing when interacting with other people or with the environment.

Summary in Dutch (Nederlandse samenvatting)

I thank the DeepL translator (www.DeepL.com) break the language barrier.

Oefening baart kunst. Hoe meer je oefent met pianospelen, of het leren van een nieuwe taal, hoe meer expertise je zult hebben. Terwijl een soortgelijk perspectief in de neurowetenschap ervan uitging dat "Neuronen die samen vuren, samen bedraden" (ook bekend als de Hebbiaanse leerregel, Hebb, 2002). Dat wil zeggen, door uitputtende herhaling, zal de neurale sequentie/verbinding in je hersenen ook robuuster worden. De ideomotorische theorie (IM) biedt een elegante en eenvoudige verklaring om te begrijpen hoe de geest het lichaam bestuurt (zie historische ontwikkeling in Shin et al. , 2010). In het kort, voortbouwend op de Duitse filosofie en de Britse psychologie, stelde William James dat het denken aan een actie voldoende zou zijn om het eigenlijke motorische gedrag te produceren dat tot deze actie leidt en dat controle vooral nodig was om actie te remmen, niet om actie te initiëren. Als zodanig werden het denken aan een actie en het uitvoeren ervan beschouwd als met elkaar verweven. Voortbordurend op deze aanname, opperden moderne theoretici het idee van "gemeenschappelijke codering" van waarneming en actie (Prinz, 1990; Prinz, Aschersleben, & Koch, 2009) en introduceerden de term event files, verwijzend naar de gedeelde en homogene representaties van acties en percepties (aka, de Theory of Event Coding, TEC) (Hommel et al., 2001; Hommel, 2019).

Hedendaagse benaderingen van ideomotorische effecten zijn gebaseerd op een standaard paradigma met twee fasen, met inbegrip van de primaire verwervingsfase en de tweede testfase (Elsner & Hommel, 2001). Dat wil zeggen, associatieve relaties tussen responsen (Rs) en uitkomsten (Os) worden eerst getraind door middel van herhalingen, waarna de selectie van de respons door een uitkomst wordt vergemakkelijkt als de uitkomst gekoppeld is aan dezelfde respons als voorheen, of wordt verstoord als de uitkomst gekoppeld is aan het tegenovergestelde. Het ideomotorische principe is ingeroepen in domeinen zo divers als actiecontrole (e.g., Pfiter, 2019), imitatie en empathie (e.g., Iacoboni, 2009), imiterend leren (e.g., Paulus 2014), taalverwerking en gereedschapgebruik (e.g., Badets, 2016, Badet & Osiurak, 2017). De associatieve link van actie representaties en actie begrip zou ook kunnen worden toegepast in sociale robotica contexten, dat het observeren van een robot beweging een soortgelijke actie representatie uitlokte als men de beweging van een mens observeerde (e.g., Wykowska, Chellali, Al-Amin, Müller, 2014).

Echter, kijkend naar een diepgaande analyse van het groeiende corpus van het mechanistische ideomotorische effect, documenteert het bijna gemeenschappelijk op de vrije-keuzetaak en een geforceerde-keuzetaak waarin deelnemers instructies krijgen die ofwel consistent ofwel inconsistent zijn met de eerder verworven mapping. Ook hebben mislukkingen in het waarnemen van IM-activatie bij het omschakelen van de testfase van between-subject design naar within-subject design gespookt met het idee dat associaties een eenvoudige verklaring bieden voor de automatische aard van ideomotorische activatie. Tenminste, het is niet de enige mogelijke verklaring. Wij betogen hier, dat deze twee taken het gebruik van expliciete propositionele kennis niet uitsluiten.

Het proefschrift richt zich op de vraag of de automatische associatie werkelijk het enige cruciale mechanisme is voor het verwerven van de kennis tussen acties en volgende uitkomsten, of dat andere factoren meestal bijdragen aan het activeren van de verworven kennis. We hebben eerst de recente literatuur doorgenomen om een overzicht te krijgen van alternatieve verklaringen en evidenties die hierbij ook een rol zouden kunnen spelen ten opzichte van het traditionele associatieve leermechanisme (Hoofdstuk 2). Vervolgens, in het resterende deel van het proefschrift, testen drie empirische hoofdstukken systematisch de mogelijkheid van alternatieve verklaringen bij het verwerven van de actie-uitkomst associaties. Specifiek onderzochten we (Hoofdstuk 3 en 4) verbale instructies, belonende uitkomsten, equimodaliteit van de uitkomsten en primes, en extinctie-effect gebaseerd op het twee-fasen paradigma. Onze resultaten toonden aan dat mensen spontaan het "onderbuik" gevoel konden krijgen over de contingentie tussen elke actie en de daaropvolgende uitkomst, zelfs zonder directe instructies (Hoofdstuk 3). Echter, hoewel dergelijke voorwaardelijke verbanden in het geheugen kunnen worden opgeslagen, is het moeilijk om ze op te halen en om te zetten in openlijk gedrag, zelfs met behulp van belonen en gelijkwaardigheid (Hoofdstuk 4). In plaats daarvan kunnen deelnemers, als ze dezelfde voorwaardelijke koppeling opnieuw zien (bijv. R--> O), automatisch primaire actie-uitkomstkennis in gedachten activeren en vervolgens de secundaire respons beïnvloeden, wat betekent dat bewustzijn van de actie-uitkomstkennis een van de cruciale factoren is voor ideomotorische activering (Hoofdstuk 4). In lijn met de aanname van causale gevolgtrekking uit de voorgaande hoofdstukken, kwamen we ook met een nieuw paradigma door de verwervingsfase en de testfase in één trial te laten plaatsvinden om te testen of ideomotorische effecten onmiddellijk kunnen ontstaan (Hoofdstuk 5). Conceptueel gerepliceerd het snelle leren door Wolfensteller en Ruge (2011), vonden we inderdaad onmiddellijke IM effecten, zelfs de voorwaardelijke koppeling tussen acties en uitkomsten verschoof van 100% naar 80%. We vonden ook dat deze effecten onmiddellijk konden omslaan zonder omschakelingskosten zodra de relaties veranderden.

