DEVELOPMENT OF THE ONLINE IMITATION-INHIBITION TASK TO INVESTIGATE MODULATIONS OF AUTOMATIC IMITATION

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Table of Contents

Preface

Chapter 2 is based on the following manuscript:

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All authors developed the idea to all experiments. The second author and I programmed the basic version of the first experiment, which I adapted for the second, third, and fourth experiment. The third and last author contributed with valuable suggestions to both steps. I conducted all experiments, finalized data analysis for all experiments, and wrote the manuscript. The last author assisted in the analysis of the data and contributed valuable suggestions to the interpretation of the data. The second, third and last author contributed valuable suggestions to the overall interpretation of all data and the manuscript multiple times before submission and during the review process.

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The last author and I developed the idea to all experiments. The second author and I programmed the second and third experiment. I programmed the first and fourth experiment. The last author contributed with valuable suggestions to this step. I conducted all experiments, finalized data analysis for all experiments, and wrote the manuscript. The second author assisted in the analysis of the data for the second and third experiment and contributed valuable suggestions to the interpretation of the data of both experiments. The last author contributed valuable suggestions to the interpretation of all data, the overall interpretation of all data and the manuscript multiple times before submission and during the review process.

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All authors developed the idea to both experiments. I programmed both experiments. The second and the last author contributed with valuable suggestions to this step. I conducted both experiments, finalized data analysis for both experiments, and wrote the manuscript. The second author assisted in the analysis of the data and contributed valuable suggestions to the interpretation of the data of both experiments. The last author contributed valuable suggestions to the interpretation of the data of both experiments. The second and the last author contributed valuable suggestions to the manuscript multiple times before submission and during the review process.

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All authors developed the idea to all experiments. I programmed all experiments. The first and the last author contributed with valuable suggestions to this step. I conducted all experiments and finalized data analysis for all experiments. The first author wrote the manuscript. The third author contributed valuable suggestions to the interpretation of the data and the first version of the manuscript. I, the third and the last author reviewed the manuscript and contributed valuable suggestions to it before submission and during the review process.

Please note that some changes in headings, citation style, and formatting were undertaken to fit the layout of this dissertation. Supplemental materials, if any, were added to the text. No changes were made to the content of the articles and manuscripts.

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List of Figures

List of Tables

Abstract

People automatically imitate a wide range of different behaviors. One of the most commonly used measurement methods to assess imitative behavior is the imitation-inhibition task (Brass et al., 2000). However, in its original form, the task is only suited to be carried out in a laboratory setting – a time-consuming and costly procedure. In this dissertation, I will present a method for investigating automatic imitation in an online environment (i.e., introducing an online imitation-inhibition task). This online version allows for a more economically investigation of automatic imitation. Using this task, I investigated different modulations to address theoretically relevant questions and contribute to the research field of automatic imitation. Specifically, there have been problems replicating some findings with the imitation-inhibition task recently, particularly regarding findings that involved social modulations. This is likely due to the fact that social effects with the imitation-inhibition task are very small. However, the online-imitation task provides an opportunity to examine the impact of social and other modulations on automatic imitation with high statistical power. It can thus be used to detect very small effect sizes. In six chapters, I will first review the literature on automatic imitation. I will then test the impact of different social and non-social modulations (i.e., belief in free will, finger movement size, finger movement speed, animacy beliefs, and group membership) on automatic imitation using the online imitation-inhibition task. Finally, I will discuss possible reasons for the failed and successful replications and provide further insight into possible explanations and implications.

Deutsche Zusammenfassung

Menschen imitieren automatisch viele unterschiedliche Verhaltensweisen. Eine der am häufigsten verwendeten Messmethoden zur Beurteilung von Imitation ist der Imitation-Inhibition Task (Brass et al., 2000). In seiner ursprünglichen Form ist der Task jedoch nur für die Labordurchführung geeignet – welche zeit- und kostenaufwändig ist. In dieser Dissertation werde ich eine Methode zur Untersuchung der automatischen Imitation in einer Online-Umgebung vorstellen (d.h. einen online Imitation-Inhibition Task). Diese Online-Version ermöglicht eine ökonomischere Untersuchung von automatischer Imitation. Mithilfe des Tasks untersuchte ich verschiedene Modulationen, um theoretisch relevante Fragen zu klären und einen Beitrag zum Forschungsfeld der automatischen Imitation zu leisten. In letzter Zeit gab es Probleme bei der Replikation einiger Befunde mit dem Imitation-Inhibition Task, insbesondere bei Befunden, die soziale Modulationen beinhalteten. Dies ist wahrscheinlich auf die Tatsache zurückzuführen, dass soziale Effekte mit dem Imitation-Inhibition Task sehr klein sind. Der online Imitation-Inhibition Task bietet die Möglichkeit, die Auswirkungen sozialer und anderer Modulationen auf automatische Imitation mit hoher statistischer Aussagekraft zu untersuchen. Er kann verwendet werden, um sehr kleine Effekte zu entdecken. In sechs Kapiteln werde ich zunächst einen Überblick über die Literatur zur automatischen Imitation geben. Anschließend werde ich die Auswirkungen verschiedener sozialer und nicht-sozialer Modulationen (d.h. Glaube an den freien Willen, Größe der Fingerbewegung, Geschwindigkeit der Fingerbewegung, Animacy Beliefs und Gruppenzugehörigkeit) auf automatische Imitation mit dem Imitation-Inhibition Task testen. Abschließend werde ich mögliche Gründe für die fehlgeschlagenen und erfolgreichen Replikationen diskutieren und weitere Einblicke in mögliche Implikationen geben.

Chapter 1

General Introduction

It happens to everyone at some point: you're sitting in the waiting room with nothing to do, watching other people around you. Suddenly, someone starts yawning, and you have a strong urge to do the same.

Human beings have an innate tendency to imitate a wide range of behaviors, including facial expressions (Dimberg, 1982; Hess & Blairy, 2001), speech patterns (Bock, 1986; Giles & Powesland, 1975), emotions (Hess & Blairy, 2001; Hess & Fischer, 2013), gestures (Bernieri, 1988; Cracco, Genschow, et al., 2018), postures (LaFrance, 1982), basic movements (Brass et al., 2000; Genschow et al., 2012; Genschow & Florack, 2014; Genschow & Schindler, 2016), and even complex action patterns (Hansen et al., 2016). Early research suggests that this imitative tendency serves an important purpose by enhancing learning in early childhood (Carpenter et al., 1998; Meltzoff, 1988, 1995; Meltzoff & Keith Moore, 1994) and adulthood (Bandura, 1962, 1963). However, it’s even more important for fostering stronger human connections and a sense of affiliation between humans (for a review, see Duffy & Chartrand, 2015a).

Typically, when measuring imitation, researchers must choose between assessing imitation via face-to-face mimicry experiments during social interactions (i.e., motor mimicry; e.g., Chartrand & Bargh, 1999) or to use a more basic and cognitive measurement method, the imitation-inhibition task (Brass et al., 2000, 2001) to measure automatic imitation.

The direct assessment of motor imitation in social interactions was the first approach to measuring imitation (Hull, 1933; Lipps, 1907; Scheflen, 1964). However, most research on motor mimicry did not begin until the late 1990s (e.g., Chartrand & Bargh, 1999; Lakin & Chartrand, 2003). More recent research has shown that the measurement of motor mimicry produces unreliable results (Genschow, van Den Bossche, et al., 2017). In addition, measuring motor mimicry in the laboratory is an effortful, time-consuming and costly procedure, making high-powered studies less affordable.

In contrast, the imitation-inhibition task, the approach to measure covert imitative response tendencies (Heyes, 2011), produces more reliable (Genschow, van Den Bossche, et al., 2017) and strong effects (Cracco, Bardi, et al., 2018). Since its first use at the beginning of the 21st century (Brass et al., 2000; Stürmer et al., 2000), this task finds application across various fields, including, social (e.g., Cracco, Genschow, et al., 2018; De Souter et al., 2021; Genschow et al., 2022) or cognitive psychology (e.g., Brass et al., 2000; Cracco et al., 2015), among others. Nevertheless, one limitation of the current version of the imitation-inhibition task, similar to typical motor mimicry measurement methods, is that that the implementation of these kind of tasks is limited to effortful laboratory settings. In light of the replication crisis in psychological research (e.g., Open Science Collaboration, 2015), there's a growing demand for large samples (e.g., Asendorpf et al., 2013), which is challenging when assessing imitation behavior in the laboratory. Furthermore, laboratory experiments predominantly involve student participants, a practice that has faced criticism on multiple occasions (e.g., Henry, 2008; Stevens, 2011). These circumstances raised the question of whether it is possible to design an online adaptation of the imitation-inhibition task producing results that are similarly reliable and strong as its laboratory version. Designing such a task would open doors for conducting research with larger and more diverse samples in an easy and economical manner. This is the methodological problem that I would like to address with this dissertation.

While there is an established consensus within the literature that the imitation-inhibition task effectively measures imitative behavior (Cracco & Brass, 2019), the current discourse is marked by an ongoing and lively debate regarding the extent to which different factors, particularly social factors, can influence or manipulate automatic imitation (Bouquet et al., 2011; Chiavarino et al., 2013; e.g., Cracco, Bardi, et al., 2018; De Souter et al., 2021; Genschow, Schuler, et al., 2019; Gleibs et al., 2016; Heyes, 2005; Klapper et al., 2014; Poljac et al., 2009). This uncertainty is caused by numerous failed replications that complicate demonstrating robust effects (e.g., Butler et al., 2015; De Souter et al., 2021; Galang & Obhi, 2020; Genschow, Pauels, et al., 2023; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019). In this dissertation, I will present the current state of knowledge concerning modulations of automatic imitation and try to address the question whether automatic imitation measured with the imitation-inhibition task can be modulated using its online version. Thus, it is not only a methodological question as to whether automatic imitation can indeed be modulated but also a question of social relevance, if the focus is on modulation with social variables, assuming that automatic imitation is a method of measuring covert imitative response tendencies (Brass et al., 2000; Cracco & Brass, 2019).

Within this dissertation I first develop and validate a method to measure automatic imitation in an online environment, which I then use to investigate different social and non-social modulations with automatic imitation in order to gain new insights. For this purpose, I will first give a definition and an overview of imitation in Chapter 1.1. In Chapter 1.2, I will explain motor mimicry and automatic imitation as the two different measurement methods of imitation. After that, I will focus exclusively on automatic imitation with the imitation-inhibition task because covering motor mimicry and automatic imitation would go beyond the scope of this work. Therefore, in Chapter 1.3, I will address automatic imitation measured with the imitation-inhibition task in detail, including an explanation of the different automatic imitation effects. In Chapter 1.4, I will focus on the theories regarding the mechanisms behind automatic imitation, including the perception-behavior link, ideomotor theory and associative sequence learning theory, as well as mirror neuron networks and theories on modulation of automatic imitation. Subsequently, Chapter 1.5 will consider modulations with the imitation-inhibition task. In this chapter I will provide an overview of the literature on modulations of automatic imitation with the imitation-inhibition task in the laboratory. To conclude this chapter, I will point out replication problems of some of these modulations and why an online version of the imitation-inhibition task is needed to enable high-powered replications.

Chapter 2 comprises the manuscript in which I validated the online version of the imitation-inhibition task and showed that the task (a) works online, (b) yields comparable results to its laboratory version, (c) can similarly achieve automatic imitation when controlling for a typical moderator (i.e., spatial compatibility), and (d) shows that a typical social modulation with the online task (i.e., the so-called animacy effect: The finding that a human hand is imitated more strongly than a robotic hand; e.g., Klapper et al., 2014), is also evident with the online version of the imitation-inhibition task. Chapter 3 is based on a manuscript that includes a social manipulation of the online imitation-inhibition task and shows that movement size (but not movement speed) affects automatic imitation. However, the animacy effect does not disappear when controlling for a movement size confound in typical animacy stimuli. Chapter 4 shows a top-down correlative approach using the online imitation-inhibition task demonstrating that the relationship between automatic imitation and interindividual differences is not as universal as previously thought, using the free will inventory (Nadelhoffer et al., 2014). Lastly, Chapter 5 is based on an article showing that group membership does not alter automatic imitation, despite using different stimulus manipulations.

In Chapter 6, I will summarize the different studies, derive implications for the online and the imitation-inhibition task in general, take a look at *p*-curve analyses of social modulations with the imitation-inhibition task and reflect on limitations and further implications regarding automatic imitation and the generalizability of the findings.

1.1 Imitation: An Overview

Imitation is defined as the copying of a model’s body action, behavior or movement by an observer. Thus, imitation comprises two components: (a) the observation of a body movement, and (b) the deliberate execution of the topographically similar body movement by the observer (Heyes, 2001, 2011; Iacoboni, 2009). The observer's corresponding reaction is causally related to the observation of the body movement of the model and is not executed at the exact same time, but with a (short) delay (Heyes, 2001). Imitation does not have to take place consciously and should be contiguous to the situation (i.e., a predictive relationship between the participant’s movements and those of the confederate, Catmur & Heyes, 2013). While social imitation is primarily unique to humans (Brass & Heyes, 2005), other animals such as monkeys (Custance et al., 1999), chimpanzees (Carpenter et al., 1995), dogs (Galef, 1988), or birds (Akins & Zentall, 1996) are also able to imitate fellow animals as well as humans, albeit to a limited extent (Heyes, 2001).

1.1.1 Differentiation of Imitation from Other Phenomena

Emulation. Imitation is often confused with emulation (Fridland & Moore, 2015). In emulation, behavior observation promotes the execution of behavior that is not similar in the structure of the body's movement but similar in its effects on an object in the environment. Or in other words, emulation is the copying of body movements effects on environmental objects (Gerrans, 2013; Thompson & Russell, 2004; Tomasello, 1996). If someone lifts a bucket by the handle another person imitating would also lift the bucket in the same way, replicating the body movement. Instead, if it is emulation, the other person might lift the bucket in a different way as long as the goal (lifting the bucket) is achieved. Both emulation and imitation are based on the fact that an observation of an action primes the same or a similar action. However, in emulation it is the features of the object, not the features of the body movements, that prime the corresponding motor representation (Fridland & Moore, 2015; Gerrans, 2013; Heyes, 2009). Thus the motor representations activated by action observation encode similar object transformations and not movement transformations (Heyes, 2011; Tucker & Ellis, 1998). In paradigms and study designs on imitation in which body movements are interchanged with object properties, imitation could actually be confounded with emulation (e.g., Bird, Brindley, et al., 2007). However, usually imitation tasks use movements or body positions as the behavior to be copied, thus no object interaction is involved (e.g., Brass et al., 2000; Chartrand & Bargh, 1999; Cracco, Bardi, et al., 2018; Duffy & Chartrand, 2015a).

Synchrony. Similarly, imitation is sometimes confused with synchrony (O’Sullivan et al., 2018). As in imitation, synchrony arises when people adapt their behavior to that of other people. However, with synchrony the behavior is often performed several times in a row in the same rhythm and is identical in timing (Bernieri, 1988; Cracco et al., 2022; Marques-Quinteiro et al., 2019; Miles et al., 2010; Wiltermuth & Heath, 2009). For example, people sometimes synchronize when they walk side by side and start to raise and lower their legs at the same time in the same rhythm. Thus, synchrony also involves perceiving and copying a behavior, albeit in a different form than imitation. This is because, unlike imitation, the joint time aspect is important; it is only synchrony when people move in time with one another (Hove & Risen, 2009; Miles et al., 2010). Research on synchrony demonstrated similar important social functions as imitation: Synchrony enhances bonding (Lakens & Stel, 2011; Marques-Quinteiro et al., 2019), empathy (Lumsden et al., 2014), and creates affiliation (Hove & Risen, 2009). However, synchrony paradigms differ in the displayed behavioral schema, as synchrony paradigms often involve maintaining time-parallel synchrony of the same behavior for a longer time-span (e.g., Lakens & Stel, 2011).

1.1.2 Imitation and its Functions

One of the most important functional components of imitation is learning (Bandura, 1962, 1963). Children demonstrably use imitation learning in their first year of life to imitate the behavior of their parents or peers (Carpenter et al., 1998; Meltzoff, 2002; Meltzoff & Keith Moore, 1994; Over & Carpenter, 2013) and can also imitate actions guided by ascription of goals or intentions to the model at an early age. For example, children tend to mimic actions that may seem devoid of meaning when they perceive those actions as being executed intentionally, as opposed to believing they were performed unintentionally or by accident (Buttelmann et al., 2013; Meltzoff, 1988). Lastly, it has been proposed that imitation contributes to the development of theory of mind, because it leads to inferring first-person and third-person experience out of the imitative relationship between action observation, action execution, the model and the own person (Barresi & Moore, 1993; Meltzoff, 2002; Tomasello et al., 1993). But even later in life, imitation is one of the most important learning components and is widely believed to be one of the learning mechanisms humans use to learn and alter behavior during adulthood (Bandura, 1962).

The second important function of imitation is binding people personally together in social interactions (Duffy & Chartrand, 2015a). Not only do people generally imitate people they like more rather than people they dislike (e.g., Likowski et al., 2008; Stel et al., 2010), imitation also enhances liking (e.g., Chartrand & Bargh, 1999; Kulesza et al., 2023), empathy (Butler 2015), and sympathy between strangers (e.g., Sparenberg, Topolinski, et al., 2012). Additionally, it fosters pro-social behavior as imitated persons help more, donate more (van Baaren, Holland, et al., 2004), or tip more (van Baaren, Holland, et al., 2003). This change in pro-social behavior can even be demonstrated in children (Carpenter et al., 2013). Differences in imitation might also detect neurodevelopmental disorders like autism (Spengler, Bird, et al., 2010; Williams, 2008) or also narcissistic traits (Hogeveen & Obhi, 2013; Obhi et al., 2013). In addition, imitation improves the sense of belonging (Lakin et al., 2003), as individuals with an affiliation goal increase their non-conscious tendency to mimic. Furthermore, being imitated promotes trust (Kulesza et al., 2023), and increases the chances of striking a deal in negotiation settings (Maddux et al., 2008). Moreover, being imitated enhances empathetic responses (De Coster et al., 2013). Even if people are told that they will be imitated, there is no backfiring effect (Kulesza et al., 2016).

1.1.3 Is Imitation Goal-Directed?

Debates persist regarding whether imitation generally is exclusively goal-oriented (Genschow, Hansen, et al., 2019). Initially, researchers believed imitation was entirely goal-oriented, supported by studies with children attributing goals to actions and copying accordingly or performing the correct goal-directed behavior when the model is hindered (Bekkering et al., 2000; Carpenter et al., 1998). Neurological studies also found that brain areas that are active during performing goal-directed behavior are also active during observing goal-directed behavior (di Pellegrino et al., 1992; Gallese et al., 1996). Thus, researchers suggested that observed actions are decomposed into a hierarchy of features, with goals outweighing the manner of achieving the action (Wohlschläger et al., 2003). However, Bird and colleagues found imitation behavior to be more flexible, influenced by task-independent factors and mediated by task-general processes (Bird, Brindley, et al., 2007). In line with this research, other research found that for the execution of an action, the observation of the action movement beforehand is more important than perceiving a goal (Cracco, Bardi, et al., 2018; Genschow, Hansen, et al., 2019; Heyes, 2001), as the mere observation of an action (which does not have to be goal-oriented) activates the same motor processes in the brain (Gazzola & Keysers, 2009; Keysers & Gazzola, 2010). Cross-contextual imitation studies demonstrated that people imitate movements without a common goal (Genschow et al., 2012; Genschow & Schindler, 2016). In summary, both goal-directed imitation and movement-directed imitation may both be used interchangeably depending on context, measurement method (i.e., motor mimicry or automatic imitation) and model characteristics (Cracco, Bardi, et al., 2018; Leighton & Heyes, 2010). For example, both goal-directed and movement-directed imitation are impacted by psychological distance

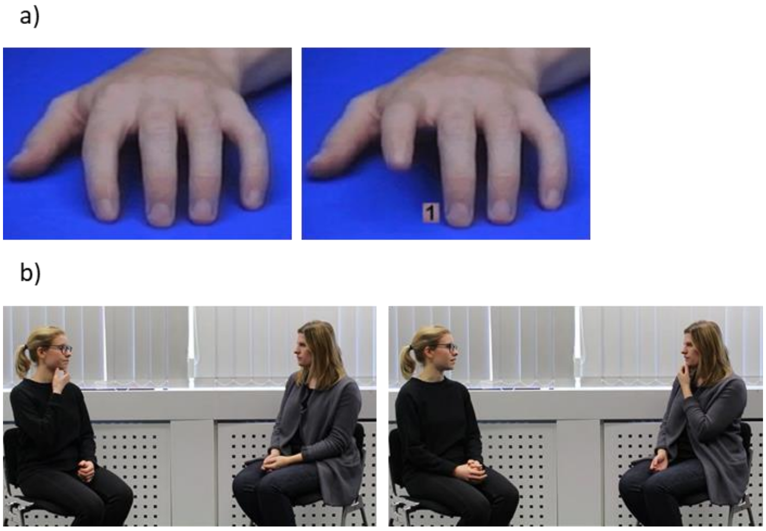
(Genschow, Hansen, et al., 2019). However, it is important to note that the observation of an action is necessary in both cases in order to imitate the movement, as well as to infer a goal in this action (Massen & Prinz, 2009).

1.2 Measurement Methods of Imitation

When measuring imitation, researchers either measure motor mimicry or automatic imitation (see Figure 1.1). In this section, I will first introduce motor mimicry and secondly automatic imitation: While motor mimicry is often referred to as the natural social form of imitation, automatic imitation is referred to as the controlled cognitive form of imitation (Heyes, 2011). Thereafter, I will differentiate between the two measurement methods. This chapter introduces automatic imitation in general terms and provides a rough overview in order to compare it with motor mimicry and draw a distinction between the two. Subsequent to this chapter, this dissertation will only further address automatic imitation with the imitation-inhibition task. Accordingly, Chapter 1.3 explains automatic imitation and the imitation-inhibition task in detail.

Figure 1.1

Examples for an Automatic Imitation Paradigm and Motor Mimicry Paradigm



Note. Both examples a) and b) depict imitation. a) illustrates stimuli typically used in imitation-inhibition tasks to measure automatic imitation (e.g., Brass et al., 2000). The left panel shows a hand of an action model lying on the table. The right panel shows the fixation stimulus (i.e., the hand in resting position), the left panel shows the hand moving a finger and the number cue; both pictures are shown one after the other in the task. The number indicates which finger the observing person should move. b) shows two example pictures of a typical motor mimicry situation in the laboratory originally used as video stimuli in Genschow and Alves (2020). Two people sit opposite each other. The confederate or action model (left person) performs the initial action. The participant (right person) usually follows by copying this behavior.

1.2.1 Motor Mimicry

Lipps (1907) introduced the concept of mimicry, proposing that copying movements and expressions creates internal cues, allowing individuals to experience the emotions of those they imitate — a phenomenon he termed "objective motor mimicry". About 60 years later, Scheflen (1964) extended this work, highlighting the unconscious use of postural information for social alignment within groups. He emphasized that postural positions communicate messages about liking and understanding during social interactions.

Nowadays, there is extensive evidence for motor mimicry, which is often only referred to as mimicry (for a review, see Chartrand & van Baaren, 2009; Duffy & Chartrand, 2015a). Motor mimicry can be defined as the notion that people imitate others' behaviors (i.e., posture, facial expression, emotions, and verbal features) during social interactions.

Measuring Motor Mimicry. Motor mimicry is commonly measured in dyads, with one person being a confederate, often unaware of the hypothesis, and the other being the participant. Included in motor mimicry paradigms are a mimicry (i.e., the confederate imitates) and a no-mimicry (i.e., without imitation) condition (Chartrand & Lakin, 2013; Chartrand & van Baaren, 2009; Kulesza et al., 2016). For instance, Chartrand and Bargh (1999) had participants describe photos together with a confederate, with the latter repeatedly touching their own face or moving their foot. Videos revealed that participants mimicked the confederate's behavior (e.g., touching their own face after observing the confederate). Studies also explore the impact of *being* mimicked, such as Van Baaren et al. (2004) found increased pro-social behavior in participants after they were being mimicked (vs. not being mimicked). Recent research employs video recordings of people performing certain actions to assess mimicry more easily (e.g., Likowski et al., 2008) or extending investigations to third-person perspectives (Genschow & Alves, 2020)

Four Types of Motor Mimicry. Motor mimicry encompasses four types: facial mimicry, emotional mimicry, verbal mimicry, and behavioral mimicry (Duffy & Chartrand, 2015a). Facial mimicry involves copying someone's facial expressions, primarily for understanding and inferring their emotions (Chartrand & van Baaren, 2009; Duffy & Chartrand, 2015a; Hess & Blairy, 2001). It's debated whether imitation starts in newborns, with some believing in neonatal imitation (Meltzoff & Moore, 1977, 1979), while other research has found neonatal imitation to be more of a reflex than intentional imitation (Heyes, 2001; Meltzoff & Moore, 1979; Oostenbroek et al., 2016). However, this fact continues to be debated (Nagy et al., 2020). Nevertheless, infants already engage in facial mimicry (e.g, Isomura & Nakano, 2016).

When engaging in emotional mimicry individuals imitate the emotional expressions of others, often influenced by affiliation goals and emotional intentions (Duffy & Chartrand, 2015a; Hess & Fischer, 2013). Sometimes emotional mimicry is considered a subtype of facial mimicry (Hess & Blairy, 2001). Offensive expressions are typically not mimicked (Duffy & Chartrand, 2015a), and context plays a crucial role in determining which emotions are imitated (Bourgeois & Hess, 2008; Hess & Fischer, 2014).

Verbal mimicry relates to the imitation of another person's way to talk (Neumann & Strack, 2000). In conversations, people tend to pick up on each other's accents (Giles & Powesland, 1975), how quickly they respond (Chartrand & Lakin, 2013), speech speed (Webb, 1969), speaking breaks (Niederhoffer & Pennebaker, 2002), and how long they talk (Webb, 1972).

Behavioral Mimicry is extensively researched and includes copying postures, gestures, physical movements, and mannerisms (for a review, see Chartrand & Lakin, 2013; Chartrand & van Baaren, 2009; Duffy & Chartrand, 2015a). Behavioral mimicry often leads to increased liking (e.g., Chartrand & Bargh, 1999; Kulesza et al., 2022) or promotes pro-social behavior (e.g., Catmur & Heyes, 2013; van Baaren, Holland, et al., 2004).

Moderators of Motor Mimicry. Individuals are more likely to mimic when they aim to affiliate (Cheng & Chartrand, 2003; Kavanagh & Winkielman, 2016; Lakin & Chartrand, 2003), or when they have eye contact to the confederate (Bavelas et al., 1986; Mauersberger et al., 2022). Interindividual differences also play a role, such as high cognitive empathy correlating with more behavioral mimicry and high affective empathy with increased facial mimicry (Chartrand & Bargh, 1999; Sonnby-Borgström et al., 2003). Extraversion (Duffy & Chartrand, 2015b) and attachment styles (Hall et al., 2012) may influence mimicry as well. Finally, disliked individuals or those from an outgroup are imitated less, although being mimicked reduces negative out-group bias (e.g., Bretter et al., 2023; Likowski et al., 2008; Stel et al., 2010; van der Schalk et al., 2011). Positive emotions generally encourage mimicry, but not all positive emotional states have the same effect: Feelings of pride lead to less mimicry (Dickens & DeSteno, 2014) while sad emotions generally lower the tendency to imitate (Likowski et al., 2008).

However, these studies have mostly not been replicated despite their low power and barely significant *p*-values. The likely reason for this is that motor mimicry studies are uneconomical and costly (Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017).

Automaticity of Motor Mimicry. Automaticity in mimicry involves unconscious, efficient, unintentional, and uncontrollable processes (Bargh & Chartrand, 1999; Heyes, 2011; Moors & De Houwer, 2006; T. D. Wilson, 2002). Recent research expands this view, considering speed, attention, and stimulus-driven nature as integral facets (Moors & De Houwer, 2006). Motor mimicry aligns with these criteria, occurring without awareness, consciousness, control, or intention (Chartrand & Dalton, 2009; Duffy & Chartrand, 2015b), as participants reported in post-test interviews (Chartrand & Bargh, 1999). Moreover, mimicry may be very fast (Dimberg & Thunberg, 1998). Motor mimicry is proposed to be stimulus-driven, since certain repeated behaviors by the confederate are imitated more frequently by the participant than others (Chartrand & Bargh, 1999; Chartrand & Dalton, 2009). In addition, motor mimicry is efficient as it is performed easily under cognitive load (e.g., Levelt & Kelter, 1982).

1.2.2 Automatic Imitation

In addition to face-to-face motor mimicry, there is a second measurment method to imitation, namely automatic imitation. Automatic imitation refers to the finding that performing an action that is different from an observed action of another person interferes with the individuals' tendency to imitate (Brass et al., 2000, 2001; Stürmer et al., 2000). The term automatic imitation is defined as an initial task being modulated by action observations that may or may not match the required response (for a review, see Heyes, 2011), or in other words: People imitate others, even if it affects their own performance (Cracco, Bardi, et al., 2018; Heyes, 2011). In more detail, automatic imitation refers to any kind of stimulus-response compatibility effect in which task-irrelevant action stimuli facilitate similar responses and impede dissimilar responses (for a meta-analysis, see Cracco, Bardi, et al., 2018).

Automatic Imitation Paradigms. Automatic imitation is measured with reaction times, error rates and sometimes kinematics (i.e., limb movement) using stimulus-response compatibility paradigms combining perception and action, oftentimes within social settings (Cracco, Bardi, et al., 2018; Darda & Ramsey, 2019; Heyes, 2011). Typically, automatic imitation effects are measured using forced choice reaction time paradigms (i.e., participants performing different responses with their fingers to different cues; e.g., Brass et al., 2000), simple response reaction time paradigms (i.e., participants performing the same response with their fingers to a cue; e.g., Brass et al., 2001) or simple response kinematic paradigms (i.e., participants performing the same response to a cue with their limbs, like arms or hands; e.g., Bouquet et al., 2011). Typically, the participants see another person's hand (or limb in kinematic designs) additionally to the cue, performing the same or a different movement (for a review, see Cracco, Bardi, et al., 2018). This results in congruent trials (i.e., participant and model performing the same movement) and incongruent trials (i.e., both perform different movements).

A recent meta-analysis showed that kinematic automatic imitation paradigms might produce spatial processes rather than genuine automatic imitation effects, although it does not completely rule out that automatic imitation effects can be found using this type of paradigm (Cracco, Bardi, et al., 2018). However, the most important and most often used task design is an adapted forced-choice reaction time paradigm, often referred to as the imitation-inhibition task, which I will explain in detail in Chapter 1.3.1.

Automaticity of Automatic Imitation. As already outlined, automatic processes are unconscious practices that happen quickly, do not require attention, and cannot be avoided (Bargh & Chartrand, 1999; Heyes, 2011; Moors & De Houwer, 2006; T. D. Wilson, 2002). More recent research considers automaticity as a multifaceted construct or concept that combines classic features like consciousness, efficiency, and intentionality, with speed, attention, and a stimulus-driven nature (Moors & De Houwer, 2006). All these features also apply to automatic imitation. First, in automatic imitation paradigms, participants imitate the observed movements of another person regardless of whether this facilitated or impaired their own response selection, even while having a clear different instruction to react to different (number) cues. These, findings are widely taken as indication that this type of imitation is (apart from its stimulus-driven task structure) – uncontrollable, involuntary and unintentional, which speaks to its automatic nature (Heyes, 2011). Second, research also found that automatic imitation still occurs, and may even be facilitated, under high cognitive load. Thus it appears to be efficient (Catmur, 2016; van Leeuwen et al., 2009). Moreover, a recent meta-analysis showed that automatic imitation is weakened when the duration between stimulus movement and number cue is increased. The authors concluded that in automatic imitation, the relevant processes must be fast, which would fulfill another component of automaticity (Cracco, Bardi, et al., 2018). Furthermore, they showed that automatic imitation is diminished but not completely eliminated when attention is shifted away from the stimulus movement (Cracco, Bardi, et al., 2018). In general, automatic imitation is sensitive to variations in attention (Catmur, 2016; Cracco & Brass, 2017), but persists in its absence, so attention or awareness is not a direct prerequisite for automatic imitation to occur (Cracco, Bardi, et al., 2018).

Nevertheless, whether automatic imitation is an unconscious process is still debatable, as participants typically report explicitly to feel the urge to imitate the stimulus movement and automatic imitation is found to be absent when the movement action is presented subliminally (Cracco, Bardi, et al., 2018; Mele et al., 2014). Moreover, participants typically notice that they are slower and make more errors in incongruent trials (Cracco, Bardi, et al., 2018). Still, automatic imitation is an unintentional process that occurs rapidly and does not depend on attention per se, warranting the term automatic in its name.

Is Automatic Imitation a Goal-Directed Process? Research on automatic imitation of goal-directed vs. goalless actions is mixed. Some research supports the view that automatic imitation is driven by goals (e.g., Bouquet et al., 2011) and other research supports the view that it is rather driven by movements (e.g., Chiavarino et al., 2013; Liepelt & Brass, 2010a). A recent meta-analysis of automatic imitation found that goalless actions produce stronger automatic imitation than goal-directed actions (Cracco, Bardi, et al., 2018). The authors argued that it may be the fast reaction in the automatic imitation task that leads to goals not being encoded in terms of actions goals, but rather in terms of movement characteristics, which is why goalless actions are imitated more strongly. However, with enough processing time, the pattern can be reversed. This may explain the different results regarding the topic (Cracco et al. 2018).

1.2.3 Motor Mimicry and Automatic Imitation: A Comparison

As both concepts, motor mimicry and automatic imitation, are part of the construct imitation, but are nevertheless very different, it is worth comparing the two.

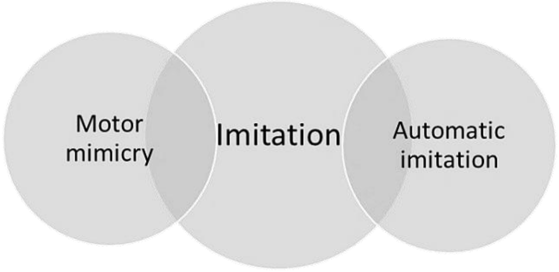
Motor mimicry (a) is primarily observed in natural social situations such as interactions with a confederate and (b) its dependent variable involves measuring action frequency within an interaction or other subjective ratings of executed mimicry actions (e.g., Chartrand & Bargh, 1999; Genschow, van Den Bossche, et al., 2017). While it shows high external validity (Chartrand & van Baaren, 2009; Heyes, 2001, 2011), and offers insights into social mechanisms (Chartrand & van Baaren, 2009; Duffy & Chartrand, 2015a), motor mimicry faces challenges. Even though motor mimicry studies are underpowered and have just significant p-values (e.g., Chartrand & Bargh, 1999; van Baaren, Holland, et al., 2003, 2004) there’s a lack of replications. Thus, high-powered studies are needed to validate the findings. Additionally, possible confounding variables, varying experimental setups, and potential confederate biases complicate the research landscape (Salazar Kämpf et al., 2018). Motor mimicry experiments are time-consuming (Westfal, Mischkowski, et al., 2022), uneconomical, and unreliable (Cracco & Brass, 2019; Genschow, van Den Bossche, et al., 2017), partly due to interference factors in social interactions (Cracco & Brass, 2019; Genschow, van Den Bossche, et al., 2017; Salazar Kämpf et al., 2018), but also because of too few target actions (i.e., imitative behavior) being shown in typical motor mimicry experiments (Genschow, van Den Bossche, et al., 2017).

In comparison to motor mimicry, automatic imitation (a) is studied in an artificial environment at the computer oftentimes using reaction time tasks and (b) its dependent variable is reaction time and/or accuracy of a movement within trials (Cracco, Bardi, et al., 2018). While lacking the complexity of full social interactions (Cracco & Brass, 2019; Heyes, 2011; Ramsey, 2018), leading to limited external validity and generalizability (Genschow, van Den Bossche, et al., 2017; Heyes, 2011), this paradigm ensures reliability, consistency, and ease of evaluation (Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017; Heyes, 2011). Automatic imitation never claimed to be a social measurement and was not implemented as such, but to measure covert imitation tendencies offering insights into cognitive mechanisms with stable and robust effects (Cracco & Brass, 2019). Nevertheless, its artificial nature does not diminish its adaptability to social inquiries (e.g., J. L. Cook & Bird, 2012; Cracco et al., 2015; Genschow, Schuler, et al., 2019; Gleibs et al., 2016; Press et al., 2005). The paradigm's uniform construction and its consistent procedures across different studies enhance further its reliability and its strong effects (for a review, see Cracco, Bardi, et al., 2018; Heyes, 2011).

To summarize, the advantages of automatic imitation are the disadvantages of motor mimicry and vice versa and both measurement methods complement each other in measuring imitation due to their different structure: While motor mimicry is often referred to as the natural social form of imitation, automatic imitation is referred to as the controlled cognitive form of imitation (Heyes, 2011). Nevertheless, the two imitation types are not correlated (Genschow, van Den Bossche, et al., 2017). This lack of correlation could be due to the procedural differences, the limited reliability of motor mimicry paradigms, but also possible different underlying processes (Genschow et al., 2017). For example, automatic imitation paradigms are referred to as cognitive because they require explicit control and executive functions to correctly perform the tasks involved (Brass & Heyes, 2005; Heyes, 2011), but such psychological processes are not required in motor mimicry. Moreover, studies suggest that different brain parts and processes are involved in both imitation forms (Hogeveen et al., 2015): Automatic imitation is influenced by corticospinal facilitation of the observed action (Bardi et al., 2017), whereas motor mimicry appears to be controlled by more general sensorimotor arousal (van Schaik et al., 2017). Despite not being correlated, research agrees that both paradigms measure imitation (e.g., Cracco, Bardi, et al., 2018; Cracco & Brass, 2019; Heyes, 2011). Preliminary evidence for this claim stems from the fact that automatic imitation and motor mimicry have similar results in terms of social modulation or priming: for example on in- and out-groups (Genschow & Schindler, 2016; Yabar et al., 2006), empathy (Müller et al., 2013; Sonnby-Borgström et al., 2003), self-other focus (Ashton–James et al., 2007; Genschow, Schuler, et al., 2019) or perspective taking (Cracco et al., 2015; Stel & Vonk, 2009). Moreover, both imitation constructs are believed to be based on the same links between observation and action and are thought to be caused by a shared representation of observed and performed actions (Brass & Heyes, 2005; Chartrand et al., 2005; Chartrand & Bargh, 1999; Chartrand & Lakin, 2013; Chartrand & van Baaren, 2009; Greenwald, 1970; Heyes, 2011; Prinz, 1997, 2005). Thus theoretically, it might be that automatic imitation and motor mimicry just measure different parts of the construct imitation (see Figure 1.2).

Figure 1.2

The Possible Venn-Diagram of Imitation, Motor Mimicry, and Automatic Imitation



1.3 Automatic Imitation With the Imitation-Inhibition Task

This chapter deals in detail with automatic imitation, measured with the imitation-inhibition task, as this is the focus of this dissertation. Whenever I mention the imitation-inhibition task throughout this dissertation, I am specifically referring to an adapted version of the forced-choice reaction time paradigm of Brass el al. (2000), which is probably most often used in the automatic imitation literature (cf., Cracco, Bardi, et al., 2018).

1.3.1 The Imitation-Inhibition Task

The imitation-inhibition task is a method for assessing an individual's tendency to imitate in a trial-by-trial manner. It was developed to assess imitation in a laboratory setting without having to employ confederates and to provide a method of measuring covert imitation response tendencies (Cracco & Brass, 2019). The imitation-inhibition task (Brass et al., 2000) assesses effector compatibility using finger movement in a forced choice reaction time paradigm. Participants respond to two numeric cues with distinct finger movements over multiple trials. Typically, they are instructed to raise their index finger in response to the number cue "1" and their middle finger in response to the number cue "2." At the same time, participants see a task-irrelevant stimulus, a picture of another person's hand on the screen either lifting the same finger (congruent trial), the other finger (incongruent trial), or no finger at all (neutral trial). Oftentimes only congruent and incongruent trials are included in the task (Cracco, Bardi, et al., 2018). The primary dependent variable of interest is reaction time, as it offers reliable results (Genschow, van Den Bossche, et al., 2017). Error rates are the second dependent variable but can sometimes be less reliable, as they depend on participants making an adequate number of errors, which does not always occur (e.g., Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017; Westfal, Cracco, et al., 2022).

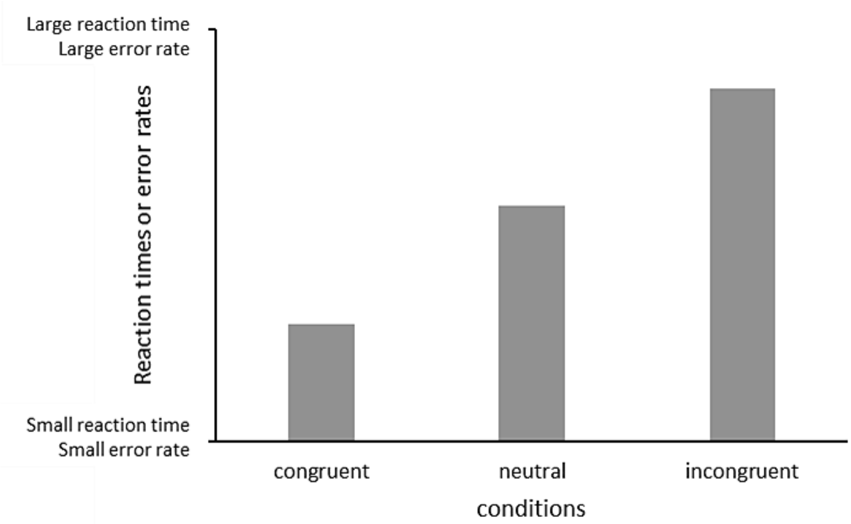
Originally designed for laboratory settings, the imitation-inhibition task measured movement onsets using custom-built light sensors (Brass et al., 2000). Recent research demonstrated that similar robust effects can be achieved by measuring key releases on computer keyboards (e.g., Butler et al., 2015; Genschow, van Den Bossche, et al., 2017), a method we adapted for online environments to use it in our online version of the imitation-inhibition task.

1.3.2 Automatic Imitation Effects With the Imitation-Inhibition Task

The most common and most important observed effect in the imitation-inhibition task is the congruency effect, indicating faster reaction times and fewer errors in congruent trials compared to incongruent trials (for a meta-analysis, see Cracco, Bardi, et al., 2018). Additionally, researchers can evaluate two other indices of automatic imitation by including a neutral condition where the hand on the screen remains motionless (e.g., Genschow, van Den Bossche, et al., 2017). Typically, participants exhibit faster responses with fewer errors in congruent trials compared to neutral trials (i.e., the facilitation effect), and in neutral trials compared to incongruent trials (i.e., the interference effect). The congruency effect reflects the overall strength of imitation. However, how much of this strength is movement facilitation (i.e., that a participant can react faster and more accurately by following the imitation), or how much is movement interference (i.e., that participants must first inhibit imitation before they can perform correctly the required response), is not reflected by the congruency effect, but by the other two indices (Cracco, Bardi, et al., 2018; Cracco & Brass, 2019; Heyes, 2011). These three effects are schematically shown in Figure 1.3 and may be calculated by subtracting the respective trial conditions per participant from each other (e.g., Genschow, van Den Bossche, et al., 2017).

Figure 1.3

Schematic Representation of the Automatic Imitation Effects



Note. As the participants can follow their automatic tendency to imitate in congruent trials, they have a facilitated reaction and respond faster than in neutral trials. As participants in incongruent trials first have to inhibit their interfering automatic tendency to imitate, they are slower than in neutral trials.

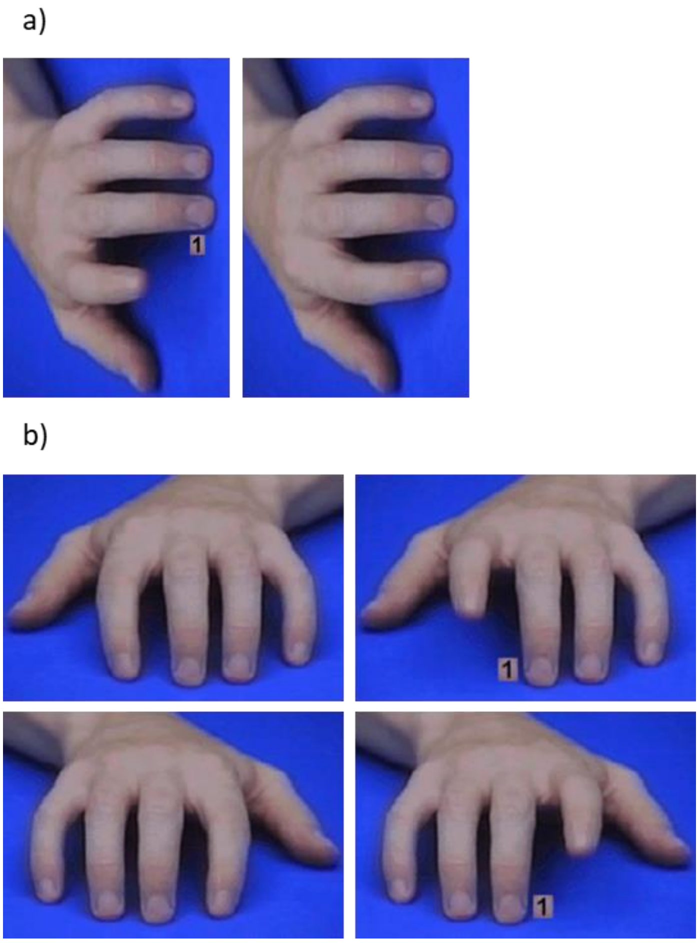
1.3.3 Contributing Processes to Automatic Imitation

Essentially, the typical automatic imitation effect can generally be attributed, at least partly, to three key processes (Cracco, Bardi, et al., 2018): movement compatibility, which refers to the compatibility of the type of movement (e.g., observing a lifting movement while simultaneously executing a lifting movement); effector compatibility, which involves the compatibility of body parts (e.g., observing the index finger while performing a corresponding index finger action); and spatial compatibility, which pertains to the compatibility of location (e.g., observing a movement on the left side of space while executing a movement on the left side of space). It is important to note that in early research with the imitation-inhibition task, imitative compatibility (i.e., automatic imitation), was confounded with spatial compatibility (Brass et al., 2000, 2001). Stimuli at this time were made in a way that when they were imitatively congruent (i.e., cue indicating the same finger lifting movement as the hand on the picture is lifting), they were also spatially congruent (i.e., the movement happened on the same side of the screen). The same applied to incongruent stimuli: Imitatively incongruent movements were also spatially incongruent. This raised the fundamental question of whether automatic imitation is only driven by spatial compatibility, which would suggest that automatic imitation could be considered as a form of the Simon effect (i.e., that responses to stimuli occur faster when the stimulus and response are in the same location, even if the stimulus location is irrelevant to the performance of the task; e.g., Borgmann et al., 2007).