Deze resultaten laten ons toe verder te gaan dan de mechanistische verklaring van ideomotorische effecten. Volgens de bevindingen van onze en andere labs, biedt het nieuwe inzichten in de rol van causale gevolgtrekkingen tijdens de ideomotorische activatie, eerder dan louter bidirectionele associaties gevormd in het geheugen. Het lijkt er dus op dat onderzoek naar actiecontrole baat zou kunnen hebben bij meer zorgvuldige experimenten en theorievorming om te helpen begrijpen op welke manieren signalen in een bepaalde situatie doelgericht gedrag kunnen uitlokken. Het blijft zeker de moeite waard om de robuuste kwaliteit van het bewijs voor causale gevolgtrekking in de ideomotorische literatuur verder te onderzoeken. Voor het automatisch verspreiden van activatie via louter verbeelding moet worden gewaakt; toekomstig onderzoek zou op zijn minst de gezamenlijke impact van proposities en associaties op de ideomotorische effecten moeten overwegen. Wat de brede implicatie betreft, laat de exploratie van de factoren gerelateerd aan causale inferenties ons toe het mechanisme van informatieverwerking te onderzoeken bij interactie met andere mensen of met de omgeving.

References

References

Aarts, H. (2012). Goals, motivated social cognition, and behavior. In S. T. Fiske & M. C. Neil (Eds.), *The SAGE Handbook of Social Cognition* (pp. 75–95). SAGE Publications Inc. https://doi.org/10.4135/9781446247631.n5

Aarts, H., & Dijksterhuis, A. (2000). Habits as knowledge structures: Automaticity in goal-directed behavior. *Journal of Personality and Social Psychology*, *78*, 53–63. https://doi.org/10.1037/0022-3514.78.1.53

Allen, M., Poggiali, D., Whitaker, K., Marshall, T. R., & Kievit, R. A. (2019). Raincloud plots: a multi-platform tool for robust data visualization. *Wellcome Open Research*, *4*, 63. https://doi.org/10.12688/wellcomeopenres.15191.1

Ansfield, M. E., & Wegner, D. M. (1996). The feeling of doing. In P. M. Gollwitzer & J. A. Bargh (Eds.), *The psychology of action: Linking cognition and motivation to behavior* (pp. 482–506). The Guilford Press.

Bargh, J. A. (1994). The four horsemen of automaticity: Awareness, intention, efficiency, and control in social cognition. In R. S. Wyer Jr. & T. K. Srull (Eds.), *Handbook of Social cognition: Basic processes; Applications* (pp. 1–40). Lawrence Erlbaum Associates, Inc.

Bargh, J. A. (2005). Bypassing the will: Towards demystifying the nonconscious control of social behavior. In R. R. Hassin, J. S. Uleman, & J. A. Bargh (Eds.), *The new unconscious* (pp. 37–60). Oxford University Press.

Bargh, J. A. (2006). What have we been priming all these years? On the development, mechanisms, and ecology of nonconscious social behavior. *European Journal of Social Psychology*, *36*, 147–168. https://doi.org/10.1002/ejsp.336

Bargh, J. A., Lee-Chai, A., Barndollar, K., Gollwitzer, P. M., & Trötschel, R. (2001). The automated will: Nonconscious activation and pursuit of behavioral goals. *Journal of Personality and Social Psychology*, *81*, 1014–1027. https://doi.org/10.1037/0022-3514.81.6.1014

Barsalou, L. W. (1999). Perceptual symbol systems. *Behavioral and Brain Sciences*, *22*, 577–660.

Barsalou, L. W. (2008). Grounded cognition. *Annual Review of Psychology*, *59*, 617–645. https://doi.org/10.1146/annurev.psych.59.103006.093639

Blakemore, S.-J., Wolpert, D. M., & Frith, C. D. (1998). Central cancellation of self-produced tickle sensation. *Nature Neuroscience*, *1*, 635–640.

Blakemore, S.-J., Wolpert, D., & Frith, C. (2000). Why can’t you tickle yourself? *Neuroreport*, *11*(11), R11–R16.

Brass, M., & Haggard, P. (2008). The what, when, whether model of intentional action. *Neuroscientist*, *14*, 319–325. https://doi.org/10.1177/1073858408317417

Carpenter, B. (1852). On the influence of suggestion in modifying and directing muscular movement, independently of volition. *Proceedings of the Royal Institution*, *1*, 147–154.

Cartoni, E., Balleine, B., & Baldassarre, G. (2016). Appetitive pavlovian-instrumental transfer: A review. *Neuroscience and Biobehavioral Reviews*, *71*, 829–848.

https://doi.org/10.1016/j.neubiorev.2016.09.020

Carver, C. S., & White, T. L. (1994). Behavioral inhibition, behavioral activation, and affective responses to impending reward and punishment: The BIS/BAS Scales. *Journal of Personality and Social Psychology*, *67*, 319–333. https://doi.org/10.1037/0022-3514.67.2.319

Cleeremans, A., Destrebecqz, A., & Boyer, M. (1998). Implicit learning: News from the front. *Trends in Cognitive Sciences*, *2*, 406–416. https://doi.org/10.1016/S1364-6613(98)01232-7

Cooper, R. P., & Shallice, T. (2006). Hierarchical schemas and goals in the control of sequential behavior. *Psychological Review*, *113*, 887–916. https://doi.org/10.1037/0033-295X.113.4.887

Custers, R., & Aarts, H. (2005). Positive affect as implicit motivator: On the nonconscious operation of behavioral goals. *Journal of Personality and Social Psychology*, *89*, 129–142. https://doi.org/10.1037/0022-3514.89.2.129

Custers, R., & Aarts, H. (2010). The unconscious will: How the pursuit of goals operates outside of conscious awareness. *Science*, *329*, 47–50.

Custers, R., & Aarts, H. (2011). Learning of predictive relations between events depends on attention, not on awareness. *Consciousness and Cognition*, *20*, 368–378. https://doi.org/10.1016/j.concog.2010.05.011

Custers, R., Eitam, B., & Bargh, J. A. (2012). Conscious and unconscious processes in goal pursuit. In H. Aarts & A. J. Elliot (Eds.), *Goal-directed behavior* (pp. 231–266). Taylor and Francis. https://doi.org/10.4324/9780203869666

Damasio, A. R. (1994). *Descartes' error: Emotion, rationality and the human brain*. New York: Putnam.

Demiris, Y., & Dearden, A. (2005). From motor babbling to hierarchical learning by imitation: a robot developmental pathway. In L. Berthouze, F. Kaplan, H. Kozima, H. Yano, J. Konczak, G. Metta, J. Nadel, G. Sandini, G. Stojanov, & C. Balkenius (Eds.), *Proceedings of the Fifth International Workshop on Epigenetic Robotics: Modeling Cognitive Development in Robotic Systems* (Vol. 123, pp. 31–37). http://cogprints.org/4961/

Dickinson, A., Shanks, D., & Evenden, J. (1984). Judgement of act-outcome contingency: The role of selective attribution. *The Quarterly Journal of Experimental Psychology*, *36*, 29–50.

Dijksterhuis, A., & Bargh, J. A. (2001). The perception-behavior expressway: Automatic effects of social perception on social behavior. In M. P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 33, pp. 1–40). Academic Press. https://doi.org/10.1016/s0065-2601(01)80003-4

Dogge, M., Custers, R., & Aarts, H. (2019). Moving forward: On the limits of motor-based forward models. *Trends in Cognitive Sciences*, *23*, 743–753. https://doi.org/10.1016/J.TICS.2019.06.008

Dogge, M., Custers, R., Gayet, S., Hoijtink, H., & Aarts, H. (2019). Perception of action-outcomes is shaped by life-long and contextual expectations. *Scientific Reports*, *9*(1), 1–9. https://doi.org/10.1038/s41598-019-41090-8

Doksum, K. (1974). Empirical probability plots and statistical inference for nonlinear models in the two-sample case. *The annals of statistics*, 267-277.

Doksum, K. A., & Sievers, G. L. (1976). Plotting with confidence: Graphical comparisons of two populations. *Biometrika, 63*(3), 421-434.

Doksum, K. (1977). Some graphical methods in statistics: A review and some extensions. *Statistica Neerlandica, 31*(2), 53-68.

Dutzi, I. B., & Hommel, B. (2009). The microgenesis of action-effect binding. *Psychological Research*, *73*, 425–435. https://doi.org/10.1007/s00426-008-0161-7

Eder, A. B., & Dignath, D. (2017). Influence of verbal instructions on effect-based action control. *Psychological Research*, *81*, 355–365. https://doi.org/10.1007/s00426-016-0745-6

Eder, A. B., Erle, T. M., & Kunde, W. (2020). Reward strengthens action-effect binding. *Motivation Science*, *6*, 297–302. https://doi.org/10.1037/mot0000153

Eitam, B., Kennedy, P. M., & Higgins, T. (2013). Motivation from control. *Experimental Brain Research*, *229*, 475–484.

Eitam, B., Schul, Y., & Hassin, R. R. (2009). Goal relevance and artificial grammar learning. *Quarterly Journal of Experimental Psychology*, *62*, 228–238. https://doi.org/10.1080/17470210802479113

Elsner, B., & Hommel, B. (2001). Effect anticipation and action control. *Journal of Experimental Psychology: Human Perception and Performance*, *27*, 229–240. https://doi.org/10.1037/0096-1523.27.1.229

Elsner, B., & Hommel, B. (2004). Contiguity and contingency in action-effect learning. *Psychological Research*, *68*, 138–154. https://doi.org/10.1007/s00426-003-0151-8

Faul, F., Erdfelder, E., Lang, A. G., & Buchner, A. (2007). *G*Power 3: A flexible statistical power analysis program for the social, behavioral, and biomedical sciences*. *39*, 175–191. https://doi.org/10.3758/BF03193146

Fiedler, K., Messner, C., & Bluemke, M. (2006). Unresolved problems with the “I”, the “A”, and the “T”: A logical and psychometric critique of the Implicit Association Test (IAT). *European Review of Social Psychology*, *17*(1), 74–147. https://doi.org/10.1080/10463280600681248

Frings, C., Hommel, B., Koch, I., Rothermund, K., Dignath, D., Giesen, C., Kiesel, A., Kunde, W., Mayr, S., Moeller, B., Möller, M., Pfister, R., & Philipp, A. (2020). Binding and retrieval in action control (BRAC). *Trends in Cognitive Sciences*, *24*, 375–387. https://doi.org/10.1016/j.tics.2020.02.004

Gallese, V., Fadiga, L., Fogassi, L., & Rizzolatti, G. (1996). Action recognition in the premotor cortex. *Brain*, *119*, 593–609. https://doi.org/10.1093/BRAIN/119.2.593

Gallistel, C. R., & Matzel, L. D. (2013). The neuroscience of learning: Beyond the hebbian synapse. *Annual Review of Psychology*, *64*(1), 169–200. https://doi.org/10.1146/annurev-psych-113011-143807

Gawronski, B., & Bodenhausen, G. V. (2011). The associative–propositional evaluation model: Theory, evidence, and open questions. *Advances in Experimental Social*

Psychology, *44*, 59–127. https://doi.org/10.1016/B978-0-12-385522-0.00002-0

Gilbert, D. T., & Wilson, T. D. (2007). Prospection: experiencing the future. *Science*, *317*, 1351–1354. https://doi.org/10.1126/science.1144161

Gold, J. I., & Shadlen, M. N. (2007). The neural basis of decision making. *Annual Review of Neuroscience*, *30*, 535–574. https://doi.org/10.1146/annurev.neuro.29.051605.113038

Gollwitzer, P. (1990). Action phases and mindsets. In T. Higgins & R. Sorrentino (Eds.), *Handbook of motivation and cognition: Foundations of social behavior* (pp. 53–92). The Guilford Press.

Gollwitzer, P. M. (1999). Implementation intentions: Strong effects of simple plans. *American Psychologist*, *54*(7), 493–503. https://doi.org/10.1037/0003-066x.54.7.493

Greenwald, A. G. (1970). Sensory feedback mechanisms in performance control: With special reference to the ideo-motor mechanism. *Psychological Review*, *77*, 73–99. https://doi.org/10.1037/h0028689

Harleß, E. (1861). Der apparat des willens. *Zeitschrift Für Philosophie Und Philosophische Kritik*, *38*, 50–73.