This has since been debunked as research employed new techniques to control for spatial compatibility (for an overview, see Cracco, Bardi, et al., 2018). One of the most commonly used techniques involves rotating the presented hand stimuli by 90 degrees. This alteration results in the observed finger movements no longer aligning with the same spatial location as the executed finger movements (e.g., J. L. Cook & Bird, 2012; e.g., Heyes et al., 2005; Press et al., 2005). Another approach taken by researchers is to present participants with both left and right hand stimuli (e.g., Bertenthal et al., 2006; Boyer et al., 2012; Catmur & Heyes, 2011). For an overview of the different ways to control for spatial compatibility, see Figure 1.4. Using this techniques research has shown that spatial compatibility does not fully account for automatic imitation (Bertenthal et al., 2006; Boyer et al., 2012; Catmur & Heyes, 2011; J. L. Cook & Bird, 2012; Heyes et al., 2005; Press et al., 2005). Remarkably, past research has consistently shown robust automatic imitation effects even when spatial compatibility was negatively correlated with imitative compatibility, using both left and right hand stimuli, as confirmed by a meta-analysis (Cracco, Bardi, et al., 2018). In other words, the congruency effect in a typical imitation-inhibition task can even be detected when the observed action is spatially incompatible with the executed action. Thus, spatial compatibility does not account for automatic imitation although automatic imitation can be enhanced by spatial compatibility.

Figure 1.4

Examples of Spatially Controlled Stimulus Material



Note. Both, a) and b), depict possibilities to control for spatial compatibility in an imitation-inhibition task. a) This approach uses pictures rotated by 90°, thus not confounding the spatial location with the imitative movement anymore. b) This approach uses mirrored and non-mirrored pictures to be able to measure both imitative and spatial compatibility.

However, both movement and effector compatibility (both part of the general imitation definition, i.e., copying the exact same observed body movements of another person) contribute to automatic imitation (Cracco, Bardi, et al., 2018; Heyes, 2011; Leighton & Heyes, 2010). Typically, automatic imitation paradigms concentrate on just one of these two compatibility processes: Experiments on effector compatibility keep the movement constant while manipulating the effector (e.g., lift index or middle finger), while experiments on movement compatibility keep the effector constant while manipulating the movement (i.e., tapping or lifting of the index finger). The typical imitation-inhibition task uses effector compatibility (cf., Brass et al., 2000). Both experiment types have found reliable automatic imitation effects (Brass et al., 2000, 2001; Catmur & Heyes, 2011; Heyes et al., 2005; Hogeveen & Obhi, 2013; Stürmer et al., 2000). Effector and movement compatibility also appear to be at least partially independent (e.g., J. L. Cook & Bird, 2012; Cracco, Bardi, et al., 2018; Leighton & Heyes, 2010; Press et al., 2010). A recent meta-analysis compared reaction times of automatic imitation in effector compatibility tasks with reaction times of automatic imitation in movement compatibility tasks and found that automatic imitation was stronger when measured in effector compatibility experiments than in movement compatibility experiments (Cracco, Bardi, et al., 2018).

1.4 Theories on Automatic Imitation and its Underlying Mechanisms

This chapter focuses on the various theories that attempt to explain the phenomenon of automatic imitation and the mechanisms behind it. Different theories have been put forward to explain automatic imitation. These theories either focus on cognitive (e.g., Greenwald, 1970), social (e.g., Chartrand & Bargh, 1999), or neuropsychological processes (e.g., Heyes, 2010). Here, I will discuss theories associated with automatic imitation, but not theories focused on very specific imitation phenomena or solely on motor mimicry (e.g., the active intermodal matching model (Brass & Heyes, 2005; Meltzoff, 2002; Meltzoff & Moore, 1979), which is mainly concerned with neonatal facial expression mimicry). I will also discuss theories that relate specifically to the modulation of automatic imitation. Eventually, I will give a short conclusion of the theories on mechanisms underlying automatic imitation.

1.4.1 Perception-Behavior Link

The perception-behavior link is, arguably, the most classic and very general applicable imitation theory and forms the basis for many other theories. It assumes that the observation of an action leads to the activation of this action in the observer because both, perception and behavior, share the same representation (Chartrand et al., 2005; Chartrand & Bargh, 1999; Chartrand & van Baaren, 2009; Dijksterhuis & van Knippenberg, 1998). Since the observation of an action and the execution of an action itself are linked, the mere observation of the action leads to an increased probability of executing the same action (Chartrand et al., 2005). The coding system for perceiving behaviors is therefore the same as the one for performing behaviors, hence the name perception-behavior link. This theory was mainly used as an explanation for mimicry (Chartrand et al., 2005; Chartrand & Bargh, 1999; Chartrand & van Baaren, 2009; Duffy & Chartrand, 2015a), but is also used to explain automatic imitation (Heyes, 2011; e.g., Jackson et al., 2006; Westfal et al., 2021). However, many other theories have been developed from or alongside this theory that are more likely to be responsible for automatic imitation, such as the ideomotor theory (Greenwald, 1970; Prinz, 2005; Shin et al., 2010; see Chapter 1.4.3).

1.4.2 The Associative Sequence Learning Theory

This theory is a universal theory and therefore refers to imitation in general, i.e. includes all forms of imitation (Brass & Heyes, 2005). It sees imitation as the result of associative learning processes. This theory refers not only to transparent actions, such as finger movements in automatic imitation, but also to opaque actions, such as facial expressions, where the own sensory feedback is not the same as the observed image, as we cannot see our own face (Brass & Heyes, 2005; Heyes, 2005; Iacoboni, 2009). The associative sequence learning theory sees each action observation divided into two action representation components: The first encodes the visual information and internalizes what the action looks like; the second contains the information about how the action feels and how it can be imitated (i.e., somatosensory information and motor commands). The two are not directly linked. Associative sequence learning theory assumes that the association between the visual representation of the action and its motor representation arises because they are learned together over time as we observe ourselves performing the action. Thus, the execution of an action contains sensory perceptual information that is associated over time with the motor command that triggers its execution (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018; Heyes, 2010; Iacoboni, 2009). This includes both, the mirrored emotional expression seen on another person's face as well as the objective of lifting the finger and the subsequent finger lift. The reciprocal connections between observing and performing an action learned through these experiences are primarily thought to be a possible mechanism accounting for the phenomenon of automatic imitation (Cracco, Bardi, et al., 2018), but may also explain motor mimicry (Iacoboni, 2009).

1.4.3 Ideomotor Theory

The ideomotor theory is an independent theory that extends associative sequence learning theory and builds on the perception-behavior link (Iacoboni, 2009). This theory is probably most often referred to as an explanation for automatic imitation (Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017). Ideomotor theory assumes that imitation is based on a shared representation of observed and performed actions. Furthermore, like associative sequence learning theory, the ideomotor theory proposes that associative learning creates these links between the visual representation of the action and its motor representation (Brass & Heyes, 2005). However, by going one step further, ideomotor theory proposes that these learning processes lead to the development of ideomotor representations that directly represent the actions along with their sensorimotor consequence (Greenwald, 1970; Prinz, 1997; Shin et al., 2010). In other words, the visual image of an action is directly part of its motor representation. Thus, the observation of an action immediately primes the execution of this action, resulting in automatic imitation (Brass & Muhle-Karbe, 2014; Genschow, van Den Bossche, et al., 2017). According to ideomotor theory, observed stimuli can vary in their extent to which they correspond to the sensory output of an action (i.e., ideomotor compatibility). Thus, observed actions with higher ideomotor compatibility can access the corresponding ideomotor representation more easily, which results in stronger imitation (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018; Greenwald, 1970). The ideomotor theory is mainly associated with automatic imitation and is mentioned in motor mimicry primarily in connection with the perception-behavior link (e.g., Chartrand & Bargh, 1999).

1.4.4 Mirror Neuron Networks

Ever since mirror neurons were found in the primate brain (di Pellegrino et al., 1992; Gallese et al., 1996), they have repeatedly been associated with imitation (e.g., Bien et al., 2009; Brass & Heyes, 2005; Catmur et al., 2009; Cracco, Bardi, et al., 2018; Heyes, 2011; Heyes & Catmur, 2022). Mirror neurons are found in large numbers in motor areas of the frontal, premotor and parietal cortex and fire not only during the execution of actions but also during the observation of actions (Casile, 2013; Caspers et al., 2010; Rizzolatti & Craighero, 2004). Although the existence of mirror neurons has been challenged by some researchers (Heyes & Catmur, 2022), researchers widely agree that there is a mirror neuron system (or network), meaning that the observation of an action leads to similar activation in the brain as its execution (Casile, 2013; Gazzola & Keysers, 2009; Iacoboni, 2009; Keysers & Gazzola, 2010; Kilner et al., 2009; Molenberghs et al., 2012).

Over the last decades, there has been much speculation about the function of mirror neurons. It‘s being discussed that they are not only responsible for imitation, but also for action understanding (Catmur et al., 2007; Heyes & Catmur, 2022), empathy (Gallese, 2003; Gazzola et al., 2006), language development (Rizzolatti & Arbib, 1998) and the simulation of actions (M. Wilson & Knoblich, 2005). While research with Transcranial magnetic stimulation (TMS; Aziz-Zadeh et al., 2002; Catmur et al., 2007; Fadiga et al., 1995; Strafella & Paus, 2000) and functional magnetic resonance imaging (fMRI ; Gazzola & Keysers, 2009; Keysers & Gazzola, 2010) has demonstrated that mirror neurons are associated with imitative actions, it is uncertain to what extent these connections form an actual whole organized system (Heyes, 2010; Heyes & Catmur, 2022). Similarly, it is still not clear which exact function they fulfill during imitation (Brass & Heyes, 2005; Heyes, 2010). However, researchers agree that mirror neurons are clustered at certain locations in the brain and that they perform important tasks in the execution of actions (Gazzola & Keysers, 2009; Heyes, 2010; Heyes & Catmur, 2022; Iacoboni, 2009; Keysers & Gazzola, 2010). Moreover, it is generally accepted that although they can generate imitation, their primary function might not be to imitate per se. As such, it appears most likely that human imitation is the interplay between mirror neurons and more complex cognitive abilities (Buccino et al., 2004; Iacoboni, 2009).

1.4.5 Theories of Modulation of Automatic Imitation With the Imitation-Inhibition Task

In addition to general theories explaining automatic imitation and its mechanism, there are also theories that relate specifically to the explanation of modulations of automatic imitation. In this chapter I will discuss these theories in more detail.

Dual-Route Models of Stimulus-Response Compatibility. Dual-route models of stimulus-response compatibility (SRC) by definition have two routes: A controlled conditional route and an uncontrolled unconditional route (Proctor & Vu, 2006). The basis of the theory is an underlying long-term stimulus-response link. Heyes (2011) associated dual-route models of SRC with automatic imitation. Before she explains modulations with automatic imitation, she first makes own assumptions about the general functioning and mechanism of automatic imitation itself with the dual-route model. In her view, the imitation-inhibition task activates both routes and thus two motor representations: the controlled route through its instructions to react to the number cue, and the automatic route, which is activated by the irrelevant hand stimulus in the background. When these two routes match in congruent trials, the final response reaction is facilitated as both activated motor representations are the same; if they do not match, it becomes impaired as the incorrect automatic motor representation has to be inhibited. But for the modulation of automatic imitation the underlying stimulus-reponse link is important, not the two routes who cause the automatic imitation effect. A modulation of the magnitude of automatic imitation can occur in two ways: Input or output modulation of the underlying stimulus-response link. Input modulation is the modulation of the degree of automatic imitation by influencing the processing of the initial action stimulus. Output modulation refers to modulating the extent to which the motor activation of a corresponding action is inhibited or allowed in order to influence the final responsive behavior (Heyes, 2011). Thus, attention, for example, might modulate automatic imitation on an input level, as they influence the perception of the initial action stimulus. On the other hand self-other focus might modulate automatic imitation on an output level, as it may influence the extent to which the motor activation of a corresponding action is inhibited or not and therefore exhibits a different behavioral response (Heyes, 2011).

Motivational Theories. According to motivational theories, individuals use imitation as a means of affiliating with others. Affiliation refers to different forms of social benefits that brings individuals together with other people (Lakin & Chartrand, 2003). As a result, motivational theories assume that people imitate other people more when they have an affiliation goal (Chartrand & Bargh, 1999; Genschow, Pauels, et al., 2023; Wang & Hamilton, 2012). Thus, individuals unconsciously imitate others in order to use imitation as a tool to get others to like them more or to get other social benefits out of the interaction. The mechanism is assumed to be unintentional and unconscious (Wang & Hamilton, 2012).

An example of a motivational theory is the social top-down response modulation (STORM) model (Wang & Hamilton, 2012). According to the STORM model, the visual input activates not only the motor action, but also an evaluative mentalizing system (that includes, for example, person evaluation, priming, context information, etc.), which is then further supplied with social evaluation through the perception-action association. This evaluation system ultimately modifies the association through social top-down response modulation.

Motivational theories find support in classic motor mimicry studies that demonstrate positive social consequences of imitation: being imitated leads to the participant liking the imitator (Chartrand & Bargh, 1999), feeling close to the imitator (van Baaren, Holland, et al., 2004), or giving more money to the imitator (van Baaren, Holland, et al., 2003), to name just a few examples. More recent studies also apply motivational theories to automatic imitation (De Souter et al., 2021; Genschow et al., 2022; Genschow & Schindler, 2016; Gleibs et al., 2016; Leighton et al., 2010).

Self-Other Overlap Theories. Self-other overlap theories propose that the tendencies to imitate are learned responses that develop as a result of self-observation and interaction with other, often similar, individuals (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018; Genschow et al., 2021; Heyes, 2010, 2011). As a result, individuals perceived to be similar to oneself should be imitated to a larger extent.

Self-other overlap theories are compatible with both associative sequence learning theory (cf., Chapter 1.4.2) and ideomotor theory (cf., Chapter 1.4.3). According to associative sequence learning theories, imitation is a learned response that develops from self-observation and interaction with other, often similar, individuals (Brass & Heyes, 2005). In the ideomotor theory, imitation is the result of an action observation that directly stimulates the execution of the action due to its ideomotor representations. The ideomotor compatibility is key in this context, thus, the extent to which observed actions resemble the sensory outcomes of an action (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018; Heyes, 2011). This means that observed action models that are highly similar to the participant lead to quicker access of the corresponding ideomotor representation in the participant because the ideomotor compatibility of the observed action is higher (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018). In both theories, the result is a stronger automatic imitation of similar models.

Self-other overlap theories applied to automatic imitation see evidence in the fact that automatic imitation is sensitive to observer and model similarity (Chaminade & Cheng, 2009; Cracco et al., 2015; Genschow et al., 2021). Moreover, an extensive body of literature shows that human models are imitated more strongly than non-human models, which is also attributed to a larger self-other overlap between human models and participants (Chaminade & Cheng, 2009; Gowen & Poliakoff, 2012; Klapper et al., 2014; Press, 2011; Press et al., 2005, 2006). A recent meta-analysis also states that self-other overlap is more important than previously thought and shows that also gender overlap and response overlap (i.e., stimulus movement matches in visual characteristics and outcomes produced by these actions) partly elicit a stronger automatic imitation (Cracco, Bardi, et al., 2018).

1.4.6 Conclusion: Theories on Automatic Imitation and its Underlying Mechanisms

With respect to theories about the mechanics and general processes underlying automatic imitation, research to date never assessed experimentally, and thus has left open, which theory offers the best explanation of automatic imitation. Although most work on automatic imitation cites the ideomotor theory (e.g., Aicken et al., 2007; Bird, Brindley, et al., 2007; Brass & Heyes, 2005; Brass & Muhle-Karbe, 2014; Cracco, Bardi, et al., 2018; Cracco et al., 2015; Genschow, van Den Bossche, et al., 2017; Iacoboni, 2009; Prinz, 2005; Spengler, Brass, et al., 2010; Westfal et al., 2021), in general, the theories reviewed in this chapter are very similar, and some researchers assume that they overlap strongly (e.g., Brass & Heyes, 2005; Iacoboni, 2009), or even see some theories as one and the same (e.g., Dijksterhuis & Bargh, 2001). Irrespective of the different theories proposed to explain automatic imitation, researchers agree that automatic imitation is a result of action observation and direct activation of the corresponding motor plan (Bertenthal et al., 2006;

Brass et al., 2000; Chartrand & Bargh, 1999; Chartrand & van Baaren, 2009; Kilner et al., 2009; Liepelt et al., 2008). Indeed, several studies confirm this claim: Behavioral studies (e.g., Brass et al., 2000, 2001; Kilner et al., 2003), fMRI studies (Dinstein et al., 2007; e.g., Gazzola & Keysers, 2009; Keysers & Gazzola, 2010; Kilner et al., 2009), Electroencephalography (EEG) studies (Lepage & Théoret, 2006), motor TMS studies (e.g., Catmur et al., 2007; Fadiga et al., 1995; Maeda et al., 2002), and single-cell recordings in both monkeys (di Pellegrino et al., 1992) as well as humans (Mukamel et al., 2010), all show that the observation of an action leads to a similar motor representation as the execution of that action.

With regard to theories on modulations of automatic imitation, all theories are quite different concerning how a possible change in the magnitude of automatic imitation could occur. Whereas in the dual-route model a modulation is simply based on a change in the perception of the stimulus-response link, leaving the social component completely uncovered, motivational theories cover this social component precisely, but hardly describe the mechanism behind. Self-other overlap theories, on the other hand, rely on general theories that explain the mechanism behind automatic imitation and extend these theories with a modulation mechanism based on similarity perception. Additionally, these theories, like the general theories regarding the mechanism underlying automatic imitation, are also not tested directly in experiments. Nevertheless, particularly motivational theories and self-other overlap theories are frequently used and considered more important than dual-route models as possible explanations for different modulations, oftentimes several at once (e.g., Cracco, Bardi, et al., 2018; De Souter et al., 2021; Genschow et al., 2022; Gleibs et al., 2016). Indeed, the theories may be better applicable to individual modulations and not all theories may explain all modulations of automatic imitation. However, there is no research on this.

Future research ought to test whether processes proposed by these theories actually relate to automatic imitation and test several of the theories in one single experiment.

To summarize, although there is a consensus among researchers that modulations of automatic imitation are possible, the theoretical landscape remains vague and less consistent than the one regarding general theories of the mechanisms underlying automatic imitation.

1.5 Modulation of Automatic Imitation With the Imitation-Inhibition Task

Since one of the key points of this dissertation is the modulability of the imitation-inhibition task in its online version, in the next section, I will review the literature on what kind of modulations, which are mainly of social nature, have already been found using the imitation-inhibition task in the laboratory. I will also discuss the replication problems with some of these findings. First, however, I will define top-down and bottom-up modulations, as these are different ways to manipulate automatic imitation and therefore deserve a short definition.

1.5.1 Bottom-up vs. Top-down Modulations

Researchers differentiate between bottom-up or top-down modulations. Typically, when a researcher uses a bottom-up manipulation they directly manipulate the moving hands in the imitation-inhibition task (Press et al., 2006). Thus, directly changing the motor input by changing its appearance, using different kinematics, or changing the stimulus saliency (Gowen & Poliakoff, 2012; Klapper et al., 2014). Examples are the animacy effect experiments with non-human and human hand images (e.g., Kilner et al., 2003; Klapper et al., 2014; Press et al., 2005) or different skin colors in group membership experiments (e.g., Genschow et al., 2022).

Top-down modulations are not directly implemented in the imitation-inhibition tasks, but rather beliefs, prior knowledge or attention that alter the top-down processing (Wang & Hamilton, 2012) of the stimulus (Gowen & Poliakoff, 2012; Klapper et al., 2014; Press et al., 2006). Typically top-down modulations are achieved with different instructions or different priming methods before or during the experiment (e.g., Genschow et al., 2021; Klapper et al., 2014; Liepelt & Brass, 2010b).

1.5.2 Modulations in the Laboratory: An Overview

In the laboratory, the imitation-inhibition task has been used in many experiments with different modulations to determine their effects on or their relationships to automatic imitation. There are both experimental approaches and correlative approaches, which may be both top-down or bottom-up.

Among the correlative approaches, studies were mainly conducted on personality traits or personality disorders. For example, Hogeveen and Obhi (2013) found that the degree to which others are automatically imitated varies in relation to narcissistic traits. Other researchers found reduced automatic imitation in individuals with autistic spectrum disorder (Bird, Leighton, et al., 2007; J. Cook et al., 2013; J. L. Cook & Bird, 2012; Williams et al., 2004), or in patients with schizophrenia (Enticott et al., 2008). Research also shows that cognitive and emotional empathy traits (Cracco et al., 2015; Müller et al., 2013; Nishimura et al., 2018; Sonnby-Borgström et al., 2003), interoceptive awareness (Ainley et al., 2014), as well as the ability to take the perspective of another person (e.g., Cracco et al., 2015), correlate with automatic imitation.

Nevertheless, there is also a large body of research showing that automatic imitation with the imitation-inhibition task does not always successfully yield a correlation with some personality traits. For example, levels of pro-social behavior could not be predicted by automatic imitation (Galang & Obhi, 2020), as well as personality traits like extraversion or agreeableness (Butler et al., 2015) or personality traits related to self-other focus (Genschow, van Den Bossche, et al., 2017). Moreover, other research was not able to replicate some of the previous findings, such as the relation of automatic imitation with perspective taking (Genschow, van Den Bossche, et al., 2017; Newey et al., 2019), empathy (Butler et al., 2015; Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017), or autism (Butler et al., 2015; Cracco, Bardi, et al., 2018).

There are also a lot of experimental modulation approaches with the imitation-inhibition task in the laboratory. Researchers found stronger automatic imitation effects for other-focus vs. self-focus (Bortoletto et al., 2013; Cracco et al., 2015; Genschow, Schuler, et al., 2019; Spengler, Brass, et al., 2010), for pro-social vs. anti-social priming (Leighton et al., 2010), for human vs. non-human hands (Liepelt & Brass, 2010b; Press et al., 2010), for human vs. robotic hands (Bird, Brindley, et al., 2007), and for in-group vs. out-group members (Genschow & Schindler, 2016; Gleibs et al., 2016; Rauchbauer et al., 2016). Moreover, automatic imitation is enhanced in goalless actions when compared to goal-directed actions (Chiavarino et al., 2013), for pro-social mindsets when compared to anti-social mindsets (Butler et al., 2016; Cracco, Genschow, et al., 2018), for direct as compared to averted gaze (Wang et al., 2011), for someone who has experience in the task compared to no experience (Heyes et al., 2005), when the attention is directed to the hands compared to no attention direction (Longo & Bertenthal, 2009), when participants are acting in expectation of cooperation compared to competition (Gleibs et al., 2016), when intention is perceived in the actions as compared to unintended actions (Liepelt et al., 2008), or when the focus is directed towards similarities compared to differences to the model (Genschow et al., 2021).

On the other hand, there are some manipulations that yielded no effects, as for example Liepelt and Brass (2010a) found no difference in automatic imitation between possible and impossible movements. Moreover, regarding direct experimental manipulations in the laboratory, some researchers could not replicate some of these findings. For example, the difference between direct compared to averted gaze (e.g., Trilla et al., 2020), pro-social mindset vs. anti-social mindset (Westfal et al., 2024), or cooperative vs. competitive situations (Era et al., 2020) could not be replicated. Moreover, the research on observing in-group members enhancing automatic imitation in comparison to observing out-group members is rather mixed, with some studies supporting the finding (e.g., Gleibs et al., 2016) and others not supporting the finding (e.g., De Souter et al., 2021). The research regarding goalless actions vs. goal-directed actions enhancing automatic imitation is mixed as well (cf., Chapter 1.2.2).

1.5.3 Replication Problems

The above cited literature highlights that modulation of automatic imitation with the imitation-inhibition task is not as straightforward as it seems at first glance. On the one hand, there is consistent evidence supporting the idea that focusing on others, in comparison to focusing on oneself (e.g., Genschow, Schuler, et al., 2019; Spengler, Brass, et al., 2010), or observing human actions as opposed to non-human actions (e.g., Bird, Leighton, et al., 2007; Kilner et al., 2003; Klapper et al., 2014; Press et al., 2005), enhances automatic imitation. On the other hand, the evidence for the idea that direct gaze compared to averted gaze (e.g., Trilla et al., 2020; Wang et al., 2011), leads to increased automatic imitation is somewhat unclear due to several unsuccessful replication attempts. One possible explanation for this inconclusive evidence might be that modulation effects of automatic imitation, particularly social modulations, are relatively small, as a recent meta-analysis indicates (Cracco, Bardi, et al., 2018). In addition, many modulations of automatic imitation in the laboratory originate from a time when less attention was paid to the power of the experiments and only a few participants were collected with a rather low stimulus repetition number (Cracco, Bardi, et al., 2018; Heyes, 2011). Thus, these findings are strongly underpowered. There are therefore two reasons why it becomes necessary to conduct high-powered experiments with the imitation-inhibition task to be able to shed some light on modulations of automatic imitation with the imitation-inhibition task. Thus, in addition to the development of the online imitation-inhibition task that I address in Chapter 2, in my dissertation I would also like to investigate high-powered four selected modulations of automatic imitation with the online task: In Chapter 3, Chapter 4 and Chapter 5 of this dissertation, I examine different social and non-social modulations of automatic imitation measured with the online imitation-inhibition task. As the online imitation-inhibition task allows for collecting large samples, the likelihood of finding possible small modulation effects should be increased.

Chapter 3 is a study on whether different finger movement sizes make a difference in automatic imitation and the animacy effect. We noticed that some animacy stimuli material is not controlled for finger movement size. In fact, in some studies, finger movement size is even confounded with the conditions such that a larger finger movement is associated with the human hand and a smaller finger movement with the robotic hand. We therefore tested whether both finger movement size and finger movement speed have an effect on automatic imitation and, in a final experiment, whether the animacy effect remains strong despite the finger movement confound. The results are important for all stimuli with the imitation-inhibition task, since confounds of this type can occur not only with the animacy effect. While it may not be the reason why some modulations cannot be replicated, such a confound certainly complicates replications with the imitation-inhibition task.

Chapter 4 is a top-down investigation whether there is a link between free will belief and automatic imitation in two high-powered experiments. In these experiments, we used a correlational design to investigate whether interindividual differences based on perceiving intention in others are differently associated with automatic imitation. Since belief in free will is associated with a varying perception of intention in others, we used a free will belief questionnaire to assess whether free will belief correlates positively with automatic imitation. Thus, this chapter examines interindividual differences but in a high-powered manner in the hope of shedding some light on the replication problems with interindividual differences in automatic imitation and understanding whether low power caused these replication problems or not.

Chapter 5 examines whether observing cultural in-group and out-group members makes a difference in automatic imitation. For this purpose, different manipulations are used to experimentally investigate the effects of observing the hand of a cultural in-group member compared to the hand of a cultural out-group member. The manipulations here are mainly top-down using instructions and different beliefs about the group membership, but we also used a bottom-up approach, trying to manipulate directly the stimulus material. Since group membership is one of the modulations that has been producing mixed replication results lately, we hoped to progress research on automatic imitation in this area with own high-powered replications.

1.5.4 The Necessity of an Online Version of the Imitation-Inhibition Task

We decided to develop an online version of the imitation-inhibition task not only because of the need for high-powered experiments in the field of automatic imitation. Generally speaking, the most striking drawback of the existing imitation-inhibition task is that it is tied to controlled and effortful laboratory environments. Data collection in laboratory experiments is typically time-consuming, labor-intensive, and often restricted to university-based samples (e.g., Thomas, 2011). Moreover, laboratory samples make cross-cultural and international studies much more effortful (e.g., Hanel & Vione, 2016). Thus, working in the laboratory with the imitation-inhibition tasks leads to WEIRD (Western, Educated, Industrialized, Rich, Democratic) samples, as does other laboratory work (e.g., Henrich et al., 2010; Muthukrishna et al., 2020). As a result, the samples are not representative when it comes to the comparability of automatic imitation. Furthermore, conducting experiments with students and relying on student samples in laboratory experiments has faced criticism (e.g., Henry, 2008; Stevens, 2011). Given the current replication crisis in psychological research which led to initiatives like the Open Science Collaboration (2015), there's generally an increasing need for larger and more diverse participant samples, as also emphasized by other researchers (e.g., Asendorpf et al., 2013). But fulfilling the demand for larger samples becomes a significant challenge, both in terms of resources and expenses, when only conducting complex and resource-intensive laboratory experiments. Online research offers a solution to these problems, as it is faster, cheaper and more economical (e.g., Dandurand et al., 2008; De Man et al., 2021). Therefore, we found it appropriate to propose an online version of the imitation-inhibition task to enable automatic imitation research to progress away from effortful laboratory studies. In the following Chapter 2 I address the development of the online imitation-inhibition task in detail. In this chapter I present an imitation-inhibition task I programmed for the online environment as well as four studies we conducted to validate this task. In this chapter I show that the online imitation-inhibition task (a) performs well in an online environment, (b) achieves comparable results to its laboratory version, (c) shows stable automatic imitation effects even under control of spatial compatibility, and (d) enables social modulations with the imitation-inhibition task in an online environment.

Chapter 2

Validation of an Online Imitation-Inhibition Task

As already discussed, an online version of the imitation-inhibition task would be very practical for many reasons. An online imitation-inhibition task allows to collect large samples in order to find modulations of automatic imitation with very small effects. Moreover, it enables high-powered replications that are necessary to consolidate and verify effects. Additionally, psychological research nowadays largely takes place online (e.g., De Man et al., 2021), as online research has many advantages over laboratory research, including more differentiated cross-cultural samples (Hanel & Vione, 2016) and more economical studies (Dandurand et al., 2008).

For this reason, in this chapter I present a way to implement the imitation-inhibition task adapted from Brass et al. (2000) in an online environment.

This chapter is based on the following manuscript:

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Please note that some changes in headings, citation style, and formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the article.

Abstract

People automatically imitate a wide range of different behaviors. One of the most commonly used measurement methods to assess imitative behavior is the imitation-inhibition task (Brass et al., 2000). However, in its original form, the task is only suited to be carried out in a laboratory setting–a time-consuming and costly procedure. Here, we present an approach for conducting the imitation-inhibition task in online settings. We programmed the online version of the imitation-inhibition task in JavaScript and implemented the task in online survey software (i.e. Qualtrics). We validated the task in four experiments. Experiment 1 (*N* = 88) showed that the typical automatic imitation effects can be detected with good psychometric properties. Going one step further, Experiment 2 (*N* = 182) directly compared the online version of the imitation-inhibition task with its laboratory version and demonstrates that the online version produces similar strong and reliable effects. In Experiments 3 and 4, we assessed typical moderator effects that were previously reported in laboratory settings: Experiment 3 (*N* = 93) demonstrated that automatic imitation can be reliably detected in online settings even when controlling for spatial compatibility. Experiment 4 (*N* = 104) found in line with previous research that individuals imitate hand movements executed by a robot less strongly than movements executed by a human. Taken together, the online version of the imitation-inhibition task offers an easy-to-use method that allows measuring automatic imitation with common online survey software tools in a reliable and valid fashion.

Keywords. Automatic imitation, imitation-inhibition task, online research, survey software

2.1 Introduction

People have the automatic tendency to imitate a variety of different behaviors, including simple movements (Brass et al., 2000; Genschow et al., 2012; Genschow & Florack, 2014; Genschow & Schindler, 2016), facial expressions (Dimberg, 1982), emotions (Dimberg, 1982; Hess & Fischer, 2013), and gestures (Bernieri, 1988; Cracco, Genschow, et al., 2018). Early research indicates that such imitative behaviors fulfill an important function, as it fosters learning (e.g., Bandura, 1962). More recent research illustrates that imitation also fulfills a crucial social function, as it bonds human more strongly together by creating feelings of affiliation (for a review, see Duffy & Chartrand, 2015a).

A disadvantage of current tasks measuring imitative behavior is that their application is limited to effortful laboratory settings. Since the confidence crisis in psychological research (e.g., Open Science Collaboration, 2015), there is a call for large samples (e.g., Asendorpf et al., 2013), which makes conducting effortful laboratory experiments particularly challenging and costly. Moreover, laboratory experiments usually involve student samples which has been criticized repeatedly (e.g., Henry, 2008; Stevens, 2011). One of the most often used tasks measuring imitative behavior is the imitation-inhibition task (Brass et al., 2000, 2001). Here we present a reliable and valid online version of this task and show that it can be implemented in common online survey software tools. This allows measuring imitation flexibly via online platforms in different countries with diverse samples and cultures. Moreover, it facilitates the assessment of large sample sizes when high-powered studies are needed. Thereby, the online task is not only more time-efficient, but also generally more economical than its laboratory version.

2.1.1 Automatic Imitation and its Underlying Processes

The imitation-inhibition task is the most often used task to measure individuals' automatic tendency to imitate on a trial-by-trial basis (Brass et al., 2000; for a meta-analysis, see Cracco, Bardi, et al., 2018). The imitation-inhibition task is regularly used in many different fields, including social (e.g., Cracco, Genschow, et al., 2018; De Souter et al., 2021; Genschow et al., 2022), cognitive (e.g., Brass et al., 2000; Cracco et al., 2015; Genschow, van Den Bossche, et al., 2017), developmental (e.g., Simpson & Riggs, 2011), neuro- (e.g., Darda & Ramsey, 2019; Hogeveen et al., 2015), and personality psychology (e.g., Hogeveen & Obhi, 2013; Obhi et al., 2013; Westfal et al., 2021). In the imitation-inhibition task, participants respond to two numerical cues with two different finger movements across multiple trials. Typically, they respond to the number “1” by lifting their index finger and to the number “2” by lifting their middle finger. At the same time as the number appears on the screen, participants see another person's hand either lifting the same finger (congruent trial), the other finger (incongruent trial), or no finger (neutral trial). The main dependent variable is reaction time, since this can be measured very reliably (e.g., Genschow, van Den Bossche, et al., 2017). Error rates are also measured as a dependent variable although they are not always as reliably interpretable, since they depend heavily on participants making enough errors, which is not always the case (e.g., Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017).

The most often reported effect with the imitation-inhibition task is the congruency effect, which refers to faster reaction times and fewer error rates in congruent, as compared to incongruent trials (for a meta-analysis, see Cracco, Bardi, et al., 2018). In addition, researchers can assess two other automatic imitation indices by implementing the neutral condition in which the hand on the screen does not move. Typically, participants respond faster and with fewer errors to congruent than to neutral trials (facilitation effect) and faster and with fewer errors to neutral than to incongruent trials (interference effect). Since the observation of a movement facilitates the execution of the same movement and inhibits the execution of another movement, it is generally agreed that the imitation-inhibition task measures automatic imitation (Cracco, Bardi, et al., 2018; Cracco & Brass, 2019; Heyes, 2011). Originally, the imitation-inhibition task was used in laboratory settings by measuring movement onsets with custom-build light sensors (e.g., Brass et al., 2000, 2001). More recent research demonstrates that the task produces similar reliable effects by measuring key release times on computer keyboards in the laboratory (e.g., Butler et al., 2015; Genschow, van Den Bossche, et al., 2017)—a procedure that we adapted for the use in the online environment.

In principle, three different components may generally contribute to the typical automatic imitation effects: Movement compatibility (e.g., observing a lifting movement while executing a lifting a movement), effector compatibility (e.g., observing the index finger while executing an index finger movement), and spatial compatibility (e.g., observing an effector or a movement on the left side of space while executing a movement on the left side of space). To control for spatial compatibility, past research used different methods (cf., Cracco, Bardi, et al., 2018). In the most often used method, researchers rotate the presented hand stimuli by 90°. By doing so, the observed finger movements are no longer in the same spatial location as the executed finger movements (e.g., Press et al., 2005). Another method that has been used is to present not only left hands but also right hands to the participants (e.g., Catmur & Heyes, 2011). Past research found robust effector and movement compatibility effects even when controlling for spatial compatibility (for a meta-analysis, see Cracco, Bardi, et al., 2018) further supporting the idea that the imitation-inhibition task is a measure of automatic imitation.

While there is consensus in the literature that the imitation-inhibition task measures imitative behavior (Cracco & Brass, 2019), there is currently a debate about the degree to which social factors can modulate or manipulate automatic imitation. On the one hand, for example, there is consistent evidence for the idea that focusing on others, as compared to the self (e.g., Genschow, Schuler, et al., 2019; Spengler, Brass, et al., 2010) or observing human, as compared to non-human actions (e.g., Bird, Leighton, et al., 2007; Kilner et al., 2003; Klapper et al., 2014; Press et al., 2005) increases automatic imitation. On the other hand, the evidence for the idea that ingroup as compared to outgroup members (e.g., De Souter et al., 2021; Genschow, Pauels, et al., 2023; Genschow & Schindler, 2016; Gleibs et al., 2016; Rauchbauer et al., 2016) or direct as compared to averted gaze (e.g., Trilla et al., 2020; Wang et al., 2011) increases automatic imitation is due to several failed replications rather unclear. One possible reason for the inconclusive evidence is that the effect size of social modulation is rather small (Cracco, Bardi, et al., 2018), which calls for a method that makes it possible to administer the imitation-inhibition task within large samples. The online version of imitation-inhibition task we present here can fill this gap by facilitating data collection of large and diverse samples.

2.1.2 Administering the Imitation-Inhibition Task Online

As data collection in laboratory experiments is usually time consuming, effortful, and mainly limited to university samples (e.g., Thomas, 2011), which makes it very difficult to conduct studies across different countries and cultures (Hanel & Vione, 2016) a steadily increasing number of psychological studies nowadays takes place online (e.g., Buhrmester et al., 2018). Indeed, with the help of different survey platforms (e.g., Qualtrics; www.qualtrics.com), it is now easy to implement high-powered studies online even with little programming knowledge. However, while online surveys can easily be used to assess simple questionnaires, there do not exist many solutions to assess behavior, such as automatic imitation for instance, in online settings.

However, there are several solutions for creating response time tasks using online construction software solutions that include integrated toolkits (e.g., Labvanced, https://www.labvanced.com/; Inquisit, https://www.millisecond.com/). Still, one disadvantage of these software solutions is that the integrated online toolkits do not offer all features for every response time task. Moreover, participants might have to download a program or file when they participate in a study, which many participants shy away from. A solution for these problems is to program reaction-time based measures from scratch with other tools, such as JavaScript libraries (e.g., jsPsych, www.jspsych.org; de Leeuw, 2015; de Leeuw & Motz, 2016; Hilbig, 2016; Pinet et al., 2017). However, without programming knowledge, the implementation of such measures can be a time-consuming and sometimes challenging hurdle that not every researcher is willing to overcome. As there does not exist a behavioral measure to assess automatic imitation in an easy and swift manner in online settings, we programmed and validated the imitation-inhibition task using the jsPsych library in such a way that it can be implemented easily on an own server or a simple survey platform (i.e., Qualtrics). Thus, researchers and students interested in assessing automatic imitation do not have to program the task themselves, but can make use of the validated version we present here even if they have limited programming knowledge.

2.1.3 Construction of the Online Version of the Imitation-Inhibition Task

To program the online version of the imitation-inhibition task, we used the JavaScript library JsPsych (de Leeuw, 2015) by applying existing plugins and generating new plugins (i.e., jspsych-check-response, jspsych-image-keyboard-release and, jspsych-fixation-image-keyboard-release; (Cracco, 2020) to custom-build the task. We have uploaded the whole task in addition with a list of all plugins we used and developed to the Open Science Framework (OSF; https://osf.io/q7fju/) to make the programming process transparent. After creating the code, we modified it so that it can be used in Qualtrics. The OSF folder also contains detailed instructions and a tutorial for this purpose. In addition, we uploaded to the OSF folder analyses scripts to preprocess and analyze the gathered data. When using all the tools we provide on OSF, researchers without much programming knowledge or with little time available can very easily and quickly apply and analyze the online version of the imitation-inhibition task.

2.1.4 The Procedure of the Online Version of the Imitation-Inhibition Task

In line with previous research conducted in the laboratory (Butler et al., 2015; Genschow, van Den Bossche, et al., 2017), the online version of the imitation-inhibition task measures response latencies and error rates by detecting key releases on participants' computer keyboard. Thus, to complete the online imitation-inhibition task, participants need a computer with a keyboard. They will not be able to participate on a tablet without keyboard or on a cell phone, as finger lifting movements cannot be captured well with these devices. We advise researchers to mention this information when recruiting participants.

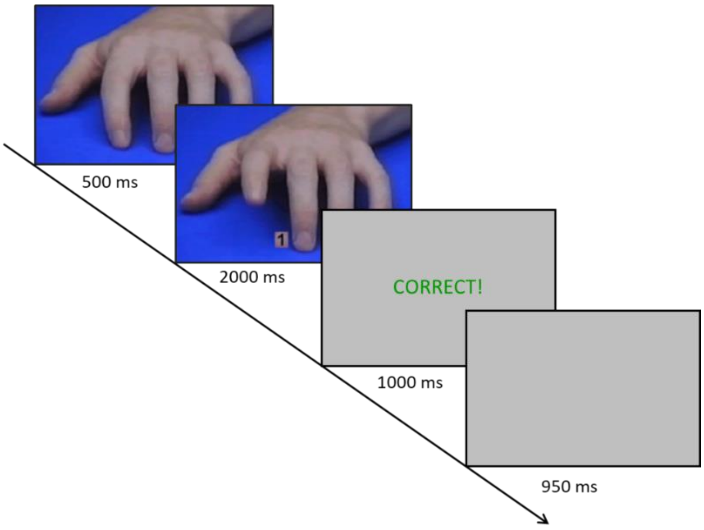
Although the online version of the imitation-inhibition task is similar to the original laboratory-based task developed by Brass et al. (2000, 2001), we added a new practice procedure with two consecutive practice phases, because in online studies, participants cannot ask the experimenter for clarification and often read the instructions poorly (or not at all), which increases the risk of misunderstanding the instructions (e.g., Birnbaum, 2004; De Man et al., 2021; Reips, 2000). Additionally, in reaction time tasks, it is more common to press a key and not to hold and release it, so especially in the online setting, it was important to us that participants had a clear understanding of the procedure of the task. To start the task, participants have to click a button in the center of the screen to switch to full screen mode. This reduces the risk that participants get distracted from other programs and browser taps that might be open while running through the task. Afterwards, participants receive general instructions about the experiment and the imitation-inhibition task. Crucially, participants are instructed to respond with their right index finger and middle finger only. To illustrate the general instructions, participants see a gif of a participant performing the task correctly. Next, participants complete two practice phases.

In the first practice phase, participants are instructed to first press and hold down the “g” key with their right index finger and the “h” key with their right middle finger. As soon as they press down both keys, the participants see a fixation cross for 500 ms, followed by a picture of the number cue “1” or “2”. Within a time window of 2000 ms, participants need to respond as fast as possible to the number cue “1” by lifting their index finger and to the number cue “2” by lifting their middle finger. After lifting their finger, participants get accuracy feedback (i.e., CORRECT in green or WRONG in red letters) displayed for 1000 ms. If participants do not lift any finger, they receive an instruction to lift a finger. Then, a post-trial gap of 950 ms follows until the next trial starts again. After 10 trials, participants receive feedback about their overall performance. If a participant commits more than two errors, (s)he has to repeat the practice phase until (s)he reaches the threshold of at least eight correct trials.

The procedure of the second practice phase consists of 12 trials and is similar to the procedure of the first practice phase (see Figure 2.1). In contrast to the first practice phase though, the number cues are now combined with images of another person's hand. Specifically, instead of the fixation cross, another person's hand in resting position is shown for 500 ms. Afterwards, a picture of the same hand either lifting the index or the middle finger with the number cues presented between the index and middle finger is shown for a maximum of 2000 ms. The participants are again instructed to react to the number cue as fast as possible by lifting their fingers as before. Participants receive accuracy feedback exactly as in the first practice phase. To ensure that participants understand the task, they have to repeat this practice phase until they commit less than 4 errors.

Figure 2.1

Schematic Representation of a Congruent Practice Trial



After the two practice phases, participants start with the experimental phase. The procedure of a typical experimental block is the same as the second practice block, except that participants receive no accuracy feedback. Between each block, participants can take a self-paced break. In addition to the pictures with the model raising the middle finger or the index finger, researchers can implement a neutral condition in which they present pictures of a model not raising a finger. This way, pictures of three different trial types can be shown: In congruent trials, participants are instructed respond with the same finger as the model. In incongruent trials participants are instructed to respond with another finger. In neutral trials participants respond to the cue with a finger while the model's hand does not move at all.

2.1.5 A Survey-Software Solution for the Imitation-Inhibition Task

The task can directly be implemented on one's own (or any other) server by using the programmed jsPsych version. On the Open Science Framework (OSF; https://osf.io/q7fju/), we provide a detailed tutorial on how to implement the task on a server. In addition, we programmed a version that can be implemented in online survey platforms. Here, we present a solution to implement the imitation-inhibition task in one of the most often-used online survey platforms platforms—that is, Qualtrics. Although we focus here on instructions about the implementation of the task within Qualtrics or on a server, with a little adjustment, the task should theoretically also be implementable on other server providers such as mindprobe (https://mindprobe.eu/), cognition.run (https://www.cognition.run/), or pavlovia (www.pavlovia.org) or other survey platforms that support JavaScript add-ins.

In Qualtrics, the task is implemented by using HTML and JavaScript code to a "text entry" survey question, so that a second window overrides the actual question and shows the task. We created different .qsf files for different purposes: The whole original task (with congruent, incongruent, and neutral trial conditions), the original task without a neutral condition, a version of the original task with pictures controlled for spatial compatibility (rotated by 90°) with and without a neutral condition, the task assessing imitative and spatial compatibility separately, a basic version of the task that can contain a picture above the hand stimuli (again with and without a neutral condition), and a version of the task in which two different hand conditions can be implemented (e.g., robotic vs. human hand). Furthermore, we created two versions of the basic original task.qsf files (one with and on without neutral trials) that contain the practice phase and the experimental blocks separated and independently of each other in individual "text entry" questions without instructions, to make the task even more customizable. We have uploaded the different task versions as .qsf files to our OSF folder along with a detailed tutorial (i.e., "Tutorial - Qualtrics implementation") that describes each task version and the implementation of them step-by-step. Here, we provide further important information for the implementation and usage of the online version of the imitation-inhibition task on Qualtrics.

Which of the Code Should Be Added? Our downloadable Qualtrics-.qsf files can be used to automatically create a survey that already contains the task. Contents of the code itself can also be changed by code-savvy persons in Qualtrics by clicking "</> JavaScript" under question behavior of the "text entry" questions that contains the task. Everything is explained in more detail in the tutorial on the OSF.