Hebb, D. O. (2002). *The organization of behavior* (1st ed.). Psychology Press. https://doi.org/10.4324/9781410612403

Herbart, Johann Friedrich. (1825). *Psychologie als wissenschaft: neu gegründet auf erfahrung, metaphysik und mathematik* (Vol. 2). AW Unzer.

Herwig, A., & Horstmann, G. (2011). Action-effect associations revealed by eye movements. *Psychonomic Bulletin and Review*, *18*, 531–537. https://doi.org/10.3758/s13423-011-0063-3

Herwig, A., Prinz, W., & Waszak, F. (2007). Two modes of sensorimotor integration in intention-based and stimulus-based actions. *Quarterly Journal of Experimental Psychology*, *60*, 1540–1554. https://doi.org/10.1080/17470210601119134

Herwig, A., & Waszak, F. (2009). Intention and attention in ideomotor learning. *Quarterly Journal of Experimental Psychology*, *62*, 219–227. https://doi.org/10.1080/17470210802373290

Heyes, C. (2010). Where do mirror neurons come from? *Neuroscience & Biobehavioral Reviews*, *34*, 575–583. https://doi.org/10.1016/j.neubiorev.2009.11.007

Hommel, B. (1993). The relationship between stimulus processing and response selection in the Simon task: Evidence for a temporal overlap. *Psychological Research*, *55*, 280–290. https://doi.org/10.1007/BF00419688

Hommel, B., Müsseler, J., Aschersleben, G., & Prinz, W. (2001). The Theory of Event Coding (TEC): A framework for perception and action planning. *Behavioral and Brain Sciences*, *24*, 849–878. https://doi.org/10.1017/S0140525X01000103

Hommel, B. (2013). Ideomotor action control: On the perceptual grounding of voluntary actions and agents. In W. Prinz, M. Beisert, & A. Herwig (Eds.), *Action science: Foundations of an emerging discipline* (pp. 113–136). The MIT Press.

Hommel, B. (2019). Theory of Event Coding (TEC) V2.0: Representing and controlling perception and action. *Attention, Perception, & Psychophysics*, *81*, 2139–2154. https://doi.org/10.3758/s13414-019-01779-4

Hommel, B. (2021). GOALIATH: A theory of goal-directed behavior. *Psychological Research*, 1–24. https://doi.org/10.1007/s00426-021-01563-w

Hommel, B., Alonso, D., & Fuentes, L. (2003). Acquisition and generalization of action effects. *Visual Cognition*, *10*, 965–986.

Hommel, B., Müsseler, J., Aschersleben, G., & Prinz, W. (2001). The Theory of Event Coding (TEC): A framework for perception and action planning. *Behavioral and Brain Sciences*, *24*, 849–878. https://doi.org/10.1017/S0140525X01000103

Hommel, B., & Wiers, R. W. (2017). Towards a unitary approach to human action control. *Trends in Cognitive Sciences*, *21*, 940–949. https://doi.org/10.1016/j.tics.2017.09.009

Iacoboni, M. (2009). Imitation, empathy, and mirror neurons. *Annual Review of Psychology*, *60*, 653–670. https://doi.org/10.1146/annurev.psych.60.110707.163604

James, W. (1890). *The principles of psychology*. Henry Holt and Company.

James, W. (2007). *The principles of psychology. Vol. 1*. New York Cosimo Classics.

JASP Team. (2020). JASP (Version 0.14). https://jasp-stats.org/

Jeffreys, H. (1961). *Theory of probability* (3rd ed.). Cambridge University Press.

Kassambara, A. (2021). *rstatix: Pipe-friendly framework for basic statistical tests*.

Koch, C., & Tsuchiya, N. (2007). Attention and consciousness: two distinct brain processes. *Trends in Cognitive Sciences*, *11*, 16–22. https://doi.org/10.1016/j.tics.2006.10.012

Kruglanski, A. W., Shah, J. Y., Fishbach, A., Friedman, R., Woo, Y. C., & Sleeth-Keppler, D. (2002). A theory of goal systems. *Advances in Experimental Social Psychology*, *34*, 331–378.

Kühn, S., Elsner, B., Prinz, W., & Brass, M. (2009). Busy doing nothing: Evidence for nonaction-effect binding. *Psychonomic Bulletin and Review*, *16*, 542–549. https://doi.org/10.3758/PBR.16.3.542

Kunde, W. (2001). Response-effect compatibility in manual choice reaction tasks. *Journal of Experimental Psychology: Human Perception and Performance*, *27*, 387.

Kunde, W. (2004). Response priming by supraliminal and subliminal action effects. *Psychological Research*, *68*, 91–96. https://doi.org/10.1007/s00426-003-0147-4

Kunde, W., Kiesel, A., & Hoffmann, J. (2003). Conscious control over the content of unconscious cognition. *Cognition*, *88*, 223–242.

Kunde, W., Koch, I., & Hoffmann, J. (2004). Anticipated action effects affect the selection, initiation, and execution of actions. *The Quarterly Journal of Experimental Psychology Section A*, *57*, 87–106.

Lakens, D. (2013). Calculating and reporting effect sizes to facilitate cumulative science: A practical primer for t-tests and ANOVAs. *Frontiers in Psychology*, *4*, p. 863. https://doi.org/10.3389/fpsyg.2013.00863

Laycock, T. (1845). On the reflex function of the brain. *The British and Foreign Medical Review*, *19*, 298.

Lee, M. D., & Wagenmakers, E.-J. (2013). *Bayesian cognitive modeling: A practical course*. Cambridge University Press.

Liefooghe, B., Braem, S., & Meiran, N. (2018). The implications and applications of learning via instructions. *Acta Psychologica*, *184*, 1–3.

https://doi.org/10.1016/j.actpsy.2017.09.015

Locke, E. A., & Latham, G. P. (1984). *Goal setting: A motivational technique that works!* Prentice-Hall Englewood Cliffs, NJ.

Locke, E. A., & Latham, G. P. (1990). *A theory of goal setting & task performance.* Prentice-Hall, Inc.

Loersch, C., & Payne, B. K. (2011). The situated inference model: An integrative account of the effects of primes on perception, behavior, and motivation. *Perspectives on Psychological Science*, *6*, 234–252. https://doi.org/10.1177/1745691611406921

Lotze, R. H. (1852). *Medicinische psychologie*. Jazzybee Verlag.

Marien, H., Aarts, H., & Custers, R. (2014). Reward-driven modulation of adaptive control: How prospective monetary gains interact with unpredictable control demands. *Motivation and Emotion*, *38*(6), 771–778. https://doi.org/10.1007/S11031-014-9439-9

Marien, H., Custers, R., & Aarts, H. (2018). Understanding the formation of human habits: An analysis of mechanisms of habitual behaviour. In B. Verplanken (Ed.), *The psychology of habit: theory, mechanisms, change, and contexts* (pp. 51–69). Springer International Publishing. https://doi.org/10.1007/978-3-319-97529-0_4

Marien, H., Custers, R., & Aarts, H. (2019). Studying human habits in societal context: Examining support for a basic Stimulus–Response mechanism. *Current Directions in Psychological Science*, *28*, 614–618. https://doi.org/10.1177/0963721419868211

Melnikoff, D. E., & Bargh, J. A. (2018). The mythical number two. *Trends in Cognitive Sciences*, *22*, 280–293. https://doi.org/10.1016/j.tics.2018.02.001

Meltzoff, A. N., & Moore, K. M. (1997). Explaining facial imitation: a theoretical model. *Early Development and Parenting*, *6*(3-4), 179–192. https://www.ncbi.nlm.nih.gov/pmc/articles/PMC3953219/

Mitchell, C. J., De Houwer, Jan, & Lovibond, P. F. (2009). The propositional nature of human associative learning. *Behavioral and Brain Sciences*, *32*, 183–246. https://doi.org/10.1017/S0140525X09000855

Moeller, B., & Frings, C. (2019). From simple to complex actions: Response-response bindings as a new approach to action sequences. *Journal of Experimental Psychology: General*, *148*, 174–183. https://doi.org/10.1037/xge000048

Moeller, B., Pfister, R., Kunde, W., & Frings, C. (2016). A common mechanism behind distractor-response and response-effect binding? *Attention, Perception, & Psychophysics*, *78*, 1074–1086. https://doi.org/10.3758/s13414-016-1063-1

Monsell, S. (2003). Task switching. *Trends in Cognitive Sciences*, *7*, 134–140.

Monsell, S., Taylor, T. J., & Murphy, K. (2001). Naming the color of a word: Is it responses or task sets that compete? *Memory & Cognition*, *29*, 137–151.

Moors, A. (2016). Automaticity: Componential, causal, and mechanistic explanations. *Annual Review of Psychology*, *67*, 263–287.

Muhle-Karbe, P. S., & Krebs, R. M. (2012). On the influence of reward on action-effect binding. *Frontiers in Psychology*, *3*, 450. https://doi.org/10.3389/fpsyg.2012.00450

O'Regan, J. K., & Noë, A. (2001). A sensorimotor account of vision and visual consciousness. *Behavioral and Brain Sciences*, *24*(5), 939–973.

Pfister, R. (2019). Effect-based action control with body-related effects: Implications for empirical approaches to ideomotor action control. *Psychological Review*, *126*, 153–161. https://doi.org/10.1037/rev0000140

Pfister, R., Kiesel, A., & Hoffmann, J. (2011). Learning at any rate: Action-effect learning for stimulus-based actions. *Psychological Research*, *75*, 61–65. https://doi.org/10.1007/s00426-010-0288-1

Pfister, R., Kiesel, A., & Melcher, T. (2010). Adaptive control of ideomotor effect anticipations. *Acta Psychologica*, *135*, 316–322. https://doi.org/10.1016/j.actpsy.2010.08.006

Powers, W. T. (1973). Feedback: Beyond behaviorism. *Science*, *179*, 351–356. https://doi.org/10.1126/science.179.4071.351

Prinz, W. (1990). A common coding approach to perception and action. In O. Neumann & W. Prinz (Eds.), *Relationships between perception and action: current approaches* (pp. 167–201). Springer Berlin Heidelberg. https://doi.org/10.1007/9783642753480_7

Prinz, W. (1997). Perception and action planning. *European Journal of Cognitive Psychology*, *9*, 129–154. https://doi.org/10.1080/713752551

Prinz, W., Aschersleben, G., & Koch, I. (2009). Cognition and action. In E. Morsella, J. A. Bargh, & P. M. Gollwitzer (Eds.), *Oxford handbook of human action* (pp. 33–70). Oxford University Press.

Qin, K., Marien, H., Custers, R., & Aarts, H. (2021). Environmental control of human goal pursuit: Investigating cue-based forced responses in a Pavlovian-to-instrumental transfer paradigm. *Motivation Science*, *7*(3), 281–290. https://doi.org/10.1037/mot0000219

R Core Team. (2020). *R: A language and environment for statistical computing*. https://www.r-project.org/

Riechelmann, E., Weller, L., Huestegge, L., Böckler, A., & Pfister, R. (2019). Revisiting intersubjective action-effect binding: No evidence for social moderators. *Attention, Perception, & Psychophysics*, *81*, 1991–2002.

Rizzolatti, G., & Craighero, L. (2004). The mirror-neuron system. *Annual Review of Neuroscience*, *27*, 169–192. https://doi.org/10.1146/annurev.neuro.27.070203.144230

Rizzolatti, G., Fadiga, L., Gallese, V., & Fogassi, L. (1996). Premotor cortex and the recognition of motor actions. *Cognitive Brain Research*, *3*, 131–141.