How Are Reaction Times Measured? Reaction times are measured using the provided and adapted plugin from JsPsych (de Leeuw, 2015). With this plugin, reaction times are recorded following de Leeuw (2015) by recording timestamps when the stimulus appears and when the subject responds (i.e., releases the button) and storing the differences in milliseconds. The reaction times that are recorded are very accurate, reliable, and already validated in previous experiments (e.g., Pinet et al., 2017). For example, de Leeuw and Motz (2016) showed that although JavaScript exhibits a small delay, JavaScript using JsPsych is just as sensitive to reaction times between conditions as any laboratory platform. Similar results were presented by Hilbig (2016). After the reaction times of the individual trials are recorded, they are stored block-wise as .csv file in the respective embedded variables and can be read out later individually. In this way, no trial is lost and trial-wise exclusion criteria can be applied.

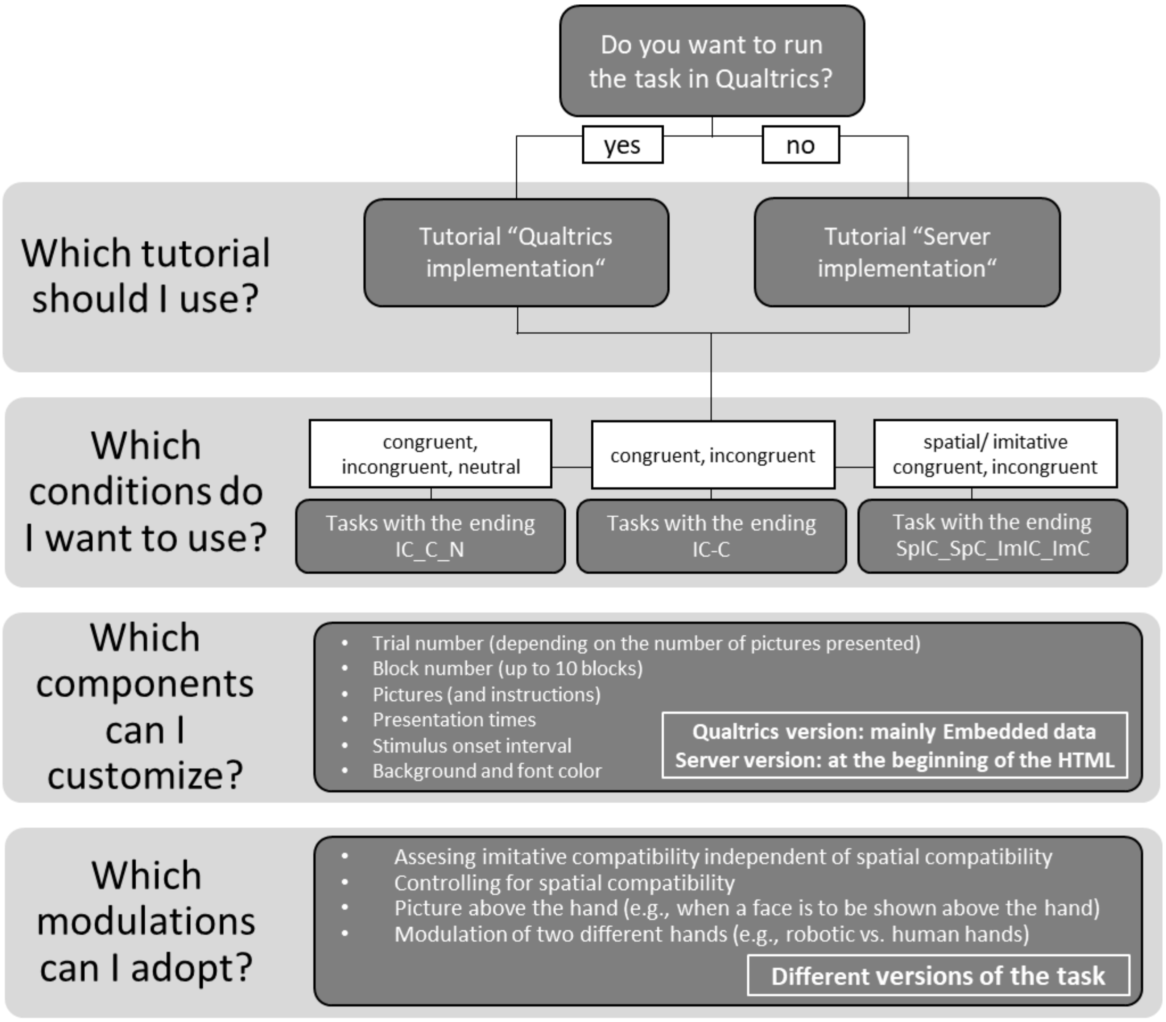
How Exactly Is the Data Stored? We store the trial-wise data per block individually in comma-separated .csv files. The script is written to store up to 10 blocks of the imitation-inhibition task. Theoretically more than 10 blocks can be stored, but this would require adapting the script and adding new embedded variables to the survey flow in Qualtrics. Since we could not create an infinite number of storage embedded variables, we decided to limit the script to 10 variables. Above that it would be too exhausting for the participants anyway to further participate in such a reaction time task. Since the trial number per block can also be adjusted, 10 blocks should be sufficient. This block-wise trial data is stored in a single embedded variable for each block per participant and can be read and processed individually later in R (R Core Team, 2022). In addition to the code of the imitation-inhibition task, on OSF, we provide detailed technical instructions and codes on how to implement the task, how to preprocess the data, and how to analyze it.

How Are Pictures Handled? The pictures are read in and loaded automatically into the computer’s cache before the practice phases trials start, so they are directly available and do not interfere with the reaction times. Theoretically, other images can be used than the ones we were using during the validation experiments. Our images all have a size of 400 x 267 pixels, but images of a different size can also be used.

How Can the Individual Parts of the Experiment Be Customized? Researchers can adjust the task in a number of ways if they wish. For example, the pictures of the practice phases and the experimental phase can be exchanged with any other picture. The default are the pictures we used in the validation experiments. Likewise, custom pictures can be inserted as long as they are on an appropriately accessible location (e.g., Github, an accessible cloud, or personal server). Moreover, we provide a number of variables in the survey flow that can be customized. In particular, researchers can vary the timing (e.g., of the fixation, trial duration, and post-trial gap), the block count, the number of stimulus repetitions within one block (i.e., the trial count within one block), as well as the color of the background or the font of the task. All of these settings can be conveniently changed in Qualtrics itself without having to open the task in the JavaScript window of Qualtrics. For this, we use the "Embedded data" feature provided by Qualtrics (i.e., a facility provided by Qualtrics to easily add or save data; https://www.qualtrics.com/support/survey-platform/survey-module/survey-flow/standard-elements/embedded-data/). A detailed tutorial explaining how the mentioned features can be changed, is available on our OSF. Figure 2.2 shows the possibilities of customization and leads to the different tutorials that contain more information about the customization handling.

Figure 2.2

Flowchart of the Customization Possibilities With the Online Imitation-Inhibition Task



The flow of the task, as well as the two practice phases cannot be changed directly. For this, an interested researcher would have to adapt the JavaScript code. Likewise, the instructions of the task cannot be changed directly in Qualtrics Embedded data variables. However, the places where the instructions can be changed are marked accordingly in the JavaScript code and are easily placed to find. To ease these kind of changes, we added short remarks in the tutorial in the OSF. Moreover, as mentioned we provide second .qsf files for the basic tasks in which only the two practice phases in one "text entry" questions and ten single blocks of the task in other "text entry" questions are available without instructions or breaks. Using this .qsf file or its code, researchers can create their own imitation-inhibition task with individual instructions or break messages. By changing the instructions, using the modulated tasks, or using the task with the separated blocks, the task can be framed in various ways allowing, for example, top-down manipulations to be easily implemented.

How Can the Data of the Task Be Preprocessed and Analyzed? On OSF (https://osf.io/q7fju/), we provide R-scripts (https://cran.r-project.org/) to preprocess and analyze the data. Theoretically, no knowledge of R is necessary for this approach.

2.1.6 Empirical Validation

To validate the online version of the imitation-inhibition task, we conducted four experiments. In Experiments 1 and 2, we tested the task for its functionality and compared it with a laboratory sample in terms of reliability and effect size. In Experiment 3, we tested whether the task produces the to-be-expected findings when controlling for spatial compatibility. Finally, in Experiment 4, we investigated whether one of the most-often reported social moderators—namely bottom-up animacy—influences automatic imitation measured with the online version of the imitation-inhibition task.

2.2 Experiment 1

In Experiment 1, we tested if the online version of the imitation-inhibition task reliably produces the to-be-expected effects (i.e., congruency effect, facilitation effect, interference effect). The experiment was pre-registered at aspredicted (https://aspredicted.org/blind.php?x=/APD_RJG).

2.2.1 Method

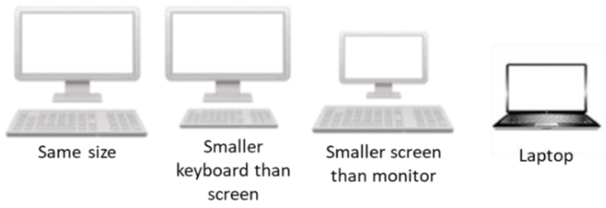
Participants. We aimed to detect a medium to small effect size of *d* = 0.32, which is well below the average effect sizes found in the laboratory (Cracco, Bardi, et al., 2018), as we wanted to be sure to find the effect in the online environment. With a power of β = 0.90, 86 participants are needed to detect such an effect. To compensate for potential dropouts based on our pre-registered exclusion criteria, we pre-registered a sample of 100 participants. We recruited participants via Amazon's Mechanical Turk in return for a compensation of $1.00. Only MTurkers located in the US with a Hit Approval Rate over 85 % were invited to participate in the study. Participants could only take part in the study with a laptop or computer. In line with our pre-registration, data from a total of 12 participants were excluded from data analysis because they had less than 33 trials in one or more of the conditions (*n* = 7), or indicated that they did not use the right hand during the experimental blocks (*n* = 5). The final sample consisted of 88 participants (40 female, 48 male) with an age ranging from 20 to 64 (*M* = 35.41, *SD* = 10.62). Fourteen participants were left-handed and 74 participants were right-handed.

Procedure. The procedure was the exact same as described above. The experiment consisted out of two practice blocks with accuracy feedback to familiarize participants with the task. As described above, participants had to repeat the first practice phase until they reached the threshold of 8 out of 10 accurate trials and they had to repeat the second practice phase until they reached the threshold 8 out of 12 accurate trials. Two participants had to repeat the first practice block, 41 participants had to repeat the second practice block, and 7 participants had to repeat both practice blocks. Afterwards, participants ran through 5 experimental blocks without receiving feedback. Per block, we presented thirty trials in random order. Participants could take a self-paced break between the experimental blocks. In total, the experiment consisted of 150 trials (50 incongruent, 50 congruent, and 50 neutral trials). To prepare the data for analysis, we removed extremely fast and slow reaction times in line with our pre-registration. That is, we removed trials with reaction times below 100 ms (0.46%) and latencies below (0.02%) or above (0.93%) 3 *SD*s of a participant's mean. For the analyses of the latencies we removed erroneous trials as well (7.11%).

At the end, participants indicated demographical data (i.e., gender and age), which hand they used during the experimental blocks (left hand, right hand, or both hands). In addition, participants estimated the ratio between their keyboard and their screen (same size, smaller keyboard than screen, smaller screen than monitor, and laptop; see Figure 2.3), indicated what kind of keyboard they had used (i.e., regular keyboard with prominent keys, regular keyboard with flat keys, Mac keyboard with prominent keys, and Mac keyboard with flat keys), if they used a computer with an external monitor or a laptop, and their handedness (right- or left-handed). Additionally, we extracted from the User-Agent string which browser the participants were using. On average, participants needed *M* = 12.37 (*SD* = 5.04) minutes to complete the task. The task can be found together with the used stimuli, plugins, and further material on the OSF (https://osf.io/q7fju/).

Figure 2.3

Stimuli for Estimation of the Ratio Between Keyboard and Screen

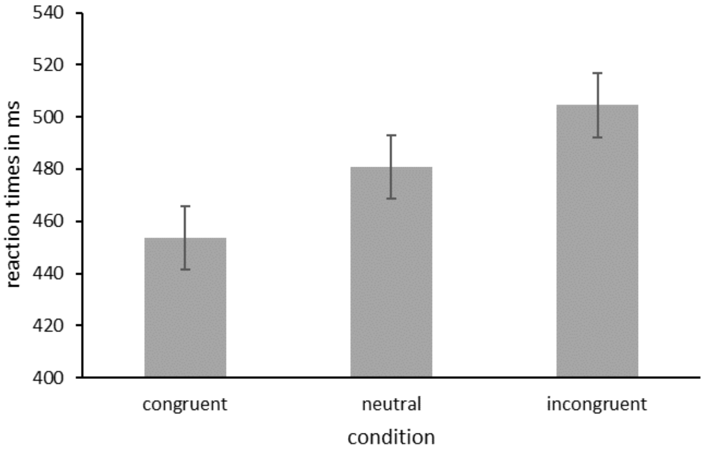


2.2.2 Results

Latencies. For the response times, we applied three pre-registered *t*-tests for dependent samples to test for the presence of the typical imitation-inhibition effects (see Figure 2.4). With respect to the congruency effect, the results indicate that participants responded faster in congruent trials (*M* = 453.77 ms, *SD* = 113.44), than in incongruent trials (*M* = 504.50 ms, *SD* = 114.88), *t*(87) = 13.24, *p* < .001, d_z = 1.41, CI 95% [1.11, 1.71]. Also, the facilitation effect was significant: participants responded faster in congruent trials (*M* = 453.77 ms, *SD* = 113.44) than in neutral trials (*M* = 480.85 ms, *SD* = 112.91), *t*(87) = 12.09, *p* < .001, d_z = 1.29, CI 95% [1.00, 1.57]. Finally, with respect to the interference effect, participants responded faster in neutral trials (*M* = 480.85 ms, *SD* = 112.91), as compared to incongruent trials (*M* = 504.50 ms, *SD* = 114.88), *t*(87) = 8.89, *p* < .001, d_z = 0.95, CI 95% [0.69, 1.20].

Figure 2.4

Response Times of Experiment 1

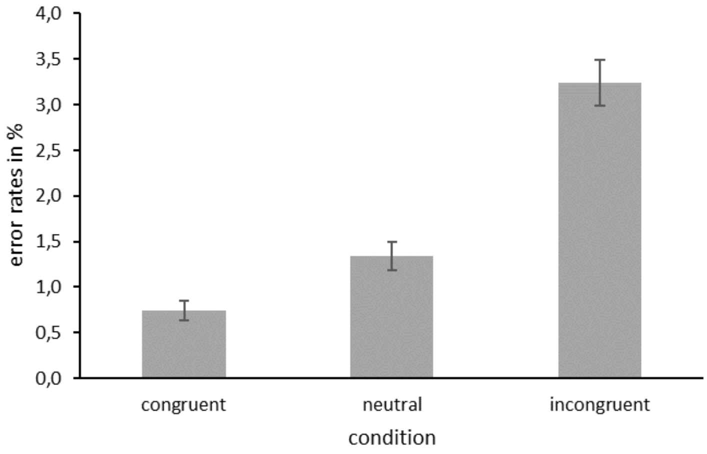


Note. Latencies (+/- SE). All tests are one-tailed.

Error Rates. In a second step, we ran the same analyses for the error rates. In line with the latencies, we detected a congruency effect as participants made fewer errors in congruent trials (*M* = 0.74 %, *SD* = 0.98) than in incongruent trials (*M* = 3.24 %, *SD* = 2.38), *t*(87) = 9.89, $p < .001$, d_z = 1.05, CI 95% [0.79, 1.31]. Moreover, we found a facilitation effect showing that participants committed fewer errors in congruent trials (*M* = 0.74 %, *SD* = 0.98), as compared to neutral trials (*M* = 1.34 %, *SD* = 1.44), *t*(87) = 4.55, $p < .001$, d_z = .49, CI 95% = [0.26, 0.70]. Finally, we found an interference effect as well: In neutral trials, participants made fewer errors (*M* = 1.34 %, *SD* = 1.44), than in incongruent trials (*M* = 3.24 %, *SD* = 2.38), *t*(87) = 8.29, $p < .001$, d_z = .88, CI 95% = [0.64, 1.13]. The error rates of the effects are shown in Figure 2.5.

Figure 2.5

Error Rates of Experiment 1



Note. Error rates in percentage (+/- SE). All tests are one-tailed.

Reliability. To investigate the reliability of the task, we calculated split-half reliabilities of the respective effects on the basis of odd and even trials using the Spearman Brown coefficient in line with previously research on the imitation-inhibition task (Genschow, van Den Bossche, et al., 2017). For the latencies, the congruency effect achieved a reliability of ρ^* = .72, the facilitation effect a reliability of ρ^* = .38, and the interference effect a split-half reliability of ρ^* = .49. For the error rates, the congruency effect achieved a reliability of $\rho^* = .64$, the facilitation effect a reliability of $\rho^* = .45$, and the interference effect a split-half reliability of $\rho^* = .42$. The reliabilities of the effects of the task are comparable with the reliability reported for laboratory experiments in prior research (Genschow, van Den Bossche, et al., 2017).

Explorative Analyses. In additional analyses, we tested whether the ratio between participants' keyboard and their screen, the keyboard they had used, the browser they had used, their handedness, whether they used an external monitor or a laptop, and the number of repetitions in the first and the second practice block influenced the imitation-inhibition indices. None of these factors affected any of the three automatic imitation indices for latencies, *Fs* < 2.25, *ps* > .145, and for error rates, *Fs* < 3.94, *ps* > .050.

2.2.3 Discussion

By applying the online version of the imitation-inhibition task, we replicated the typical imitation-inhibition effects for latencies as well as error rates. The results of the task were not influenced by the ratio between participants' keyboard and their screen, the computer keyboard and browser used, their handedness, and the number of repetitions in one of the practice blocks. This illustrates the robustness of the task. Moreover, the effects of the task were in terms of effect size and reliability in the same range as reported for laboratory experiments in prior research (e.g., Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017). However, to actually compare the online version of the imitation-inhibition task with its laboratory equivalent, an experiment with both tasks is needed—an approach we followed in Experiment 2.

2.3 Experiment 2

In Experiment 2, we empirically compared the imitation-inhibition task in an online setting with a laboratory setting and tested whether one of the tasks produces stronger and more reliable effects. The experiment was pre-registered at Aspredicted (https://aspredicted.org/blind.php?x=/XCC_BBJ).

2.3.1 Method

Participants. Applying the same power analysis as for Experiment 1, we collected an online sample of 100 participants. We recruited the participants via Amazon's Mechanical Turk in return for a compensation of $1.00. The Hit Approval Rate and the location was the same as in Experiment 1. Participants could again only take part in the study with a laptop or computer. In line with our pre-registration, data from a total of 16 participants were excluded from data analysis because they had less than 33 valid trials in one or more of the conditions (*n* = 13), or did not use the right hand during the experimental blocks (*n* = 4). One person met both exclusion criteria. The final online sample consisted of 84 participants (40 female, 42 male, 2 other) with an age ranging from 21 to 73 (*M* = 35.74, *SD* = 12.07). Eight participants were left-handed and 76 participants were right-handed. To ensure that the data collection for both samples (i.e., online and laboratory) was similar, we recruited the participants for the online experiment on several different days spread over two weeks to match the days on which we assessed the participants in the laboratory.

We collected the same number of participants in the laboratory as we did online to obtain a balanced sample. Specifically, for the laboratory sample we recruited 100 participants on the campus of the university of Cologne (Germany) in return for a compensation of a chocolate bar or a coffee. In line with our pre-registration, data from 2 participants were excluded from data analysis because they had less than 33 valid trials in one or more of the conditions. The final laboratory sample consisted of 98 participants (46 female, 52 male) with an age ranging from 16 to 55 (*M* = 23.42, *SD* = 5.51). Fourteen participants were left-handed and 84 participants were right-handed.

Procedure. We used the exact same procedure as in Experiment 1 for both, the online and the laboratory sample. To prepare the data for analysis, as pre-registered, we removed trials with reaction times below 100 ms for the latency and error rate analyses (online sample: 1.11 %; laboratory sample: 0.07%). We also removed latencies below (online sample: 0.03 %; laboratory sample: 0 %) and above (online sample: 0.97 %; laboratory sample: 0.91 %) 3 *SD*s of a participant's mean. For the analyses of the latencies we discarded erroneous trials as well (online sample: 8.09 %; laboratory sample: 5.43 %).

2.3.2 Results

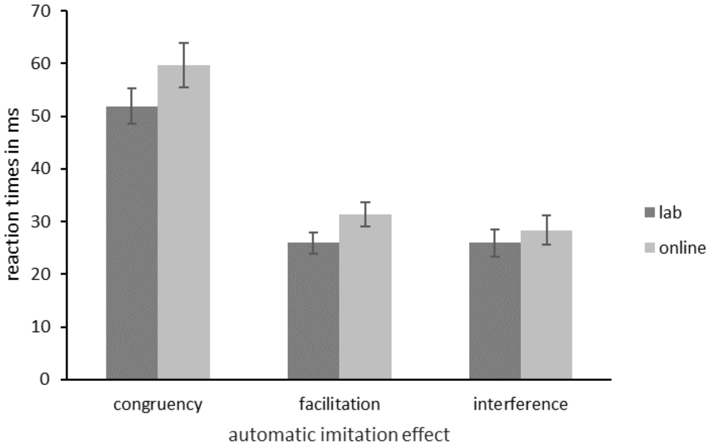
Latencies. We completed several pre-registered tests. First we tested for the presence of the typical imitation-inhibition effects. In line with Experiment 1, the typical imitation effects were significant in both samples. In the online sample, participants responded faster in congruent trials (*M* = 463.26 ms, *SD* = 104.51) than in incongruent trials (*M* = 522.95 ms, *SD* = 111.09), *t*(83) = 14.32, *p* < .001, d_z = 1.56, CI 95% [1.24, 1.88]. Also, the facilitation effect reached significance: The participants responded faster in congruent (*M* = 463.26 ms, *SD* = 104.51), than in neutral trials (*M* = 494.57 ms, *SD* = 104.10), *t*(83) = 13.43, *p* < .001, d_z = 1.47, CI 95% = [1.15, 1.77]. Finally, we found a significant interference effect as participants responded faster to neutral trials (*M* = 494.57 ms, *SD* = 104.10), as compared to incongruent trials (*M* = 522.95 ms, *SD* = 111.09), *t*(83) = 9.98, *p* < .001, d_z = 1.09, CI 95% = [0.82, 1.36].

In the laboratory sample, we found the same effects. Participants responded faster in congruent trials (*M* = 416.36 ms, *SD* = 46.36) than in incongruent trials (*M* = 468.21 ms, *SD* = 60.37), *t*(97) = 15.50, *p* < .001, d_z = 1.57, CI 95% [1.27, 1.86]. They responded faster in congruent trials (*M* = 416.36 ms, *SD* = 46.36), than in neutral trials (*M* = 442.29 ms, *SD* = 50,18), *t*(97) = 13.11, *p* < .001, d_z = 1.32, CI 95% = [1.05, 1.59], and faster to neutral trials (*M* = 442.29 ms, *SD* = 50,18) than to incongruent trials (*M* = 468.21 ms, *SD* = 60.37), *t*(97) = 10.22, *p* < .001, d_z = 1.03, CI 95% = [0.78, 1.27].

A direct comparison of the typical imitation-inhibition effects from the online and laboratory samples showed no meaningful differences in terms of the congruency effect, *t*(180) = 1.48, *p* = .140, d_z = 0.22, CI 95% = [-0.07, 0.51], the facilitation effect, *t*(180) = 1.78, *p* = .078, d_z = 0.26, CI 95% = [- 0.03, 0.56] and the interference effect, *t*(180) = 0.65, *p* = .520, d_z = 0.10, CI 95% = [- 0.20, 0.39] (Figure 2.6).

Figure 2.6

Reaction Times Comparison Between Online and Laboratory Sample



Note. Latencies (+/- SE). All tests are two-tailed.

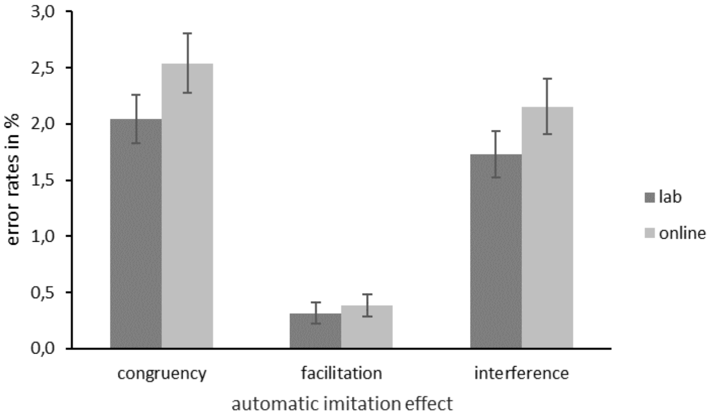
Error Rates. First, we conducted the pre-registered tests. The results of these tests mirrored those of the latencies. In the online sample we found the congruency effect: The participants committed fewer errors in congruent trials (M = 0.63 %, SD = 0.85) than in incongruent trials (M = 3.17 %, SD = 2.60), t(83) = 9.61, p < .001, d_z = 1.05, CI 95% = [0.78, 1.31]. Moreover, we found a facilitation effect: The participants made fewer errors in congruent trials (M = 0.63 %, SD = 0.85), as compared to neutral trials (M = 1.02 %, SD = 1.19), t(83) = 3.86, p < .001, d_z = 0.42, CI 95% = [0.20, 0.64]. Finally, we found an interference effect as participants made fewer errors in neutral trials (M = 1.02 %, SD = 1.19) than in incongruent trials (M = 3.17 %, SD = 2.60), t(83) = 8.77, p < .001, d_z = 0.96, CI 95% = [0.70, 1.21].

In the laboratory sample we found all three imitation effects for the error rates as well. The congruency effect: The participants made fewer errors in congruent trials (M = 0.85 %, SD = 1.06), than in incongruent trials (M = 2.90 %, SD = 2.16), t(97) = 9.48, p < .001, d_z = 0.96, CI 95% = [0.72, 1.20]. The facilitation effect: The participants committed fewer errors in congruent trials (M = 0.85 %, SD = 1.06), as compared to neutral trials (M = 1.17 %, SD =1.06), t(97) = 3.29, p < .001, d_z = 0.33, CI 95% = [0.13, 0.53]. And the interference effect: The participants made fewer errors in neutral (M = 1.17 %, SD = 1.06) than in incongruent trials (M = 2.90 %, SD = 2.16), t(97) = 8.46, p < .001, d_z = 0.86, CI 95% = [0.62, 1.08].

A direct comparison of the typical imitation-inhibition effects revealed no difference in the error rates between the laboratory and online sample in terms of the congruency effect, t(180) = 1.46, p = .145, d_z = 0.22, CI 95% = [- 0.08, 0.51], the facilitation effect, t(180) = 0.50, p = .618, d_z = 0.07, CI 95% = [- 0.22, 0.37], and the interference effect, t(180) = 1.34, p = .182, d_z = 0.20, CI 95% = [- 0.09, 0.49] (see Figure 2.7).

Figure 2.7

Error Rates Comparison Between Online and Laboratory Sample



Note. Error rates in percentage (+/- SE). All tests are two-tailed.

Reliabilities. As can be seen in Table 2.1, the reliability of the imitation-inhibition effects of the two samples are comparable. The negative reliabilities of the facilitation effect in the error rates can best be explained by the overall low number of errors which possibly led to a low correlation between the split-half trials (Krus & Helmstadter, 1993).

Table 2.1

Reliabilities of the Online and the Laboratory Sample

	Latencies		Error rates	
	Laboratory	Online	Laboratory	Online
Congruency	*p** = .81	*p** = .81	*p** = .62	*p** = .60
Facilitation	*p** = .41	*p** = .38	*p** = - .03	*p** = - .51
Interference	*p** = .61	*p** = .65	*p** = .54	*p** = .48

*Note. p** is the Spearman-Brown Coefficient. The split-half reliabilities are based on odd and even trials.

Explorative Analyses. Similarly, as in Experiment 1, we conducted different additional exploratory analyses. These analyses again showed that neither the ratio between keyboard and screen, the keyboard participants had used, the browser participants had used, handedness, whether they had used an external monitor or a laptop, nor the number of repetitions in the first and the second practice block influenced the imitation-inhibition indices in terms of latencies, *Fs* < 2.07, *ps* > .072, and error rates, *Fs* < 2.08, *ps* > .070.

2.3.3 Discussion

As in Experiment 1, Experiment 2 found the typical imitation-inhibition effects with the online version of the imitation-inhibition task. When comparing the effects with a laboratory sample, there were no differences in terms of effect size and reliability. In line with the results obtained in Experiment 1, the results were furthermore not influenced by the ratio between participants' keyboard and their screen, the keyboard they had used, their handedness, whether the used an external monitor or a laptop, or the number of repetitions in one of the practice blocks. This again illustrates the robustness of the task.

In sum, Experiments 1 and 2 demonstrate that the online version of the imitation-inhibition task produces effects that are comparable to those of laboratory experiments. Yet an open question is whether the online version is sensitive to detect crucial moderator influences. To shed light onto this open question, we conducted Experiments 3 and 4.

2.4 Experiment 3

A disadvantage of our previous experiments is that the stimuli confounded imitative with spatial compatibility. That is, participants' finger movements were not only (in)congruent with the model's effector (i.e., index and middle finger), but also with the spatial location of the finger movement (i.e., left and right). The goal of Experiment 3 was to test whether the online task produces reliable effects even when controlling for spatial compatibility.

Previous research conducted in the laboratory found that effects of imitative compatibility still occur when spatial compatibility is controlled (e.g., Catmur & Heyes, 2011; Cracco, Bardi, et al., 2018; Jiménez et al., 2012). To test whether the same is true for the online task, we applied a procedure in line with previous research (Bertenthal et al., 2006; Boyer et al., 2012; Catmur & Heyes, 2011). That is, we presented participants not only with left hands but also with right hands (i.e., mirrored left hands) in Experiment 3. This way, we can separately compute imitative compatibility and spatial compatibility effects.

2.4.1 Method

Participants. In order to detect even small effects with a high power, we performed an a priori power calculation: For an effect size of $\eta^2 = 0.025$ with a power of $\beta = 0.90$, we needed 72 participants. To compensate for potential dropouts, we pre-registered a sample of 100 participants. We recruited the 100 participants via Amazon's Mechanical Turk in return for a compensation of $1.00. The Hit Approval Rate and the location was the same as in Experiment 1.

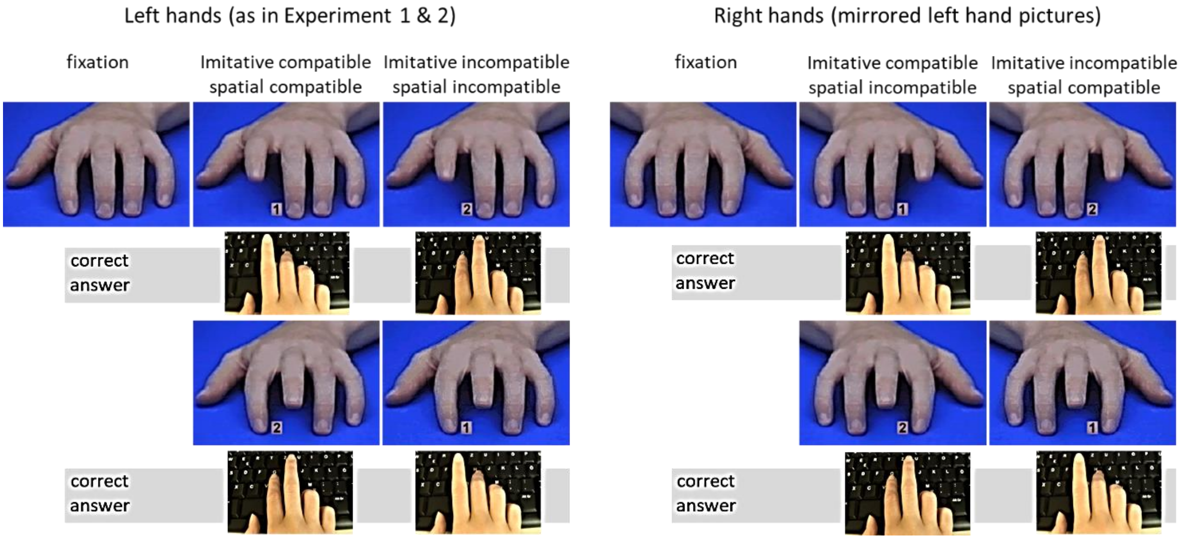
In line with our pre-registration (https://aspredicted.org/blind.php?x=6qx27q), data from a total of 7 participants were excluded from data analysis because they had less than 33 valid trials in one or more of the conditions (*n* = 4), or did not use the right hand during the experimental phase (*n* = 4). One participant met both criteria. The final online sample consisted of 93 participants (41 female, 46 male, 6 other) with an age ranging from 21 to 75

(*M* = 38.58, *SD* = 11.66). Ten participants were left-handed and 83 participants were right-handed.

Procedure. The procedure of the online task was the same as in Experiment 1 and 2, except that not only left, but also right hands were presented on the screen in the second practice block and also in the experimental block (see Figure 2.8). In addition, the task comprised only congruent and incongruent, but no neutral trials.

Figure 2.8

Illustration of the Used Pictures in Their Normal and Mirrored Form



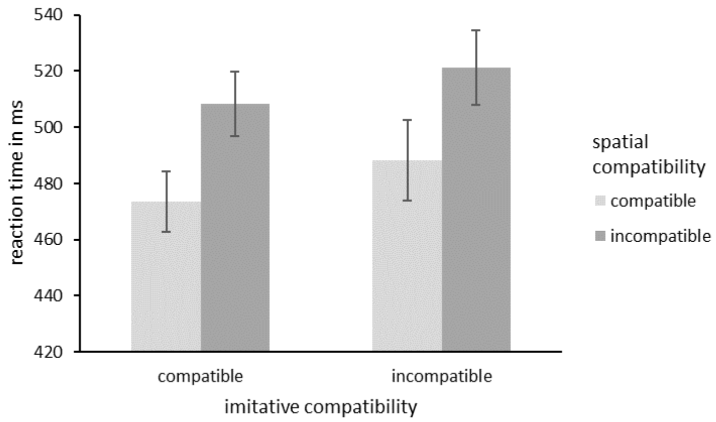
The first practice block remained the exact same as in Experiment 1 and 2. The second practice block consisted of 16 trials in total (8 left hands and 8 right hands) and participants had to repeat it until they committed less than 8 errors. The experimental blocks consisted of 200 trials, with 50 trials for each of the four conditions. The trials were presented in a randomized order across five blocks (40 trials each). Participants could take a self-paced break between each block. To prepare the data for analysis, we pre-registered the same trial-based exclusion criteria as in the other experiments. That is, we discarded trials with reaction times below 100 ms (0.77%). We did not remove latencies below and above 3 *SD*s of the participant's mean, as there were no such trials detected. For the analyses of the latencies, we removed erroneous trials as well (9.02%).

2.4.2 Results

Latencies. To test our hypotheses, we firstly conducted a 2 (imitative compatibility: congruent vs. incongruent) x 2 (spatial compatibility: compatible vs. incompatible) repeated measures ANOVA for the latencies. We found a main effect for imitative compatibility, $F(1, 92) = 12.39$, $p < .001$, $\eta_p^2 = .12$, indicating that participants responded faster in imitative congruent trials ($M = 490.89$ ms, $SD = 106.23$) than in imitative incongruent trials ($M = 504.72$ ms, $SD = 132.49$). Also, the main effect for spatial compatibility was significant, $F(1, 92) = 229.28$, $p < .001$, $\eta_p^2 = .71$. This means that participants responded faster in spatial compatible trials ($M = 480.82$ ms, $SD = 120.28$) than in spatial incompatible trials ($M = 514.79$ ms, $SD = 117.86$). The interaction was not significant, $F(1, 92) = .20$, $p = .656$, $\eta_p^2 = .002$. In line with the non-significant interaction, the pre-registered planned contrast analyses showed that irrespective of spatial compatibility, participants respond significantly faster to imitative congruent movements than to imitative incongruent movements in both the spatial compatible condition, $F(1, 92) = 7.78$, $p = .006$, $\eta_p^2 = .08$, and the spatial incompatible condition, $F(1, 92) = 13.61$, $p < .001$, $\eta_p^2 = .13$, indicating that automatic imitation measured with the online version of the imitation-inhibition task measures imitative behavior even when movements were presented spatially incompatible (see Figure 2.9).

Figure 2.9

Reaction Times Imitative Compatibility x Spatial Compatibility

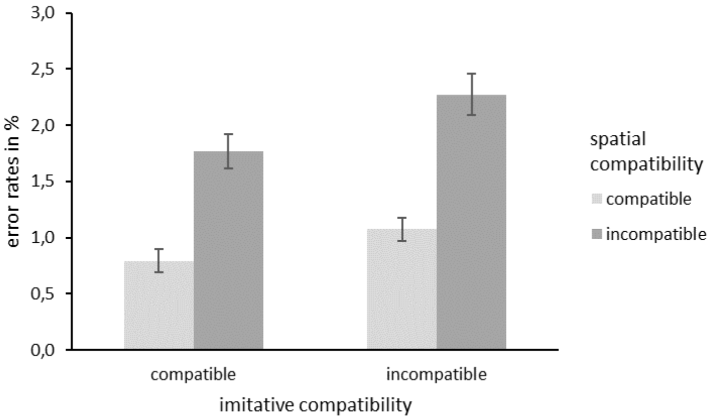


Note. Latencies (+/- SE).

Error Rates. The same analyses on the error rates yielded similar effects. Again, we found both the main effect for imitative compatibility, *F*(1, 92) = 14.20, $p < .001$, $\eta_p^2 = .13$, and the main effect for spatial compatibility, *F*(1, 92) = 71.92, $p < .001$, $\eta_p^2 = .44$. This means that participants made fewer errors in imitative congruent trials (*M* = 1.28 %, *SD* = 1.06) than in imitative incongruent trials (*M* = 1.67 %, *SD* = 1.22). Likewise, they made fewer errors in spatially congruent trials (*M* = 0.93 %, *SD* = 0.86) than in spatially incongruent trials (*M* = 2.02 %, *SD* = 1.47). The interaction was not significant, *F*(1, 92) = 1.92, $p = .169$, $\eta_p^2 = .02$. The planned contrasts revealed that irrespective of spatial compatibility, participants made fewer errors in congruent trials than in incongruent trials in both the spatial congruent condition, *F*(1, 92) = 7.04, $p = .009$, $\eta_p^2 = .07$, and the spatial incongruent condition, *F*(1, 92) = 10.63, $p = .002$, $\eta_p^2 = .10$ (see Figure 2.10).

Figure 2.10

Error Rates Imitative Compatibility x Spatial Compatibility



Note. Error rate percentages (+/- SE).

2.4.3 Discussion

Experiment 3 replicated the typical effects found in the laboratory with the online imitation-inhibition task. That is, in line with previous research conducted in laboratory settings (e.g., Catmur & Heyes, 2011; Cracco, Bardi, et al., 2018; Jiménez et al., 2012), our experiment demonstrated that automatic imitation assessed with the online task is still robust when imitative compatibility is assessed orthogonally to spatial compatibility. In other words, the results of Experiment 3 show that the online version of the imitation-inhibition task is a measure of automatic imitation, because spatial compatibility cannot explain the detected congruency effects. Yet an open question is whether automatic imitation measured with the online version of the imitation-inhibition task as a social process is also modulated by social factors. To investigate this question, we conducted Experiment 4.

2.5 Experiment 4

The purpose of Experiment 4 was to test whether the online imitation-inhibition task can be socially modulated when applying a manipulation that has been frequently used in the laboratory. Specifically, we investigated whether automatic imitation is different for robotic as compared to human hands. In laboratory settings, typically, a smaller automatic imitation effect is detected for robotic hands compared to human hands (Bird, Leighton, et al., 2007; Chaminade & Cheng, 2009; Kilner et al., 2003, 2003; Press et al., 2005, 2006).

2.5.1 Methods

Participants. To be able to detect even very small effects, we performed an a priori power calculation: For an effect size of $\eta^2 = 0.01$ with a power of $\beta = 0.90$, we needed 178 participants. Considering potential drop-outs, we recruited 200 participants via Amazon's Mechanical Turk in return for a compensation of $1.00. As for the first three experiments, only MTurkers located in the US with a Hit Approval Rate over 85 % were invited to participate in the study and participants could only take part in the study with a laptop or computer. We applied the same exclusion criteria as in the first three experiments. In total, data from 96 participants were excluded from data analysis because they did not use the right hand during the experimental phase (*n* = 18), or had less than 33 valid trials in one or more of the conditions (*n* = 88). Thirteen participants met both criteria. Three participants had technical problems, which is why their data was submitted empty.[1] The final online sample consisted of 104 participants (36 female, 64 male, 3 other, 1 missing) with an age

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[1] Please note that we had to exclude more participants as in our previous experiments, even though we used the same exclusion criteria as in the first three experiments. A potential reason for the higher dropout is that participants were less motivated and attentive as compared to our previous experiments, as there are many participants who did not respond at all to nearly one third of all trials. Also, a couple of these participants had to repeat one or both practice phases more than 10 times, which also suggests that they were neither motivated nor attentive to the study.

ranging from 22 to 69 (*M* = 38.40, *SD* = 11.42). Six participants were left-handed and 98 participants were right-handed.

Procedure. The procedure of the online task was similar to the procedure of Experiment 1 and 2 with a few exceptions. That is, we included congruent and incongruent, but no neutral trials. The two practice blocks remained the same as in Experiments 1 and 2. Another difference concerned the stimuli presented. Instead of presenting human hands only, in Experiment 4 we presented participants with a robotic and an artificial human hand. We randomized on a trial-by-trial basis whether the human hand or the robotic hand was presented. The hand pictures have already been used in different prior experiments with the imitation-inhibition task (e.g., Klapper et al., 2014). We presented the images rotated by 90°—an often-used technique to reduce the impact of spatial compatibility (for more information see, Cracco, Bardi, et al., 2018). [2]

The experimental blocks consisted of 160 trials (40 congruent robotic hand trials, 40 incongruent robotic hand trials, 40 congruent human hand trials, and 40 incongruent human hand trials). Participants could take a self-paced break between each block. To prepare the data for analysis, we used the same trial-based exclusion criteria as in the three other experiments. That is, we removed latencies below (0.14%) and above (1.17%) 3 *SD*s of the participant's mean. We also discarded trials with reaction times below 100 ms (2.74%). For the analyses of the latencies, we removed erroneous trials as well (18.24%).

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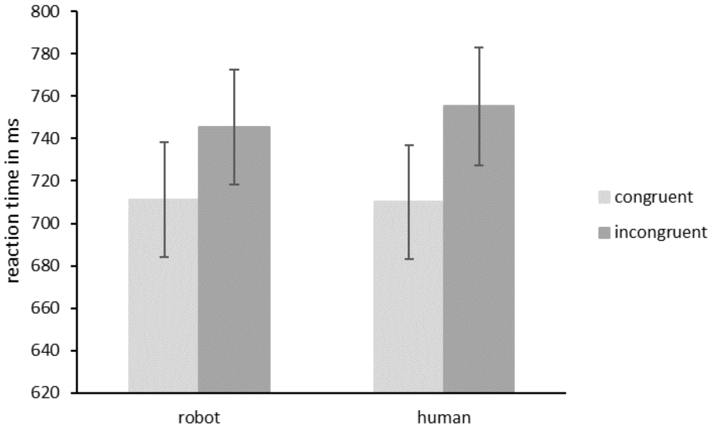
[2] To get further information about the social modulation, we also asked three questions about perceived similarity of the hands individually shown and randomized before the demographics. Specifically, we presented again the different hands and asked the following: "How similar or dissimilar do you find the hands?", "How similar is this hand to yours?" (for robotic and human hand). Participants rated all questions on sliders from 1 ("very dissimilar") to 100 ("very similar"). They perceived their own hand to be more similar to the artificial human hand (*M* = 77.00, *SD* = 21.54), compared to the robotic hand (*M* = 33.04, *SD* = 35.84), $t(103) = 12.40$, $p < .001$, $d_z = 1.22$, CI 95% [0.96, 1.47], although they found the hands neither similar nor dissimilar to each other (*M* = 44.37, *SD* = 34.76), $t(103) = -1.65$, $p = .101$, $d_z = -0.16$, CI 95% [- 0.36, 0.03].

2.5.2 Results

Latencies. To test the hypothesis that the automatic imitation effect is stronger for human hands, as compared to robotic hands, we firstly conducted a 2 (imitative compatibility: congruent vs. incongruent) x 2 (hand condition: human vs. robotic) repeated measures ANOVA for the latencies. We found a main effect for imitative compatibility (i.e., congruency effect), $F(1, 103) = 146.08$, $p < .001$, $\eta_p^2 = .59$, indicating that participants responded faster in congruent trials ($M = 710.63$ ms, $SD = 275.30$) than in incongruent trials ($M = 750.32$ ms, $SD = 280.13$). Likewise, the main effect for hand condition was significant, $F(1, 103) = 7.42$, $p = .008$, $\eta_p^2 = .07$, which means that participants responded faster in robotic hand trials ($M = 728.35$ ms, $SD = 275.80$) than in human hand trials ($M = 732.60$ ms, $SD = 323.55$). More importantly, the interaction was significant as well: This means that the congruency effect was stronger for human hands ($M = 45.14$, $SD = 37.32$) than robotic hands ($M = 34.26$, $SD = 40.42$), $F(1, 103) = 7.87$, $p = .006$, $\eta_p^2 = .07$ (see Figure 2.11).

Figure 2.11

The Difference in the Trial Conditions for the Latencies



Note. Latencies *(+/- SE).*

Error Rates. We ran the same analyses for the error rates as well. We found again the main effect for congruency, $F(1, 103) = 48.19$, $p < .001$, $\eta_p^2 = .32$, meaning that participants made fewer errors in congruent trials ($M = 1.13$ %, $SD = 1.15$) than in incongruent trials ($M = 2.34$ %, $SD = 1.66$). We did not find a main effect for the hand condition, $F(1, 103) = 0.87$, $p = .353$, $\eta_p^2 = .008$. The error rate between robotic hand trials ($M = 1.68$ %, $SD = 1.21$) and human hand trials ($M = 1.79$ %, $SD = 1.30$) did not differ significantly this time. The interaction between the congruency and the hand condition was also not significant, $F(1, 103) = 1.88$, $p = .173$, $\eta_p^2 = .02$, which means that the congruency effect did not vary between human ($M = 1.03$, $SD = 2.15$) or robotic hands ($M = 1.38$, $SD = 2.25$) within the error rates.