Rodway, P. (2005). The modality shift effect and the effectiveness of warning signals in different modalities. *Acta Psychologica*, *120*, 199–226. https://doi.org/10.1016/j.actpsy.2005.05.002

Rousselet, G. A., Pernet, C. R., & Wilcox, R. R. (2017). Beyond differences in means: robust graphical methods to compare two groups in neuroscience. *European Journal of Neuroscience*, *46*(2), 1738–1748. https://doi.org/10.1111/ejn.13610

Sato, A., & Itakura, S. (2013). Intersubjective action-effect binding: Eye contact modulates acquisition of bidirectional association between our and others' actions. *Cognition*, *127*, 383–390. https://doi.org/10.1016/j.cognition.2013.02.010

Schmalz, X., Manresa, B., & Zhang, L. (2021). What is a Bayes factor? *Psychological*

Methods. https://doi.org/10.1037/met0000421

Schönbrodt, F. D., Wagenmakers, E.-J., Zehetleitner, M., & Perugini, M. (2017). Sequential hypothesis testing with Bayes factors: Efficiently testing mean differences. *Psychological Methods*, *22*, 322–339. https://doi.org/10.1037/met0000061

Schwarz, K. A., Burger, S., Dignath, D., Kunde, W., & Pfister, R. (2018). Action-effect binding and agency. *Consciousness and Cognition*, *65*, 304–309. https://doi.org/10.1016/J.CONCOG.2018.10.001

Seabrooke, T., Hogarth, L., Edmuns, C. E. R., & Mitchell, C. J. (2017). Evidence of a goal-directed process in human pavlovian-instrumental transfer. *Journal of Experimental Psychology: Animal Learning and Cognition*, *43*, 377–387. https://doi.org/10.1037/xan0000147

Seabrooke, T., Hogarth, L., & Mitchell, C. J. (2016). The propositional basis of cue-controlled reward seeking. *Quarterly Journal of Experimental Psychology*, *69*, 2452–2470. https://doi.org/10.1080/17470218.2015.1115885

Shin, Y. K., Proctor, R. W., & Capaldi, E. J. (2010). A review of contemporary ideomotor theory. *Psychological Bulletin*, *136*, 943–974. https://doi.org/10.1037/a0020541

Singmann, H., Bolker, B., Westfall, J., & Aust, F. (2018). *afex: Analysis of factorial experiments*. https://cran.r-project.org/package=afex

Stock, A., & Stock, C. (2004). A short history of ideo-motor action. *Psychological Research*, *68*, 176–188. https://doi.org/10.1007/s00426-003-0154-5

Sun, D., Custers, R., & Aarts, H. (2022). Automaticity in Ideomotor Action? Moderators in the Two-Stage Paradigm. *Manuscript Under Revision*

Sun, D., Custers, R., Marien, H., & Aarts, H. (2020). Ideomotor action: evidence for automaticity in learning, but not execution. *Frontiers in Psychology*, *11*, 185. https://doi.org/10.3389/fpsyg.2020.00185

Sun, D., Custers, R., Marien, H., Liefooghe, B., & Aarts, H. (2022). Examining mechanistic explanations for ideomotor effects. *Journal of Experimental Psychology: Human Perception and Performance*.

Theeuwes, M., De Houwer, Jan, Eder, A., & Liefooghe, B. (2015). Congruency effects on the basis of instructed response-effect contingencies. *Acta Psychologica*, *158*, 43–50. https://doi.org/10.1016/j.actpsy.2015.04.002

Tibboel, H., & Liefooghe, B. (2020). Attention for future reward. *Psychological Research*, *84*, 706–712. https://doi.org/10.1007/s00426-018-1094-4

Trick, L., Hogarth, L., & Duka, T. (2011). Prediction and uncertainty in human pavlovian to instrumental transfer. *Journal of Experimental Psychology: Learning Memory and Cognition*, *37*, 757–765. https://doi.org/10.1037/a0022310

Turatto, M., Benso, F., Galfano, G., & Umiltà, C. (2002). Nonspatial attentional shifts between audition and vision. *Journal of Experimental Psychology: Human Perception and Performance*, *28*, 628–639. https://doi.org/10.1037/0096-1523.28.3.628

Umiltà, M. A., Kohler, E., Gallese, V., Fogassi, L., Fadiga, L., Keysers, C., & Rizzolatti, G. (2001). I know what you are doing: A neurophysiological study. *Neuron*, *31*, 155–165. https://doi.org/10.1016/S0896-6273(01)00337-3

Umiltà, M., Intskirveli, I., Grammont, F., Rochat, M., Caruana, F., Jezzini, A., Gallese, V., & Rizzolatti, G. (2008). When pliers become fingers in the monkey motor system. *Proceedings of the National Academy of Sciences*, *105*, 2209–2213.

Vallacher, R. R., & Wegner, D. M. (1987). What do people think they're doing? Action identification and human behavior. *Psychological Review*, *94*, 3–15. https://doi.org/10.1037/0033-295X.94.1.3

van Doorn, J., van den Bergh, D., Böhm, U., Dablander, F., Derks, K., Draws, T., Etz, A., Evans, N. J., Gronau, Q. F., Haaf, J. M., Hinne, M., Kucharský, Š., Ly, A., Marsman, M., Matzke, D., Gupta, A. R. K. N., Sarafoglou, A., Stefan, A., Voelkel, J. G., & Wagenmakers, E.-J. (2021). The JASP guidelines for conducting and reporting a Bayesian analysis. *Psychonomic Bulletin & Review*, *28*, 813–826. https://doi.org/10.3758/s13423020017985

Verschoor, S., Weidema, M., Biro, S., & Hommel, B. (2010). Where do action goals come from? Evidence for spontaneous action–effect binding in infants. *Frontiers in Psychology*, *1*. https://doi.org/10.3389/fpsyg.2010.00201

Vogel, D., Rudolf, M., & Scherbaum, S. (2020). The impact of the verbal instruction and task characteristics on effect-based action control. *Cognitive Processing*, 1–15. https://doi.org/10.1007/s10339-020-00960-0

Waldmann, M. R., Holyoak, K. J., Fratianne, A., & Fra-Tianne, A. (1995). Causal models and the acquisition of category structure. In *Journal of Experimental Psychology: General*.

Watson, P., Wiers, R. W., Hommel, B., & Wit. (2018). Motivational sensitivity of outcome-response priming: Experimental research and theoretical models. *Psychonomic Bulletin & Review*, 1–14. https://doi.org/10.3758/s13423-018-1449-2

Watson, P., & Wit. (2018). Current limits of experimental research into habits and future directions. *Current Opinion in Behavioral Sciences*, *20*, 33–39. https://doi.org/10.1016/j.cobeha.2017.09.012

Wiese, W., & Metzinger, T. (2017). *Vanilla PP for philosophers: A primer on predictive processing*.