2.5.3 Discussion

Using the online version of the imitation-inhibition task, Experiment 4 aimed at replicating a social modulation of automatic imitation, which has been often found in laboratory experiments. Similar to typical effects found in the laboratory (e.g., Cracco, Bardi, et al., 2018), individuals imitated human hands more strongly than robotic hands when analyzing the latencies. For the error rates, we found the typical congruency effect, but no significant influence of the robotic versus human hand. In this respect, it is important to note that the error rate is often not sensitive to the modulation of imitative behavior (Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017). Besides, due to the exclusions, we did not reach the amount of participants that we planned, which might have also contributed to not finding a possible small effect. Thus, one can conclude that the online version of the imitation-inhibition task mirrors the results of typical laboratory experiments investigating the modulation of automatic imitation.

2.6 General Discussion

People automatically imitate others’ behaviors (Cracco, Bardi, et al., 2018; Heyes, 2011). The most-often used task to measure automatic imitation tendencies is the imitation-inhibition task, a task typically used in costly and time-consuming laboratory settings. In this article we present an online version of the imitation-inhibition task, which can be implemented using online survey software (e.g., Qualtrics). In four experiments, we validated the online version of the imitation-inhibition task. The results show that the task works efficiently (Experiment 1) and achieves similar results in terms of effect size and reliability as compared to laboratory settings (Experiment 2). Moreover, the online version of the imitation-inhibition task taps similarly into social processes as its laboratory equivalent since it detects automatic imitation reliably even when controlling for spatial compatibility (Experiment 3), and is modulated by animacy in the sense that individuals imitate robotic finger movements less strongly than human finger movements (Experiment 4).

To implement the online version of the imitation-inhibition task we provide two solutions. First, we provide on the OSF programmed versions that run by themselves on any server. Second, we provide a solution that allows running the imitation-inhibition task in the online survey tool Qualtrics. Although common survey software is not intentionally designed to perform reaction time tasks such as the imitation-inhibition task, we have shown that it can be used very easily when implementing the additional codes. On OSF, we provide all the codes necessary to run the task in Qualtrics and on a server along with detailed tutorials and templates. Additionally, we present analyses scripts to preprocess and analyze the data in R.

2.6.1 Practical Advantages of the Online Version of the Imitation-Inhibition Task

The advantages of the online version of the imitation-inhibition task are manifold. First, the imitation-inhibition task with the survey software solution we provide her can be very easily and conveniently implemented and adjusted by researchers with limited programming knowledge. Thereby, the task can also be used for teaching purposes and in student research projects.

Second, when using traditional programming software solutions that allow running reaction time based experiments online, participants sometimes need to install or download a file or software or redirection to external platforms might be necessary. As many participants are hesitant to download such files, the range of participants that take part in these kind of experiments is limited. This does not apply to the programmed solutions we provide here, as participants do not need to download any file, nor are they redirected to external platforms ensuring that no data is getting lost.

Third, the task works equally well for all computers (either laptops or stationary PCs) keyboards, laptop vs. external monitors, and browsers, as we found no difference in automatic imitation with respect to the participants’ equipment in neither the first nor the second experiment.

2.6.2 Broad Scope of the Online Version of the Imitation-Inhibition Task

Besides the practical advantages, the online imitation-inhibition task offers the potential to increase the use of the imitation-inhibition task in several ways. Thereby, the scope of research questions is broadened. First, as noted earlier, imitation studies are typically conducted with small, in-person samples in the laboratory (e.g., Liepelt & Brass, 2010a). However, such small samples are less informative, lead to inaccurate parameter estimates, and have been criticized as a factor leading to replicability issues (Brandt et al., 2014; Szucs & Ioannidis, 2017). The online imitation-inhibition task offers the possibility to conduct high-powered experiments without much effort. This may be especially useful for research questions for which small effects are expected and thus large samples are needed. For example, the online imitation-inhibition task can help resolving the debate about the degree to which social factors affect automatic imitation. Although there are a several studies showing the influence of social variables on automatic imitation (e.g., Bird, Leighton, et al., 2007; Cracco et al., 2015; Hogeveen & Obhi, 2013; Leighton et al., 2010; Liepelt & Brass, 2010a; Rauchbauer et al., 2016), other studies have had trouble replicating these findings (e.g., Butler et al., 2015; Galang & Obhi, 2020; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019). A reasonable assumption for the mixed results is that the to be expected effect size for these findings is most likely small. As the online imitation-inhibition task allows testing large samples in an effortless manner, it is well suited for investigating research questions for which a small effect size is expected and thereby shed light on some unsolved debates.

Second and related to the first point, the imitation-inhibition task presented here is well suited for conducting high-powered replications of previous experiments. Replications sometimes require more participants than have been actually included in the original articles, if the original study was not powered sufficiently (Brandt et al., 2014; Simonsohn, 2015). The collection of such large samples is possible and simplified by the online version auf the task.

Third, the online version of the imitation-inhibition task offers new avenues for a diverse set of research questions that cannot be answered in the laboratory. For example, the question which cultures imitate more or less can only be tested by measuring imitation in many different countries. Such an approach is difficult to implement in laboratory settings, but rather easily achieved when applying the online task.

Fourth, psychological research has been frequently criticized in recent times for collecting WEIRD samples (i.e., participants from western, educated, industrialized, rich, and democratic nations), especially the United States (Cheon et al., 2020; Muthukrishna et al., 2020; Rad et al., 2018). Thus, psychological data does not really represent the entire world population (Muthukrishna et al., 2020). With an online version of the imitation-inhibition task, it becomes now easier to collect representative samples from different parts of the world via panel providers, thus avoiding the use of typical college student samples.

2.6.3 Limitations

Despite its advantages, the online imitation-inhibition task may have some limitations that we would like to mention here. First of all, it should be noted that there may also be situations in which an in-person imitation-inhibition task in the laboratory is desirable. For example, researchers may wish to involve other observed behaviors that cannot be measured online or may desire greater control over the environment (e.g., the location of the experiment, correct handling of the experimental material, possibility to ask questions in case of ambiguity, more complicated manipulations). Nevertheless, researchers who wish to perform the imitation-inhibition task in the laboratory could theoretically use the survey software online imitation-inhibition task or the server-based version in the laboratory as well.

Second, and related to the first point is that the online task can only be used to measure finger lifting movements. Previous research sometimes measured imitation of other movements, such as hand opening and closing motions (e.g., Press et al., 2006) or finger lowering instead of finger lifting (e.g., Boyer et al., 2012). Given that our task can only detect key releases made on a computer keyboard, the range of movements is limited to actions that involve key releases (or with adjustment of the JavaScript also key presses). Any other movement behavior should be measured in the laboratory or with a different task. However, as most researchers use finger lifting movements to study automatic imitation, our task is nevertheless adaptable for most research questions on automatic imitation.

Third, from the comparison of the samples of Experiment 2 and especially the sample of Experiment 4, we can derive one primary caution: Although the imitation-inhibition task itself produces good and comparable results for both students in the lab sample and crowdsource participants (e.g., MTurk) online, a somewhat larger proportion of MTurk participants took the imitation-inhibition task less seriously. That is, the exclusion rate was considerably higher with the same exclusion criteria for the online sample than for the laboratory (i.e., highest for the fourth experiment), due to the fact that some participants made more intentional errors online (e.g., not raising their finger at all), which is why they do not achieve comparable trial numbers in the different trial conditions. As lower quality data compared to laboratory research is a known problem in online research (De Man et al., 2021; Gosling et al., 2004; Gosling & Mason, 2015), we advise to pre-register the exclusion criteria in experiments with the online imitation-inhibition task and to take into account a likely higher exclusion rate when calculating the sample, as it is common practice in other online experiments. At least, when applying the exclusion criteria we used here, the results detected with the imitation-inhibition task are similar to those of its laboratory equivalent. In addition, it might also make sense to think about alternative online panel providers than MTurk, since MTurk is known to collect lower quality data compared to other providers (e.g., Chmielewski & Kucker, 2020; Woo et al., 2015). Furthermore, we recommend implementing the same strict practice phases we used in our experiments (i.e., participants cannot start with the actual experimental phase if they have not successfully completed the minimum number of trials in both practice phases) to make sure that even participants who did not read the instructions carefully will understand the task.

Fifth, participants can only take part on computers with keyboards; participants who prefer tablets or mobile phones cannot participate in the online version of the imitation-inhibition task. Nevertheless, one does not have to consider this as a limitation, since this is a key feature of the task and it even might not be considered as automatic imitation when a tablet or a mobile phone would be used to conduct an imitation-inhibition task, neither in the laboratory nor online.

A final limitation is that our provided codes currently work without adaptation only when running experiments on Qualtrics or a personal server. Nevertheless, based on the codes and tools we provide on OSF, we are confident that an implementation within other online survey platforms or freely available server providers (e.g. mindprobe, https://mindprobe.eu/ or cognition.run, https://www.cognition.run/) is feasible with only little adjustments.

2.6.4 Conclusion

The validation of the online imitation-inhibition task allows researchers across the globe to investigate imitative behavior online without much effort. This moves research away from the same samples collected on campus and allows testing a wide range of research questions in a variety of different samples and cultures. The online imitation-inhibition task is a reliable and valid method for conducting imitation research that saves costs, enables recruitment of large online samples, and simplifies data management. Compared to laboratory samples, the effects obtained with online measurements are similar in terms of size and reliability. Thus, the online imitation-inhibition task is a well-performing alternative to its laboratory version. With the use of the materials we have provided here, the implementation of the online procedure will be simplified to facilitate high quality imitation research in the future.

2.7 Acknowledgment

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2.8 Declarations

Data Availability. The datasets generated and analyzed during the current study are available in the OSF repository “Online Imitation-Inhibition Task” [https://osf.io/q7fju/].

Code Availability. The codes generated during the current study are available in the OSF repository “Online Imitation-Inhibition Task” [https://osf.io/q7fju/].

Conflicts of Interest. The authors have no competing interests to declare that are relevant to the content of this article.

Funding. The research leading to these results received funding from the German Research Foundation (DFG) under Grant Agreement No. GE 3040/2-1 & GE 3040/6-1.

Ethics Approval. The experiments are typical reaction time studies with a duration of less than 20 minutes. Because subjects were fully informed of their rights, the personal data collected (demographics), and the possibility of publishing the data in anonymized, averaged form, using a Participant Information and Informed Consent Form validated by the ethics committee and data protection office of the university where the data were collected, no separate ethics approval had to be obtained for this type of reaction time study. All procedures performed in studies involving human participants were in accordance with the 1964 Helsinki Declaration and its later amendments or comparable ethical standards.

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

Consent for Publication. Informed consent was obtained from all subjects participating in the study that personal data (demographics) and reaction time data may be published in anonymized averaged form, and that data such as reaction time data, age, gender, handedness, and any additional data collected in the experiments regarding keyboard and screen, etc. may be published anonymously as a data sheet. The data were never traceable to an individual.

Authors' Contributions. All authors developed the study concept. All authors contributed to the study design. M. Westfal drafted the manuscript, and E. Cracco, J. Crusius, and O. Genschow provided critical revisions. All authors approved the final version of the manuscript for submission.

2.9 Open Practices Statements

The data and materials for all four experiments are available at OSF (https://osf.io/q7fju/), with the exception of some of the images used in Experiment 4, which we were not allowed to upload due to copyright reasons. Experiment 1 (https://aspredicted.org/blind.php?x=/APD_RJG), Experiment 2 (https://aspredicted.org/blind.php?x=/XCC_BBJ), and Experiment 3 (https://aspredicted.org/blind.php?x=6qx27q) were pre-registered.

Chapter 3

Movement Size Influences Automatic Imitation

Chapter 2.5 showed that modulations with the online imitation-inhibition task are possible. However, there have been problems with replicating some modulation effects using the imitation-inhibition task in the laboratory (see Chapter 1.5.2 and Chapter 1.5.3). Now that I have been able to show that the online version of the task generates similar results to its laboratory counterpart, I examine different modulations with the online task.

In this chapter, I investigate a basic modulation with the task: finger movement size. Indeed, in many studies on automatic imitation and the animacy effect, the researchers did not control for the size of the finger movement (e.g., Klapper et al., 2014; Press et al., 2006). Thus, the study in this chapter investigates whether (a) the size of the finger movement, (b) the speed of the finger movement, (c) the two together, have an influence on automatic imitation, and (d) whether the confound of the size of the finger movement in the animacy stimuli is responsible for the animacy effect.

This chapter is based on the following manuscript:

Westfal, M., Lemmens, V. N., & Genschow, O. (2023). *Movement size influences automatic imitation*. [Manuscript submitted for publication]. Institute for Management & Organization (IMO), Leuphana University Lueneburg.

Please note that some changes in headings, citation style, and formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the article.

Abstract

Individuals automatically imitate each other. With reference to the so-called animacy effect, a common finding in the literature is that individuals imitate human hands more strongly than non-human hands. In this article, we show that most of the hand stimuli used in previous research confounded animacy with the size of finger movements meaning that the movements of human fingers were larger than those of non-human hands. This raises the question of whether differences in the size of finger movements influence automatic imitation and thereby account for the animacy effect. To answer this question, we carried out four experiments (total N = 540). Experiment 1 shows that large movements elicit stronger automatic imitation tendencies than small movements. As the size of a movement is usually confounded with the speed of a movement, we conducted two additional experiments. These experiments demonstrated that movement size influences automatic imitation independent of movement speed. Experiment 4 demonstrates that even when controlling for movement size, the animacy effect can still be detected and remains strong. Theoretical implications are discussed.

Keywords. Automatic imitation, imitation-inhibition task, online research, finger movement size, animacy effect

3.1 Introduction

Humans have the automatic tendency to imitate a variety of different behaviors including, facial expressions (Dimberg, 1982), simple movements (Brass et al., 2000; Genschow et al., 2012; Genschow & Florack, 2014; Genschow & Schindler, 2016), emotions (Dimberg, 1982; Hess & Fischer, 2013), and gestures (Bernieri, 1988; Cracco, Genschow, et al., 2018). Such imitative behavior serves important social purposes as it bonds humans more strongly together by fostering feelings of affiliation (Duffy & Chartrand, 2015a). With respect to the so-called animacy effect, previous research has shown that people have the tendency to imitate human agents more strongly than non-human agents (e.g., Bird, Leighton, et al., 2007; Haffey et al., 2013; Press et al., 2005, 2006). While the phenomenon of imitation is well documented in the psychological literature, its underlying processes are still a matter of investigation. Here, we test how differences in motion profiles of observed movements influence automatic imitation. Specifically, we test whether the size of a movement affects automatic imitation and thereby may account for the animacy effect.

3.1.1 Measures of Automatic Imitation

The most often used task to measure individuals’ automatic tendency to imitate others is the imitation-inhibition task (Brass et al., 2000; for a meta-analysis, see Cracco, Bardi, et al., 2018). Using this task allows assessing automatic imitation on a trial-by-trial basis by producing reliable (Genschow, van Den Bossche, et al., 2017) and strong effects (Cracco, Bardi, et al., 2018). In the imitation-inhibition task, participants respond to imperative cues presented on the computer screen by executing specific finger movements over multiple trials. Typically, they lift their index finger in response to the number "1" and their middle finger in response to the number "2." In the same time as the number cue appears on the screen, participants see another person's hand lifting either the same finger (congruent trial) or the other finger (incongruent trial). The typical finding observed in studies utilizing the imitation-inhibition task is the congruency effect meaning that participants respond faster and with fewer errors in congruent, as compared to incongruent trials (for a meta-analysis, see Cracco, Bardi, et al., 2018). It is widely agreed that the imitation-inhibition task is a measure of automatic imitation in the sense that the congruency effect reflects the facilitation of executing an observed movement and the suppression of executing a conflicting movement (Cracco, Bardi, et al., 2018; Cracco & Brass, 2019; Heyes, 2011).

In principle, three different components could contribute to the congruency effect: Movement compatibility (e.g., observing a lifting movement while executing a lifting movement), effector compatibility (e.g., observing the index finger while moving the index finger), and spatial compatibility (e.g., observing an effector or a movement on the left side of space while executing a movement on the left side of space). Past research on automatic imitation (e.g., Catmur & Heyes, 2011; Cracco, Bardi, et al., 2018; Heyes, 2011) demonstrated that movement and effector compatibility both contribute to the congruency effect. However, spatial compatibility does not contribute to automatic imitation, as effector and movement compatibility effects are detected even if movements are presented spatially incompatible (for a meta-analysis, see Cracco, Bardi, et al., 2018).

3.1.2 Processes Underlying Automatic Imitation

Automatic imitation is commonly explained by a shared representation of observed and executed actions. For example, ideomotor theory (Greenwald, 1970; Prinz, 1990, 1997) posits that the visual image of a perceived action is part of its own motor representation. As a consequence, the mere observation of a specific action is sufficient to prime the execution of the same action. The idea that observing an action elicits a similar mental representation as its execution has been confirmed in many different neurophysiological studies applying different methods such as fMRI (e.g., Gazzola & Keysers, 2009; Keysers & Gazzola, 2010), motor TMS (e.g., Catmur et al., 2007; Fadiga et al., 1995), and single-cell recordings (Mukamel et al., 2010).

A claim often put-forward in the literature is that imitation as a social process should be influenced by social factors. Two different theories can explain such social modulation. Motivational theories (e.g., Chartrand & Dalton, 2009; Wang & Hamilton, 2012) argue that individuals engage in imitation either consciously or unconsciously in order to affiliate with others. As a consequence, individuals are expected to imitate others more strongly when they have an affiliation goal. Self-other overlap theories (e.g., Brass & Heyes, 2005; Greenwald, 1970; Heyes, 2010; Prinz, 1990, 1997) postulate that imitative tendencies are learned responses that emerge as a consequence of self-observation and interaction with other, often similar individuals (Brass & Heyes, 2005; R. Cook et al., 2014; Heyes, 2010; Ray & Heyes, 2011). Hence, when other people are perceived as more similar, individuals imitate more strongly (Genschow et al., 2021).

Interestingly, research testing theories of social modulation produced rather mixed results. On the one hand, research found that participants imitate stronger when they observe an in-group member as compared to an out-group member (Genschow & Schindler, 2016; Rauchbauer et al., 2016), when they are in a pro-social as compared to an anti-social mindset (e.g., Butler et al., 2016; Cracco, Genschow, et al., 2018; Leighton et al., 2010), or when they focus on others as compared to the self (for a meta-analysis, see Genschow, Schuler, et al., 2019; Spengler, Brass, et al., 2010)—to name just a few examples. On the other hand, more recent studies documented difficulties in replicating some of this research (e.g., Butler et al., 2015; Cracco, Bardi, et al., 2018; De Souter et al., 2021; Galang & Obhi, 2020; Genschow, Pauels, et al., 2023; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019).

While some findings are difficult to replicate, there is extensive evidence for the animacy effect indicating that observing movements of human agents elicit stronger automatic imitation tendencies than movements executed by non-human agents (e.g., Bird, Leighton, et al., 2007; Chaminade & Cheng, 2009; Klapper et al., 2014; Liepelt & Brass, 2010b; Press, 2011; Press et al., 2006). In such research, non-human stimuli consist of wooden hands (Liepelt & Brass, 2010b), but most most-often of robotic hands (e.g., Press et al., 2006). Non-human hand stimuli are usually contrasted with pictures of actual human hands (Press et al., 2007) or artificial hands that look human-like (Klapper et al., 2014). Interestingly, a close look at these stimuli reveals that the used images oftentimes do not control for the size of the finger movement (e.g., Bird, Leighton, et al., 2007; Cracco et al., 2015; Haffey et al., 2013; Jansson et al., 2007; Klapper et al., 2014; Press et al., 2005, 2006, 2007). For example, for the stimuli used by Klapper et al. (2014), the size of the finger movements is larger for human, as compared to non-human actions. When looking at different hand stimuli used by other authors (e.g., Bird, Leighton, et al., 2007; Cracco et al., 2015; Press et al., 2005, 2006, 2007), the same movement confound can be detected. This raises the question whether the size of a finger movement influences automatic imitation and whether this would explain previous findings on the animacy effect.

3.1.3 How Are Movements Different in Size Processed?

A prerequisite for imitation to occur is that individuals need to guide their attention towards another person's movement (Bek et al., 2016; Cracco, Bardi, et al., 2018; Heyes, 2011; Longo & Bertenthal, 2009). In line with this notion, neurophysiological investigations reveal that activations in the mirror neuron system arise the more attention is directed at another person's movement (Fadiga et al., 2005; Keysers & Gazzola, 2010; Kilner et al., 2007). Factors contributing to a more attentive perception of a movement might be the size and the speed of the movement.

With respect to the size of an movement, research has repeatedly shown that large movements are more salient than small ones (Franconeri & Simons, 2003; Krüger et al., 2016; Taylor & Fiske, 1978; Wixson, 2000) and are thus more likely attended and perceived (Burnham, 2020; Gaspelin & Luck, 2018; Kerzel & Schönhammer, 2013). Moreover, the more attention is given to a movement, the easier our brain can process and represent the movement in the motor cortex (Wertheim, 1981). This is true for movements of the body, but also for movements of smaller limbs like the fingers, as all movements and its muscles are processed and represented in the motor cortex (Kakei et al., 1999; Omrani et al., 2017). For example, Bourdin et al. (2019) showed that observing large arm movements activates individuals' motor representation in the motor cortex more strongly than observing small movements. Based on this research and ideomotor theory that puts forward that an increased activation of a movement's motor representation accounts for imitation, we predicted that large movements elicit stronger automatic imitation effects than small movements.

3.1.4 Present Research

To test whether the size of finger movements influence automatic imitation, we carried out four experiments using the imitation-inhibition task. In a typical imitation-inhibition task, participants first see a picture of a hand in resting position before another picture of the same hand with a lifted finger is shown (for a review see, Cracco, Bardi, et al., 2018). How high the finger in the second picture is lifted, determines how large the finger movement will be. That is, the higher the finger is lifted, the larger is the resulting movement. In Experiment 1, we manipulated the height of the raised fingers to investigate whether large, as compared to small movements increase automatic imitation.

Since in a typical imitation-inhibition task, researchers show only two pictures shortly after each other, the size of the movement is confounded with speed. That is, large movements are faster than short movements, because large movements cover a longer distance in the same time as short movements. This raises the question whether the size of an observed movement influences automatic imitation even when controlling for its speed. Experiment 2 tested whether fast movements increase automatic imitation as compared to slow movements. Experiment 3 manipulated the speed and the size of movements independently from each other to shed light onto the question whether the size of observed movements influence automatic imitation independently of its speed.

Finally, Experiment 4 examined whether the animacy effect can still be detected when we control for the movement’s size.

All experimental data and all materials can be accessed on the OSF (https://osf.io/cqkm4/?view_only=149dd0a648cc4b0082a7576b76db3922/).

3.2 Experiment 1

In Experiment 1, we tested whether large, as compared to small movements influence automatic imitation in the imitation-inhibition task. We predicted that a large finger movement elicits a stronger congruency effect than a small finger movement.

3.2.1 Method

Participants and Design. We aimed to detect a medium to small effect size of d_z = 0.35. With a power of β = 0.90, 72 participants are needed to detect such an effect in a within-subjects design. To compensate for potential dropouts, we assessed a sample of 100 participants. We recruited all participants online via Amazon's Mechanical Turk in return for a compensation of $1.00. We only invited MTurkers located in the US with a Hit Approval Rate over 85 %. Participants could only take part in the experiment with a laptop or computer (otherwise we would not have been able to record reaction times). By applying the exclusion criteria put forward by Westfal et al. (2021), we excluded data from a total of 27 participants, because they had less than 26 trials (i.e., less than two third of all trials in one condition) in one or more of the conditions (*n* = 18), or indicated that they did not use their right hand during the experimental blocks (*n* = 12). Three participants met both criteria. Two participants had technical problems, thus their datasets were submitted empty. The final sample consisted of 73 participants (32 female, 38 male, 1 prefer to self-describe, 2 no answer) with an age ranging from 23 to 66 (*M* = 37.88, *SD* = 11.15). Six participants were left-handed and 67 participants were right-handed.

The design of the experiment consisted of a 2 (trial condition: congruent vs. incongruent) x 2 (movement size: small vs. large) within-subjects design.

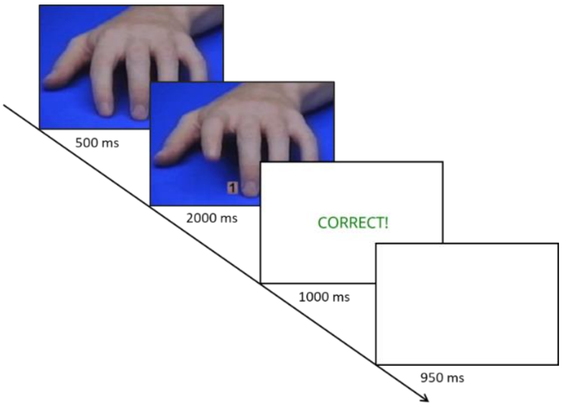
Procedure and Materials. To initiate the task, participants were required to click a button located at the center of the screen, which enabled full-screen mode. Subsequently, participants read general instructions regarding the experiment and the imitation-inhibition task. We emphasized that participants should respond using only their right index finger and middle finger. To facilitate understanding, we presented participants with a gif demonstrating the correct execution of the imitation-inhibition task.

After the general instructions, participants engaged in two practice phases. Both practice phases and the subsequent experimental phases involved fully randomized trials. In the first practice phase, participants were instructed to press and hold the "g" key with their right index finger and the "h" key with their right middle finger. They were required to respond as quickly as possible to the number cue "1" by lifting their index finger and to the number cue "2" by lifting their middle finger. Each trial was structured in the following way: participants were firstly presented with a fixation cross for 500 ms, followed immediately by the number cue "1" or "2". Subsequently, participants had a response window of 2000 ms in which they had to lift their right index or middle finger. Upon lifting a finger, participants instantly received feedback, displayed as either "CORRECT" in green letters or "WRONG" in red letters. If participants did not to lift any finger, they received a reminder to lift a finger. The feedback remained visible for 1000 ms, followed by a post-trial gap of 950 ms before the next trial began. After completing 10 trials, participants received accuracy feedback. If a participant made more than two errors, they had to repeat the practice phase until they achieved the minimum of eight correct trials.

During the second practice phase participants responded to images displaying another person's hand in a mirrored position (i.e., a left hand) in addition to the numbers "1" and "2". These pictures depicted the model lifting either the index or middle finger. Participants were instructed to respond by lifting their fingers in accordance with the number cue as in the first practice phase. The second practice phase comprised a total of 12 trials. To ensure participants understand the task requirements, they had to repeat this practice block until they made fewer than 4 errors. Figure 3.1 illustrates the precise presentation timings and trial structure employed in this second practice phase.

Figure 3.1

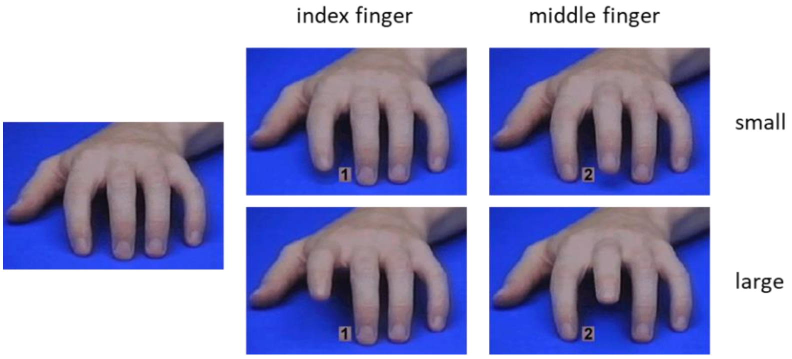
Schematic Representation of a Congruent Practice Trial



Following the practice phase, participants ran through four experimental blocks without receiving any feedback. Each block consisted of 40 trials presented in random order. Participants were allowed to take self-paced breaks between the experimental blocks. The participants saw the same hand in the background as in the second practice phase. However, in contrast to the second practice phase, we manipulated the size of the finger lifting movement. That is, we randomized on a trial-by-trials basis whether the size of finger movement was large or small. The small finger movement was 16 px and the large finger movement comprised 56 px for both the index and the middle finger. Thus, the large movements were 3.5 times larger than the small movement. Figure 3.2 depicts the difference in the finger lifting movements. Overall, the experiment consisted of 160 trials, evenly distributed across four within-participants trial types: 40 large movement-incongruent, 40 large movement-congruent, 40 small movement-incongruent, and 40 small movement-congruent trials.

Figure 3.2

Stimuli that Were Used in Experiment 1



To prepare the data for analysis, we removed extremely fast and slow reaction times in line with previous research (e.g., Catmur & Heyes, 2011; Cracco et al., 2015; Cracco, Genschow, et al., 2018; Genschow, Pauels, et al., 2023; Genschow, van Den Bossche, et al., 2017; Westfal, Cracco, et al., 2022; Westfal et al., 2021). That is, we removed trials with reaction times below 100 ms (1.91%) and latencies below (1.42%) or above (2.58%) 3 *SD*s of a participant's mean. For the analyses of the latencies we removed erroneous trials as well (14.66%).

At the end, participants indicated demographical data (i.e., gender and age), which hand they had used during the experimental blocks (left hand, right hand, or both hands) and their handedness. The stimuli together with the data and further material can be found on the Open Science Framework (https://osf.io/cqkm4/?view_only=149dd0a648cc4b0082a7576b76db3922/).

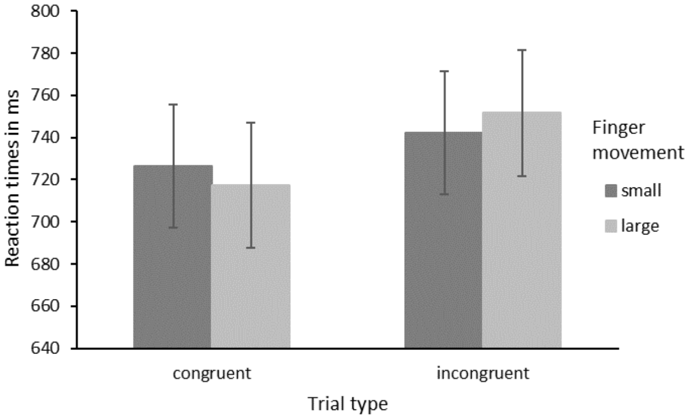
3.2.2 Results

Latencies. To test our hypotheses, we firstly conducted a 2 (trial condition: congruent vs. incongruent) x 2 (movement size: small vs. large) repeated measures ANOVA for the latencies. We found a main effect for trial condition, $F(1, 72) = 41.04$, $p < .001$, $\eta_p^2 = .36$, indicating that participants responded faster in congruent trials ($M = 721.84$ ms, $SD = 251.31$) than in incongruent trials ($M = 746.93$ ms, $SD = 251.69$). The main effect for movement size was not significant, $F(1, 72) = 0.002$, $p = .960$, $\eta_p^2 < .001$. However, more important for our hypothesis, the interaction between trial condition and movement size was significant, $F(1, 72) = 9.41$, $p = .003$, $\eta_p^2 = .12$, showing that the size of the finger movement influences the congruency effect.

To investigate the interaction in more detail, we conducted three follow-up *t*-tests. Firstly, we applied two *t*-tests for dependent samples to test for the presence of the typical imitation-inhibition effects within the small and large finger lifting movement. With respect to the small finger lifting movement, the results indicate that participants responded faster in congruent trials ($M = 726.36$ ms, $SD = 250.37$), than in incongruent trials ($M = 742.26$ ms, $SD = 249.48$), $t(72) = 3.48$, $p < .001$, $d_z = 0.41$, CI 95% [0.17, 0.64]. Also, with respect to the large finger lifting movement, participants responded faster in congruent trials ($M = 717.32$ ms, $SD = 253.41$) than in incongruent ones ($M = 751.59$ ms, $SD = 255.20$), $t(72) = 6.51$, $p < .001$, $d_z = 0.76$, CI 95% [0.50, 1.02]. Thus, in both small and large finger movement trials the congruency effect was present. Finally, a direct comparison of large and small finger movements in a one-tailed *t*-test showed a meaningful difference as participants showed a stronger congruency effect for large finger movements ($M = 34.27$ ms, $SD = 44.95$), as compared to the small finger movements (*M* = 15.91 ms, *SD* = 39.09), *t*(72) = 3.07, *p* = .002, d_z = 0.36, CI 95% [0.12, 0.60]. The reaction times are depicted in Figure 3.3.

Figure 3.3

The Latencies of Experiment 1



Note. Latencies (+/- SE). All tests are one-tailed.

Error Rates. In a second step, we ran the same analyses for the error rates. For the 2 (trial condition: congruent vs. incongruent) x 2 (movement size: large vs. small) repeated measures ANOVA we found a main effect for trial condition (i.e., overall congruency effect), *F*(1, 72) = 34.16, *p* < .001, η_p^2 = .32, with participants making fewer errors in congruent trials (*M* = 1.99 %, *SD* = 2.08) than in incongruent trials (*M* = 3.45 %, *SD* = 2.70). The main effect for movement size was not significant, *F*(1, 72) = 1.63, *p* = .206, η_p^2 < .02. The interaction between trial condition and movement size was significant, *F*(1, 72) = 7.75, *p* = .007, η_p^2 = .10, showing, that movement size influences the congruency effect of error rates as well.

Follow-up tests detected in line with the latencies a significant congruency effect in small finger movement trials, with participants making fewer errors in congruent trials (*M* = 2.14 %, *SD* = 2.45) than in incongruent trials (*M* = 3.06 %, *SD* = 2.85), *t*(72) = 3.17,

p = .001, d_z = 0.37, CI 95% [0.13, 0.61]. Moreover, we found a congruency effect in large finger movement trials as well, with participants making fewer errors in congruent trials (M = 1.84 %, SD = 2.12), than in incongruent trials (M = 3.85 %, SD = 3.19), t(72) = 5.81, p < .001, d_z = .68, CI 95% = [0.42, 0.93]. A direct comparison of large and small finger movements showed a meaningful difference in a one-tailed t-test for error rates as participants showed a stronger congruency effect for large finger movements (M = 2.00 %, SD = 2.94), as compared to the small finger movements (M = 0.91 %, SD = 2.45), t(72) = 2.78, p = .003, d_z = 0.33, CI 95% [0.09, 0.56].

3.2.3 Discussion

In line with our predictions, Experiment 1 found stronger congruency effects for large, as compared to small finger movements. This indicates that the size of the observed finger movement influences automatic imitation. However, as participants in Experiment 1 saw only two pictures in succession (i.e., a base hand followed by a hand with a lifted finger), the large movement might have been perceived as faster as the small finger movement, because the finger traveled a longer distance in the same time. Thus, movement size was confounded with movement speed. To test whether finger movement speed influences automatic imitation as well as movement size, we conducted Experiment 2.

3.3 Experiment 2

To investigate whether the speed of a movement influences automatic imitation, we first calculated the speed with which the fingers moved in Experiment 1. Afterwards, we manipulated the speed of finger movements accordingly and tested whether fast movements elicit stronger congruency effects than slow movements. The study was pre-registered at AsPredicted (https://aspredicted.org/PDG_YCD).

3.3.1 Method

Participants and Design. We applied the same power calculation as for Experiment 1. To compensate for potential dropouts, we pre-registered a sample of 100 participants. We recruited the participants via Amazon’s Mechanical Turk in return for a compensation of $1.20. The Hit Approval Rate and the location was the same as in Experiment 1.

In line with our pre-registration (https://aspredicted.org/PDG_YCD), data from a total of 46 participants were excluded from data analysis because they had less than 27 valid trials in one or more of the conditions (*n* = 34), or did not use the right hand during the experimental phase (*n* = 19). Seven participants met both criteria. The final sample consisted of 54 participants (21 female, 33 male) with an age ranging from 25 to 68 (*M* = 37.41, *SD* = 11.64). Four participants were left-handed and 48 participants were right-handed. Two participants were both handed.

The design of Experiment 2 consisted of a 2 (trial condition: congruent vs. incongruent) x 2 (movement speed: slow vs. fast) within-subjects design.

Procedure and Materials. To manipulate movement speed, we created videos with different frame rates. In a first step, we calculated the speed for the different movements applied in Experiment 1. This analysis revealed that for the small movements, the fingers moved with a speed of 0.04 m/s, whereas they moved with 0.15 m/s for the large movement. In a second step, we created videos of finger movements that incorporated the two different speeds. With the help of blender (https://www.blender.org/), we created a 3D hand with movable fingers and photographed this hand in 17 picture frames of a movement up to the finger height of 87 px. The Microsoft Legacy Video Editor was then used to create the actual videos. To achieve the desired speed of the finger movements, we used a total of 17 picture frames for the slow finger movement and 6 picture frames for the fast finger movement. Each picture frame was shown for 21,5 ms for the fast movements and 3000 ms for the slow movement. The stimuli can be viewed on the OSF (https://osf.io/cqkm4/?view_only=149dd0a648cc4b0082a7576b76db3922/)

The procedure of Experiment 2 was the same as in Experiment 1, except that we presented participants with the video stimuli in the experimental blocks. In the experimental blocks, each video began with the hand in a resting position for 1 second, after which the movement was shown (either fast or slow). At the end of the video a still hand with the respective finger lifted and the imperative cues (i.e., number “1” or “2”) was shown for 2 seconds or until participants responded.

The experimental phase consisted of 160 trials presented in four blocks. We presented participants with 40 trials per condition (i.e., congruent slow finger movement, incongruent slow finger movement, congruent fast finger movement, and incongruent fast finger movement). Participants could take a self-paced break between the blocks.

To prepare the data for analysis, we pre-registered the same trial-based exclusion criteria as in the other experiments. That is, we discarded trials with reaction times below 100 ms (4.56%) and removed latencies below (0.14%) and above 3 *SD*s (1.24%) of the participant’s mean. For the analyses of the latencies, we removed erroneous trials (22.37 %).

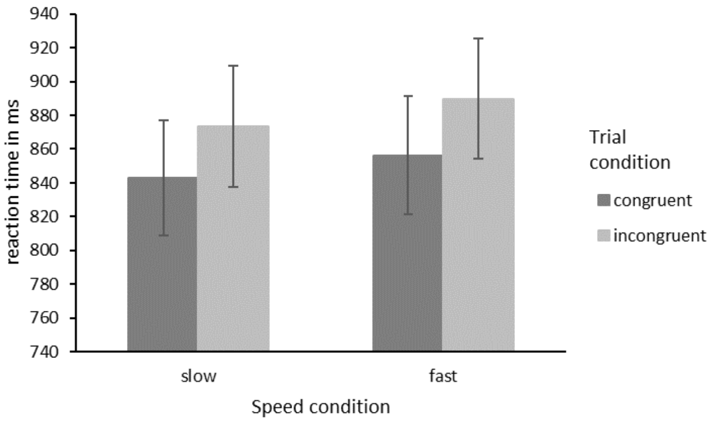
3.3.2 Results

Latencies. To test our hypotheses, we firstly conducted a pre-registered 2 (trial condition: congruent vs. incongruent) x 2 (movement speed: slow vs. fast) repeated measures ANOVA for the latencies. We found a main effect for trial condition, $F(1, 53) = 29.86$, $p < .001$, $\eta_p^2 = .36$ indicating that participants responded faster in congruent trials (M = 856.30 ms, SD = 266.00) than in incongruent trials (M = 884.99 ms, SD = 274.44). Also, the main effect for movement speed was significant, $F(1, 53) = 17.06$, $p < .001$, $\eta_p^2 = .24$. This means that participants responded faster in slow trials (M = 864.34 ms, SD = 269.67) than in fast trials (M = 876.95 ms, SD = 269.92). The interaction was not significant, $F(1, 53) = .19$, $p = .663$, $\eta_p^2 = .004$.

A pre-registered t-test for the comparison of the congruency effect between slow and fast movements revealed that the congruency effect for slow movements (M = 27.41 ms, SD = 46.52) did not differ significantly from the congruency effect of fast movements (M = 29.98 ms, SD = 41.80), $t(53) = 0.44$, $p = .663$, $d_z = 0.06$, CI 95% = [- 0.21, 0.33], indicating that automatic imitation is not influenced by the speed of the finger movement in the imitation-inhibition task (see Figure 3.4).

Figure 3.4

Reaction Times Trial Condition x Speed Condition



Note. Latencies (+/- SE).

Error Rates. The same pre-registered analyses on the error rates yielded similar effects. Again, we found the main effect for the overall congruency effect, $F(1, 53) = 24.79$, $p < .001$, $\eta_p^2 = .32$, but no main effect for movement speed, $F(1, 53) = 0.15$, $p = .697$, $\eta_p^2 = .003$. This means that participants made fewer errors in congruent trials ($M = 1.95$ %, $SD = 1.73$) than in incongruent trials ($M = 3.52$ %, $SD = 2.40$). But they did not differ in their error rates regarding slow trials ($M = 2.69$ %, $SD = 1.87$) and fast trials ($M = 2.78$ %, $SD = 2.00$). The interaction between trial condition and speed was also not significant, $F(1, 53) = 1.62$, $p = .209$, $\eta_p^2 = .03$. The pre-registered *t*-test for the comparison of the congruency effect between slow and fast movements of the error rates showed no difference between the congruency effect of the slow trials ($M = 1.26$ %, $SD = 2.66$) and fast trials ($M = 1.87$ %, $SD = 3.13$), $t(53) = 1.27$, $p = .209$, $d_z = 0.17$, CI 95% = [- 0.10, 0.44].

Robustness Check. Although we had to exclude a rather large number of participants, the results of the analyses remained the same when all participants are included in the analyses. For the latencies we still found the main effect for trial condition, $F(1, 99) = 6.21$, $p = .014$, $\eta_p^2 = .06$ and the main effect for movement speed, $F(1, 99) = 4.60$, $p = .035$, $\eta_p^2 = .04$. The interaction remained not significant, $F(1, 99) = 1.53$, $p = .220$, $\eta_p^2 = .02$.

For the error rates we still found the main effect for the overall congruency effect as well, $F(1, 99) = 6.75$, $p = .011$, $\eta_p^2 = .06$, and no main effect for movement speed, $F(1, 99) = 1.41$, $p = .238$, $\eta_p^2 = .01$. The interaction between trial condition and speed remained also not significant, $F(1, 99) = 3.21$, $p = .076$, $\eta_p^2 = .03$.

3.3.3 Discussion

Experiment 2 investigated whether the speed of finger lifting movements influences automatic imitation (i.e., the congruency effect). The results showed, that automatic imitation is not influenced by the speed we manipulated.

3.4 Experiment 3

While Experiment 1 shows that the size of an observed movement affects automatic imitation, Experiment 2 indicates that movement speed does not influence automatic imitation. However, a disadvantage of our previous experiments is that we assessed the speed and the size of the movements in separate experiments. Thus, theoretically, it remains open whether we would still find an effect of movement size when controlling for movement speed. In Experiment 3, we examined whether movement size influences automatic imitation independently of speed. The experiment was pre-registered at AsPredicted (https://aspredicted.org/4XL_7PP).

3.4.1 Methods

Participants. To be able to detect even possibly very small effects for a possible interaction between finger movement size and finger movement speed, we performed an a priori power calculation: For an effect size of $\eta^2 = 0.01$ with a power of $\beta = 0.90$, 178 participants are needed. Considering potential drop-outs, we recruited 200 participants via Amazon's Mechanical Turk in return for a compensation of \$2.50. As for the first two experiments, participants could only take part in the study with a laptop or computer and only MTurkers located in the US with a Hit Approval Rate over 85 % were invited to participate in the study. We applied nearly the same exclusion criteria as in the first two experiments, except that the needed trials per condition changed because we had less trials than in the first experiments to shorten the experiment to a comfortable length for the participants. In total, data from 70 participants were excluded from data analysis because they did not use the right hand during the experimental phase (*n* = 39), or had less than 20 valid trials in one or more of the conditions (*n* = 49). Eighteen participants met both criteria. For two participants data were submitted empty due to technical problems. The final sample consisted of 128 participants (58 female, 70 male) with an age ranging from 23 to 66 (*M* = 37.27, *SD* = 9.27). Seven participants were left-handed and 119 participants were right-handed. Two were both-handed.

Procedure and Materials. To create stimuli, we took the same 3D hand we used in Experiment 2 to create picture frames that end on the height of 16 px for the small finger movement and 56 px for the large finger movement (i.e. the same size as in Experiment 1). We used 10 picture frames for the large movement and 5 picture frames for the small movement. To manipulate the speed of the movements, we adjusted the duration of each picture frame so that the slow movements lasted 0.5 s and the fast movements 0.13 s. In total, we created videos of four different movements: a small and fast movement, a large and fast movement, a small and slow movement, and a large and slow movement. All stimuli are on the OSF (https://osf.io/cqkm4/?view_only=149dd0a648cc4b0082a7576b76db3922/).

The procedure of the imitation-inhibition task was similar to the one applied in Experiment 1 and 2 with a few exceptions. As in Experiment 2, each trial began with the hand shown in a resting position for 1 second. Afterwards, the movement was shown. At the end of the video, the video showed a still hand with the respective finger lifted and the imperative cue (i.e., number “1” or “2”) for 2 seconds or until participants responded.

The experimental phase consisted of 240 trials with 30 trials for each within-participants condition (i.e., congruent-fast-small movements, incongruent-fast-small movements, congruent-fast-large movements, incongruent-fast-large movements, congruent-slow-small movements, incongruent-slow-small movements, congruent-slow-large movements, incongruent-slow-large movements), within 5 experimental blocks. Participants could take a self-paced break between each block.

To prepare the data for analysis, we pre-registered the same trial-based exclusion criteria as in our previous experiments. That is, we removed latencies below (0.13 %) and above (1.28 %) 3 *SD*s of the participant's mean. We also discarded trials with reaction times below 100 ms (2.35 %). For the analyses of the latencies, we removed erroneous trials as well (15.17 %).

3.4.2 Results

Latencies. Firstly, to test whether the congruency effect is present within each finger movement condition (i.e., within the movement speed trial conditions and the movement size trial conditions)[3], we carried out four *t*-tests. We found a congruency effect for fast movements, with faster responses for congruent (M = 881.57 ms, SD = 267.04) compared to incongruent trials, (M = 899.62 ms, SD = 259.96), $t(127) = 5.36$, $p < .001$, $d_z = 0.47$, CI 95% = [0.29, 0.66]. Also, the congruency effect for slow movements was significant, with faster responses for congruent (M = 874.98 ms, SD = 269.63) as compared to incongruent trials, (M = 887.31 ms, SD = 262.34), $t(127) = 3.44$, $p < .001$, $d_z = 0.30$, CI 95% = [0.13, 0.48]. Moreover, the congruency effect for large movements was also significant, with faster

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[3] Please note: We accidentally pre-registered only two *t*-tests, although we were referring to four *t*-tests, as can be also inferred from the wording.

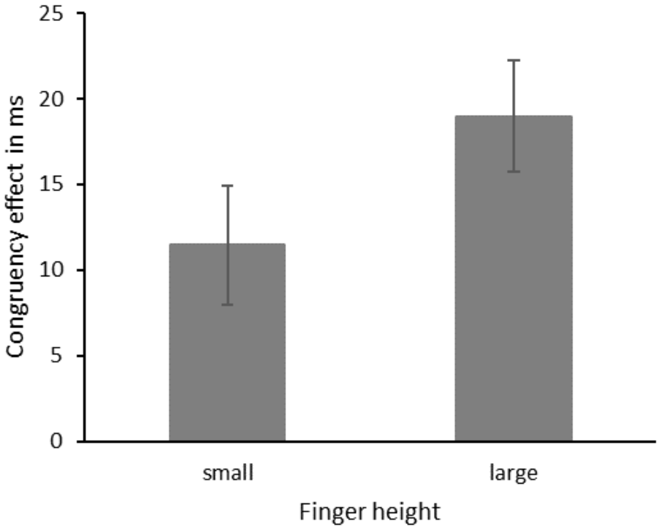
responses for congruent (*M* = 877.61 ms, *SD* = 269.94) as for incongruent trials, (*M* = 896.58 ms, *SD* = 261.72), *t*(127) = 5.81, *p* < .001, d_z = 0.51, CI 95% = [0.33, 7051]. Finally, the congruency effect for small movements was significant, with participants responding faster in congruent (*M* = 878.99 ms, *SD* = 266.78), compared to incongruent trials (*M* = 890.46 ms, *SD* = 260.58), *t*(127) = 3.31, *p* < .001, d_z = 0.29, CI 95% = [0.12, 0.47].

Next, we conducted a pre-registered 2 (trial condition: congruent vs. incongruent) x 2 (movement speed: slow vs. fast) x 2 (movement size: small vs. large) repeated measures ANOVA for the latencies. We found a main effect for trial condition (i.e., overall congruency effect), *F*(1, 127) = 27.48, *p* < .001, η_p^2 = .18, indicating that participants responded faster in congruent trials (*M* = 878.29 ms, *SD* = 267.74) than in incongruent trials (*M* = 893.40 ms, *SD* = 260.54). The main effect for movement size was not significant, *F*(1, 127) = 1.26, *p* = .263, η_p^2 = .01, which means that participants response did not differ between small finger trials (*M* = 884.65 ms, *SD* = 262.85) and large finger trials (*M* = 887.03 ms, *SD* = 265.00). The main effect of movement speed was significant, *F*(1, 127) = 21.23, *p* < .001, η_p^2 = .14, indicating that participants responded faster in slow movement trials (*M* = 880.94 ms, *SD* = 264.89) than in fast movement trials (*M* = 890.74 ms, *SD* = 262.96). More important for our hypothesis was the significant interaction between movement size and congruency, *F*(1, 127) = 4.52, *p* = .035, η_p^2 = .03. This indicates that the congruency effect was influenced by the size of the movement. The interaction between finger movement speed and congruency was not significant, *F*(1, 127) = 2.20, *p* = .141, η_p^2 = .02, neither was the interaction between movement size and movement speed, *F*(1, 127) = 0.16, *p* = .693, η_p^2 = .001, nor the three-way interaction, *F*(1, 127) = 3.07, *p* = .082, η_p^2 = .02.

Lastly, we pre-registered a direct comparison between the congruency effects of the finger movement conditions (i.e., fast vs. slow and small vs. large). As the significant interaction between movement size and congruency already indicated, the only significant one-tailed *t*-test resulted for the congruency effect between the large and small movements, with a stronger congruency effect for large finger movements (*M* = 18.98 ms, *SD* = 36.95), compared to small finger movements (*M* = 11.47 ms, *SD* = 39.20), *t*(127) = 2.18, *p* = .016, d_z = 0.19, CI 95% = [0.02, 0.37] (see Figure 3.5). The two-tailed *t*-test for the movement speed condition revealed no significant difference between the congruency effect for slow (*M* = 12.33 ms, *SD* = 40.52), compared to fast movements (*M* = 18.05 ms, *SD* = 38.12), *t*(127) = 1.47, *p* = .143, d_z = 0.13, CI 95% = [- 0.04, 0.30].

Figure 3.5

The Difference in the Congruency Effect for Small and Large Finger Trials



Note. Latencies *(+/- SE).* Test is one-tailed.

Error Rates. We ran the same analyses for the error rates as well. Firstly, to test whether the congruency effect is present within each finger movement condition (i.e., within movement speed condition and movement size condition), we carried out four *t*-tests for the error rates as well. We found a congruency effect in the fast speed condition, with participants making fewer errors in congruent (*M* = 1.85 %, *SD* = 2.08) compared to incongruent trials (*M* = 2.63 %, *SD* = 2.39), *t*(127) = 3.55, $p < .001$, $d_z = 0.31$, CI 95% = [0.14, 0.49]. We found a congruency effect within the slow speed condition as well, with fewer errors for congruent (*M* = 1.64 %, *SD* = 1.87), compared to incongruent trials, (*M* = 2.32 %, *SD* = 2.49), *t*(127) = 3.22, $p < .001$, $d_z = 0.29$, CI 95% = [0.11, 0.46]. Furthermore, the congruency effect in the large movement size condition was significant, with participants making fewer errors in congruent (*M* = 3.48 %, *SD* = 3.55), than in incongruent trials, (*M* = 4.97 %, *SD* = 4.25), *t*(127) = 4.35, $p < .001$, $d_z = 0.39$, CI 95% = [0.20, 0.56], as well as the congruency effect for the small movement size condition, with fewer errors in congruent (*M* = 1.82 %, *SD* = 2.00), compared to incongruent trials (*M* = 2.35 %, *SD* = 2.29), *t*(127) = 2.70, $p = .004$, $d_z = 0.24$, CI 95% = [0.06, 0.41].

We conducted a pre-registered 2 (trial condition: congruent vs. incongruent) x 2 (movement speed: slow vs. fast) x 2 (movement size: small vs. large) repeated measures ANOVA for the error rates as well. We found again the main effect for congruency, *F*(1, 127) = 18.83, $p < .001$, $\eta_p^2 = .13$, meaning that participants made fewer errors in congruent trials (*M* = 1.74 %, *SD* = 1.78) than in incongruent trials (*M* = 2.48 %, *SD* = 2.13). We did not find a main effect for the movement size condition, *F*(1, 127) = 0.19, $p = .665$, $\eta_p^2 = .001$. The error rate between small trials (*M* = 2.08 %, *SD* = 1.84) and large trials (*M* = 2.14 %, *SD* = 1.83) did not differ significantly. We again found a main effect for the movement speed condition, *F*(1, 127) = 4.39, $p = .038$, $\eta_p^2 = .03$, with participants making fewer errors in slow trials (*M* = 1.98 %, *SD* = 1.83) compared to fast trials (*M* = 2.24 %, *SD* = 1.86). The interaction between the congruency and the movement size condition was significant, *F*(1, 127) = 4.61, *p* = .034, η_p^2 = .04. But the congruency effect between congruency and movement speed was not significant, *F*(1, 127) = 0.13, *p* = .719, η_p^2 = .001, neither was the overall interaction between all three conditions, *F*(1, 127) = 1.64, *p* = .203, η_p^2 = .01.

For the direct comparison between the congruency effects of the finger movement conditions (i.e., fast vs. slow and small vs. large) we conducted different *t*-tests. As the interaction indicated, we found a significant one-tailed *t*-test within the finger movement size conditions, with a stronger congruency effect for large finger movements (*M* = 1.49 %, *SD* = 3.86), compared to small finger movements (*M* = 0.53, *SD* = 2.21), *t*(127) = 4.73, *p* < .001, d_z = 0.42, CI 95% = [0.24, 0.60]. The two-tailed *t*-test for the movement speed condition revealed no significant difference between the congruency effect for slow (*M* = 0.69 %, *SD* = 2.42), compared to fast trials (*M* = 0.79 %, *SD* = 2.50), *t*(127) = 0.36, *p* = .719, d_z = 0.03, CI 95% = [- 0.14, 0.21].

3.4.3 Discussion

In Experiment 3, we tested whether the size of a finger movement influences automatic imitation even when controlling for the speed of the movement. Our results replicated those from Experiment 1 and 2 as we found that only the size of finger movements, but not its speed influences automatic imitation. As several previous experiments on the animacy effect (e.g., Cracco et al., 2015; Klapper et al., 2014; Press et al., 2006) confounded animacy with the size of finger movements, the question arises whether the animacy effect can still be detected if the stimuli control for movement size. To answer this question, we conducted Experiment 4.

3.5 Experiment 4

In several previous investigations on the animacy effect, the robotic hands incorporated a smaller finger movement than the human hands (e.g., Klapper et al., 2014; Press et al., 2005). To test whether the animacy effect can still be detected when this confound is removed from the stimuli, in Experiment 4, we used previously applied stimuli that were uncontrolled for finger movement size (Klapper et al., 2014). In addition, we presented the same stimuli, but adapted them in such a way that the size of the finger movement was the same in the robotic as compared with the human hand.

3.5.1 Method

Participants and Design. To detect even small effects of at least $d_z = 0.15$ with a power of $\beta = 0.90$ (including estimated high drop-out rate), we assessed a high-powered experiment with an online sample of 400 participants. We recruited all participants via Amazon's Mechanical Turk in return for a compensation of $1.20. The Hit Approval Rate and the location was the same as in the previous experiments. Participants could only take part in the study with a laptop or computer. In line with our previous experiments, we excluded data from a total of 112 participants before data analysis because they had less than 26 valid trials in one or more of the conditions ($n = 91$), or did not use the right hand during the experimental phase ($n = 46$). Twenty-five participants met both exclusion criteria. Eight participants had technical problems during the task (i.e., their datasets were submitted empty) and were thus also excluded. The final sample consisted of 280 participants (125 female, 148 male, 7 prefer to self-describe) with an age ranging from 21 to 76 ($M = 40.53$, $SD = 12.18$). Seventeen participants were left-handed and 263 participants were right-handed.

The design consisted of a 2 (trial condition: congruent vs. incongruent) x 3 (hand condition: human vs. robotic vs. robotic corrected) within-subjects design.

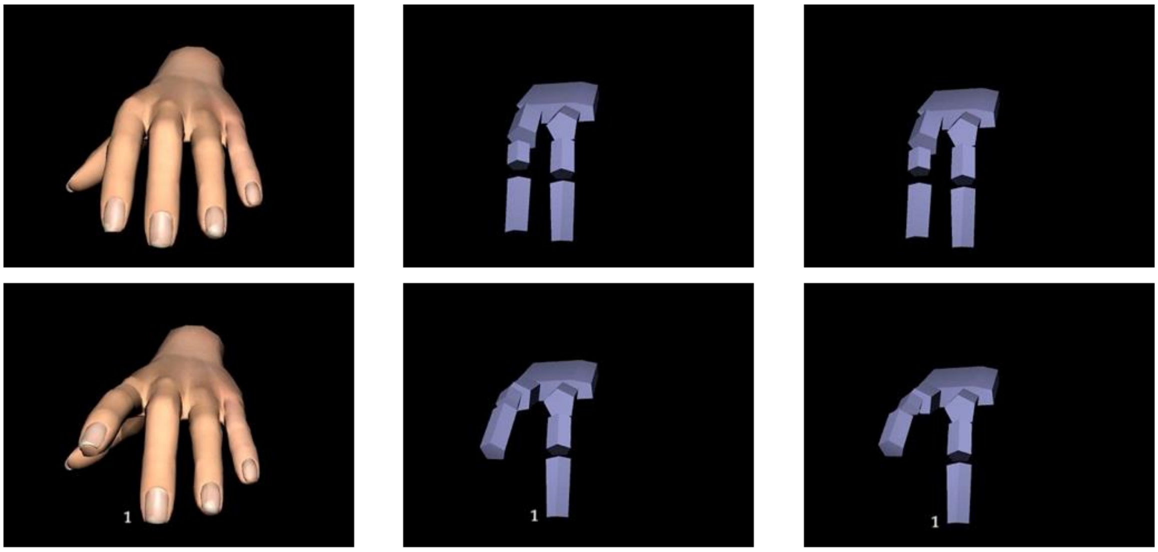
Procedure and Material. We used the exact same procedure as in Experiment 1, except that the experimental blocks used different stimuli and more total trials. As stimuli, we used the exact same robotic and human-looking hand stimuli from Klapper et al. (2014). The size of the finger movement in the original robotic hand condition is 62 px. That of the human hand is 70 px. Additionally, in another condition, we used the robotic hand stimuli, but changed the size of the movements in GIMP (https://www.gimp.org/) so they matched the size of the human-like hand stimuli. (see, Figure 3.6).

The experimental phase consisted of 240 trials with 40 trials for each within-participants condition (i.e., congruent-human movements, incongruent-human movements, congruent-robotic (original) movements, incongruent-robotic (original) movements, congruent-robotic (corrected) movements, and incongruent-robotic (corrected) movements), within 4 experimental blocks. Participants could take a self-paced break between each block.

To prepare the data for analysis, we applied the same approach as in our previous experiments based on the recommendations for the online imitation-inhibition task from Westfal et al. (Westfal, Cracco, et al., 2022), and other previous research (e.g., Catmur & Heyes, 2011; Cracco et al., 2015; Westfal et al., 2021). That is, we removed trials with reaction times below 100 ms for the latency and error rate analyses (3.43 %). We also removed latencies below (0.12 %) and above (1.10 %) 3 *SD*s of a participant's mean. For the analyses of the latencies we discarded erroneous trials as well (14.74 %).

Figure 3.6

Example Stimuli of the Index Finger and the Base Images That Were Used in Experiment 2



human hand **robot hand** **robot corrected**

3.5.2 Results

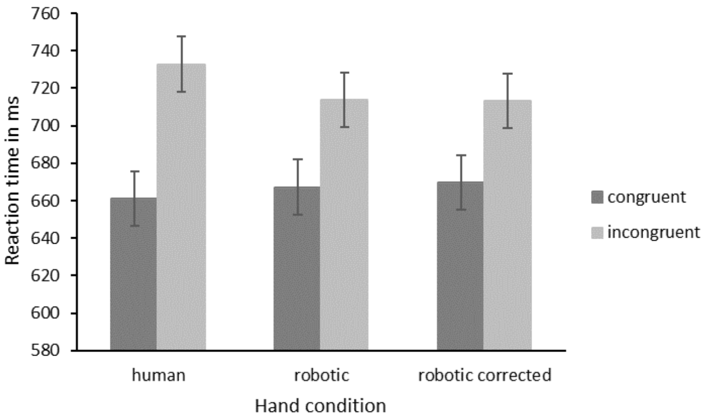
Latencies. First, we conducted a 2 (trial condition: congruent vs. incongruent) x 3 (hand: human vs. robotic vs. robotic corrected) repeated measures ANOVA for the latencies. The ANOVA yielded a significant main effect for trial condition, $F(1, 279) = 594.45$, $p < .001$, $\eta_p^2 = .68$, indicating that participants responded faster in congruent trials ($M = 665.96$ ms, $SD = 244.49$) than in incongruent trials ($M = 719.92$ ms, $SD = 243.62$). The main effect for hand condition was also significant, $F(2, 558) = 17.10$, $p < .001$, $\eta_p^2 = .06$. This means that participants responded slowest for human hand trials ($M = 697.01$ ms, $SD = 244.00$), fastest for robotic hand trials ($M = 690.39$ ms, $SD = 244.46$), with the robotic corrected hand trials latencies between the other two hand conditions ($M = 691.42$ ms, $SD = 242.46$). Finally, the interaction between trial condition and hand was significant as well, $F(2, 558) = 82.35$, $p < .001$, $\eta_p^2 = .23$, showing that the hand condition did influence the congruency effect.

To inspect the interaction in more detail, we tested for the presence of the imitation-inhibition congruency effect within each hand condition. The results showed that the congruency effect was significant within all three hand conditions. In the human hand condition, participants responded faster in congruent trials (*M* = 661.22 ms, *SD* = 244.74) than in incongruent trials (*M* = 732.60 ms, *SD* = 245.19), $t(279) = 27.58$, $p < .001$, $d_z = 1.65$, CI 95% [1.47, 1.83]. Also, in the robotic hand condition the participants responded faster in congruent (*M* = 667.12 ms, *SD* = 248.12), than in incongruent trials (*M* = 713.66 ms, *SD* = 242.90), $t(279) = 17.02$, $p < .001$, $d_z = 1.02$, CI 95% = [0.87, 1.16]. Finally, we found a significant congruency effect in the robotic corrected hand condition as well, as participants responded faster to congruent trials (*M* = 669.53 ms, *SD* = 242.18), as compared to incongruent trials (*M* = 713.30 ms, *SD* = 244.51), $t(279) = 17.61$, $p < .001$, $d_z = 1.05$, CI 95% = [0.91, 1.20].

In a final series of analyses, we compared directly the strength of the congruency effect between all three hand conditions. The congruency effect was stronger for the human hand (*M* = 71.58, *SD* = 43.44) than for the robotic hand (*M* = 46.54, *SD* = 45.76), $t(279) = 10.69$, $p < .001$, $d_z = 0.64$, CI 95% = [0.51, 0.77]. The congruency effect was also stronger for the human hand (*M* = 71.58, *SD* = 43.44) than for the robotic corrected hand (*M* = 43.77, *SD* = 41.60), $t(279) = 12.21$, $p < .001$, $d_z = 0.73$, CI 95% = [0.60, 0.86]. However, the congruency effect did not differ between the robotic hand (*M* = 46.54, *SD* = 45.76) and the robotic corrected hand (*M* = 43.77, *SD* = 41.60), $t(279) = 1.09$, $p = .277$, $d_z = 0.07$, CI 95% = [- 0.05, 0.18] (see Figure 3.7).

Figure 3.7

Reaction Times Comparison Between the Hand Conditions



Note. Latencies (+/- SE). All tests are two-tailed.

Error Rates. We conducted the same test for the error rates as we did for the latencies. The results mirrored those of the latencies. The 2 (trial condition: congruent vs. incongruent) x 3 (hand: human vs. robotic vs. robotic corrected) repeated measures ANOVA yielded a main effect of trial condition, $F(1, 279) = 332.16$, $p < .001$, $\eta_p^2 = .54$, indicating that participants responded with fewer errors in congruent trials ($M = 1.38$ %, $SD = 1.54$) than in incongruent trials ($M = 3.65$ %, $SD = 2.69$). We also found a significant main effect for hand condition, $F(2, 558) = 30.25$, $p < .001$, $\eta_p^2 = .10$, which indicates that participants made the most errors in human hand trials ($M = 2.97$ %, $SD = 2.33$), the least in the robotic corrected hand trials ($M = 2.20$ %, $SD = 2.09$), and the robotic hand trials ($M = 2.37$ %, $SD = 2.11$) between the other two conditions. The interaction between trial condition and hand was also significant, $F(2, 558) = 55.41$, $p < .001$, $\eta_p^2 = .17$, showing that the congruency effect was influenced by the hand condition.

To investigate the interaction in more detail, we first examined the congruency effect for each of the hand conditions: For the human hand, participants committed fewer errors in congruent trials (*M* = 1.19 %, *SD* = 1.67) than in incongruent trials (*M* = 4.76 %, *SD* = 3.76), *t*(279) = 17.20, *p* < .001, d_z = 1.03, CI 95% = [0.88, 1.17]. Similarly, for the robotic hand, participants made fewer errors in congruent trials (*M* = 1.47 %, *SD* = 1.96), as compared to incongruent trials (*M* = 3.28 %, *SD* = 2.95), *t*(279) = 11.26, *p* < .001, d_z = 0.67, CI 95% = [0.54, 0.80]. Finally, we found the congruency effect also for the robotic corrected hand trials with participants making fewer errors in congruent trials (*M* = 1.48 %, *SD* = 1.94) than in incongruent ones (*M* = 2.91 %, *SD* = 2.90), *t*(279) = 9.14, *p* < .001, d_z = 0.55, CI 95% = [0.42, 0.67].

A direct comparison of the congruency effect in the error rates between all three hand conditions revealed a stronger congruency effect in human hand trials (*M* = 3.57 %, *SD* = 3.47) than robotic hand trials (*M* = 1.81 %, *SD* = 2.69), *t*(279) = 7.62, *p* < .001, d_z = 0.46, CI 95% = [0.33, 0.58]. Also, participants' congruency effect was stronger for the human hand (*M* = 3.57 %, *SD* = 3.47), than for the robotic corrected hand (*M* = 1.44 %, *SD* = 2.63), *t*(279) = 9.85, *p* < .001, d_z = 0.59, CI 95% = [0.46, 0.72]. Finally, the congruency effect of the error rates was not statistically different for the robotic hand (*M* = 1.81 %, *SD* = 2.69), as compared to the robotic corrected hand (*M* = 1.44 %, *SD* = 2.63) based on conventional levels of significance, *t*(279) = 1.86, *p* = .064, d_z = 0.11, CI 95% = [- 0.01, 0.23].

Robustness Check. Although we had to exclude such a large number of participants, the results of the analyses remained nearly the same when all participants are included. For the latencies we still found a main effect for trial condition, *F*(1, 389) = 235.91, *p* < .001, η_p^2 = .38, and a significant interaction between trial condition and hand, *F*(2, 778) = 82.35, *p* < .001, η_p^2 = .23. Only the main effect for hand condition disappeared for the latencies, due to the dilution of the latencies by e.g. speeder (i.e., persons who did not want to answer the task attentively and only as fast as possible), $F(2, 778) = 1.56$, $p = .211$, $\eta_p^2 = .004$. This main effect was not important for the hypothesis.

Moreover, the congruency effect was still stronger for the human hand than for the robotic hand, $t(390) = 4.88$, $p < .001$, $d_z = 0.25$, CI 95% = [0.15, 0.35], and for the robotic corrected hand, $t(389) = 5.99$, $p < .001$, $d_z = 0.30$, CI 95% = [0.20, 0.41]. However, the congruency effect remained without difference between the robotic hand and the robotic corrected hand, $t(389) = 1.19$, $p = .237$, $d_z = 0.06$, CI 95% = [- 0.04, 0.16].

The effects on error rates did not differ in their results from the above reported results when all participants were included.

3.5.3 Discussion

Experiment 4 tested if the confound in the movement size of robotic versus human hand stimuli accounts for the animacy effect. For this, we assessed automatic imitation of human and robotic hand movements that included the confounded as well as corrected robotic hand movements that matched the movement size of the human hand. The results demonstrate that the size of the finger movement does not account for the animacy effect. This indicates that although the size of the movement influences automatic imitation, the difference in the movement size between the human and robotic hand was not large enough to account for the animacy effect.

3.6 General Discussion

One of the most often replicated effects within the literature on automatic imitation is the finding that individuals imitate hand movements of humans more strongly than those of non-humans (e.g., Cracco et al., 2015; Haffey et al., 2013; Jansson et al., 2007; Klapper et al., 2014; Press et al., 2005, 2006, 2007). Interestingly, when investigating the stimuli used in these studies, we detected a confound as human hands typically involved larger movements than robotic hands (e.g., Cracco et al., 2015; Klapper et al., 2014; Press et al., 2006). This raises the question whether the size of a movement influences automatic imitation and accounts for the animacy effect. To shed light onto this question we carried out four experiments. In three experiments, we found that movement size influences automatic imitation even when controlling for the speed of the movement. Experiment 4 found that the animacy effect can be detected even when controlling for movement speed. Despite some limitations, these results have several theoretical and methodological implications.

3.6.1 Theoretical and Methodological Implications

Our findings have implications for different lines of research. First, our findings extend previous theories explaining automatic imitation. Ideomotor theory (Greenwald, 1970; Prinz, 1990, 1997) explains imitation with the notion that observing a movement leads to a similar mental representation as when executing the movement. Our research extends this theory by showing that the mental representation of a movement, and as a consequence its imitation, depends on the size of the movement. That is, large movements lead to stronger automatic imitation effects than small movement.

This finding may be explained by attentional processes. Previous research demonstrated that the more attention is directed to a movement, the stronger it is mentally represented (Wertheim, 1981) and, as a consequence, imitated (Bek et al., 2016; Cracco, Bardi, et al., 2018; Heyes, 2011; Longo & Bertenthal, 2009). As large movements are more salient (Franconeri & Simons, 2003; Krüger et al., 2016; Taylor & Fiske, 1978; Wixson, 2000) and thus elicit more attention than small movements (Burnham, 2020; Gaspelin & Luck, 2018; Kerzel & Schönhammer, 2013), it is reasonable to assume that large movements are imitated more strongly, because they elicit more attention.

Since in a typical imitation-inhibition task, participants first see a picture of a hand in resting position before another picture of the same hand with a lifted finger is shown (for a review see, Cracco, Bardi, et al., 2018), large finger movements are faster than small movements. Thus, in principle our findings might be due to the speed and not the size of a movement. However, Experiments 2 and 3 show that in our experiments speed of the movements cannot account for the effects. In particular, Experiment 3 showed that movement size influences automatic imitation independent of movement speed. This is in line with research showing that the human brain processes movement amplitude independently of movement speed (Stark-Inbar & Dayan, 2017).

The goal of the present research was to test whether the size of an observed movement influences automatic imitation. Thus, we tested whether movement size operates independently of movement speed. Although we did not find an influence of speed on automatic imitation, it might be that differences in the speed of a movement affect automatic imitation nevertheless when speed is manipulated in a more salient and extreme way. Indeed, research has shown that fast, as compared to slow body movements, are more salient, leading to a stronger mental representation of the movement (Feria, 2013; Krüger et al., 2016; Taylor & Fiske, 1978). Based on this research, one could expect larger congruency effects for fast movements. On the other hand, research has also shown that the more time participants have to process a movement, the stronger is the congruency effect (for a meta-analysis, see Cracco, Bardi, et al., 2018). This research would suggest that slow movements should lead to stronger imitation effects than fast movements, because slow movements can be better processed and mentally represented. Future research may aim at testing these two hypotheses against each other. Going one step further, our research raises the general question of how differences in motion profiles influence automatic imitation. Besides the size and the speed of a movement, a motion profile also involves the acceleration of a movement (e.g., Carlton, 1981; Rosenbaum, 1975). Future research could, thus, not only test how the speed of a movement, but also its acceleration influences automatic imitation.

Our research also adds to a current debate in the literature about whether automatic imitation is modulated by social factors. On the one hand previous research found evidence for social modulation as results show that participants imitate more strongly when observing human as compared with non-human hands (e.g., Bird, Leighton, et al., 2007; Chaminade & Cheng, 2009; Klapper et al., 2014; Liepelt & Brass, 2010b; Press et al., 2006), when observing in-group as compared with out-group members (e.g., Genschow & Schindler, 2016; Rauchbauer et al., 2016), when being in a pro-social as compared with an anti-social mindset (e.g., Butler et al., 2016; Cracco, Genschow, et al., 2018; Leighton et al., 2010), when focusing on similarities rather than differences (Genschow et al., 2021), or when focusing on others rather than the self (for a meta-analysis, see Genschow, Schuler, et al., 2019; Spengler, Brass, et al., 2010). On the other hand, recent studies have struggled to replicate some of these findings (e.g., Butler et al., 2015; Cracco, Bardi, et al., 2018; De Souter et al., 2021; Galang & Obhi, 2020; Genschow, Pauels, et al., 2023; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019) questioning the claim that automatic imitation is modulated by social factors. In the same time, the animacy effect has been replicated many times suggesting that animacy is one of the few factors that reliably modulates automatic imitation (e.g., Bird, Leighton, et al., 2007; Chaminade et al., 2005;

Cracco et al., 2015; Klapper et al., 2014; Liepelt & Brass, 2010b; Press et al., 2006, 2007). However, since we found that previous research confounded animacy with the size of movements, the questions arose whether animacy is actually a robust moderator of imitative behavior. In Experiment 4 we could detect a robust animacy effect even when controlling for the size of the movement. This indicates that animacy remains as one of the most robust social modulations of automatic imitation.

Finally, our findings have important implications for researchers using the imitation-inhibition task. When implementing button-up manipulations within this task, researchers should ensure that the size of the finger movements is matched. Otherwise, it remains unclear whether it is the experimental manipulation or the size of the movement that drives the observed effect. In the same time, as in Experiment 4 the difference in size between the robotic hand and the corrected robotic hand was not large enough to find differences between the two hands, researchers interested in studying the effect of movement size on automatic imitation should manipulate the size of the movements strong enough to detect the predicted effects.

3.6.2 Limitations

Besides these implications our study nevertheless has a few limitations. First, one may argue that some of our experiments were underpowered as we did not always reach the calculated number of participants, due to numerous pre-registered exclusions in all four experiments. Reasons for the somewhat extensive exclusions may be due to the online environment, but also due to the MTurk platform we used for recruitment, which is known to promote high exclusion rates (e.g., Chmielewski & Kucker, 2020). In the same time, it is important to note that the exclusion criteria are based on general practices in reaction time research and years of experience (e.g., Cracco, Genschow, et al., 2018; Gabay & Behrmann, 2014; Genschow, van Den Bossche, et al., 2017; Selst & Jolicoeur, 1994; Westfal et al., 2021). Some of the exclusion criteria are necessary because otherwise they falsify the aim of the task to measure imitation. These include, for example, participants who worked with both hands or responses below 100 ms, which is below any human reaction time (e.g., De Pretto et al., 2021; Gabay & Behrmann, 2014). Although the task and its congruency effect are largely stable against noise (Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017), as also shown by the robustness checks from Experiments 2 and 4, it remains difficult to isolate social effects in the presence of noise, as the result may become unreliable under these circumstances (Borgmann et al., 2007). Thus, even if in some of our experiments the total number of trials and sample size is rather small, the results with exclusions will be more reliable than those without, since they only exclude individuals who performed the task inattentively or disregarded the instructions. Nevertheless, further experiments in this regard should assess larger samples to reinforce and amplify the effects we found.

Second, we derived our prediction that large movements elicit stronger imitation effects based on research showing that large movements are more salient than small ones (Franconeri & Simons, 2003; Krüger et al., 2016; Taylor & Fiske, 1978; Wixson, 2000) and are thus more likely attended and mentally represented (Burnham, 2020; Gaspelin & Luck, 2018; Kerzel & Schönhammer, 2013). Although our findings are in line with this reasoning, we did not directly test if attention actually accounts for our effects. Future research could, thus, test the mediating processes more directly.

3.6.3 Conclusion

People have the tendency to automatically imitate other people's behaviors. The exact underlying mechanism of this ubiquitous phenomenon are still a matter of investigation. By finding that people imitate stronger when they see large (as compared to small) movements, we show that small differences in the size of an observed movement are sufficient to change the strength in which people imitate others.

3.7 Acknowledgment

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3.8 Declarations

Ethics Approval: The experiments are typical reaction time studies with a duration of less than 20 minutes. Because subjects were fully informed of their rights, the personal data collected (demographics), and the possibility of publishing the data in anonymized, averaged form, using a Participant Information and Informed Consent Form validated by the ethics committee and data protection office of the university where the data were collected, no separate ethics approval had to be obtained for this type of reaction time study. All procedures performed in studies involving human participants were in accordance with the 1964 Helsinki Declaration and its later amendments or comparable ethical standards.

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Declarations of Interest. None

Chapter 4

Imitation and Interindividual Differences: Belief in Free Will Is not Related to Automatic Imitation

The previous chapter showed that finger movement size made a difference in automatic imitation, but was not responsible for the animacy effect, despite a confound in the animacy stimuli. Thus, the animacy effect remains one of the best-researched modulations with the imitation-inhibition task (e.g., Klapper et al., 2014; Press et al., 2006). However, the question now is whether other effects can be replicated with the same success in high-powered online experiments.

Therefore, this chapter investigates whether perceiving intentions in others' actions is linked to automatic imitation. Since free will belief as an interpersonal factor increases the perception that a behavior is internally driven (e.g., Genschow, Rigoni, et al., 2017), we investigated in a correlative design if individuals with increased belief in free will also show an increase in automatic imitation of others.

This chapter is based on the following article:

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Please note that some changes in headings, citation style, and formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the article.

Abstract

It is well known that individuals have the tendency to automatically imitate each other and that such imitative behavior is fostered by perceiving intentions in others' actions. That is, past research has shown that perceiving an action as internally driven enhances the shared representation of observed and executed actions increasing automatic imitation. An interpersonal factor that increases the perception that a behavior is internally driven is belief in free will. Consequently, we hypothesized that the more individuals believe in free will, the more they automatically imitate others. To test this prediction, we conducted two high-powered (total *N* = 642) and preregistered studies in which we assessed automatic imitation with the imitation-inhibition task. Contrary to our predictions, belief in free will did not correlate with automatic imitation. This finding contributes to current findings challenging the assumption that automatic imitation is modulated by interindividual differences. Further theoretical implications are discussed.

Keywords. Automatic imitation, free will belief, intention, top-down modulation, interindividual differences

4.1 Introduction

People automatically imitate a variety of different behaviors, including gestures (Bernieri, 1988; Cracco, Genschow, et al., 2018), simple movements (Brass et al., 2000; Genschow et al., 2012; Genschow & Florack, 2014; Genschow & Schindler, 2016), facial expressions (Dimberg, 1982), and emotions (Dimberg, 1982; Hess & Fischer, 2013). Imitation is considered to be a crucial learning mechanism through which children's personality develops (Bandura, 1962, 1963) and through which children adapt to society (Forman & Kochanska, 2001; Kuczynski et al., 1987). Moreover, imitation serves an important social function in the sense that it bonds humans more strongly together by fostering prosocial behavior (Duffy & Chartrand, 2015a; Wang & Hamilton, 2012). For example, imitation enhances helping (van Baaren, Holland, et al., 2004), reciprocal liking (Sparenberg et al., 2012), empathy (Iacoboni, 2009), and the acceptance of other people‘s perspective (Lamm, Batson, et al., 2007), which is why imitation is often declared to be a social glue (Kavanagh & Winkielman, 2016). A current debate concerns whether interindividual differences can predict the propensity to imitate others (Cracco, Bardi, et al., 2018). In the current research, we investigated the role of belief in free will, which is closely tied to perceptions of intentionality as an important component of automatic imitation.

One of the most often used tasks that allows measuring individuals' automatic tendency to imitate in a very reliable way (Genschow, van Den Bossche, et al., 2017) is the imitation-inhibition task (Brass et al., 2000). In this task, participants respond to two imperative cues with two different finger movements across multiple trials. For example, they are instructed to respond to the number "1" by lifting the index finger and to the number “2” by lifting the middle finger. At the same time, the participants see another person’s hand either lifting the same (congruent trial), the other finger (incongruent trial), or no finger at all (neutral trial). Typically, individuals respond more quickly and with fewer errors within congruent trials than within incongruent ones (i.e., congruency effect). The response time to neutral trials lies between the response times to congruent and incongruent trials. The congruency effect can be explained with individuals’ automatic tendency to imitate others. That is, in incongruent trials, individuals need to inhibit this tendency, which results in slower reaction times. Likewise, the automatic tendency to imitate is facilitated in congruent trials, which results in faster response onsets (for a meta-analysis, see Cracco, Bardi, et al., 2018). Traditionally, the imitation-inhibition task was used in laboratory settings by measuring movement onsets with custom-built light sensors (e.g., Brass et al., 2000, 2001). More recent research demonstrates the reliability of the task by assessing movement onsets with key releases on computer keyboards in laboratory (Butler et al., 2015; Genschow, van Den Bossche, et al., 2017; Longo et al., 2008; Longo & Bertenthal, 2009), as well as online settings (Westfal, Cracco, et al., 2022).

By using the imitation-inhibition task, previous research tested whether automatic imitation is predicted by interindividual differences. For example, Hogeveen and Obhi (2013) found that the degree to which others are automatically imitated varies between narcissistic and non-narcissistic participants. Other researchers found reduced automatic imitation effects in individuals with autistic spectrum disorder as compared to healthy controls (Bird, Leighton, et al., 2007; J. Cook et al., 2013; J. L. Cook & Bird, 2012). Also, empathic traits and the ability to take another person’s perspective have been found to correlate with automatic imitation (Cracco et al., 2015). Both of these links have been explained by variation in self-other overlap that should theoretically alter automatic imitation. These findings suggest that certain interindividual characteristics, theorized to relate to processes of automatic imitation, can explain variance in the propensity to imitate others.

However, recent research casts doubt on the link between automatic imitation and several interindividual differences. For example, Butler et al. (2015) could not find support for the idea that broad personality traits related to social behavior (i.e., extraversion and agreeableness) and disorders of social cognition (i.e., autistic-like and schizotypal traits) correlate with automatic imitation. Moreover, the authors could not replicate the findings that narcissistic and empathic traits correlate with automatic imitation. Similar results were obtained by Genschow, van Den Bossche, et al. (2017) who did not find significant correlations between automatic imitation and a variety of stable interpersonal differences including empathy, perspective taking, autism-like traits, and traits related to self-other focus. These findings raise the question of why it is difficult to find links between automatic imitation and interindividual factors. One reason could be that the relation between interindividual differences and automatic imitation is rather small and studies investigating this relation did not have enough power to detect such small effects. Another reason may be that the previously assessed personality factors are not linked closely enough to the mechanisms underlying automatic imitation. Thus, in the present research we aimed at testing, using high-powered samples, whether automatic imitation is predicted by belief in free will—a factor that is closely linked to a key process underlying automatic imitation: perceiving intentionality in others’ actions.

4.1.1 Processes Underlying Automatic Imitation

Automatic imitation is generally explained by a common representation of observed and executed actions. For instance, according to the so-called perception-behavior link (Chartrand et al., 2005; Chartrand & Bargh, 1999) an observed action leads to the same mental representation as the executed action. This shared mental representation, in turn, increases the likelihood of executing the observed action. Likewise, ideomotor theory (Greenwald, 1970; Prinz, 1990, 1997) argues that the visual image of a perceived action is part of its own motor representation. Thus, the observation of a certain action should be sufficient to prime the execution of the same action.

Experiments against the backdrop of ideomotor theory (for an overview, see Prinz et al., 2004) indicates that two principles account for the representation of others: perceptual and intentional induction. Whereas perceptual induction indicates that individuals execute the actions they see, intentional induction means that observers act in order to achieve the anticipated outcome of an action. In line with the latter principle, neuro-physiological investigations indicate that activations in the mirror-neuron system are largely based on inferring intentions in others' actions (Kilner et al., 2007; Lamm, Fischer, et al., 2007). In line with this reasoning, research on anticipated action has suggested that merely inferring another person's intention to initiate an action might be sufficient to trigger the execution of the very same action (Genschow, Bardi, et al., 2018; Genschow, Klomfar, et al., 2018; Genschow & Brass, 2015; Genschow & Groß-Bölting, 2021).

It is important to note that these perceived intentions can be driven by higher-level top-down processes, such as a higher-level beliefs (Bien et al., 2009; Buttelmann et al., 2013; Liepelt et al., 2008; Meltzoff, 1988; Spengler, Brass, et al., 2010). For example, research in developmental psychology suggests that children are more likely to imitate seemingly meaningless actions if they believe that the actions had been performed intentionally as compared to if they believed that the actions had been performed unintentionally or accidentally (Buttelmann et al., 2013; Meltzoff, 1988). Similarly, adults are more likely to imitate a behavior in which they can recognize a clear intention, as compared to a rather unintentional behavior (Bien et al., 2009).

Research from Liepelt et al. (2008) further supports the crucial role of perceived intentions within the imitation-inhibition task. In several experiments, the researchers implemented two conditions. In the intentional condition, the observed fingers were moved freely by the model. In the unintentional condition the model's fingers were pulled by an apparatus. The researchers found larger congruency effects in the condition in which the fingers were lifted intentionally indicating that perceived intentions are a crucial factor for automatic imitation to occur. Based on these findings, Liepelt et al. argue that perceiving intentions in other persons' movements is based on attributional processes. That is, smaller automatic imitation effects were found "when participants interpreted the observed movement as externally driven than when they attributed the movements as internally generated" (p. 587). In sum, previous research indicates that if observers believe that movements are internally driven (as compared to externally driven), they perceive stronger intentions, which enhance the shared representation of observed and executed action. As a consequence, automatic imitation is increased.

4.1.2 Belief in Free Will

A factor that increases internal attributions and perceived intentions in others is belief in free will (Genschow, Rigoni, et al., 2017, 2019). Free will beliefs are stable trait-like metacognitive judgments (Brembs, 2011) about the extent to which individuals intentionally guide their thoughts and actions (Frith, 2012). Individuals who believe in free will, believe that other people are responsible for their actions (Carey & Paulhus, 2013; Nahmias et al., 2008), because they can decide and control their own behavior (Paulhus & Carey, 2011). Thus, not surprisingly, the more people believe in free will, the more they perceive other persons’ actions as internally driven (Genschow, Rigoni, et al., 2017, 2019). For instance, in several studies, Genschow, Rigoni, et al. (2017) presented participants with different behaviors and then asked them to which degree they perceive the behaviors as internally or externally driven. The results indicate that individuals generally perceive behaviors as more strongly internally driven than externally driven. Crucially, however, this effect was more strongly pronounced, the more participants believed in free will. In another series of studies, Genschow et al. (2019) presented participants with different video clips. For example, in Study 1 participants watched video clips of football players touching the ball with their hands. It was varied whether the players objectively touched the ball accidentally or intentionally. The researchers found that the more participants believed in free will, the more intentions they perceived in football players touching the ball with their hand irrespective of whether the players actually touched the ball accidentally or intentionally. This indicates that belief in free will correlates with perceived intentions irrespective of how ambiguous the action is.

4.1.3 Present Research

Taken together, the literature on automatic imitation reports mixed results with respect to the question of whether interindividual differences modulate automatic imitation. A reason for the difficulty to find correlations between automatic imitation and interindividual differences may lay in the fact that most of the previously assessed scales did not tap closely enough into crucial processes underlying automatic imitation. Regarding the processes underlying automatic imitation, it is widely accepted that perceived intentions are a crucial mechanism in the sense that perceiving an action as internally driven enhances the shared representation of observed and executed actions and, thus, automatic imitation (e.g., Liepelt et al., 2008). The literature on free will beliefs indicates that belief in free will increases the perception that a behavior is internally driven (Genschow, Rigoni, et al., 2017, 2019). That is, the more individuals believe in free will, the stronger their perceived intentions of other people's behavior.

Based on this research, we predicted that belief in free will is positively correlated with automatic imitation. It is important to note that this does not mean that a person who does not believe in free will would not perceive any intention and would therefore not imitate others at all. Rather, we expected individuals to lay on a continuum with individuals who do not believe in free will perceiving fewer intentions than individuals who strongly believe in free will. Consequently, we predicted that the more a person believes in free will, the more strongly he or she will engage in automatic imitation.

Interestingly, the connection between imitation and belief in free will has been discussed already by various researchers (Baer et al., 2008; Cracco, Bardi, et al., 2018;

Feldman, 2016; Rigoni et al., 2012, 2013; Spengler, Brass, et al., 2010). For example, Cracco, Bardi, et al. (2018) claim that “evidence suggests that psychological beliefs on topics such as free will can have pervasive effects on human performance” (p. 26). However, the link between belief in free will and automatic imitation has never been empirically tested. To fill this gap, we conducted two high-powered and preregistered studies.

Study 1 tested the predicted relation in a high-powered sample. Study 2 aimed at replicating the findings obtained in Study 1 in an even larger sample. For both studies, we applied an online version of the imitation-inhibition task and measured free will belief with the Free Will Inventory (FWI; Nadelhoffer et al., 2014). In addition, we added another belief in free will measure in Study 2 to cross-validate our findings. To test whether the predicted effect is driven by facilitation, interference, or both, we measured in both studies all three typical automatic imitation effects (i.e., congruency, facilitation, interference). We have made all material and data of this paper together with a codebook openly accessible on the Open Science Framework (OSF; https://osf.io/5nhdb/). Both studies were pre-registered at aspredicted.org.

4.2 Study 1

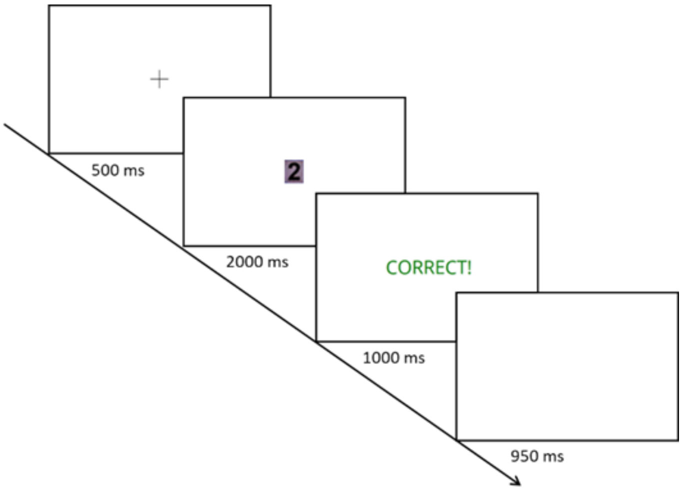
4.2.1 Method

Participants. In line with the recommendations for stable correlations put forward by Schönbrodt and Perugini (2013) we aimed at recruiting 250 participants. Such a number of participants would give us 80% power (one-tailed) for an effect size of *r* = 0.16. In order to compensate for preregistered participant exclusions and a potentially high drop-out rate, we aimed at collecting data from 300 participants via Amazon's Mechanical Turk. Only MTurkers located in the US with a Hit Approval Rate over 85% were invited to participate in the study. Participants could only take part in the study with a laptop or computer. For their participation, the participants received \$1.00. Data from 293 participants were recorded (for seven participants no data was recorded). 60 participants met at least one of the pre-registered exclusion criteria (cf. https://aspredicted.org/8jj58.pdf) and were, thus, excluded prior to data analysis. That is, among the excluded participants, 56 had fewer than 33 trials in at least one condition and 4 did not use the right hand during the experimental phase. The final sample consisted of 233 participants (103 female, 121 male, 9 diverse) with an age between 20 and 73 (*M* = 36.69, *SD* = 10.80). Twenty-three participants were left-handed and 210 participants were right-handed.

Procedure. The whole online study was programmed with JavaScript using the JavaScript library jsPsych (de Leeuw, 2015). The participants first completed two practice phases to familiarize themselves with the imitation-inhibition task. In the first practice phase, they were instructed to respond within 10 trials as quickly as possible to the randomly appearing number cue “1” by lifting their index finger and “2” by lifting their middle finger. After each trial, they received accuracy feedback. If they made more than two errors within 10 trials, they had to repeat the phase until they reached the threshold of at least eight correct trials. Afterwards, the participants were asked which hand they had used and were requested to use the right hand if they had used the left hand or both hands so far. The exact presentation times and a schematic structure of a practice phase trial is displayed in Figure 4.1.