Wilcox, R. R. (2012). *Introduction to robust estimation and hypothesis testing*. Academic press.

Wilson, M., & Knoblich, G. (2005). The case for motor involvement in perceiving conspecifics. *Psychological Bulletin*, *131*, 460–473. https://doi.org/10.1037/0033-2909.131.3.460

Wolfensteller, U., & Ruge, H. (2011). On the timescale of stimulus-based action-effect learning. *Quarterly Journal of Experimental Psychology*, *64*, 1273–1289. https://doi.org/10.1080/17470218.2010.546417

Wolpert, D. M., Ghahramani, Z., & Jordan, M. I. (1995). An internal model for sensorimotor integration. *Science*, *269*, 1880–1882.

Wood, W., & Rünger, D. (2016). Psychology of habit. *Annual Review of Psychology*, *67*, 289–314. https://doi.org/10.1146/annurev-psych-122414-033417

Zedelius, C. M., Veling, H., Custers, R., Bijleveld, E., Chiew, K. S., & Aarts, H. (2014). A new perspective on human reward research: How consciously and unconsciously perceived reward information influences performance. *Cognitive, Affective and Behavioral Neuroscience*, *14*, 493–508. https://doi.org/10.3758/s13415-013-0241-z

Ziessler, M., & Nattkemper, D. (2002). Effect anticipation in action planning. In W. Prinz & B. Hommel (Eds.), *Common mechanisms in perception and action* (Vol. 19, pp. 645–672). Oxford University Press.

Zwaan, R. A., & Taylor, L. J. (2006). Seeing, acting, understanding: Motor resonance in language comprehension. *Journal of Experimental Psychology: General*, *135*, 1–11. https://doi.org/10.1037/0096-3445.135.1.1

Zwosta, K., Ruge, H., & Wolfensteller, U. (2013). No anticipation without intention: Response-effect compatibility in effect-based and stimulus-based actions. *Acta Psychologica*, *144*, 628–634. https://doi.org/10.1016/j.actpsy.2013.09.014

Acknowledgements

Acknowledgements

The story dated back to that autumn in 2017. It was my first time studying abroad with the financial support of the Chinese Scholarship Council (i.e., CSC). My initial goal was to dig further into the field of (visual) perception, then I met the “Ghost in the machine” – the ideomotor principle in terms of the interaction between mind and body. I was impressed by its simplicity and elegance and started to play with it for my whole Ph.D. project. The last five years were something joy, something blues, something cheerful, also something anxious; but with all these mixtures of feelings, I had a great company of many people helping me cope with everything.

First and foremost, I would like to thank my supervisors, Henk, and Ruud, for all your guidance, patience, tips, encouragement, also critiques, and comments throughout my whole Ph.D. track. Your insightful feedbacks and helps pushed me to sharpen my thinking and brought my work to a higher level. **Henk**, thank you for taking care of my project when my initial face perception project had to be terminated for some unpredictable reasons. You gave me support to start a new proposal and introduce Ruud to help me build it step by step. Thank you for every sharp comment on every presentation, and papers. I hope we can have further cooperation in the future. **Ruud**, thank you for boarding on my project, even though I barely knew this new area. I appreciated your effort to help me tackle every uncertainty and difficulty. You taught me to how to think critically, how to write structurally, and how to show off one’s work clearly. Thank you for the special goodbye gift – the customized T-shirt with Ouija boards on it. I was happy to have a physical fare ware with you. When the world comes to normal, welcome to visit China! I also hope we can meet somewhere again and have further cooperation.

The second credits I would like to express my gratitude’s are my brilliant cooperators and the people that I go to for advice and help. **Hans**, thank you for taking care of my projects, especially the first one, I still remembered that you took the time to double-check my code and report, teaching me the importance of carefulness. And you also give good feedbacks on other projects. **Baptist**, thank you for all the inspiring comments and suggestions. As an expert on instruction effect, you helped me a lot to think from a different perspective. **Tina,** thank you for offering me a chance to cooperate. You promoted the binding-experiment idea and your generosity made me have a large, powered replication study. It belonged to the first-author publication of my project! Thank you, all the IT groups, especially **Jim and Song**. You are the superheroes of (almost) every researcher (at least in my opinion). Every time I came across problems with devices, experiments, or labs. I knew where the right way was to ask for help.

Thank you also to all the **GOALLAB** members for all the inspiring comments and inputs and help me to learn about topics from a different perspective. I would especially thank you to my officemates from G1.11. Thank you, **Sammy, Myrthel, and Chris**, for all the helpful discussions about stats and programming over (coffee/) break. Thank you, **Ilona, and Nil**, for all the conversations about work and life, thank you for all the encouragement (also some gossip and complaints). We even built our own office garden with those lovely and cute plants. Though they did not overcome the challenges of the long-term pandemic, C'est la vie.

My dear **Ph.D. fellows**, thank you for making my Ph.D. life colorful. I did enjoy every Thursday meeting, both the in-site and virtual ones, the informal chatting and formal presentations, and also the highlights and lowlights per week. I embraced the happiness and support, and I learned the professional comments and feedbacks. I am also missing the board games event and drinks.

I would also like to thank all my colleagues and staffs at the **SHOP** department. As one of the SHOPer, I appreciate the atmosphere of diversity and welcome to international students. Thank you for these kind words and wishes during the celebration of the Chinese lunar new year!