Figure 4.1

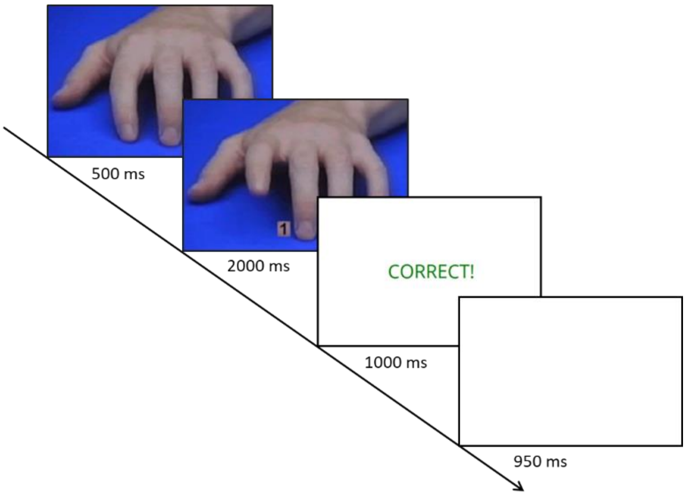
Schematic Representation of a Practice Trial With the Number "2"



In the second practice phase, images of another person's hand were additionally shown to the numbers "1" and "2" (all pictures had a resolution of 400 x 267 pixels). This picture depicted a model lifting either the index, the middle finger, or no finger. The participants were again asked to react to the number cue by lifting their fingers. This setup resulted in three different trial types: in congruent trials, participants responded with the same finger as the model. In incongruent trials, participants responded with another finger, and in neutral trials the participants responded with a finger while the model's hand did not execute any finger movement. This second practice phase consisted of 12 trials in total whereby neutral, congruent and incongruent trials were presented randomly. After each trial, participants received accuracy feedback. The participants had to repeat this practice phase until they made fewer than 4 errors. The exact presentation times and a schematic structure of a practice phase trial with hands is displayed in Figure 4.2.

Figure 4.2

Schematic Representation of a Congruent Practice Trial With Hand Pictures



The subsequent experimental phase was built the same way as the second practice phase, with the exact same stimuli presentation times and the same interstimulus interval, except that participants received no further accuracy feedback. In total, the experimental phase consisted of 150 trials (50 incongruent, 50 congruent, and 50 neutral trials), which were presented in a randomized order. To prepare the data for analysis, we removed extremely slow reaction times in line with our preregistration. That is, latencies below (0.02%) and above (0.95%) 3 *SD*s of the participant’s mean. We also removed trials with reaction times below 100 ms (2.36%). For the analyses of the latencies we removed erroneous trials (10.65%).

After participants completed the imitation-inhibition task, we measured their belief in free will with the Free Will Inventory (FWI; Nadelhoffer et al., 2014). That is, they indicated their agreement for 15 statements on 7-point rating scales ranging from 1 (*strongly disagree*) to 7 (*strongly agree*). The FWI consists of three subscales measuring belief in free will (Cronbach's α = 0.90), determinism (Cronbach's α = 0.86), and dualism (Cronbach's α = 0.94).

Dualism refers to the idea that humans have souls that are distinct from their material body. Determinism (contrary to free will) describes that there is only one possible outcome of the future. To prepare the data for analysis, for each subscale we computed sum scores by adding all answers for the respective subscale.[4]

At the end, participants indicated demographical data and a few general questions on how they experienced working on the task.

4.2.2 Results

To test our hypotheses, we conducted inference statistics using SPSS besides the correlational analyses. Additionally, we applied Bayesian statistics using JASP, an open source statistical package (Version 0.11.1.0; JASP Team, 2019). We also used JASP for the hypothesized correlational analyses to directly test for positive correlations.

Latencies. In a first step, we tested within three preregistered *t*-tests for the presence of imitation-inhibition effects in the response times. With respect to the congruency effect, the results indicate that the subjects responded more quickly in congruent trials (M = 451.97 ms, SD = 72.00), than in incongruent trials (M = 511.28 ms, SD = 79.13), $t(232) = 27.65$, $p < .001$, $d_z = 1.81$, 95% CI [1.68, 1.94]. Also, the facilitation effect was significant: the subjects responded more quickly in congruent trials (M = 451.97 ms, SD = 72.00), than in neutral trials (M = 481.30 ms, SD = 72.40), $t(232) = 23.29$, $p < .001$, $d_z = 1.53$, 95% CI [1.40, 1.65]. Finally, with respect to the interference effect, the participants responded more quickly to neutral trials (M = 481.30 ms, SD = 72.40), as compared to

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[4] We also pre-registered an overall free will score to correlate it with the automatic imitation effects, in order to establish comparability with previous research, as this approach has been used by some researchers (Baumeister et al., 2009; Genschow, Rigoni, et al., 2017; Lynn et al., 2013; Rigoni et al., 2012, 2015). As an overall score we computed the mean of the free will, dualism, and reversed determinism subscale.

incongruent trials (*M* = 511.28 ms, *SD* = 79.13), *t*(232) = 18.73, $p < .001$, $d_z = 1.23$, 95% CI [1.10, 1.36]. In addition to the pre-registered analyses we corrected for multiple testing according to Bonferroni (Bland & Altman, 1995): all *t*-tests were also significant at a level of $\alpha = 0.017$.

Error Rates. In a second step, we ran the same analyses for the error rates. In line with the latencies, we detected a congruency effect. That is, the subjects responded with fewer errors in congruent trials (*M* = 0.56%, *SD* = 0.71) than in incongruent trials (*M* = 3.00%, *SD* = 2.32), *t*(232) = 16.58, $p < .001$, $d_z = 1.09$, 95% CI [0.96, 1.22]. Moreover, we found a facilitation effect meaning that the subjects responded with fewer errors in congruent trials (*M* = 0.56%, *SD* = 0.71), compared to neutral trials (*M* = 0.96%, *SD* = 1.05), *t*(232) = 5.74, $p < .001$, $d_z = 0.38$, 95% CI [0.25, 0.51]. Finally, we found an interference effect: the participants committed fewer errors in neutral trials (*M* = 0.96%, *SD* = 1.05) than in incongruent trials (*M* = 3.00%, *SD* = 2.32), *t*(232) = 14.78, $p < .001$, $d_z = 0.97$, 95% CI [0.84, 1.09]. Again all *t*-tests were also significant when the α-level is corrected according to Bonferroni (i.e., $\alpha = 0.017$).

Relation Between Automatic Imitation and Free Will Belief. To prepare the data for analysis, we first calculated, for the latencies and the error rates, the congruency effect by subtracting the mean values of congruent trials from those of the incongruent trials, the facilitation effect by subtracting the mean values of the congruent trials from those of neutral trials, and the interference effect by subtracting the mean values of the neutral trials from the mean values of the incongruent trials.

In order to test our hypothesis that belief in free will is positively correlated with automatic imitation we ran correlation analyses. We first correlated the congruency, facilitation, and interference effect of the latencies with all the FWI’s subscales (i.e., free will, dualism, and determinism). None of the FWI’s subscales correlated significantly with any of the automatic imitation indices, *ps* > .106. (see Table 4.1).

In a second series of analyses, we correlated the same scales with the imitation effects of the error rates. None of these correlations were significant either, *ps* > .188 (see Table 4.2). We do not additionally report the correlations corrected according to Bonferroni, as they were already unconservatively insignificant. Thus, our hypothesis of a positive correlation of the imitation effects with the free will scales could not be supported. Mean value, standard deviation and variance of the three subscales are listed in Table 4.3.

Table 4.1

Pearson's Correlations of the Reaction Time Imitation Effects With the Free Will Inventory (Study 1) [5]

	RT congruency	RT facilitation	RT interference	Sub free Will	Sub dualism	Sub determinism
RT congruency	—					
RT facilitation	.67 ***	—				
RT interference	.81 ***	.11 *	—			
Sub free will	- .06	- .13	.03	—		
Sub dualism	.05	.04	.04	.37 ***	—	
Sub determinism	.06	.08	.02	- .05	.23 ***	—

Note. All tests are one-tailed, for positive correlation.
* *p* < .05, *** *p* < .001, one-tailed.

Table 4.2

Pearson's Correlations of the Error Rate Imitation Effects With the Free Will Inventory (Study 1) [6]

	Error congruency	Error facilitation	Error interference	Sub free Will	Sub dualism	Sub determinism
Error Congruency	—					
Error Facilitation	.37 ***	—				
Error Interference	.88 ***	- .12	—			
Sub Free Will	- .01	- .13	.06	—		
Sub Dualism	- .003	- .03	.01	.37 ***	—	
Sub Determinism	.05	.02	.04	- .05	.23 ***	—

Note. All tests are one-tailed, for positive correlation.
*** *p* < .001, one-tailed.

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[5] Pearson's correlations of the reaction time imitation effects with the overall free will score: *r* = - .03 (rt congruency), *r* = - .09 (rt facilitation), *r* = .04 (rt interference).

[6] Pearson's correlations of the error rate imitation effects with the overall free will score: *r* = - .03 (error congruency), *r* = - .09 (error facilitation), *r* = .02 (error interference).

Table 4.3

Descriptive Statistics of the Free Will, the Dualism, and the Determinism Subscales (Study 1)

Measure	*M*	*SD*	*Var*
Sub free will	25.28	5.79	33.56
Sub dualism	21.95	8.24	67.92
Sub determinism	16.51	6.45	41.60

Bayes Factors. In a final series of analyses, we tested the null hypothesis that belief in free will is not correlated with automatic imitation by applying Bayesian statistics. That is, we report the BF_{+0}, which gives the ratio with which the alternative hypothesis is favored over the null hypothesis (i.e., the smaller the BF the more it argues in favor of the null hypothesis), indicating the constrained hypothesis that the correlation is positive-only (see Schönbrodt & Wagenmakers, 2018 for an overview). We calculated the BF_{+0} with the default priors (stretched beta prior width $r = 1$) in JASP (Version 0.11.1.0; JASP Team, 2019) because we did not expect a specific estimated size of the correlation beforehand. Moreover, we report values of the Bayes factor robustness check. The robustness check provides an assessment of the robustness of the Bayes factor under different prior specifications: if the derived conclusions do not change across a range of different prior distributions, the assumption that the analysis is robust is supported. For correlational analyses, JASP provides within the robustness check the maximal BF_{+0} that can be achieved under certain prior conditions, which is the value we report for every correlation. The BF_{+0} values for the correlation between the different free will belief scales and the imitation effects on the latencies ranged from 0.03 to 0.32, which means moderate to very strong evidence for the null hypothesis (see Table 4.4, cf. Jeffreys, 1998). The robustness check indicates that even with very low priors, the maximum BF_{+0} would not exceed anectodal evidence for H1 (max $BF_{+0} < 1.70$). The same pattern was apparent for the error rates (cf. Table 4.5).

Table 4.4

Bayes Factors for Latencies for all Subscales of the Free Will Inventory (Study 1)

	RT congruency		RT facilitation		RT interference	
Measure	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}
Sub free will	0.05	1.00 (at r = 0.0001)	0.03	1.00 (at r = 0.0001)	0.12	1.003 (at r = 0.004)
Sub dualism	0.18	1.24 (at r = 0.004)	0.13	1.06 (at r = 0.004)	0.15	1.15 (at r = 0.004)
Sub determinism	0.20	1.31 (at r = 0.004)	0.32	1.70 (at r = 0.009)	0.10	1.00 (at r = 0.0001)

Note. For all tests, the alternative hypothesis specifies that the correlation is positive.
r = prior width.

Table 4.5

Bayes Factors for Error Rates for all Subscales of the Free Will Inventory (Study 1)

	Error congruency		Error facilitation		Error interference	
Measure	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}
Sub free will	0.07	1.00 (at r = 0.0001)	0.03	1.00 (at r = 0.0001)	0.20	1.30 (at r = 0.004)
Sub dualism	0.08	1.00 (at r = 0.0001)	0.06	1.00 (at r = 0.0001)	0.10	1.00 (at r = 0.0001)
Sub determinism	0.16	1.15 (at r = 0.004)	0.10	1.00 (at r = 0.0001)	0.14	1.10 (at r = 0.004)

Note. For all tests, the alternative hypothesis specifies that the correlation is positive.
r = prior width.

4.2.3 Discussion

In Study 1, we tested the prediction that belief in free will is positively correlated with automatic imitation measured with the imitation-inhibition task. The results did not yield a significant correlation between the typical imitation indices (i.e., congruency, facilitation, and interference) and the different free will belief subscales. Bayesian analyses indicate support for H0 suggesting that belief in free will is not correlated with automatic imitation. Nevertheless, due to the large number of unanticipated participant exclusions, which resulted in a smaller sample size than we had hoped for, it might be that our sample was too small to detect a relation between free will beliefs and automatic imitation. Thus, we ran a high-powered replication of Study 1 with twice as many participants. In addition, we added a one item slider measure of belief in free will (Gooding et al., 2018) to cross-validate our findings.

4.3 Study 2

4.3.1 Method

Participants. We recruited 494 participants via Amazon's Mechanical Turk to take part in the online study in return for compensation of $1.80. On Amazon's Mechanical Turk, the same selection criteria were applied as in the first study. In line with our preregistration (https://aspredicted.org/c3hb7.pdf), data from a total of 85 subjects were excluded from data analyses because they had fewer than 33 valid trials in one or more of the conditions (*n* = 72), or did not use the right hand during the experimental phase (*n* = 13). The final sample consisted of 409 subjects (187 female, 210 male, 9 diverse, 3 prefer not to say) with an age ranging from 18 to 76 (*M* = 37.45, *SD* = 11.41). Fifty-eight participants were left-handed and 351 participants were right-handed.

Procedure. The procedure was the same as in the first study. Additionally, we implemented a free will slider that has been previously used in the research of free will beliefs (Gooding et al., 2018; Harms et al., 2017; Protzko et al., 2016; Shariff et al., 2014) to get a direct assessment of participants' general belief in free will. Specifically, participants were asked to indicate their free will belief on the following item: "*Using the slider provided, please indicate the extent to which you believe in free will*". The slider represented a continuum from 0 (*no belief in free will*) to 100 (*absolute belief in free will*). The slider measurement has the advantage that the belief in free will is indicated directly on a continuum, resulting in a direct clear and unambiguous interval-scaled variable (Gooding et al., 2018; Harms et al., 2017; Protzko et al., 2016; Shariff et al., 2014).

Afterwards, participants completed the Free Will inventory (FWI; Nadelhoffer et al., 2014). Cronbach's Alpha for the subscales was $\alpha = 0.89$ for the free will subscale, $\alpha = 0.88$ for the determinism subscale and $\alpha = 0.93$ for the dualism subscale.

To prepare the data for analysis, we removed extremely slow reaction times in line with Study 1 and our preregistration. That is, we discarded latencies below (0.02%) and above (1.01%) 3 *SD*s of the participants' mean as well as trials with reaction times below 100 ms (2.00%). For the analysis of the latencies, we discarded erroneous trials (9.69%).

4.3.2 Results

Latencies. In line with Study 1, we found the typical imitation effects. With respect to the congruency effect, subjects responded more quickly in congruent trials ($M = 461.55$ ms, $SD = 88.73$) than in incongruent trials ($M = 520.46$ ms, $SD = 94.61$), $t(408) = 31.51$, $p < .001$, $d_z = 1.54$, 95% CI [1.46, 1.65]. The facilitation effect also emerged: the subjects responded more quickly in congruent trials ($M = 461.55$ ms, $SD = 88.73$), than in neutral trials ($M = 490.32$ ms, $SD = 88.45$), $t(408) = 27.83$, $p < .001$, $d_z = 1.38$, 95% CI [1.28, 1.47]. Finally, we found a significant interference effect: the participants responded more quickly to neutral trials ($M = 490.32$ ms, $SD = 88.45$), compared to incongruent trials ($M = 520.46$ ms, $SD = 94.61$), $t(408) = 21.01$, $p < .000$, $d_z = 1.04$, 95% CI [0.94, 1.14]. All *t*-tests were also significant with the Bonferroni corrected $\alpha = 0.017$.

Error Rates. The results of the error rates mirrored those of the latencies. First, we found the congruency effect: the subjects responded with fewer errors in congruent trials (M = 0.69%, SD = 0.98), than in incongruent trials (M = 2.85%, SD = 2.32), $t(408) = 19.65$, $p < .001$, $d_z = 0.97$, 95% CI [0.87, 1.07]. Moreover, we found a facilitation effect: the subjects responded with fewer errors in congruent trials (M = 0.69%, SD = 0.98), compared to neutral trials (M = 1.12%, SD = 1.17), $t(408) = 7.78$, $p < .001$, $d_z = 0.38$, 95% CI [0.29, 0.48]. Finally, we found an interference effect: the participants responded with fewer errors in neutral trials (M = 1.12%, SD = 1.17), than in incongruent trials (M = 2.85%, SD = 2.32), $t(408) = 16.68$, $p < .001$, $d_z = 0.83$, 95% CI [0.73, 0.92]. Again, all t-tests for the error rates were also significant with the Bonferroni corrected $\alpha = 0.017$.

Relation Between Automatic Imitation and Free Will Belief. We ran the same multiple correlation analyses as in Study 1 with one exception; we included the free will slider as a further measure of free will belief. Similar to Study 1, for the latencies, none of the correlations between any of the automatic imitation indices and the free will belief subscales reached significance, $ps > .116$ (see Table 4.6). When correlating the same free will belief subscales with the imitation effects of the error rates, again, none of these correlations reached significance, $ps > .161$ (see Table 4.7). Mean value, standard deviation and variance of the three subscales are listed in Table 4.8.

Table 4.6

Pearson's Correlations of the Reaction Time Imitation Effects With the Free Will Inventory and the Free Will Slider (Study 2) [7]

	RT congruency	RT facilitation	RT interference	Sub free will	Sub dualism	Sub determinism	Free will slider
RT congruency	—						
RT facilitation	.65 ***	—					
RT interference	.84 ***	.13 ***	—				
Sub free will	- .09	- .09	- .05	—			
Sub dualism	.05	.01	.06	.27 ***	—		
Sub determinism	- .03	- .01	- .04	- .04	.28 ***	—	
Free will slider	- .01	- .04	.01	.65 ***	.25 ***	- .17	—

Note. All tests are one-tailed, for positive correlation.
*** $p < .001$, one-tailed.

Table 4.7

Pearson's Correlations of the Error Rate Imitation Effects With the Free Will Inventory and the Free Will Slider (Study 2) [8]

	Error congruency	Error facilitation	Error interference	Sub free will	Sub dualism	Sub determinism	Free will slider
Error congruency	—						
Error facilitation	.36 ***	—					
Error interference	.87 ***	- .15	—				
Sub free will	.01	.03	- .001	—			
Sub dualism	- .07	- .05	- .05	.27 ***	—		
Sub determinism	- .07	- .10	- .02	- .04	.28 ***	—	
Free will slider	.03	.05	.01	.65 ***	.25 ***	- .17	—

Note. All tests are one-tailed, for positive correlation.
*** $p < .001$, one-tailed.

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[7] Pearson's correlations of the reaction time imitation effects with the overall free will score: $r = - .01$ (rt congruency), $r = - .04$ (rt facilitation), $r = .02$ (rt interference).

[8] Pearson's correlations of the error rate imitation effects with the overall free will score: $r = .004$ (error congruency), $r = .03$ (error facilitation), $r = - .01$ (error interference).

Table 4.8

Descriptive Statistics of the Free Will, the Dualism, the Determinism Subscales, and the Free Will Slider (Study 2)

Measure	*M*	*SD*	*Var*
Sub free will	25.55	5.83	33.95
Sub dualism	22.89	8.10	65.68
Sub determinism	16.44	6.48	42.01
Free will slider	81.91	20.28	411.46

Bayes Factors. To test whether our data supports H0, we ran Bayesian analyses. The BF_{+0} values for the latencies ranged from 0.02 to 0.22, which means moderate to very strong evidence for the null hypothesis (see Table 4.9). As the Bayesian robustness checks indicate, the maximum BF_{+0} would not exceed anecdotal evidence for H1 (max $BF_{+0} < 1.62$) and most of the values would even not exceed a value above 1.00 (i.e., values less than or equal to 1 mean that there is no evidence at all for H1), which strongly supports the evidence for the null hypothesis in this study as well.

The same pattern emerged for the error rates (see Table 4.10). The BF_{+0} values strongly support the null hypothesis.

Table 4.9

Bayes Factors for Latencies for all Subscales of the Free Will Inventory and the Free Will Slider (Study 2)

	RT congruency		RT facilitation		RT interference	
Measure	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}
Sub free will	0.02	1.00 (at r = 0.0001)	0.02	1.00 (at r = 0.0001)	0.03	1.00 (at r = 0.0001)
Sub dualism	0.17	1.36 (at r = 0.004)	0.07	1.00 (at r = 0.0001)	0.22	1.62 (at r = 0.004)
Sub determinism	0.04	1.00 (at r = 0.0001)	0.06	1.00 (at r = 0.0001)	0.04	1.00 (at r = 0.0001)
Slider free will	0.05	1.00 (at r = 0.0001)	0.04	1.00 (at r = 0.0001)	0.07	1.00 (at r = 0.0001)

Note. For all tests, the alternative hypothesis specifies that the correlation is positive.
r = prior width.

Table 4.10

Bayes Factors for Error Rates for all Subscales of the Free Will Inventory and the Free Will Slider (Study 2)

	Error congruency		Error facilitation		Error interference	
Measure	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}	BF_{+0}	max BF_{+0}
Sub free will	0.08	1.00 (at r = 0.0001)	0.11	1.04 (at r = 0.004)	0.06	1.00 (at r = 0.0001)
Sub dualism	0.03	1.00 (at r = 0.0001)	0.03	1.00 (at r = 0.0001)	0.03	1.00 (at r = 0.0001)
Sub determinism	0.03	1.00 (at r = 0.0001)	0.02	1.00 (at r = 0.0001)	0.04	1.00 (at r = 0.0001)
Slider free will	0.12	1.11 (at r = 0.004)	0.17	1.38 (at r = 0.004)	0.07	1.00 (at r = 0.0001)

Note. For all tests, the alternative hypothesis specifies that the correlation is positive.
r = prior width.

4.3.3 Discussion

In a high-powered experiment, we again tested the prediction that belief in free will is positively correlated with automatic imitation as measured with the imitation-inhibition task. The setup of this second study was the same as for the first one, with two exceptions. First, we doubled the sample size. Second, we added a direct and continuous free will belief slider measure (Gooding et al., 2018). Replicating the results obtained in Study 1, we did not find a significant correlation between the typical imitation-inhibition indices (i.e., congruency, facilitation, and interference) and one of the free will belief scales, or the slider measure.

4.4 General Discussion

People automatically imitate others' behaviors (Cracco, Bardi, et al., 2018; Heyes, 2011) and this tendency increases when they perceive other people's actions as intentionally driven (Liepelt et al., 2008). Yet an open question is whether individuals who generally perceive others' behaviors as internally driven would imitate more strongly. A factor that is strongly associated with perceived intentions, and the perception that others' behavior is internally driven, is belief in free will (Genschow, Rigoni, et al., 2017, 2019). Consequently, we hypothesized that the more individuals believe in free will, the more strongly they automatically imitate others. We tested this hypothesis in two high-powered studies (total *N* = 642). In Study 1, automatic imitation did not correlate with belief in free will measured with the Free Will Inventory (FWI; Nadelhoffer et al., 2014). In Study 2, we replicated this null finding in an even larger online sample of nearly 500 participants. Additionally, we added a free will slider measure (Gooding et al., 2018) to cross-validate our findings. Again, no significant relation was found between belief in free will and automatic imitation. These findings have important theoretical implications and call for further empirical investigations.

4.4.1 Theoretical Implications

Although previous research has shown that certain personality traits such as autism (Bird, Leighton, et al., 2007), narcissism (Hogeveen & Obhi, 2013), as well as empathic traits and the ability to take another person's perspective (Cracco et al., 2015) correlate with automatic imitation, other research was not able to replicate some of these findings (Butler et al., 2015; Cracco, Bardi, et al., 2018; Galang & Obhi, 2020; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019). In line with these failed replications, we find despite high power no support for the idea that interindividual differences are linked to automatic imitation. A reason for the difficulty to detect correlations with the imitation-inhibition task may lay in the key feature of the construction of this task. The imitation-inhibition task delivers very strong effects and even under different modulations, the basic congruency effect is still very robust and reliable (Brass et al., 2000; Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017; Westfal, Cracco, et al., 2022). The strength of the task may come along with little variation between participants, which could decrease the likelihood of finding correlations with other measures (Benesty et al., 2009). Thus, the intrinsic characteristics of the imitation-inhibition task could be a reason as to why it is difficult to detect correlations with interindividual factors, such as the belief in free will, for instance. Future research may aim at restructuring the task to achieve higher interindividual variations.

Given the difficulty to find correlations between the imitation-inhibition task and personality scales, the question arises as to whether the null findings are specific to personality scales, or whether this finding generalizes to other forms of top-down modulations. This question relates to a current discussion on the usefulness of measuring automatic imitation with a stimulus-response compatibility task. Ramsey (2018) argues that it is not possible to measure imitation with reaction-time-based tasks, because the reaction time indices in the task are the result of a combination of several different underlying processes. These processes are neither necessarily related to imitation, nor to any other form of social behavior. Consequently, the task might not be a suitable candidate to measure top-down modulated imitation. In contrast to this view, Cracco and Brass (2019) argue that the imitation-inhibition task was designed as a laboratory measurement of mimicry. In their opinion, reaction times measure covert imitative response tendencies associated with some (but not all) types of overt imitation. Indeed, there is research supporting this notion showing that the imitation-inhibition task can be socially modulated with *experimentally* manipulations. For example, subjects imitate a human hand more strongly than a robot hand (Press et al., 2006), an in-group member than an out-group member (Genschow & Schindler, 2016; Rauchbauer et al., 2016), a goalless action more strongly than a goal-directed one (Chiavarino et al., 2013), social gestures more strongly compared to antisocial gestures (Cracco, Genschow, et al., 2018), or when the belief in animacy of the hand is increased (Liepelt & Brass, 2010b)—to name just a few examples. However, there is also research that has not been able to replicate some of these results. For example, recently the in-out group effect on imitation could not be replicated (De Souter et al., 2021; Genschow et al., 2022). Other research had difficulties in replicating the influence of animacy on automatic imitation (Press et al., 2006). Our findings contribute to this literature by showing that top-down modulations of automatic imitation are more difficult than previously assumed.

As previous research had difficulties in finding meaningful relations between interindividual differences and automatic imitation assessed with the imitation-inhibition task, the question arises as to whether this applies to other forms of imitation too. Indeed, individuals’ tendency to imitate others can be measured in different ways and the imitation-inhibition task is just one of many measures of imitative behavior. For, example, when measuring mimicry, that is, the spontaneously occurring tendency to imitate others in actual social situations, research within the last two decades found that a variety of different interindividual differences correlate with mimicry. For example, perspective-taking abilities (Chartrand & Bargh, 1999), affiliation motives (Johnston, 2002; Lakin et al., 2003), or field dependence (van Baaren, Horgan, et al., 2004) have all been found to correlate with mimicry behavior. However, more recent research testing the influence of several personality scales on mimicry within a highly powered experiment could not replicate previous findings (Genschow, van Den Bossche, et al., 2017). That is, mimicry did not correlate with several personality traits including empathy, perspective taking, autism traits, and traits related to self-other-focus (e.g., individualism vs. collectivism, need to belong). As there are just a few large-scale investigations on the relationship between personality traits and mimicry, future research is warranted to more thoroughly investigate whether and how interindividual differences relate to mimicry and other forms of imitation. Such research could also use other imitation paradigms that disentangle goal- from movement-based imitation (e.g., Genschow, Hansen, et al., 2019; Hansen & Genschow, 2020) to investigate top-down modulations on different forms of imitation.

4.4.2 Potential Reasons for the Null Finding

Our results suggest that belief in free will does not correlate with automatic imitation. At the same time, one may argue that the proposed correlation could be masked for a variety of reasons.

First, it might be that the link between belief in free will and automatic imitation could not be detected because the assessed measures are unreliable. However, it is important to note that the free will belief scale we used is one of the most reliable scales to measure free will belief (Liu et al., 2020; Nadelhoffer et al., 2014; Santin et al., 2018). Also, the assessed imitation-inhibition task is very reliable and more reliable than other imitation measures, such as mimicry tasks (Genschow, van Den Bossche, et al., 2017). Thus, we regard it as unlikely that unreliable measurements account for the null finding.

Second, it might be that our samples were too small to detect the hypothesized effect. However, the Bayesian analyses speak against this reasoning, as they detected moderate to very strong support for the null hypothesis. Moreover, even if one would find an effect with a larger sample, the effect would be rather small and, thus, most likely negligible.

Third, it could be that belief in free will not only relates to higher perceived intentions, but also a stronger focus on the self, which could have counteracted the predicted effect. Previous research has shown that a focus on the self (relative to others) reduces automatic imitation (Brass et al., 2009; Brass & Spengler, 2009; Genschow, Schuler, et al., 2019; Hogeveen & Obhi, 2011; Leighton et al., 2010; Spengler, Brass, et al., 2010; van Baaren, Maddux, et al., 2003; Wang & Hamilton, 2013). Based on this finding, Spengler et al. (2010) speculated that belief in free will “might help counteract predominant imitation-driven behavior” (p. 105). Based on this reasoning, it might be that a focus on the self counteracts the influence of perceived intentions on automatic imitation. If so, these two processes may cancel each other out, explaining the present null effect. However, it is important to note that the speculated link between free will belief and self-other focus has never been empirically tested. There are only some investigations suggesting a relation between free will beliefs and pro-social attitudes (Baumeister et al., 2009; Vohs & Schooler, 2008)—a link that does not seem to be very stable as several failed attempts in replicating this basic idea indicate (Crone & Levy, 2019; Nadelhoffer et al., 2020). Moreover, as recent research has shown that a focus on the self is not as strongly related to automatic imitation as this has been previously assumed (Khemka et al., 2021; Rauchbauer et al., 2020), we regard the alternative explanation that a focus on the self counteracted our assumed effect as rather unlikely. Nevertheless, future research may aim to test the link between free will beliefs, self-other focus and automatic imitation.

4.4.3 Limitations and Further Directions

Despite several implications of our studies, we need to acknowledge several limitations. First, it is important to note that our correlative approach does not allow for causal conclusions. However, as experimental approaches, in which free will beliefs are manipulated, do not typically produce stronger effects than correlational approaches (Genschow, Cracco, et al., 2023), the conclusion that belief in free will and automatic imitation are not related would most likely not be challenged by further experimental investigations.

Second, our study cannot draw any conclusions about the extent to which perceived intentions themselves influence automatic imitation, because we merely tested whether belief in free will, but not perceived intentions per se, correlates with automatic imitation. Although prior studies (Liepelt et al., 2008) have previously assessed the link between perceived intentions and automatic imitation, future research may replicate this finding to strengthen the link between perceived intentions and automatic imitation.

Third, our study is equally unable to test whether free will beliefs affect the perception of intentionality. Previous research has repeatedly shown that belief in free will is related to the perception of intentions even when people observe very abstract actions (e.g., Genschow, Rigoni, et al., 2017, 2019). Nevertheless, these studies as well as our present investigation do not allow testing of whether belief in free will increases perceived intentions in the observed finger lifting movements. Future research may consider examining this relation more directly to resolve any ambiguities in the link between free will beliefs and automatic imitation.

4.4.4 Conclusion

The two presented studies provide new insights into automatic imitation. In both of our high-powered studies, we found no correlation between automatic imitation and belief in free will, suggesting that the two constructs are likely unrelated. Therefore, in line with current research (e.g., Butler et al., 2015; Galang & Obhi, 2020; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013) our findings suggest that the relationship between automatic imitation and interindividual factors is less universal and generalizable than previously reported in the literature.

4.5 Acknowledgment

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Declarations of Interest. None

Chapter 5

Group Membership Does not Modulate Automatic Imitation

The previous chapter concluded that the relationship between interindividual factors and automatic imitation might be less generalizable and less universal than previously reported in the literature (e.g., Heyes, 2011), as no effect was found in the relationship between free will belief and automatic imitation. However, the approach in Chapter 4 was a correlational top-down modulation using a free will belief questionnaire and not a direct manipulated belief about the person whose hand is shown in the experiments.

For this reason, in this chapter I present a study on group membership, where we tried to directly manipulate the beliefs about the person whose hand is shown in the trials by (a) presenting a country flag above the hand stimulus, (b) varying the size of the flag, and (c) showing a face in addition to the flag above the stimulus. Moreover, we also tried to manipulate the hand stimuli directly by (d) using artificial black and white hands. We conducted a series of six experiments, which were included in a meta-analysis with fixed effects.

This chapter is based on the following article:

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Please note that some changes in headings, citation style, and formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the article.

Abstract

Individuals have the automatic tendency to imitate each other. A key prediction of different theories explaining automatic imitation is that individuals imitate in-group members more strongly than out-group members. However, the empirical basis for this prediction is rather inconclusive. Only few experiments have investigated the influence of group membership using classic automatic imitation paradigms and these experiments led to mixed results. To put the group membership prediction to a critical test, we carried out six high-powered experiments (total *N* = 1,538) in which we assessed imitation with the imitation-inhibition task and manipulated group membership in different ways. Evidence across all experiments indicates that group membership does not modulate automatic imitation. Moreover, we do not find support for the idea that feelings of affiliation or perceived similarity moderate the effect of group membership on automatic imitation. These results have important implications for theories explaining automatic imitation and contribute to the current discussion of whether automatic imitation can be socially modulated

Keywords: imitation, group membership, similarity, affiliation

5.1 Introduction

Individuals have the propensity to automatically imitate a wide range of different behaviors, such as facial expressions (Dimberg, 1982), emotions (Hess & Fischer, 2016), postures (LaFrance, 1982), gestures (Cracco, Genschow, et al., 2018), and simple movements (Genschow et al., 2012; Genschow, Hansen, et al., 2019; Genschow & Florack, 2014; Genschow & Schindler, 2016). The most-often used task to study automatic imitation is the imitation-inhibition task (Brass et al., 2000, 2001; for a meta-analysis see Cracco, Bardi, et al., 2018). In this task, participants are instructed to respond over many trials to two imperative cues with two different finger lifting movements. For instance, participants have to respond to the number “1” or “2” by lifting their index or middle finger. At the same time, participants see on a computer screen another person lifting either the same (i.e., congruent movement) or the other finger (i.e., incongruent movement). The typical finding in such an automatic imitation paradigm is that individuals respond faster and more accurately on congruent trials as compared with incongruent trials. Past research has demonstrated that the imitation-inhibition task is a valid (Cracco & Brass, 2019) and robust (Cracco, Bardi, et al., 2018) measure of imitation and produces larger as well as more reliable effects than other imitation tasks (Genschow, van Den Bossche, et al., 2017).

Classic perception-action theories (Chartrand & Bargh, 1999; Dijksterhuis & Bargh, 2001; Greenwald, 1970; Prinz, 1990, 1997) explain automatic imitation with the notion that the observation and execution of an action activates similar motor representations. This shared representation then increases the likelihood that observing an action leads to the execution of the very same action. The idea that observing an action activates the corresponding motor plan in the observer has been supported by many different findings, including behavioral studies (e.g., Brass et al., 2000, 2001; Craighero et al., 2002; Kilner et al., 2003), fMRI (e.g., Gazzola & Keysers, 2009; Keysers & Gazzola, 2010), motor TMS (e.g., Catmur et al., 2007; Fadiga et al., 1995), as well as single-cell recordings in monkeys (di Pellegrino et al., 1992) and humans (Mukamel et al., 2010).

While the above-reviewed literature suggests a direct link between observed and executed actions, other research suggests that this link depends on social contexts (Duffy & Chartrand, 2015a; Wang & Hamilton, 2012). One of the most-often discussed social moderators of automatic imitation is group membership. Belonging to a social group and establishing stable and cohesive bonds with members from the in-group has an evolutionary important impact on human life (Baumeister & Leary, 1995; Dunbar, 2012; Dunbar & Shultz, 2010) and recognizing a member of the in-group, such as a person from same ethnical group for instance, affects perceived distance to this person (Fini et al., 2020) and elicits a motivation to affiliate with this person (van der Schalk et al., 2011). In line with this notion, previous research on automatic imitation suggests that members from the in-group are more strongly imitated than members from the out-group (e.g., Genschow & Schindler, 2016; Gleibs et al., 2016). Different theoretical accounts have been put forward to explain this, but also other, social modulations.

Motivational theories explain *why* group membership influences automatic imitation (Chartrand & Dalton, 2009; Wang & Hamilton, 2012) by arguing that people imitate others to gain social benefits. Support for this idea comes from studies showing that being imitated causes people to feel closer (van Baaren, Holland, et al., 2004) and more affiliated to the imitator (Lakin & Chartrand, 2003), and to behave in a more prosocial manner (Lakin et al., 2008; van Baaren, Holland, et al., 2004). Based on these results, motivational theories argue that people use imitation, either consciously or unconsciously, to affiliate with others.

Because individuals have the general tendency to affiliate more with the in-group than with the out-group (van der Schalk et al., 2011), people imitate in-group members more strongly than out-group members.

Although motivational theories can explain *why* social group membership modulates imitation, they do not explain *how* this modulation is implemented. A framework that addresses the how-question is the dual-route framework (Heyes, 2011). This account argues that automatic imitation can be (socially) modulated either by input or output modulation. Input modulation refers to the degree to which action observation activates corresponding motor representations. An important factor influencing how strongly observed actions activate the motor system is attention (e.g., Chong et al., 2009). Hence, individuals imitate out-group members less, because they pay less attention to the actions of out-group members than to the actions of in-group members. Another factor that operates at the input level is similarity. Motor learning theories (Brass & Heyes, 2005; Greenwald, 1970; Heyes, 2010; Prinz, 1990, 1997) argue that imitative tendencies are learned responses that develop as a result of self-observation and interaction with other, often similar (Efferson et al., 2008), individuals (Brass & Heyes, 2005; R. Cook et al., 2014; Heyes, 2010; Ray & Heyes, 2011). Thus, in-group members are imitated more than out-group members, because in-group members are perceived as more similar than out-group members both at a physical (Press, 2011) and a conceptual level (Cracco, Bardi, et al., 2018). Output modulation, on the other hand, refers to how strongly motor activation elicited by action observation exerts an influence on behavior. Such an account would suggest that actions performed by in- and out-group members both activate the motor system similarly, but that imitative responses elicited by out-group members are subsequently inhibited.

Interestingly, despite the different explanations for the influence of group membership on automatic imitation, the empirical evidence for the group membership effect is rather unclear.

5.1.1 Empirical Evidence for the Link Between Group Membership and Automatic Imitation

On the one hand, some researchers found that individuals imitate in-group members more strongly than out-group members (Genschow & Schindler, 2016; Gleibs et al., 2016). However, this effect was only found when participants felt affiliated with the in-group (Genschow & Schindler, 2016) or when they were in a cooperative as compared with a competitive mindset (Gleibs et al., 2016). On the other hand, some researchers found in one experiment the exact opposite; meaning that participants imitated out-group members stronger than in-group members (Rauchbauer et al., 2015). However, in another experiment, the same authors found this effect only when participants were imitating target persons who displayed angry facial expression (Rauchbauer et al., 2016). Finally, recent research did not find any difference between the imitation of in- and out-group members in a multiple agent paradigm (De Souter et al., 2021; for similar results, see Weller et al., 2020).

Taken together, previous research produced rather mixed results with respect to the question of whether automatic imitation is modulated by group membership or not. Strikingly, each of the conducted experiments has limitations that hinder a clear conclusion of whether group membership modulates automatic imitation. That is, several experiments manipulated other factors such as emotions (Rauchbauer et al., 2015, 2016) or a cooperation vs. competition mindset (Gleibs et al., 2016) on top of group membership leaving open whether imitation itself is influenced by group membership. Other experiments (e.g., Genschow & Schindler, 2016) assessed only a small number of participants leaving open whether the basic effect is replicable. Finally, some of the experiments (De Souter et al., 2021; Gleibs et al., 2016) manipulated group membership with minimal group paradigms that are known to produce smaller effects than natural groups, such as ethnic groups for example (Ostrom & Sedikides, 1992). Thus, to set the group membership prediction to a stronger test, in the present research, we assessed within six high-powered experiments the classic imitation-inhibition task (Brass et al., 2000, 2001) by presenting hands from in- and out-group members belonging to different nationalities (Experiments 1 to 4) or ethnic groups (Experiment 5 and 6).

5.1.2 Present Research

The goal of the present research was to test the hypothesis that automatic imitation is stronger for in-group members than for out-group members. In addition, we assessed two different moderating variables to shed light onto potential processes underlying the influence of group membership on automatic imitation and to test different theoretical accounts that had been put forward to explain social modulations of imitation. First, we tested whether feeling affiliated with the in-group moderates automatic imitation. Motivational theories argue that individuals imitate others when they expect social benefits from the other person (Wang & Hamilton, 2012). Based on this notion, it is reasonable to predict that group membership moderates automatic imitation especially when individuals feel affiliated with the in-group (Genschow & Schindler, 2016). Second, we investigated whether perceived similarity with the in- and the out-group moderates the relation between group membership and automatic imitation. Motor learning theories (e.g., Brass & Heyes, 2005; Greenwald, 1970; Heyes, 2010; Prinz, 1990, 1997) argue that automatic imitation is facilitated when perceived or actual similarity between actor and observer is increased. In line with this reasoning, we tested whether in-group members are imitated more strongly when they are perceived as more similar to oneself as compared with out-group members.

To test our predictions, we conducted six high-powered experiments. As all experiments had similar methods and tested the same hypothesis, we analyzed the data in a meta-analysis. In all experiments, automatic imitation was assessed with the imitation-inhibition task (Brass et al., 2000, 2001). In Experiments 1 to 4, we told US participants that they would see hands from US, German, or Chinese persons. To manipulate group membership in Experiments 5 and 6, we invited black and white participants and presented them with black and white hands. In all experiments, we assessed how similar participants perceive members of the in- and the out-group. Additionally, in Experiments 1, 2, 4, 5, and 6, we assessed how strongly participants felt affiliated with the in- and the out-group.

We report all experiments we ever conducted in this line of research, all manipulations, measures, and exclusions. All experiments were conducted in accordance with the ethical standards of the 1964 Declaration of Helsinki and in line with the ethical guidelines of the German Psychological Society (DGPs). The materials and data are available on the Open Science Framework (OSF; https://bit.ly/3sfyRvj). Experiment 6 was preregistered at Aspredicted (https://aspredicted.org/blind.php?x=w9qn5n).

5.2 Method

5.2.1 Participants

In total, 1,538 participants took part in six experiments (see Table 5.1 for more details). Participants were recruited via Amazon’s Mechanical Turk (Experiments 1 to 5) or Prolific (Experiment 6). For each experiment, we aimed at detecting an effect size of at least $dz = .25$ for the difference in automatic imitation between in- and out-group members. To detect such an effect with a power of $1 - \beta = .85$ and an Alpha probability of $\alpha = .05$ (two-tailed) in a within-subject design, at least 146 participants are needed. With this power analysis in mind, we collected participants. In Experiments 5 and 6, we aimed at detecting even smaller effects with more power (i.e., effects of at least *dz* = .2 and power of at least 1 - *β* = .95). Consequently, we increased the sample sizes accordingly.

We applied the following exclusion criteria across all experiments (see Table 5.1 for numbers of exclusions): We discarded participants (1) for which less than 30% of trials remained after excluding erroneous trials, fast trials (i.e., trials faster than 100 ms and trials more than 3 SDs below the participant's mean), and slow trials (i.e., trials more than 3 SDs above the participant's mean), (2) who reported to have used two hands instead of one during the imitation-inhibition task[9], and (3) who were non-US citizens (Experiments 1-4) or reported a skin color that could not be categorized as black or white (Experiments 5-6).

5.2.2 Procedure

We conducted all experiments online. The experiments were programmed in JavaScript using the jsPsych library (de Leeuw, 2015). In all experiments, we applied a similar procedure. First, all participants provided informed consent and were informed that participation was voluntary and that all answers were processed and stored anonymously. Next, they ran through the imitation-inhibition task (Brass et al., 2000, 2001). After the task, participants indicated perceived similarity of in- and out-group members (Experiment 1-6) as well as feelings of affiliation with the in- and out-group (Experiments 1, 2, 4, 5, and 6). Finally, they indicated basic demographics, were thanked, and dismissed.

In Experiments 3 and 4, we also assessed the Inclusion of Other in the Self (IOS) scale (Aron et al., 1992) as well as pro-social attitudes towards in- and out-group members. As these scales were not central to our predictions, we report the associated results in the supplementary material only.

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[9] Including only participants who used the right hand does not change the pattern of results.

Table 5.1

Demographic Information for Experiments 1 to 6

Exp.	Sample	*N* before exclusion	*N* exclusion Criterion 1	*N* exclusion Criterion 2	*N* exclusion Criterion 3	*N* after exclusion	% female	M_{age} (*SD*); range after exclusion
1	MTurk-USA	174	20	5	8	145	43.4	37.54 (11.98); 19-70
2	MTurk-USA	147	20	5	5	121	38.8	35.19 (11.25); 18-67
3	MTurk-USA	145	25	4	7	112	41.1	39.52 (11.22); 21-70
4	MTurk-USA	146	17	4	0	126	33.3	35.13 (9.37); 18-65
5	MTurk-USA	378	62	21	11	297	36.7	37.66 (11.25); 18-69
6	Prolific-UK	791	17	17	21	737	46.1	28.03 (9.49); 17-67

Note. Some participants met more than one of the exclusion criteria, so the total number of exclusion does not add up to the overall exclusion number.

Criterion 1 = Participants for which less than 30% of trials remained after excluding erroneous trials, fast trials, and slow trials; Criterion 2 = Participants who reported to have used two hands instead of one during the imitation-inhibition task; Criterion 3 = participants who were non-US citizens (Experiments 1-4) or participants with a skin color that could neither be categorized as black or white (Experiments 5-6)

5.2.3 Measures

Imitation-Inhibition Task. We applied a validated online version (Westfal, Cracco, et al., 2022) of the imitation-inhibition task (Brass et al., 2000, 2001). The basic procedure of the online version is identical to previous research and produces similar strong and reliable results as when conducting the task in the laboratory. To accommodate the imitation-inhibition task to an online setting, it begins with a more detailed, stepwise practice procedure. First, participants ran through several different exercise blocks. In the first exercise block (10 trials), participants responded to randomly appearing number cues. That is, participants pressed and held down both the “g” key of their keyboard with their right index finger and the “h” key with their right middle finger. Afterwards, a fixation cross appeared for 500 ms, followed by the presentation of the number cue (i.e., either the number “1” or the number “2”). Participants had to lift their index finger in response to the number “1” and their middle finger in response to the number “2”. The number was presented until participants lifted a finger or for a maximum of 2,000 ms. After each trial, participants received accuracy feedback. Participants had to repeat this first exercise block until they reached the threshold of at least eight correct trials.