I would say thank you to my dear and lovely friends in our special WeChat group: the UU-fortune's-favorites (UU-小福星们)。**玉茹(Yuru)**，我亲爱的直系师姐，谢谢你在专业理论上的帮助和直白的解释，谢谢你在我求职迷茫期的真诚分享。七小福现在已经发展成大家庭啦！**仲芳(Zhongfang)**，谢谢你在元分析和求职上的答疑解惑！如果我还在学界的话，希望以后有机会能够实现跨学科的合作！也因为你，我结识了新的Zeist风趣的朋友们（**韦萱和陈婧**），特别感谢你们能够耐心解答我论文准备和提交中遇到的实际问题！**文蕊**

(Wenrui)，我亲爱的优秀的师妹，认识你真开心呀！虽然你总认为自己是假性外向，但不可否认，每次聚会你机智而又不失风趣的言谈真是令我折服！谢谢你近 4 年的陪伴，怀念那些一起畅想未来，分享美食，吐槽八卦的样子。期待我们下次见面！谢谢三位大神：**书阳(Shuyang), 恺洋(Kevin), 培凯(Peikai)**，我们在同一年的来到了乌特，谢谢你们在学术上的点拨和鼓励，谢谢你们分享的统计知识和求职经验！最后，UU-小福星家族们所有的同仁们，愿我们都能以梦为马，不负韶华！

Last but the most, I would appreciate the support and understanding of my **family**: Mom and Dad. 老季，老孙，谢谢你们能够理解我远赴海外近 5 年的求学之路！很多时候，我知道你们喜欢报喜不报忧，哪怕生病了，也只是默默忍受。辛苦了，我的父亲、母亲！ 以后的日子，希望我们都能保持康健，开启更美好的篇章！

Thank you, everyone and I hope we can meet in the future! 千言万语无法表达我的感激之情，希望我们有缘能相遇在世界的某个角落。

My room, Jiangsu, China
April 5, 2022

Curriculum Vitae

Curriculum Vitae

Dan Sun was born on the 28th of January 1991 in Guangling, a small village in Jiangsu province, China. She built her research route related to a circle of how humans deal with the social world, from facial perception to embodied cognition, from visual mechanisms to action control. Her passion for cognitive psychology started from investigating the mechanism of visual perception, where she mainly focused on the two structural-related stimuli: face and (Chinese) characters and obtained a bachelor’s degree in applied psychology at Nantong University (2013) and a master’s degree in cognitive neuroscience at Hangzhou Normal University (2017). During the research master, she also worked as a research assistant for the face perception project under the supervision of prof. dr. Caroline Blais and dr. Ye Zhang. Along the way, she also applied for the Graduate Creativity Project of HZNU to investigate the facial perception mechanism of Autism, which she was very lucky to receive. In 2017, she started her Ph.D. project in the Goallab at Utrecht University under the supervision of prof. dr. Henk Aarts and dr. Ruud Custers, where she worked on projects related to goal and habit formation, causal reasoning, and personal autonomy. Please visit her academic website via here (https://www.researchgate.net/profile/Dan-Sun-4) for more details.

KLI
Distertation
Series

KLI dissertation series

The “Kurt Lewin Institute Dissertation Series” started in 1997. The following dissertations have been published during the last two years. The complete list can be found on our website: https://kurtlewininstituut.nl

2020-01: Florian Wanders: Rebels, Renegades, and Robin Hoods: The Social-Hierarchical Dynamics Surrounding Norm Violators

2020-02: Marko Milovanović: Intrinsically Motivating Social Influence

2020-03: Simon Columbus: Subjective Interdependence and Prosocial Behaviour

2020-04: Annemijn Peters: When well begun is half done: How the adoption of sustainable energy technologies can lead to sustainable use of the technologies and other pro-environmental behaviours

2020-05: Josefine Geiger: Context matters: Three ways of how the context influences recycling behavior

2020-06: Lianne Aarntzen: Work-family guilt: A straightjacket keeping parents in traditional gender roles

2020-07: Mandy Tjew-A-Sin: Contact Comfort: Psychological Effects of Actual and Simulated Affectionate Touch

2020-08: Melissa Vink: Who brings home the bacon? How gender stereotypes straitjacket men and women into traditional relationships

2020-09: Jesús Manuel Mascareño Apodaca: Orchestrating innovation: How leaders affect creativity and innovation

2020-10: Tatiana Chopova: Doing good in business: Examining the importance of morality in business contexts

2020-11: Margarita Leib: (Dis)honesty in Individual and Collaborative Settings: A behavioral ethics approach

2020-12: Samantha Antusch: On how we experience ourselves as intentional agents. An examination of the role of intentional action in the sense of agency

2021-01: Mengchen Dong: Understanding Moral Hypocrisy: Behavioral Antecedents and Social Consequences

2021-02: Daniel Sloot: Bringing Community and Environment Together: The role of community environmental initiatives in sustainability transitions

2021-03: Burkhard Wörtler: Enhancing Blended Working Arrangements and Individual Work Performance

2021-04: Frank Doolaard: Social exclusion - put into context

2021-05: Laurens van Gestel: The psychology of nudging - An investigation of effectiveness and acceptability

2021-06: Ruddy Faure: Implicit partner evaluations: How they form and affect close relationships

2021-07: Rosabelle Illes: Between a rock and a hard place: Challenges, strategies and resolution of value conflict mediation

2021-08: Nadja Zeiske: The Intrinsic Route to Pro-Environmental Behaviour

2021-09: Katherina Tatiana Alvarez Durnov: The Psychological Impact of Receiving Aid

2021-10: Peikai Li: Looking on the Bright and Dark Sides of Working Life: Appraisals of Work Characteristics and Employee Outcomes

2022-01: Iris van Sintemaartensdijk: Burglary in virtual reality

2022-02: Lu Liu: Public participation in decision making on sustainable energy transitions

2022-03: Rabia Kodapanakkal: The role of tradeoffs and moralization in the adoption of big data technologies

2022-04: Elissa El Khawli: Why, When, and How Workers Regulate: A lifespan perspective on work design and emotion regulation at work

2022-05: Chantal van Andel: Clinical Grade Differences Between Ethnic Minority and Majority Students: Institutional-, assessor and student-related factors

2022-06: Inga Rösler: Hear me out: How to create an open mind towards moral criticism

2022-07: Tessa Coffeng: Bias in supervision: A social psychological perspective on regulatory decision-making

2022-08: Babet Kanis: Hope and Health in the Face of Adversity

2022-09: Martijn Blikmans: Do we live in the age of emotion politics? The effects of anger, disgust, hope, and nostalgia communication on political support and polarization

2022-10: Anne van Valkengoed: Reality, causes, consequences: The role of climate change perceptions in climate adaptation

2022-11: Dan Sun: How People Learn to Act on Goals: A New Examination of the Mechanistic Ideomotor Action Account