In the second exercise block, we presented images of another person’s hand in addition to the numbers “1” and “2”. That is, when participants simultaneously pressed and held down the “g” and “h” key with their right index and middle finger, another person’s hand in mirrored and resting position appeared on the screen for 500 ms. We used the hands that were used in the original Brass et al. (2001, 2000) experiments (see stimuli on OSF; https://bit.ly/3sfyRvj). Afterwards, a picture of the same hand with either the lifted index or lifted middle finger was shown. Together with the lifted finger, either the number

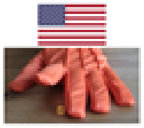
“1” or “2” was presented between the model’s index and middle finger for a maximum of 2,000 ms or until participants lifted a finger. As in the first exercise block, participants had to respond by lifting the index finger in response to the number “1” and the middle finger in response to the number “2”. The setup of the trial structure resulted in two different trial types. On congruent trials, participants executed the same finger movement as the model and on incongruent trials, they executed another finger movement. After each trial, participants received accuracy feedback. The exercise block consisted of 12 trials and was repeated until participants made less than 4 errors.

After successful completion of the two exercise blocks, the experimental blocks started. These blocks were similar to the second exercise block. However, we did not provide accuracy feedback anymore. Moreover, we presented different hands to manipulate group membership. The way we manipulated group membership varied between experiments.

In Experiments 1 to 4 we told US citizens that that they would see hands belonging to persons from different countries, including the USA, Germany, and China. To indicate which hand was from which country, the models were wearing colored gloves (i.e., blue, orange, and purple). We randomly varied across participants which color was matched with which country. To strengthen the manipulation, we presented the national flag of the respective country together with the hand (see Table 5.2 for screen shots of example trials). In Experiments 1 to 3, the flag was presented above the model’s hand. In Experiment 4, the flag was presented as screen background. The size of the flags slightly varied between experiments (see Table 5.3 for details), because we wanted to test whether the salience of the flag influences the effect of group membership on automatic imitation. To strengthen the manipulation of the nationality, in Experiment 4, we presented in addition to the flag and the hand, a picture of a face of a US, German, or Asian looking person. The pictures were taken from the Chicago face database (Ma et al., 2015). As US looking face, we chose target face CFD-WM-213, as German looking face target face CFD-WM-214, and as Asian looking face target face CFD-AM-210. For all faces, we selected the pictures with neutral facial expressions. A pretest confirmed that the faces were actually perceived as US, German, and Chinese, respectively (see supplementary material). The face pictures were presented at a size of 200 x 278 pixel. They appeared on the screen already 1,500 ms before the hands were presented and remained on the screen until participants responded to the number cue.

Table 5.2

Screenshots of Example Trials Used in Experiments 1 to 4

Country	Experiment 1	Experiments 2 and 3	Experiment 4
USA			
Germany			
China			

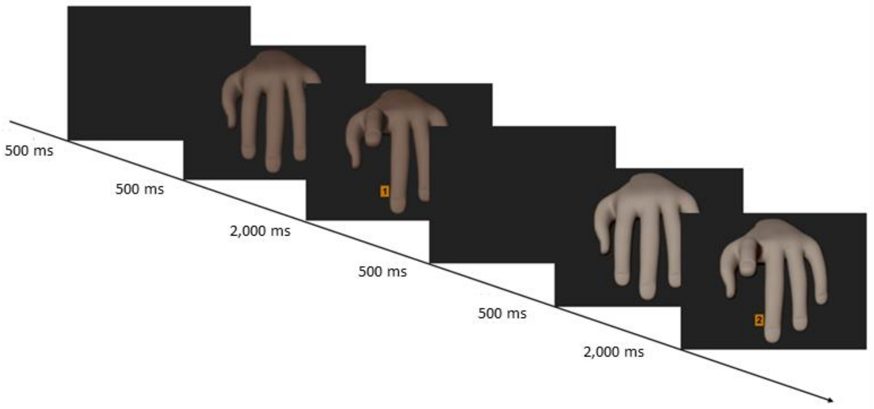
Note. Flag size slightly varied across Experiments 1 to 3 (for details, see Table 5.3).

In principle, it could be that differences in nationality are not a relevant group dimension for participants. Thus, in Experiment 5 and 6, we changed the group membership manipulation by presenting black and white participants with black and white hands. We did not photograph actual hands, but created hands differing in color (black vs. white) using the open source software Blender (Blender Foundation, 2020; Version 2.83.1) to manipulate 3D hands. The base hands were taken from Haupt (2012). With the help of the software GIMP (Version 2.10.10), we colored the hands accordingly. This approach allowed us to control for any potential confound such as shape of the hand, its size, or the height of the finger lifting movement (see Figure 5.1). To assess group membership of the participants, we asked them at the end of the experiment to indicate their ethnic background.

To make sure that our results replicate when minor changes are made to the task, we varied a few aspects of the imitation-inhibition task between experiments (see Table 5.3 for details). First, we varied the presentation time of the base hand. Second, we slightly varied the number of blocks and the number of trials per block across experiments. In Experiments 1, 3, 4, 5, and 6, we presented each hand in each block in random order. In Experiment 2, we manipulated group membership block-wise. That is, we presented in each block a hand from one group. The order of the blocks varied randomly.

Figure 5.1

Trial Structure of a Congruent Trial Consisting of a Black Hand and an Incongruent Trial Consisting of a White Hand in Experiments 5 and 6



Perceived Similarity. In all experiments, we measured perceived similarity between the self and members of the in- and out-group after the imitation-inhibition task. In

Experiments 1 to 4, participants indicated on 7-point rating scales (1 = *agree not at all*; 7 = *agree very much*) their agreement with the following two statements: "An average American/German/Chinese person is similar to myself," "An average American/German/Chinese person is different to myself." To prepare data for analyses we averaged for each country the ratings to the first item with the reverse coded ratings to the second item, so that high values indicate high similarity. In Experiments 5 and 6, participants indicated their agreement on 7-point rating scales (1 = *agree not at all*; 7 = *agree very much*) with the following two statements: "An average white/black person is similar to myself", "An average white/black person is different to myself". To prepare data for analyses, for each group, we averaged the ratings to the first item with the reversed ratings to the second item, so that high values indicate high similarity.

Feelings of Affiliation. In Experiments 1, 2, 4, 5, and 6 we assessed how affiliated participants felt with members of the in- and out-group by adopting the items used in previous research (e.g., Genschow & Schindler, 2016). In Experiments 1, 2, and 4, participants answered on 7-point rating scales (1 = *not at all*; 7 = *very much*) the following questions: "How strongly do you identify yourself with the USA/Germany/China?", "How strongly do you share the same values as people from the USA/Germany/China?". To prepare data for analyses, we averaged the ratings for each country, so that high values indicate a strong affiliation feeling. In Experiments 5 and 6, participants answered on 7-point rating scales (1 = *not at all*; 7 = *very much*) the following questions: "How strongly do you identify yourself with white/black people?" "How strongly do you share the same values as white/black people?" To prepare data for analyses, for each group, we averaged the answers to the first question with the answers to the second question, so that high values indicate a strong affiliation feeling.

Table 5.3

Specifications of Stimuli and Trials

	Experiment 1	Experiment 2	Experiment 3	Experiment 4	Experiment 5	Experiment 6
Stimuli	USA vs. Germany vs. China	USA vs. Germany vs. China	USA vs. Germany vs. China	USA vs. Germany vs. China	Black vs. white hand	Black vs. white hand
Number of trials per group	32 (16 congruent; 16 incongruent)	32 (16 congruent; 16 incongruent)	32 (16 congruent; 16 incongruent)	32 (16 congruent; 16 incongruent)	80 (40 congruent; 40 incongruent)	80 (40 congruent; 40 incongruent)
Number of blocks	3	3	3	3	4	4
Total number of trials	96	96	96	96	160	160
Erroneous trials	16.05%	14.83%	19.09%	12.19%	15.47%	7.47%
Trials faster than 100 ms	1.89%	2.44%	3.00%	1.76%	1.86%	0.11%
Trials faster than 3 SDs of participant’s mean	0.06%	0.09%	0.09%	0.08%	0.45%	0.02%
Trials slower than 3 SDs of participant’s mean	2.77%	2.55%	1.99%	1.21%	1.43%	1.08%
Flag size	150 x 90 px	200 x 120 px	200 x 120 px	1300 x 780 px	-	-
Presentation time of base hand	500 ms	1250 ms	500 ms	500 ms	500 ms	500 ms

5.3 Results

To test our hypotheses, we conducted a fixed-effects meta-analysis across all experiments. We used a fixed-effects rather than a random effects meta-analysis because the latter does not adequately control for false positive rates when the number of included studies is small, as it is the case here (Borenstein et al., 2010; Field, 2001). This implies, however, that inferences are restricted to the set of included studies and do not necessarily generalize to other studies. That said, running exploratory random-effects meta-analyses instead of fixed-effects meta-analyses did not change any of the results for the present research. We analyzed the data with *R* (R Core Team, 2020; version 3.6.3) using the metafor package (Viechtbauer, 2010).

5.3.1 Manipulation Checks

In a first series of analyses, we conducted several manipulation checks.

Automatic Imitation. First, we tested for the presence of automatic imitation by analyzing the latencies of the imitation-inhibition task (see supplementary material for error rate analyses). This analysis indicated that over all group conditions, participants responded faster to congruent than to incongruent trials, $d_z = 1.81$, $SE = 0.04$, 95% CI [1.73, 1.89], $z = 43.22$, $p < .001$.

Similarity. Second, we tested whether participants perceived members of the in-group as more similar to the self than members of the outgroup. When collapsing across all experiments (i.e., Experiments 1 to 6), the results indicate that participants perceived members of the in-group as more similar to themselves than members of the out-group, $d_z = 0.70$, $SE = 0.03$, 95% CI[0.65, 0.76], $z = 24.53$, $p < .001$. Results from Experiments 1 to 4 show that US MTurkers perceived US citizens as more similar to themselves than German citizens, $d_z = 0.74$, $SE = 0.05$, 95% CI [0.64, 0.84], $z = 14.66$, $p < .001$, or Chinese citizens, $d_z = 1.03$, $SE = 0.06$, 95% CI [0.92, 1.14], $z = 18.62$, $p < .001$.

Feelings of Affiliation. Third, we tested whether participants indicated stronger feelings of affiliation for in-group members than for out-group members. When collapsing across all experiments (i.e., Experiments 1 to 6), the results indicate that participants reported stronger feelings of affiliation with members of the in-group than with members of the out-group $d_z = 1.00$, $SE = 0.03$, 95% CI [0.94, 1.07], z = 30.39, $p < .001$. Likewise, when analyzing only the results from Experiments 1, 2, 3, and 4, US MTurkers reported stronger feelings of affiliation with US citizens than with German citizens, $d_z = 1.18$, $SE = 0.07$, 95% CI [1.05, 1.30], $z = 17.82$, $p < .001$, or Chinese citizens, $d_z = 1.43$, $SE = 0.07$, 95% CI [1.28, 1.57], $z = 19.77$, $p < .001$.

5.3.2 Main Analyses

Automatic Imitation of In- and Out-Group Members. To test whether participants imitated members of the in-group more strongly than members of the out-group, we compared participants' congruency effect (i.e., the difference between congruent and incongruent trials) for in- and out-group trials (for more details, see Table 5.4). We restricted our analyses to latencies, as this measure is more reliable than the error rates (Genschow, van Den Bossche, et al., 2017). Nevertheless, we report the same analyses for the error rates in the supplementary material. The results across Experiments 1 to 6 indicate that the congruency effect for in-group trials did not differ from the congruency effect for out-group trials, $d_z = 0.02$, $SE = 0.03$, 95% CI [-0.04, 0.07], $z = 0.57$, $p = .567$. As can be seen in Figure 5.2, the group membership effect was not significant in any of the experiments.

Furthermore, the results across Experiments 1 to 4 indicate that the US MTurkers' congruency effects for US hands (i.e., the in-group) did not differ from the congruency effects for German, $d_z = 0.01$, $SE = 0.05$, 95% CI [-0.08, 0.10], $z = 0.16$, $p = .873$, or Chinese hands, $d_z = -0.01$, $SE = 0.05$, 95% CI [-0.10, 0.07], $z = -0.32$, $p = .751$.

Figure 5.2

Forest Plot for the Difference in Automatic Imitation Between In- and Out-Group Members

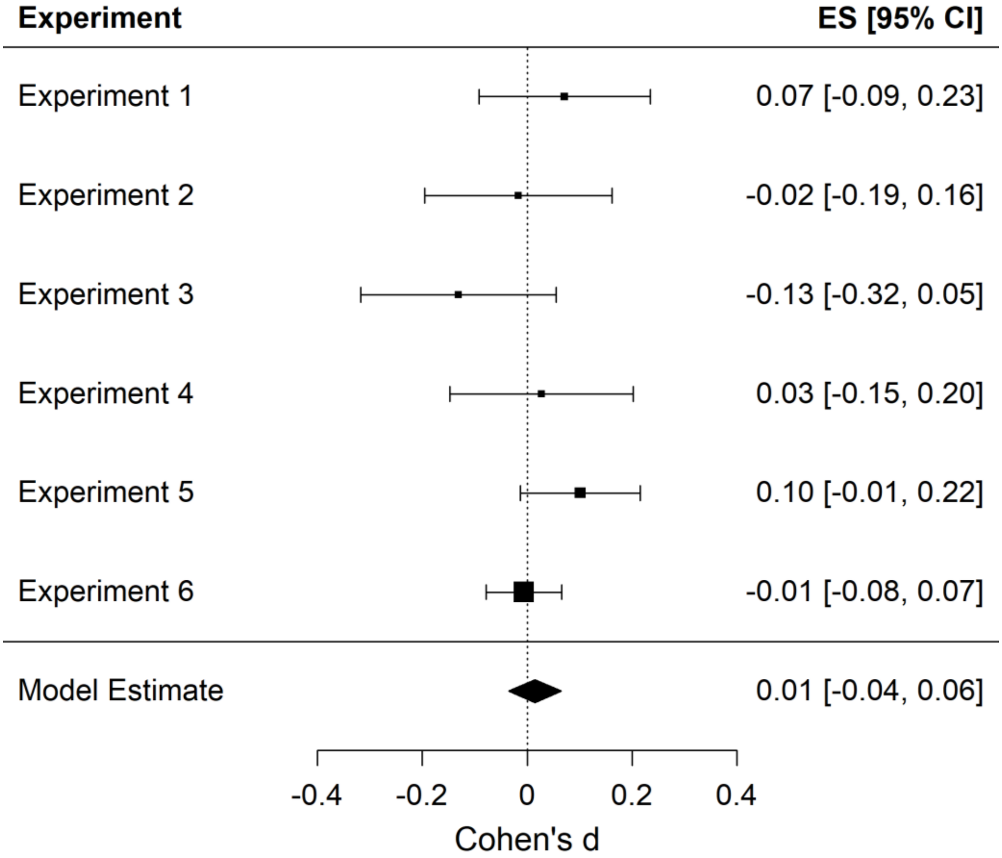


Table 5.4

Mean Values and Standard Deviations of Congruent and Incongruent Trials Within In- and Out-Group Trials

	Reaction times [ms]				Error rates [%]			
	In-group		Out-group		In-group		Out-group	
Exp.	Mean congruent trials (*SD*)	Mean in-congruent trials (*SD*)	Mean congruent trials (*SD*)	Mean in-congruent trials (*SD*)	Mean congruent trials (*SD*)	Mean in-congruent trials (*SD*)	Mean congruent trials (*SD*)	Mean in-congruent trials (*SD*)
1	564.09 (189.14)	624.68 (203.13)	567.60 (188.31)	622.61 (193.97)	2.15 (3.78)	7.07 (6.32)	1.98 (4.03)	6.10 (4.84)
2	625.42 (224.44)	702.78 (232.31)	637.42 (225.64)	715.86 (236.62)	1.06 (2.51)	5.13 (5.36)	1.04 (2.07)	5.14 (4.25)
3	629.94 (219.44)	685.28 (230.96)	629.21 (218.99)	693.62 (230.45)	3.03 (5.43)	6.75 (7.03)	2.72 (3.72)	6.34 (5.31)
4	686.05 (275.46)	759.47 (287.99)	679.79 (263.85)	751.38 (269.13)	1.30 (3.76)	4.53 (5.34)	1.19 (2.47)	4.09 (4.07)
5	761.49 (285.91)	839.02 (290.89)	766.38 (288.36)	838.64 (291.13)	1.72 (4.27)	5.76 (6.52)	1.59 (3.08)	6.03 (6.43)
6	473.73 (92.89)	550.51 (102.39)	475.38 (97.42)	552.40 (105.15)	1.01 (1.88)	5.19 (4.96)	1.06 (1.83)	5.36 (5.05)

Moderator Analyses. In a final series of analyses, we tested whether perceived similarity and feelings of affiliation moderate the influence of group membership on automatic imitation. To prepare data for analyses, we first computed across all experiments an in-out group imitation effect by subtracting the congruency effect for out-group members from the congruency effect of in-group members. Second, we computed the difference between perceived similarity for the in-group and the out-group as well as the difference between feelings of affiliation with the in- and out-group. Afterwards, we ran meta-analytical correlational analyses across all experiments. The results indicate that neither the in-out group similarity score ($r = 0.01$, $p = 0.672$), nor the affiliation score ($r = -0.01$, $p = 0.807$) correlated with the in-out group imitation score.

5.4 Discussion

A prominent prediction derived from different theories of imitation is that in-group members are imitated more strongly than out-group members. However, past research investigating this prediction produced rather mixed results. While some researchers found support for this predictions (Genschow & Schindler, 2016; Gleibs et al., 2016), others found the opposite (Rauchbauer et al., 2015, 2016) and yet others found no difference between the imitation of in- and out-group members (De Souter et al., 2021). To shed further light onto the group membership prediction, we tested it in six high-powered experiments (total $N = 1{,}538$). Across all experiments, the results indicated that group membership does not influence automatic imitation.

5.4.1 Reasons for the Null Finding

These results raise the question of why group membership does not modulate automatic imitation. First, one could argue that in our experiments group membership was not salient enough. However, this is rather unlikely, as we took great care in making clear to which group each presented hand belonged either by presenting the respective national flag together with the hand or by coloring the hand white or black. As the stimuli blatantly varied in terms of their group membership, we do not regard it as plausible that group membership was not salient enough during the imitation task. Moreover, as we found large differences between the in- and out-groups in terms of rated similarity and feelings of affiliation, it is also apparent that the groups represented meaningful and important social categories.

Second, it might be that hidden moderators influence the relationship between group membership and automatic imitation. We tested two of the most prominent moderators (i.e., similarity and feelings of affiliation), but could not find support for their influence. This is in line with related research from De Souter and colleagues (2021) who did not find an influence of affiliation motives on the relation between group membership and imitation either. In addition, De Souter et al. tested whether differences in directed attention to the in- versus out-group may moderate the relation between group membership and automatic imitation, but did not find support for this hypothesis. Nevertheless, other factors could still moderate the relationship between group membership and automatic imitation. For instance, some researchers suggested that perceiving anger in the other person (Rauchbauer et al., 2016) or being in a cooperation versus competition mindset (Gleibs et al., 2016) may influence the impact of group membership on automatic imitation. Our data do not allow testing this possibility, as they merely show that group membership itself does not influence automatic imitation. Thus, future research may investigate further moderators to test whether the relation between group membership and automatic imitation can be detected within specific conditions only.

Third, it is possible that group membership does not affect automatic imitation at all. Indeed, we regard this explanation as plausible, since we tested the prediction in large samples by manipulating group membership in two of the most explicit and extreme ways (i.e., nationality and ethnic group affiliation). If group membership does not play a role in such a setting, it most likely does not modulate automatic imitation.

5.4.2 Theoretical Implications

The finding that group membership does not influence automatic imitation has important implications for theories explaining social modulation of automatic imitation. Motivational theories of imitation (Chartrand & Dalton, 2009; Wang & Hamilton, 2012) argue that individuals use imitation as a tool to affiliate with others. As individuals have the general tendency to affiliate more with the in-group than with the out-group (van der Schalk et al., 2011), in-group members should be more strongly imitated than out-group members. Based on this reasoning, the relationship between group membership and automatic imitation should be moderated by feelings of affiliation. Motor learning theories (e.g., Brass & Heyes, 2005; Greenwald, 1970; Heyes, 2010; Prinz, 1990, 1997) predict that in-group members should be imitated more strongly than out-group members, because in-group members are perceived as more similar to the self than out-group members. Thus, differences in perceived similarity should moderate the influence of group membership on automatic imitation. Our results do neither support the assumptions derived from motivational theories nor the ones derived from motor learning theories, because (1) group membership did not influence automatic imitation and (2) neither feelings of affiliation nor perceived similarity moderated the relationship between group membership and automatic imitation. It is important to note that these results do not question the general validity of motivational and motor learning theories of imitation, but rather limit the range of their predictions by suggesting that the postulated principles of social modulation do not translate to the influence of group membership.

Interestingly, the conclusion that automatic imitation is not affected by group membership fits to several other recent findings illustrating the resilience of automatic imitation against social modulations. For example, recent research found difficulties in replicating correlations between automatic imitation and different interindividual differences including autism-like traits, narcissism, empathy, and perspective taking (Butler et al., 2015; Cracco, Bardi, et al., 2018; Galang & Obhi, 2020; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019). Likewise, Khemka et al. (2021) could not replicate the finding that sitting in front of a mirror reduces automatic imitation (Spengler, Brass, et al., 2010).

Together with this literature, our findings contribute to a current debate in the literature about the degree to which automatic imitation, and the imitation-inhibition task in particular, is driven by social processes. Ramsey (2018) argues that automatic imitation in the imitation-inhibition task is the result of a combination of several different underlying processes, which are neither necessarily related to imitation, nor to other forms of social behavior. In contrast to this view, Cracco and Brass (2019) argue that the imitation-inhibition task measures covert imitative response tendencies associated with some (but not all) types of overt imitation. Based on this view, one could conclude that imitation is a social process.

However, this does not necessarily mean that automatic imitation can be socially modulated. Our results suggest that at least in the case of group membership, automatic imitation is not socially modulated. At the same time, it is important to note that other experiments indicate that the imitation-inhibition task can be socially modulated. For example, research demonstrated that individuals engage in stronger imitative behavior when they observe human as compared to non-human actions (Klapper et al., 2014; Liepelt & Brass, 2010b; Press et al., 2005, 2006), when they observe social as compared to antisocial gestures (Cracco, Genschow, et al., 2018), or when they focus on others as compared to the self (Cracco et al., 2019; Genschow, Schuler, et al., 2019; Hogeveen & Obhi, 2011; Leighton et al., 2010; Wang & Hamilton, 2013). As some (but not all) of these studies were based on a small number of participants, future research should aim at replicating these findings with larger samples. The results will further our understanding on the social processes underlying automatic imitation.

5.4.3 Limitations and Future Directions

Besides these implications, several limitations of our experiments need to be discussed. First, one may argue that the influence of group membership on automatic imitation is smaller than expected and that we did not have sufficient power to detect such a small effect. In this respect, it is important to note that each of our experiments (total *N* = 1,538) was powered to detect even a small effect of *dz* = .25. Moreover, Experiment 5 and 6 included a sufficient number of participants to detect even effects that are smaller than *dz* = .2 with more than 90 % of power.

Second, in contrast to previous research, we did not use artificial groups or minimal group paradigms to manipulate group membership, but instead assessed existing groups such as persons from different countries or persons differing in skin color. Nevertheless, it could be that our manipulations were still too artificial to detect the predicted effect. In Experiments 1 to 4, participants needed to associate the hands in the gloves with the persons from different nationalities. In Experiments 5 and 6, we circumvented this issue by presenting participants with white and black hands. Yet, to control for any potential confound, we created the stimuli with a computer software, which might have made the hands look artificial. Thus, in future research, one may use photographs of actual hands instead.

Third, as we tested the influence of group membership in online settings in which participants did not personally interact with the imitated person, it could be that participants' motivation to affiliate was reduced. However, it is important to note that we found significant differences between in- and out-group members in terms of perceived similarity and feelings of affiliation despite the online character of our experiments. Moreover, in lab experiments that use the same imitation-inhibition task, participants do not personally interact with the other person either. Thus, we regard it as rather unlikely that the found null effects can be explained by the online setting of our experiments. Nevertheless, it might well be that the lack of personal interaction is the reason why automatic imitation is not modulated by group membership. Thus, future research could assess the effect of group membership by using other imitation tasks, such as mimicry tasks (Chartrand & Bargh, 1999; Genschow, Klomfar, et al., 2018), in which participants personally interact with a confederate.

5.4.4 Summary

A key prediction derived from different theories of automatic imitation suggests that imitation is stronger when observing actions from in-group members, as compared with out-group members. While previous research testing this prediction produced mixed results, our results clearly demonstrate that group membership per se does not influence automatic imitation. Moreover, our results show that neither perceived similarity nor feelings of affiliation moderate the influence of group membership on automatic imitation. These results challenge to some degree some of the predictions derived from motivational and learning theories of imitation.

5.5 Supplementary Material

5.5.1 Experiment 3 and 4

Facial Stimuli. In Experiment 4, we presented in addition to the hands, face pictures of either US, German, or Asian looking persons. We let ten participants (7 women, 3 men), with an age between 20 and 60 (*M* = 31.60, *SD* = 15.11), rate the faces. Participants rated for each person the degree to which it looked typically American, German, and Chinese on 7-point rating scales (1 = *not at all*; 7 = *very much*). Table 5.5 illustrates the average ratings for each face.

Table 5.5

Average Ratings of the Three Faces Used in Experiment 4

Target Face	American [*M, SD*]	German [*M, SD*]	Chinese [*M, SD*]
WM-213	5.50, 1.72	3.70, 1.70	1.00, 0.00
WM-214	4.30, 1.90	5.60, 1.35	1.10, 0.32
AM-210	1.40, 0.70	2.10, 1.20	6.30, 0.82

Note. For all faces, from the Chicago Face Database, we took the face with a neutral expression.

Additional Measures. In Experiments 3 and 4, we assessed two further measures besides feelings of affiliation and perceived similarity. First, we assessed the Inclusion of Other in the Self (IOS) scale (Aron et al., 1992). Participants saw for each country a Venn diagram-style measure that consisted of seven pairs of increasingly overlapping circles in which one circle represented the self and the other circle represents the average person from another country (i.e. US, Germany, and China). Participants indicated for each country the degree to which their self overlapped with the other person. To prepare the data for analysis, we computed an IOS index. First, we averaged the ratings for both out-groups (i.e. Germany and China) and then subtracted the averaged out-group rating from the in-group rating. High values indicate relatively more overlap of the self with members from the in-group as compared to the out-group.

Second, we assessed pro-social attitudes towards members of the in-group and the out-group. In particular, participants answered on 7-point ratings scales (0 = *0 Dollars*; 6 = *6 Dollars*) the following question: “Imagine you would get 6 Dollars. How much would you donate to a US/ German/ Chinese charity?”. To prepare the data for analysis, we computed a pro-social attitude index by averaging the ratings for the two out-groups (i.e., Germany and China). Afterwards, we subtracted this average from the rating of the in-group (i.e. USA). High values indicate a stronger prosocial attitude towards the in-group as compared with the out-group.

Results. First, we computed an in-out group imitation index by subtracting the congruency effect for out-group members from the congruency effect for in-group members. Afterwards, we correlated this imitation index with the IOS index and the pro-social attitude index. The imitation index did not correlate significantly with the IOS and pro-social attitude indices in any of the experiments (see Table 5.6 for an overview), except for the reaction times IOS index in Experiment 4, which, however, was hardly significant and opposite to the expected direction.

Table 5.6

Intercorrelations (r) Between the Imitation Indices and the IOS Index As Well As the Pro-Social Attitude Index in Experiments 3 and 4

Imitation index	IOS	Pro-social attitude
RT in Experiment 3	.10	.06
ER in Experiment 3	-.11	-.09
RT in Experiment 4	-.16*	-.01
ER in Experiment 4	.03	-.01

Note. RT (Reaction times); ER (Error rates)
* $p < .05$

Error Rate Analyses. In additional meta-analyses, we tested the prediction that group membership moderates automatic imitation by analyzing the error rates of the imitation-inhibition task. First, we tested for the presence of automatic imitation. This analysis indicated that over all group conditions, participants in Experiments 1 to 6 made fewer errors on congruent than on incongruent trials, $d_z = 1.00$, SE = 0.03, 95% CI = [0.94, 1.06], $z = 32.03$, $p < .001$

Second, we tested whether the congruency effect of the error rates differed between in- and out-group trials. The meta-analytical analyses indicate that the mean values did not differ from each other, $d_z = -0.01$, SE = 0.03, 95% CI = [-0.06, 0.04], $z = -0.31$, $p = .758$.

Finally, we investigated whether perceived similarity and feelings of affiliation moderate the influence of group membership on automatic imitation measured with the error rates. To prepare data for analyses, we computed across all experiments an in-out group imitation effect by subtracting the congruency effect for out-group members from the congruency effect of in-group members. Afterwards, we computed the difference between perceived similarity for the in-group and the out-group as well as the difference between the feeling of affiliation with the in- and out-group. Meta-analytical correlational analyses across all experiments indicate that the in-out group similarity score did not significantly correlate with the in-out group imitation score (*r* = -.03, *p* = .252). However, we the affiliation score correlated significantly with the in-out group imitation score (*r* = -.07, *p* = .008). However, the correlation went into the opposite direction as we had predicted.

Chapter 6

General Discussion

Previous research regarding automatic imitation with the imitation-inhibition task has mainly taken part in the laboratory (e.g., Brass et al., 2000; Catmur & Heyes, 2011). Over two decades, researchers found that automatic imitation is modulated by different (social) manipulations (e.g., Cracco, Bardi, et al., 2018; Heyes, 2011). Nevertheless, as laboratory research is effortful and uneconomic, and in the context of some failed replications with the imitation-inhibition task (e.g., Butler et al., 2015; De Souter et al., 2021; Genschow, van Den Bossche, et al., 2017), it is worth designing a method that can investigate high-powered modulations. The research in this dissertation takes this step and presents an online version of the imitation-inhibition task in Chapter 2. The chapter shows that the online version of the imitation-inhibition task generates robust results, that are similar to its laboratory counterpart, not only for the basic version of the task (Experiments 1 and 2), but also for its spatially controlled version (Experiment 3).

Further, the research presented in this dissertation adds to the mixed results of modulations with the imitation-inhibition task in the literature. As there have been problems replicating many of the common modulations in recent times, particularly social modulations (e.g., Butler et al., 2015; De Souter et al., 2021; Galang & Obhi, 2020; Genschow, Pauels, et al., 2023; Genschow, van Den Bossche, et al., 2017; Müller et al., 2013; Newey et al., 2019), the online version of the task has now made it possible to conduct high-powered experiments and investigate some of these modulations in detail. Using this method, Chapter 4 and Chapter 5 did not find any effect and thus fit with the mixed literature.

Chapter 4 suggests that the relationship between interindividual factors and automatic imitation is less generalizable and universal than previously reported (e.g., Heyes, 2011), as no effect was found in the relationship between belief in free will and automatic imitation despite high statistical power. Chapter 5 showed that group membership manipulated via country affiliation also has no effect on automatic imitation, thereby contributing to the failed replications regarding automatic imitation and group membership (e.g., De Souter et al., 2021). However, it must be noted that most of the manipulations in both chapters worked via top-down beliefs. Only the last two experiments in Chapter 5 attempted to manipulate ethnic group membership via skin color, with bottom-up manipulated stimuli.

In the literature, one of the strongest and most reliable, typically bottom-up manipulated, effects using the imitation-inhibition task is the animacy effect (i.e., the finding that human hands are imitated more strongly than non-human hands; e.g., Bird, Leighton, et al., 2007; Haffey et al., 2013; Klapper et al., 2014; Press, 2011; Press et al., 2005, 2006). We replicated this effect online in Chapter 2.5. In Chapter 3 I argued that the strong effect might be explained by a potential confound of finger movement size in the animacy stimuli. Nevertheless, although finger movement size does indeed lead to a change in automatic imitation (Chapter 3.2 & Chapter 3.4), the animacy effect remained stable and strong with stimuli that control for finger movement size (Chapter 3.5). Nonetheless, in Chapter 3 we found that even small differences in the size of the finger movement made a considerable difference to the magnitude of automatic imitation. This finding emphasizes that fundamental features of the imitation-inhibition task should not be omitted in research and highlights the importance of controlled stimuli when using the imitation-inhibition task to avoid confounds. Additionally, researchers should at least be aware that even though we found no effect of speed of the finger lift movement on automatic imitation (Chapter 3.3 & 3.4), the speed of the finger lift might still matter in other context. However, the animacy effect remains one of the strongest and most reliably replicable effects in the automatic imitation literature.

Thus, the imitation-inhibition task appears to be modulable, also socially, although not as universally modulable as previously thought. Apparently, bottom-up manipulated approaches might be more effective to achieve effects with the imitation-inhibition task. In the *online* imitation-inhibition task, only the bottom-up approaches showed any effects with regard to the animacy effect, but also the finger movement size. Top-down modulated effects, directly manipulated or in a correlative approach regarding interindividual differences, had little or no impact on automatic imitation. However, even bottom-up approaches do not always work flawlessly, as the manipulation of the last two experiments in Chapter 5 showed. These observations based on our studies with the online imitation-inhibition task are not necessarily generalizable explanations. Therefore, I would like to subject them to a more precise test, which is why I will carry out a respective *p*-curve analysis considering social top-down and bottom-up modulations of automatic imitation in Chapter 6.3. Before that, however, I would like to first critically review the online version of the task.

6.1 The Imitation-Inhibition Task in the Online Environment

The online task we developed is as efficient as the laboratory version, despite the different environment. Its basic automatic imitation effects are equally strong and reliable as the effects of its laboratory counterpart, as we consistently showed in four experiments in the validation of the task in Chapter 2, as well as throughout Chapters 3, 4, and 5 in each experiment. These effects, despite collected in an online environment, are comparably strong to the effects of the task performed in the laboratory (cf., Cracco, Bardi, et al., 2018).

Moreover, the online task is easy and variable to implement for researchers, as it may also be implemented and adjusted by researchers with limited programming knowledge, making it suitable for teaching and student projects. Unlike more traditional programming software toolkit solutions (e.g., Labvanced, https://www.labvanced.com/; Inquisit, https://www.millisecond.com/), participants don't need to download files or be redirected to external platforms, enhancing data accessibility and collection for researchers. Lastly, the task works consistently across various computers, keyboards, monitors, and browsers.

Beyond practical benefits, the online imitation-inhibition task broadens research possibilities in automatic imitation. It enables high-powered experiments, addressing issues of replicability and allowing exploration of research questions with small effect sizes. As small samples were criticized as being less informative, lead to inaccurate parameter estimates, and may lead to replicability issues (Brandt et al., 2014; Szucs & Ioannidis, 2017), the online version now facilitates high-powered replications of previous experiments on automatic imitation. It opens as well avenues for diverse research questions, such as cross-cultural variations in imitation, which was so far not possible with the laboratory version of the imitation-inhibition task (cf., Chapter 1.5.4). Moreover, the online task helps addressing criticisms of collecting WEIRD samples (i.e., participants from western, educated, industrialized, rich, and democratic nations; Henrich et al., 2010; Muthukrishna et al., 2020). With its global reach, researchers now can obtain representative samples from different parts of the world, moving away from reliance on typical college student samples (Hanel & Vione, 2016).

Nevertheless, the data that can be collected with the online imitation-inhibition task is only as good as the participants take their participation in the task seriously. The exclusion rate for the online samples was notably higher than for the laboratory sample (cf., Chapter 2.3), with the same exclusion criteria applied throughout all chapters. This difference is attributed to some online participants intentionally making errors, such as not raising their finger, leading to fewer trials in different conditions. Recognizing the challenge of lower data quality in online research (De Man et al., 2021; Gosling et al., 2004; Gosling & Mason, 2015), we recommend pre-registering exclusion criteria for online imitation-inhibition experiments and using appropriate online panel providers for data collection (e.g., Chmielewski & Kucker, 2020). Researchers should anticipate a potentially higher exclusion rate when determining the sample size, aligning with common practices in other online experiments (Buhrmester et al., 2018; De Man et al., 2021). Despite this, the results from the imitation-inhibition task online align closely with its laboratory counterpart.

6.2 Potential Reasons for the Problems of Modulating Automatic Imitation

The online version of the imitation-inhibition task appears to work very well. It generates similar results to its laboratory version. As such, the online task also shows similar problems to replicate some of the findings with social variables. Thus, this dissertation and its chapters join the literature on automatic imitation, which presents mixed results regarding modulations in the laboratory, particularly social modulations, and extends this to high-powered studies in the online environment. However, the key question arises as to the cause of these inconsistent results.

6.2.1 A Critique of the Imitation-Inhibition Task Format

The online imitation-inhibition task is an exact replica of its last laboratory versions, with the same presentation times for the pictures and a response via the keyboard. However, this exact design of the imitation-inhibition task as a reaction time task may be one of the reasons for the mixed results in modulations of automatic imitation, particularly regarding social modulations. In general, the imitation-inhibition task is a reaction time task regardless of whether it is conducted in the laboratory or online: The task consists of congruent, incongruent and neutral trials, instructs participants to respond to a cue, presents a distractor stimulus at the same time and measures reaction time and error rates as dependent variables (e.g., Verbruggen et al., 2005; Zhang et al., 1999). All of these features are common to typical reaction time tasks, other examples being the Simon Task (Simon, 1990), the Posner Cueing task (Posner, 1980), Stroop test (Stroop, 1935), go/no go task (Logan, 1980), or the Eriksen Flanker task (Eriksen & Eriksen, 1974), to name just the most popular ones, which have a similar structure (e.g., Hedge et al., 2018). However, many of these cognitive tasks show problems with the investigation of social variables and individual differences (e.g., Augustinova & Ferrand, 2014; De Houwer & De Bruycker, 2007; Dolk et al., 2014; Puffe et al., 2017). Strong reaction time tasks, which show robust results, yield these strong effects because the between-subject variability is low, resulting in all participants displaying similar effects (Hedge et al., 2018). However, low between-subject variability also leads to low reliability for individual differences, which is why in the end hardly any change can occur in reaction time tasks (Benesty et al., 2009; Hedge et al., 2018). In the imitation-inhibition task, it is also striking how robust and reliable the actual automatic imitation effect (i.e., congruency effect) remains, even under different modulations (Brass et al., 2000; Cracco, Bardi, et al., 2018; Genschow, van Den Bossche, et al., 2017). Moreover, the imitation-inhibition task was initially not directly designed to measure interindividual differences (Cracco & Brass, 2019). Thus it is not surprising, that the probability of correlation with other measurements may already be reduced due to the construction of the task, making it resistant to interindividual differences (like for example with free will; Chapter 4). Moreover, there is the possibility that the strength of the task, due to its form, also extends to general and social direct manipulations: If there is little to no variation in the task itself, the automatic imitation effect may be too strong to be modulated in any way. This at least reduces the probability of finding any small manipulation effects in the reaction times of the task. Following this argument, manipulations that are not strong enough might not be sufficient to induce the robust and strong imitation-inhibition task to change.

Likewise, it could be that the trials in the imitation-inhibition task are too fast-paced so that participants do not internalize a (social) manipulation. As is the case with perceiving goal-directedness in actions in automatic imitation (Cracco, Bardi, et al., 2018), the encoding of some social manipulations in the task may be too slow to have an effect on automatic imitation. The authors of a recent meta-analysis found an effect of goalless, but not goal-directed movements (Cracco, Bardi, et al., 2018). They argue that constructing a motor goal in the sense of movement coding (i.e. just following the movement without further processing) is less abstract and less difficult than in the sense of goal coding. Goal coding might need more time to be processed, thus having little influence on fast-paced automatic imitation paradigms. A similar logic might apply to the processing of social variables with the task. Possibly, the coding of social variables is slower than the faster movement coding, whereby the latter always retains the upper hand over most social modulations (i.e. there is no change in the automatic imitation effect). As Cracco, Bardi, et al. (2018) suggest, one way to investigate this issue would be to use different stimulus-onset asynchrony (SOA), as we also tried in Chapter 5 in Experiment 2. However, here we also manipulated the conditions block-wise in addition to the SOA, so changing to longer SOA may have had less impact, hence the null effect. However, other authors achieved success with the manipulation of the SOAs and found stronger influences of social variables when longer SOAs (i.e. more processing time) are present (Cracco, Genschow, et al., 2018). Besides social modulations and goal-directedness, this argument may of course also be applied to other general modulations of automatic imitation.

Furthermore, it might be that the imitation-inhibition task (both in the laboratory and online) leaves too little overlap between stimulus and response when being modulated. A recent meta-analysis (Cracco, Bardi, et al., 2018) found enhanced automatic imitation when participants' response matched the stimulus movement not only visually but also in their sensory outcomes, for example, if participants not only see the image of a finger lifted while they themselves are supposed to release a key, but the image of a finger that also releases a key with its finger lifting movements. The authors concluded that automatic imitation differs in situations in which the stimulus-response overlap is maximized vs. neglected: The effects are stronger when participants actually match the model’s performance in the picture (Cracco, Bardi, et al., 2018). This criticism can be transferred to manipulations by other variables: It might be that stimulus response overlap is too weak with some manipulations, thus not altering the general strong automatic imitation effect enough to generate an effect. This would explain why top-down manipulations in particular are difficult to replicate, as the stimulus response overlap is not aligned with the actual modulation, as the hand stimuli remain the same in every condition. It also relates directly to the similarity between model and participant and suggests that (social) manipulations that involve making the participant feel as similar or dissimilar as possible to the model in terms of stimulus-response overlap may work better than (social) manipulations that ignore stimulus-response overlap completely.

Finally, it could of course be that the reason why modulations do not work with the task is that many of the previous modulations are simply too weak. Regarding social modulations the small effects of some of these social modulations in the laboratory could also be an indication for this argument (Cracco, Bardi, et al., 2018). However, with explicit reference to the social modulability of the task, it might also indicate that automatic imitation itself is not or hardly socially modulable at all, which is why only some but not all social variables modulate imitation.

6.2.2 Modulations of Automatic Imitation and the Online Format of the Task

Of course, it is possible to criticize the online format as well in this context. Even if the online task per se produces good and reliable automatic imitation effects, participants online are not always as attentive as participants in the laboratory and barriers to understanding are not easy to overcome (e.g., Dandurand et al., 2008; De Man et al., 2021). With the practice phases, we have eliminated the possibility of not-understanding the general operation of the task, thus participants should have understood the general instruction of the task by the start of the experimental phase. Because attention problems can also occur in the laboratory, inattentive participants with too many errors are excluded from the analyses in both online and the laboratory version at the end by means of specific pre-defined exclusion criteria. Nevertheless, this does not mean that the participants also process and internalize the modulations attentively. It could be that the participants basically understand the task and how they should react to the number cues, but pay little or no attention to the modulations, so that they miss their impact. In fact, in our experiments, we did not use practice phases to ensure that the modulations were also internalized.

Similarly, there may be certain modulations that are more difficult or impossible to manipulate within an online study. Participants online, for example, are less motivated to complete complicated tasks (De Man et al., 2021), which is why modulations that require additional work (e.g., scrambled sentence tasks or priming tasks with personal contribution; cf., Leighton et al., 2010) may be less appropriate for online studies than for laboratory studies. Also, certain cover stories are impossible to implement in an online setting (e.g., a supposed later cooperation between the person raising the finger and the participant; cf., Gleibs et al., 2016). In this case, conducting the task in a laboratory setting would be more practical as well. Of course, all such problems can also occur in laboratory experiments and should generally be taken into account in the future.

6.3 Critical View of Modulations With the Imitation-Inhibition Task

Both this thesis using the online imitation-inhibition task and the literature on modulations, particularly social modulations, using the imitation-inhibition task in the laboratory show that modulations are not as unambiguous and universal as previously thought and that not all modulations can be replicated (Cracco, Bardi, et al., 2018; Heyes, 2011; Westfal et al., 2021). The results of this dissertation, taken together with the results of previous research, suggest that top-down modulations are difficult to replicate, whereas bottom-up modulations are easier to replicate.

6.3.1 *P*-Curve Analyses

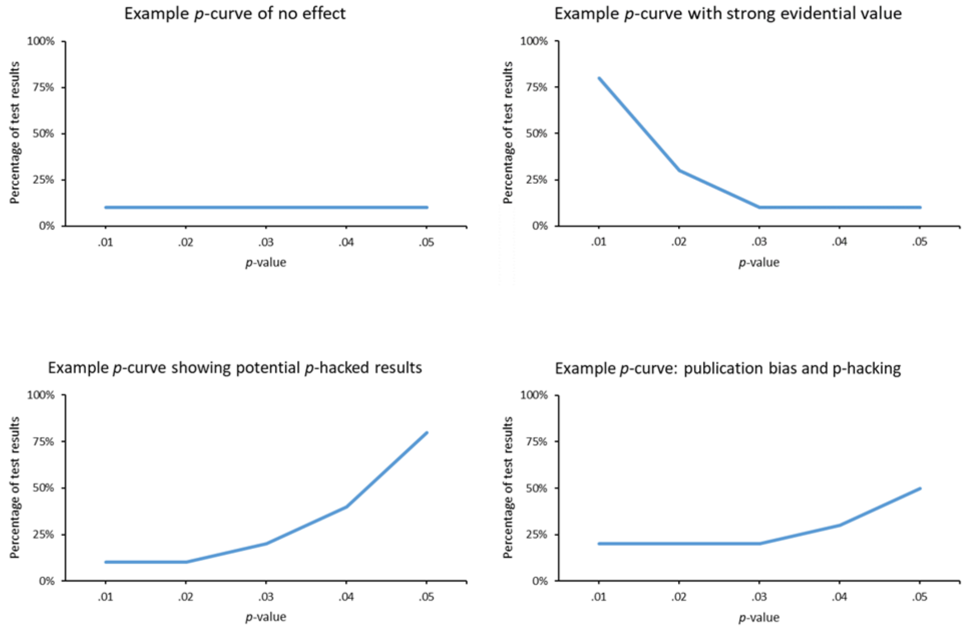
To further test this observation, I carried out a *p*-curve analysis based on the previous laboratory results on social modulations with the imitation-inhibition task. I have only included experiments on social modulations of automatic imitation to sharpen the focus of the *p*-curve and reduce the scope of the included studies, as a set of studies with too much variability in the designs can make the *p*-curve more difficult to interpret (Simonsohn et al., 2014a). A further rationale was that the debate of replication problems regarding automatic imitation in the literature oftentimes focuses on modulations with social variables in particular (e.g., Cracco, Bardi, et al., 2018; Genschow et al., 2022). A *p*-curve is the distribution of statistically significant *p*-values for a group of studies (Simonsohn et al., 2014a). These studies usually follow a predetermined hypothesis. The *p*-curve plots the significant *p*-values (.01 to .05) according to how often they occur as a percentage in the set of studies to infer the presence or absence of evidence. If the *p*-curve shows an excess of *p*-values well below the significance threshold (e.g., many *p*-values < .02; i.e., a right-skewed distribution), it may indicate the presence of true effects rather than mere chance. This is because true effects are more likely to lead to lower *p*-values (Simmons & Simonsohn, 2017; Simonsohn et al., 2014a; Veling et al., 2020). Conversely, a flat or left-skewed distribution could indicate a lack of strength of evidence. In addition, a highly left-skewed distribution may also indicate data mining (i.e., misuse of data analysis to find patterns in the data that can be presented as statistically significant), *p*-hacking (i.e., misuse of null hypothesis significance tests) or whether publication bias (i.e., studies with non-significant results are less likely to be published) exists (CEGA Berkeley, 2018; Simonsohn et al., 2014a).

The actual *p*-curve analysis interprets the strength of evidence of this distribution of *p*-values derived from multiple studies (Simmons & Simonsohn, 2017; Simonsohn et al., 2014a, 2014b). In the current approach, the *p*-curve analysis combines in its test for right-skewness both the skewness of the full (i.e., the distribution of $p < .05$ results) and half *p*-curve (i.e., the distribution of $p < .025$ results) into a single analysis. In particular, when the half *p*-curve test reveals a right-skewed distribution with $p < .05$, or both the half and full tests reveals a right-skewed distribution with $p < .1$, the total *p*-curve analysis implies the presence of evidential value (Simmons & Simonsohn, 2017; Simonsohn et al., 2014a). In this regard, the analysis also includes the half *p*-curve as this approach is more robust regarding distributions containing *p*-hacked values, as possible *p*-hacked values (i.e., *p*-values above .025) are not included in the half *p*-curve (Simonsohn et al., 2015).

The idea behind the general *p*-curve analysis is that an existing effect produces significantly more *p*-values of $p = .01$ than *p*-values that are $p = .05$. Thus, the *p*-curve of such an existing effect would be right-skewed. An effect that does not exist would produce a p-curve that would be very flat and uniformly distributed. A *p*-curve based on *p*-hacked and data mined papers, on the other hand, would be more left-skewed, as there would be significantly more *p*-values at $p = .05$. A *p*-curve that points to publication bias would also be left-skewed, as researchers would try to make their results publishable by *p*-hacking. However, at the same time such *p*-curves often have a low power, as there are fewer studies on the topic that are included in the *p*-curve. Thus, such *p*-curves are flatter than those of truly existing effects in which publication bias is inherently not a problem (Simonsohn et al., 2014a). Examples are shown schematically in Figure 6.1.

Figure 6.1

Example P-Curves of Different Set of Results



Note. These graphs are not based on actual data. The *p*-curves just depict different possibilities of how a *p*-curve can be shaped in these different situations. Other shapes can also occur and these are just examples.

In addition to the test of right-skew, the test for 33% power of the *p*-curve is deemed important. This tests for evidential value of very small effects. If the right-skew test shows that there is hardly any evidential value, the possibility still exists that the effect is simply too small to be evidential in the direct test. However, the 33% test increases the probability of finding small effects under the assumption that the effects were too underpowered (Simonsohn et al., 2014a). Thus, when a *p*-curve is not significantly right-skewed, the test of 33% examines whether the *p*-curve is flatter than one would expect if studies were powered at 33%. If a series of studies shows a *p*-curve that is significantly flatter than the curve expected from studies powered at 33%, the findings lack evidential value and are inconclusive. This implies that the effects of interest, even if they exist, are too small for existing samples. Thus, researchers interested in the effect would consequently need to conduct new studies with higher statistical power to gather data with evidential value. The authors suggest for the interpretation of the tests for flatness with the 33% power test, that if the test is $p < .05$ for the full *p*-curve or both the half *p*-curve and binomial 33% power test are $p < .1$, evidential value is inadequate or absent. The binomial 33% power test compares the observed proportion of significant results that are $p < .025$ to the expected proportions when studies have 1/3 power. Theoretically, the power of all the 33% power tests can also be chosen differently. Simonsohn et al., (2014a) suggest this test at one-third power, because in psychological statistical tests the one-third cut-off is common (e.g., Bayes factor, power analysis, *p*-value). Thus, I will also stick to this threshold.

This combined testing approach, introduced by Simonsohn et al. (2015), proves to be more resilient against *p*-hacking compared to the first introduced simpler full *p*-curve test. Finally, a *p*-curve analysis can be used to determine the general power estimate of the effect under investigation, as for any given sample size, the bigger the effect, the more right-skewed the expected *p*-curve becomes (Simmons & Simonsohn, 2017).

With the *p*-curve analysis, I will first examine all studies on social modulation with the imitation-inhibition task. Next, I will examine the studies with top-down modulations and bottom-up modulations in separate *p*-curve analyses. According to the common approach for *p*-curve analyses by Simonsohn et al. (2014a), I defined rules on which I selected the studies. I only included studies using the imitation-inhibition task (i.e., no studies with other automatic imitation paradigms), only studies investigating social modulation with the imitation-inhibition task, no correlational studies (because causality is not given, most correlational studies do not report all necessary statistics, and to prevent the *p*-curve from too many varying designs), and only studies reporting the statistics for the corresponding effect (i.e., mostly the interaction). Following the rule definition, I conducted a literature research on Pubmed and Google Scholar using the following search terms: “automatic imitation social” OR “automatic imitation social modulation” OR “social modulation imitation inhibition” OR “social imitation inhibition” OR “automatic imitation animacy” OR “automatic imitation top-down” OR “imitation inhibition top-down” OR “automatic imitation bottom-up” OR “imitation inhibition bottom-up” in the time period from 2015 to 2023 as a recent meta-analysis (Cracco, Bardi, et al., 2018) covers the time period from the first publication of the imitation-inhibition task (Brass et al., 2000; Stürmer et al., 2000) until 2015. Additionally, I screened the references of the recent meta-analysis (Cracco, Bardi, et al., 2018) for social modulations with the help of the included and coded reference tables. Based on the proposed rules, I included a total of 52 results out of 49 studies in the overall *p*-curve analysis for social modulation effects with the imitation-inhibition task; 23 results of 21 studies with bottom-up modulations and 29 results of 28 studies with top-down modulations (cf., Appendix). The *p*-curve analysis was performed with R (R Core Team, 2022; Version 4.3.0). Due to the fact that not all studies calculated the exact *p*-values, I used the method and provided R code of Simmons et al. (2014a, 2015) for the continuous test, to recalculate the *p*-values for the *p*-curve from the reported statistics and converting them to Z-scores before dividing these Z-scores by the square-root of the number of tests included (Simonsohn et al., 2014a, 2015). This method makes the resulting *p*-curve more resistant to influence of *p*-hacking and selective reporting of *p*-values. Additionally, this code provides an approximate power estimate of the set of studies with a 90% confidence interval.

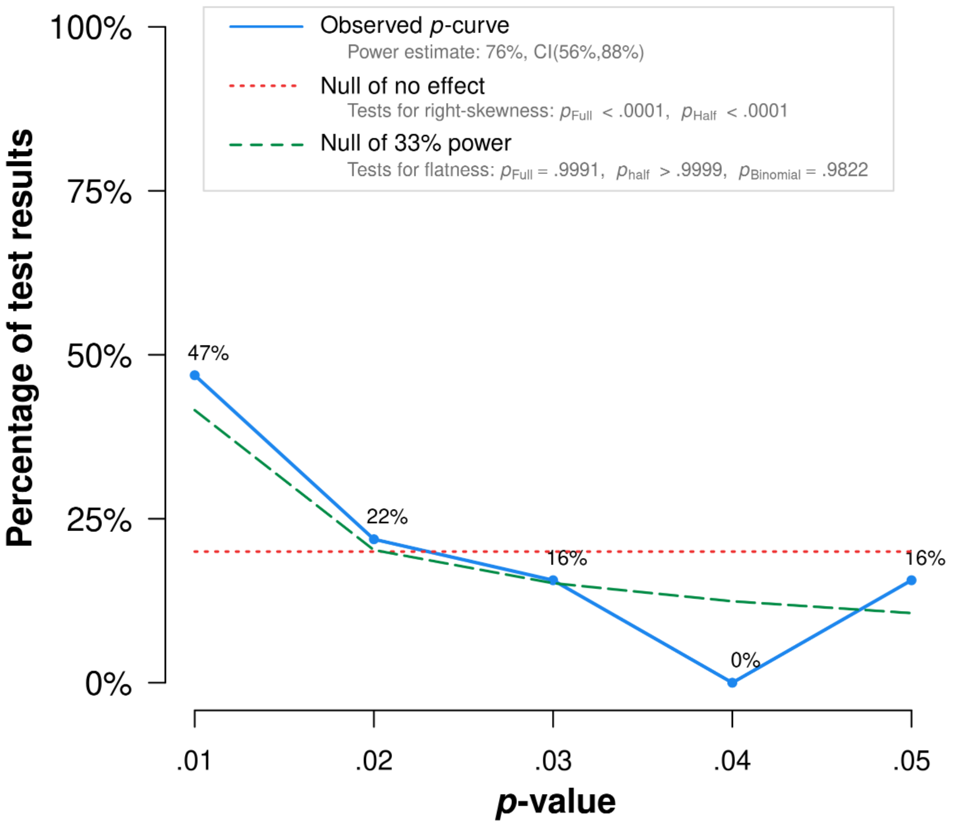
Based on the results of the dissertation, I assumed that the *p*-curve of the previous top-down modulations with the imitation-inhibition task shows no indications of evidential value, but the *p*-curve for bottom-up manipulations shows evidential value. I did not have a hypothesis about the *p*-curve of all social modulations taken together.

6.3.2 *P*-Curve of Overall Social Modulations With the Imitation-Inhibition Task

The *p*-curve of overall social modulation effects includes 32 statistically significant results (i.e., < .05), with 27 results under *p* < 0.25. Twenty additional results were entered but excluded from the *p*-curve and its analysis because they had *p*-values > .05.

Figure 6.2

The P-Curve and its Analysis for the Overall Social Modulation Results With the Imitation-Inhibition Task



Note. The *p*-curve includes 32 statistically significant results (i.e., *p* < .05), with 27 results under *p* < .025. Twenty additional results were entered but excluded from the *p*-curve and its analysis as they were *p* > .05. The blue line depicts the actual observed *p*-curve from the set of results. The red dotted line depicts how the line of *p*-values would look like if there would be no effect. The green line depicts how the *p*-curve would look like under 33% power.

The *p*-curve of the overall social modulation effects with the imitation-inhibition task and its analysis (Figure 6.2) indicate evidential value. In particular, both the half and full *p*-curve test are significant in their right-skewness (i.e., achieved with $p < .1$), with $p_{\text{Full}} < .0001$ and $p_{\text{Half}} < .0001$. Furthermore, even the half *p*-curve test reveals a right-skewed distribution with $p <.05$. In addition, the excluded *p*-values, which were not significant, must also be taken into account, as the more effects are generally not significant, the less evidence there is for the analyzed effect. More test results could be included (32) than had to be excluded (20). It can therefore be assumed that the *p*-curve is significantly right-skewed.

Similarly, the test for flatness with the 33% power distribution does not indicate that evidential value is inadequate or absent, as the test for flatness of the full *p*-curve of 33%, and the half *p*-curve and the binomial 33% power test are all non-significant, $p_{\text{Full}} = .9991$, $p_{\text{Half}} > .9999$, and $p_{\text{Binomial}} = .9822$. Thus, the *p*-curve is not significantly flattened.

The *p*-curve of the overall social modulations therefore indicates that the effect is likely to exist. The *p*-curve is also not left-skewed, which is why *p*-hacking is rather unlikely given the included data. There is also no further evidence of a publication bias, despite a slight increase of *p*-values around $p = .05$.

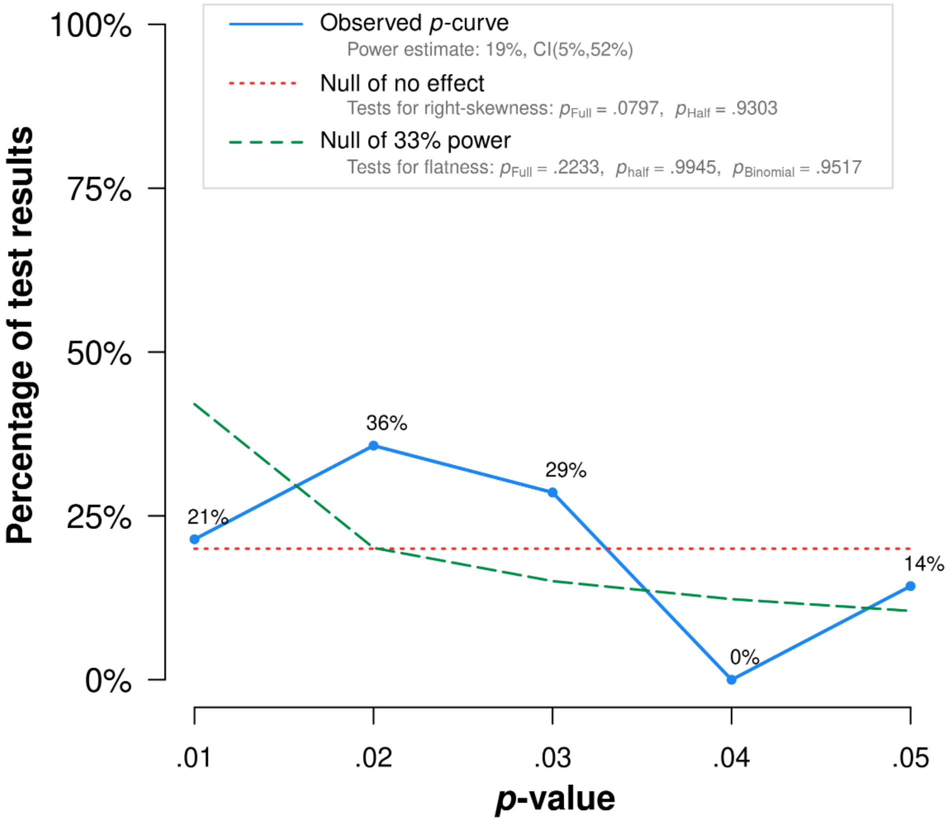
At the same time, it is important to note that the effect of the social modulations with the imitation-inhibition task is slightly underpowered, as indicated by the power estimate and the large confidence interval. However, with a power estimation of 76% (90%CI [56%, 88%]), there are three times as many *p*-values for $p = .01$ as for $p = .05$, which speaks for the effect. Nevertheless, the *p*-curve would be more meaningful with stronger powered results.

6.3.3 *P*-Curve of Top-Down Modulations With the Imitation-Inhibition Task

In the *p*-curve of top-down social modulation 14 statistically significant results (i.e., < .05) are included, with 12 results under *p* < .25. Fifteen additional results were entered but excluded from the *p*-curve and its analysis as they were *p* > .05.

Figure 6.3

The P-Curve and its Analysis for the Top-Down Social Modulation Results With the Imitation-Inhibition Task



Note. The *p*-curve includes 14 statistically significant results (i.e., *p* < .05), with 12 results under *p* < .025. Fifteen additional results were entered but excluded from the *p*-curve and its analysis as they were *p* > .05. The blue line depicts the actual observed *p*-curve from the set of results. The red dotted line depicts how the line of *p*-values would look like if there would be no effect. The green line depicts how the *p*-curve would look like under 33% power.

The *p*-curve of the top-down effects with the imitation-inhibition task and its analysis (Figure 6.3) does not indicate evidential value. In particular, neither the full *p*-curve tests, with $p_{\text{Full}} < .0797$, nor the half *p*-curve is significantly right- skewed, $p_{\text{Half}} < .9303$, thus the proposed conditions for evidential value are not met (i.e., neither the half *p*-curve test is right-skewed with $p < .05$ nor both the half and full test are right-skewed with $p < .1$). In addition, half of the *p*-values for top-down modulations had to be excluded because of being non-significant (i.e., 15 excluded, 14 included).

Nevertheless, the *p*-curve for top-down modulations is not significantly flattened, thus the tests for flatness with the 33% power distribution does not indicate that evidential value is inadequate or absent as all tests for flatness are non-significant, $p_{\text{Full}} = .2233$, $p_{\text{Half}} = .9945$, and $p_{\text{Binomial}} = .9517$. As the *p*-curve is not left-skewed, *p*-hacking is rather unlikely. Moreover, the possibility for a publication bias is rather low as well.

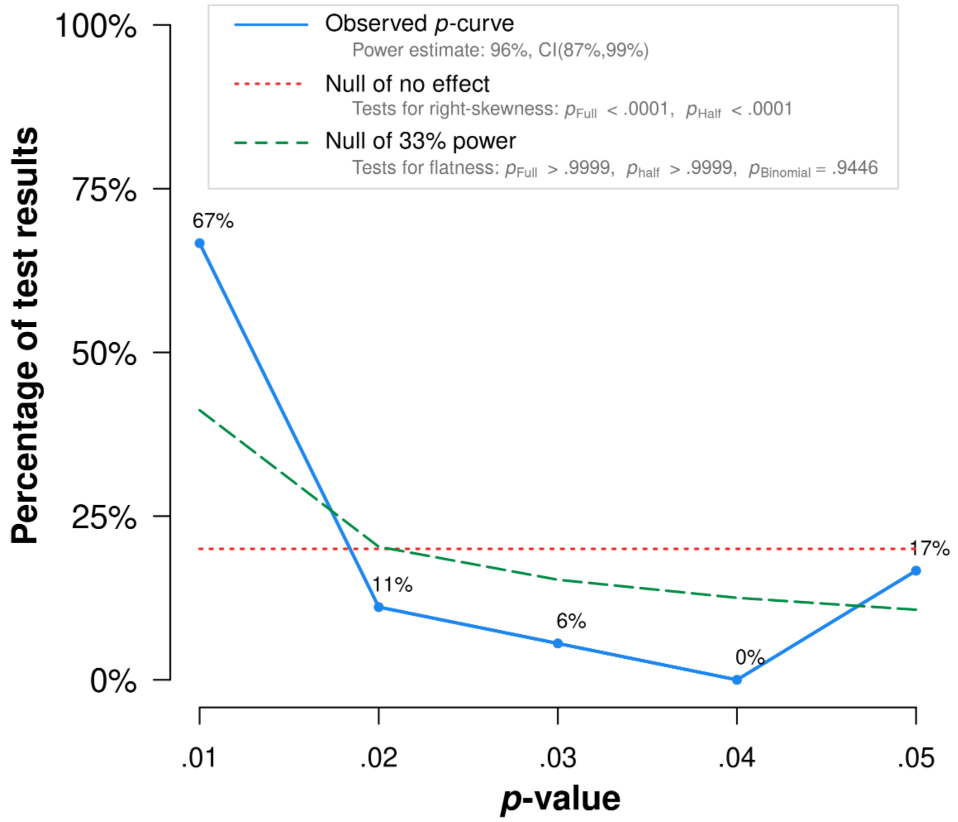
However, the top-down modulation effects with the imitation-inhibition task seem to be strongly underpowered, achieving an overall power of 19% (90%CI [5%, 52%]). Overall, although the *p*-curve is not flat and valuable effects are present, the evidential value is rather low and probably not present. Especially with such a low power, high-powered replications should be carried out to check whether and when a top-down effect really exists.

6.3.4 *P*-Curve of Bottom-Up Social Modulations With the Imitation-Inhibition Task

The *p*-curve of bottom-up social modulation effects includes 18 statistically significant results (i.e., < .05), of which 15 results are under $p < 0.25$. Five additional results were entered but excluded from the *p*-curve and its analysis as they were above .05.

Figure 6.4

The P-Curve and its Analysis for the Bottom-Up Social Modulation Results With the Imitation-Inhibition Task



Note. The *p*-curve includes 18 statistically significant results (i.e., $p < .05$), with 15 results under $p < .025$. Five additional results were entered but excluded from the *p*-curve and its analysis as they were $p > .05$. The blue line depicts the actual observed *p*-curve from the set of results. The red dotted line depicts how the line of *p*-values would look like if there would be no effect. The green line depicts how the *p*-curve would look like under 33% power.

The *p*-curve of the bottom-up modulations indicates evidential value, as it is significantly right skewed and both proposed conditions are met (Figure 6.4). In particular, both the half and full *p*-curve test significant in their right-skewness (i.e., with $p < .1$), with $p_{Full} < .0001$ and $p_{Half} < .0001$, and the half *p*-curve is significantly right-skewed under $p < 0.05$. Moreover, only five results had to be excluded because of being non-significant, 18 results could be included, with most of them under p = .025. All tests for flatness under the test of 33% are non-significant, p_{Full} > .9999, p_{Half} > .9999, and p_{Binomial} = .9446, thus evidential value is not inadequate nor absent.

The power estimate for the results with bottom-up modulations is very high, with a power estimate of 96 % (90%CI [87%, 99%]), which speaks for the effects. Although there is a rebound in the *p*-curve at *p*-values of .05, the distribution is so right-skewed that systematic *p*-hacking can be ruled out. The *p*-curve also does not indicate publication bias, as its right-skewness is rather unambiguous. Thus, the analysis of the *p*-curve of bottom-up effects with the imitation-inhibition task indicates that effects with these social modulations are very likely to exist.

6.3.5 Limitations of the *P*-Curve Analysis

The analysis of the three *p*-curves shows evidential value for the overall social modulations, and especially for the bottom-up modulations, but rather no evidential value for the top-down social modulations. This is consistent with the findings of this dissertation, but also with the problems of replication of top-down modulations with the imitation-inhibition task in the laboratory (cf., Cracco, Bardi, et al., 2018). There are also a few, albeit older, studies in the automatic imitation literature that directly compare top-down modulations with bottom-up modulations and find stronger effects for the bottom-up modulations, which further supports the findings (Klapper et al., 2014; Press et al., 2006). Generally, the numbers of excluded results also show that social top-down manipulations more often elicit no effect (i.e., approximately 53 percent of the results from the top-down modulation set of studies) than bottom-up modulations (i.e., approximately 13 percent of the results from the bottom-up modulation set of studies).

Nevertheless, the *p*-curve analysis also has some limitations. First of all, the *p*-curves strongly depend on the study results that were entered. For example, if this set of results consists of results from strongly underpowered studies, the *p*-curve does not necessarily reflect the real conditions of the effects. It could be that the *p*-curve shows no evidential value in this case, although one would actually be present. High-powered replications would be useful for confirming the results in such cases (Simmons & Simonsohn, 2017; Simonsohn et al., 2014a, 2015). This would also make sense for the social modulations with the imitation-inhibition task, specifically the top-down modulations.

Secondly, the *p*-curve is dependent on the reported *p*-values and statistics. For example, statistics that are not reported or *p*-values that are not published will not be reported in a *p*-curve and will not be examined in the analysis. This means that publication bias can only be recognized by combining several symptoms (slight *p*-hacking to make studies publishable, possible relatively flat distribution of *p*-values, generally few included studies on the effect), but also refers to older studies that often do not report all statistics (Simonsohn et al., 2014a). These studies, which would usually be important for the overall picture of the *p*-curve, cannot be included. This was also the case with some studies from the three *p*-curve analyses reported here.

Third, although the combination test introduced in Simonsohn, Simmons and Nelson’s (2015) 'Better *P*-Curves' paper is more robust to strong *p*-hacking than simply looking at the full *p*-curve, *p*-hacking can nevertheless influence slightly the power estimates as well as the effect size of the studies. Consequently, the *p*-curve can still be a little biased in this regard because of the indicated statistical results, although it will not influence strongly the analysis and *p*-curve as a whole (Simonsohn et al., 2015).

Fourth, it is important to watch out for cherry picking results with *p*-curves. Even though Simonsohn et al. (2015) were able to show in simulations that cherry picking does not influence the *p*-curve too much, it would still be important to formulate and explain appropriate rules in advance. In the case of the three reported *p*-curves, however, all *p*-values based on the formulated rules were included.

Fifth, important to consider for *p*-curves as to all *p*-values: *p*-values are only reference values and have no continuous relevance. The lower the *p*-values, the less consistent are the data with the respective null hypotheses. Thus, a $p = .053$ and a $p = .048$ are actually the same, but a $p = .000001$ is much more convincing than both (Simonsohn et al., 2014a, 2014b).

Lastly, these three *p*-curve analyses address only social modulations of automatic imitation. It is very likely that the results regarding top-down and bottom-up modulations can also be transferred to general manipulations of automatic imitation with the imitation-inhibition task, as the same replication problem applies (cf., Cracco, Bardi, et al., 2018) and manipulations are carried out in the same way, albeit without social hypothesis (Cracco, Bardi, et al., 2018; Heyes, 2011). However, only social modulations were included in the set of studies of the *p*-curve analyses, which somewhat limits the informative value and its generalizability.

In summary, the results should be viewed with caution and not taken as clear evidence. Rather, they are indications that should be handled carefully. Like any statistical analysis, a *p*-curve is not completely free of errors and does not provide a perfect and definite result that reflects reality one-to-one.

6.4 Top-Down vs. Bottom-Up: Why Bottom-Up Modulations Might Work Better

The question remains why bottom-up modulations might work better than top-down modulations. One straight-forward explanation is that they directly manipulate the extent to which the motor activation of a corresponding action is inhibited (or not). Thus, by causing a different behavioral response, they actively intervene in the cognitive mechanism of the task (e.g., Heyes, 2011; Klapper et al., 2014). Top-down manipulations, on the other hand, only affect the belief about the stimulus. This means that a change in this belief is not necessarily sufficient to completely change the perception of the stimulus and to maintain it permanently in order to bring about a different motor response (Gowen & Poliakoff, 2012; Klapper et al., 2014; Wang & Hamilton, 2012). This assumption is supported by the fact that many top-down changes are only presented in the instructions (i.e., at the beginning of the experiment; e.g., Leighton et al., 2010). Consequently, their effect might not last long enough.

In addition, the general strength of the task could counteract the top-down modulations. Thus, top-down modulations are not able to manipulate the strong stimulus-response overlap of the task. Consequently, it is more difficult to achieve effects with top-down modulations in automatic imitation. In this case, the induced belief would have to be so strong that it permanently changes the entire perception of the stimuli while the task is running. However, is this is the case only a few top-down modulations might have achieved such a permanent long-lasting perception change (e.g., Liepelt & Brass, 2010b).

Another possibility is that bottom-up modulations directly increases the perception of similarity to the hand model in the corresponding condition. Studies with the imitation-inhibition task show that models who are perceived to be similar are imitated more strongly by the participant (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018; Genschow et al., 2021). The physical characteristics of the hands are particularly important here, and a stronger stimulus-response overlap causes a stronger perception of similarity and thus stronger imitation (Cracco, Bardi, et al., 2018). Because bottom-up manipulations directly manipulate the stimuli, hands in one condition could have been perceived as more similar to oneself as compared to the other condition, and thus might also be imitated more strongly. For animacy studies, this has long been hypothesized to be an underlying effect, as human hands are more similar to the participant's than robotic hands (e.g., Klapper et al., 2014; Press et al., 2006). Conversely, for top-down responses the self-other overlap might be too low. As the stimuli themselves are not changed, there is no increased similarity in the conditions. Thus, at a meta-level, studies that include self-other overlap theory (Brass & Heyes, 2005; Cracco, Bardi, et al., 2018) in their modulations might have a higher probability of finding social effects. This contrasts with motivational theories, like the STORM theory (Wang & Hamilton, 2012) according to which a top-down affiliation goal would be responsible for stronger motivation. The results of this thesis support the assumption that studies based on motivational approaches using top-down manipulations might show weaker automatic imitation effects.

However, this is only an assumption, as most existing studies discuss either self-other overlap theories or motivational theories as a possible theoretical background for their social modulations, but never explicitly test them or deliberately adjust their stimuli according to the two theories. Only few studies actually specify whether an affiliation goal (i.e., motivational theories) or similarity overlap (i.e., self-other overlap theories) might be responsible for their social effect (e.g., Cracco, Bardi, et al., 2018; Genschow, Pauels, et al., 2023; Gleibs et al., 2016; Klapper et al., 2014; Press et al., 2006). Moreover, while both theories attempt to explain basal social imitation, they are imprecise about the moderators that belong to the two theories, nor do they make any assumptions about whether top-down modulations or bottom-up modulations or both may interact with them. A review might be important to clarify which theory explains which kind of modulation best. That is, a review might help to understand whether one of the two theories, none of the two theories or the combination of both theories has the highest explanatory value regarding modulations of automatic imitation with the imitation-inhibition task. In this case, it would be less a question of bottom-up vs. top-down modulation, but perhaps more a question of whether there is an affiliation motive or a similarity perception or it if is neither similarity nor affiliation and just a different perception of stimulus-response link as dual-route theory proposes.

Explicitly referring to our results regarding automatic imitation with the online imitation-inhibition task, it must also be mentioned that top-down modulations effects on automatic imitation are supposedly more difficult to find in online settings than bottom-up modulations. As already explained in Chapter 6.2.2, people in an online setting are less motivated to perform difficult tasks and work on the tasks with less attention and motivation (e.g., De Man et al., 2021). As a result, top-down modulations have little chance of really being consciously perceived and causing a prolonged change in perception of the stimuli. However, bottom-up modulations that permanently change the stimulus material can achieve this, as no additional motivation or special attention is required. Of course, these aspects cannot be completely ruled out for the laboratory environment either. Therefore, in both environments, especially for top-down modulations, particular effort should be invested in ensuring that the manipulation is attentively observed, consciously perceived and understood by the participant.

The *p*-curve analysis does not show why some bottom-up studies cannot be replicated. However, this dissertation provides at least some indications: If bottom-up modulations cannot be replicated, it might often be due to the stimulus material. For example, having different finger movement sizes in the different manipulated stimuli might be sufficient to already cause confounds in automatic imitation effects (cf., Chapter 3). Similarly, it may be that some bottom-up manipulated stimuli were manipulated too artificially or too subtly to achieve the corresponding effects (cf., Chapter 5). Moreover, in some included bottom-up manipulated studies, several modulations were attempted to be manipulated at once (e.g., not only animacy but also reward) which could have counteracted an effect (e.g., Haffey et al., 2013; Longo et al., 2008).

Independent of the type of social modulation (i.e., bottom-up vs. top-down), some social modulations might simply not have an effect on automatic imitation, or might not interact with automatic imitation because they do not fit with the design of the inhibition-imitation task (i.e., because its design as a reaction time tasks does not allow for the required deeper processing of social modulations and thus produces strong non-modifiable automatic imitation effects), because the manipulation is too weak (i.e., leads to effects that are too small to be detected), or because of overlap compatibility lack (Cracco, Bardi, et al., 2018) between the experimental task situation and the social effect that should be tested (i.e., watching only the hand of a group member but not the actual group member itself).

Similarly, top-down modulations could benefit from being performed in a high-powered manner in the laboratory. Liepelt and Brass (2010b), for example, achieved strong effects by presenting participants with a top-down manipulation in the laboratory where the experimenter first showed the participant which hand was in a glove (an artificial wooden hand, who he showed to the participant vs. his own human hand), although the participants then worked on the same task on the computer with the same glove pictures as stimuli. This kind of manipulation might also help to manipulate beliefs in other experiments, as it seems capable to permanently change the perception of the stimulus. Thus, it might be interesting to replicate studies that used more subtle types of manipulations like changes in instructions (Genschow et al., 2021), priming (Leighton et al., 2010), or pictures above the actual hand (Genschow, Pauels, et al., 2023), in high-powered manner in the laboratory implementing a top-down modulation using the laboratory environment to permanently change the perception of the stimulus. Thus, it may still make more sense to use top-down modulations for some questions. They should just be better and more consciously thought through in order to achieve good effects when applied in a high-powered manner.

6.5 Is Automatic Imitation Measured With the Imitation-Inhibition Task Imitation at all?

Since the automatic imitation task has difficulties to capture modulations, particularly social modulations as also indicated and supported by the *p*-curve analysis and some chapters of this dissertation, the question arises: Does the imitation-inhibition task measures imitation at all? Over the time researchers have criticized that automatic imitation measured with the imitation-inhibition task, might not be “real” imitation but rather a spatial process, or just reaction-time artefacts of a typical forced or simple response reaction time paradigm (e.g., Ramsey, 2018). Thus I will further explore these constructs, phenomenon and processes to conclude whether automatic imitation is really imitation.

6.5.1 Is Automatic Imitation a Reaction Time Artefact?

In recent years, the question has repeatedly been raised as to whether the imitation-inhibition task as a forced response reaction time task really measures actual imitation. For example, Ramsey (2018) questioned the usefulness of measuring imitation with a reaction time design. He argued that stimulus-response compatibility tasks such as the imitation-inhibition task produce reaction time indices that are a combination of many underlying processes that are not necessarily related to imitation or social behavior in general. As automatic imitation measured with the imitation-inhibition task does not correlate with motor mimicry measures (Genschow, van Den Bossche, et al., 2017), in his opinion the task has little validity with regard to social imitation and it is unclear what the reaction times really measure. Furthermore, he questioned whether the cognitive task can be modified at all by social variables. Similarly, the mechanism of how the imitation-inhibition task works (i.e. its domain-specificity), is not entirely obvious. According to Ramsey (2018), neither the reaction mechanisms nor the design of the task can tell us which cognitive systems are involved, whether the task operates domain-specific or domain-general or combines joint processes. He concludes that the imitation-inhibition task may not measure imitation or social behavior at all, but only typical reactions to a classical stimulus-response paradigm (Ramsey, 2018).

The fact that the imitation-inhibition task does not correlate with mimicry (Genschow, van Den Bossche, et al., 2017) may indeed indicate that the two paradigms do not measure the same type of imitation. Nevertheless, a lack of correlation does not mean that the reaction times of the imitation-inhibition task do not measure imitation at all. A plausible reason for the lack of correlation may be, for example, that motor mimicry is a particularly unreliable measurement method, partly due to confounding factors in social interactions (Genschow, van Den Bossche, et al., 2017; Salazar Kämpf et al., 2018), but also because too few target imitation actions are shown in typical motor mimicry experiments (Genschow, van Den Bossche, et al., 2017). Additionally, the methodological setups of the two paradigms differ completely, which is why correlations may already be limited (Genschow, van Den Bossche, et al., 2017). Besides, the author who established the imitation-inhibition task assumed that motor mimicry and automatic imitation may be part of the same construct (Cracco & Brass, 2019), but that they measure different aspects of this construct, a view that is also supported by other researchers (Genschow, van Den Bossche, et al., 2017; Heyes, 2011), as I outlined in Chapter 1.2.3. Thus, the lack of correlation does not directly affect the validity of the imitation-inhibition task for measuring imitation. The imitation-inhibition task was not originally designed to be a laboratory measure of motor mimicry, but was designed independently of motor mimicry to measure covert imitative response tendencies (Cracco & Brass, 2019). Therefore, reaction times of the imitation-inhibition task measure covert imitative response tendencies associated with some (but not all) types of overt imitation. Motor mimicry, on the other hand, is a paradigm that investigates rather overt behavioral imitation (Heyes, 2011). Accordingly, it makes sense that the two paradigms are not necessarily related, nevertheless it does not exclude that both paradigms measure imitation.

However, whether the imitation-inhibition task can be modulated, particularly socially (i.e. whether it is able to measure social imitation) is one of the questions addressed in this dissertation. Here I can conclude that even when highly powered, not all effects are replicable, and that social replicability might depend mainly on the manipulation and the type of modulation. It seems that many bottom-up direct manipulations are easier to replicate and remain very stable whereas many top-down modulations, including direct social manipulations or correlative measurement of interindividual differences, show little or no effect. Nevertheless, this dissertation can show that the imitation-inhibition task is still modifiable with social variables (cf., Chapter 2 & Chapter 3).

6.5.2 Is Automatic Imitation Only a Spatial Process?

Adding up to Ramsey's (2018) critique on the task, the imitation-inhibition task is not a standard reaction time task. While, it shares features and uses the same basic structure (Brass et al., 2000), the distractor is not a pure spatial or attentional distractor; it includes a social imitation component (Heyes, 2011). Evidence for this derives from research on compatibility effects underlying automatic imitation. It is well established that the imitation-inhibition task measures effector and movement compatibility (Cracco, Bardi, et al., 2018). These two compatibility effects are an essential part of the definition of automatic imitation and imitation in general (Heyes, 2011). They can be detected independently in imitation-inhibition paradigms but also together (Brass et al., 2000, 2001; Catmur & Heyes, 2011; J. L. Cook & Bird, 2012; Cracco, Bardi, et al., 2018; Heyes et al., 2005; Leighton & Heyes, 2010; Stürmer et al., 2000). In contrast, spatial compatibility, the compatibility effect that other reaction time-based stimulus-response tasks in particular measure (Sperling & Weichselgartner, 1995), is not responsible for automatic imitation. In fact, spatial compatibility also plays a role in the imitation-inhibition task and enhances automatic imitation effects, but when spatial compatibility effects are controlled for, the imitative compatibility persists (Bertenthal et al., 2006; Boyer et al., 2012; Catmur & Heyes, 2011; J. L. Cook & Bird, 2012; Heyes et al., 2005; Jiménez et al., 2012; Press et al., 2005). We were also able to replicate this finding online (cf., Chapter 2.4), which additionally highlights that it is a robust finding. Taken together, these results suggest that the imitation-inhibition task does not simply measure spatial stimulus-response reactions, but rather valid imitation or a part of the imitation construct.

Thus to summarize, most research agrees that automatic imitation is not a spatial process, distinguishing the imitation-inhibition task from other simple stimulus response paradigms (Cracco, Bardi, et al., 2018). Automatic imitation measures movement and effector compatibility effects (Cracco, Bardi, et al., 2018; Heyes, 2011) and is, at least partially, socially modulable, although it may not be as universally modulable as hypothesized by previous research. Automatic imitation does not measure overt imitation, but covert imitation. It is likely that the same core process or construct is addressed by motor mimicry and automatic imitation, but in different ways (Cracco & Brass, 2019; Genschow, Rigoni, et al., 2017; Heyes, 2011). Nonetheless, automatic imitation appears to be a form of imitation.

6.6 Generalizability of the Findings

This dissertation showed that automatic imitation can be modulated, even socially, although not as universally as previously assumed. Moreover, it founds that bottom-up manipulations might be more effective than top-down modulations. Furthermore, certain modulations seem to work better than others, like finger movement size and human vs. non-human stimuli.

6.6.1 Generalizability of the Findings to Other Imitation-Inhibition Tasks

Beside the finding that bottom-up modulations might achieve stronger results in automatic imitation than top-down modulations, in Chapter 3, we were able to show that the size of the finger movement makes a difference in the magnitude of automatic imitation: A larger finger movement causes stronger automatic imitation compared to a smaller finger movement. However, a different speed of finger movements made no difference in the magnitude of automatic imitation. This finding expands general theories on the mechanism of automatic imitation, like the ideomotor theory (Greenwald, 1970; Prinz, 1990, 1997), by showing that the mental representation of a movement, and as a consequence its motor plan activation and the following automatic imitation, depends on the size of the movement. It is possible that attentional processes might play a role in this linkage, as increased attention leads to stronger mental representation (Wertheim, 1981) and subsequent imitation (Bek et al., 2016; Cracco, Bardi, et al., 2018; Heyes, 2011; Longo & Bertenthal, 2009); and because large movements are more salient (Franconeri & Simons, 2003; Krüger et al., 2016; Taylor & Fiske, 1978; Wixson, 2000) and thus elicit more attention than small movements (Burnham, 2020; Gaspelin & Luck, 2018; Kerzel & Schönhammer, 2013), it is reasonable to assume that large movements are imitated more strongly, because they elicit more attention.

The finding that finger movement size enhances automatic imitation is particularly important as it shows that motion profiles can strongly influence automatic imitation. Although we could only show that finger movement size has an effect on the magnitude of automatic imitation and not finger speed, we cannot exclude the possibility that finger movement speed also has an effect on automatic imitation for other stimuli used in other contexts. Moreover, other motion profiles, for example acceleration or deceleration, might also influence automatic imitation. Thus, this finding opens up new avenues for further investigations into automatic imitation. Above all, however, it means that researchers using the imitation-inhibition task, may careful match finger movement sizes in bottom-up stimuli manipulations to distinguish between experimental effects and inherent movement size impacts. Especially for social modulations, but also general modulations, such a confound could reinforce replication problems, even if this was not the case for the animacy effect (cf., Chapter 3.5). In addition, the finger movement size should be large enough to increase the likelihood of finding an effect, with or without additional modulation. Chapter 3.2 underlines the importance of a general sufficiently manipulated movement size when studying effects on automatic imitation.

6.6.2 Generalizability of the Findings to Motor Mimicry

The generalizability of the findings of this dissertation to motor mimicry effects is limited. Indeed, many of the results cannot be directly transferred to motor mimicry, as they are very specific to automatic imitation measured with the imitation-inhibition task, like for example the effect of finger movement size. Moreover, it is rather difficult to design a motor mimicry paradigm for an online environment that generates reliable and valid results. Nevertheless, motor mimicry also uses top-down modulations and bottom-up modulations to investigate similar social modulations such as group-membership (Bourgeois & Hess, 2008; Yabar et al., 2006) imitation of high-status members (Cheng & Chartrand, 2003), or imitation after ostracism (Lakin et al., 2008; Over & Carpenter, 2009). For this reason, it would be useful to take a closer look at top-down modulations and bottom-up modulations for motor mimicry, and to examine the subtleties of these mechanisms in previous motor mimicry studies. High-powered experiments are also needed in modulated motor mimicry studies to ensure and strengthen the found effects.

Nevertheless, motor mimicry might play an important role when considering especially social modulations as a whole in relation to general imitation. This is because motor mimicry paradigms may be able to investigate those social modulations that are not compatible with the imitation-inhibition task. Despite the weaknesses of the motor mimicry paradigm (cf., Chapter 1.2.1 & Chapter 1.2.3), it would make sense to investigate some social modulations with motor mimicry paradigms rather than with automatic imitation paradigms when direct social interaction is necessary, such as group-membership. Needless to say, that the motor mimicry paradigm in such a scenario should always planned to be as reliable and efficient as possible. Since motor mimicry and automatic imitation may measure different parts of the imitation construct (e.g., Heyes, 2011; cf. Chapter 1.2.3), it makes sense that social modulations that do not work in the imitation-inhibition task, might work in motor imitation paradigms, leading to changes in mimicry. Rauchbauer and colleagues (2018), for example, found that group membership does not influence automatic imitation, but still found different brain processing patterns of the participants when imitating members of their own group compared to those of the other group. Thus in this case, but also generally speaking concerning other social modulations, the two paradigms could complement each other. The imitation-inhibition task could be used to study the more cognitively functioning social modulations or their cognitive mechanism in general, while motor mimicry could be used to study the social effects of social modulations, or to examine the effects that need direct social interactions.

Similarly, different theories of social modulation might work differently in the two paradigms: In motor mimicry, there is more evidence that motivational theories and an affiliation goal are useful to explain the findings of social modulation (Chartrand & Bargh, 1999; Lakin & Chartrand, 2003; Likowski et al., 2008). However, there is stronger evidence for self-other overlap theories regarding automatic imitation (Brass & Heyes, 2005; Genschow et al., 2021; Klapper et al., 2014; Press et al., 2006). Both paradigms should therefore not be perceived as opposites but as complements to each other in order to advance the field of social modulations in imitation and could be combined in future research. In a next step, this complimenting process may proceed from social modulations to general modulations in order to collectively uncover as many aspects of the overall construct of imitation as possible.

6.7 Limitations of the Findings

The imitation-inhibition task is not as universal modulable as previously thought. Although the animacy effect, for example, is still one of the best documented modulations (cf., Chapter 2.5 & Chapter 3.5), and finger movement size has a large impact on automatic imitation (cf., Chapter 3), but group membership (cf., Chapter 5) and interindividual differences regarding free will belief (cf., Chapter 4), on the other hand, seem not to influence automatic imitation. In addition, top-down modulations seem to work less well than bottom-up modulations whereas bottom-up modulations achieve quite strong results. Nevertheless, further high-powered experiments are necessary to consolidate the findings. There are also a few limitations of the results of this dissertation.

First, the results of this dissertation cannot necessarily be transferred reliably to all modulations with the imitation-inhibition task, nor to combinations of modulations or other adapted versions of the imitation-inhibition task. Although there is a high probability that the results may be applied to a variety of imitation-inhibition tasks and its modulations in general, further research is needed.

Second, this dissertation cannot make any precise statements about the cognitive processes, behind social processes with the imitation-inhibition task, or about general cognitive processes behind automatic imitation.

Third, similarly, this dissertation cannot provide a complete clarification of which theories underlie automatic imitation and its modulation. Although the results may suggest that self-other overlap is more likely than motivational theories for many social modulations with the imitation-inhibition task, this is mostly speculation. Likewise, it is not possible to clarify whether, for example, ideomotor theory or associative learning theories are more probable to explain automatic imitation in general.

Fourth, the generalizability of the results to real social interactions is limited. Since these are fundamental cognitive findings related to the imitation-inhibition task, the significance in real interactions cannot be directly assessed. Rather, the findings are only important in the context of modulations with the imitation-inhibition task in order to be able to investigate the cognitive properties of modulations of automatic imitation even better in the future. Nevertheless, it is likely that the findings are only applicable to other covert imitation effects, including those without modulations.

6.8 What Future Studies With the Imitation-Inhibition Task Should Consider

To conclude this dissertation, I would like to highlight what an imitation-inhibition task should look like that (a) incorporates all the results of this dissertation and (b) considers previous results using the task in the laboratory, and what should generally be considered when working with the task. Of course, this should not be understood as a necessity; the design of the imitation-inhibition task depends above all on the research question the task is supposed to investigate.

When it comes to investigating a social research question, it would first make sense to consider the theories relevant to that question when forming the hypothesis: Is the idea related to an affiliation motive (i.e., motivational theories; e.g., Lakin & Chartrand, 2003) or a similarity perception (i.e., self-other overlap theories e.g., Brass & Heyes, 2005)? The task should be structured accordingly and an adaption should be chosen that fits the respective theoretical background.

Of course, an imitation-inhibition task should always be strongly powered, but this fact is even more important when it comes to social modulations. The *p*-curve analysis has shown that there is still room for improvement and that top-down modulations in particular could possibly benefit from higher statistical power. Since social effects with the imitation-inhibition task are probably rather small (Cracco, Bardi, et al., 2018), they require high-powered studies.

Generally, if the hypothesis and the modulation permit, it might make more sense to perform bottom-up manipulations, as top-down modulations seem to produce weaker results. Additionally, correlational studies using the task should be avoided, as the task is unlikely to respond to interindividual differences due to its nature as a reaction-time task (Hedge et al., 2018).

Moreover, it would make sense to test only one moderator or a single hypothesis and to not carry out several modulations or test several hypotheses at the same time. This prevents the dilution of the effect, and avoids ignoring small effects or cross-over effects (Cracco, Bardi, et al., 2018).

Particularly in an online environment (but also in the laboratory), it should always be ensured that the participants pay sufficient attention to the manipulation. Top-down modulations in particular could benefit from this attentive consideration and cause a stronger change in stimulus perception.

For really small effects, it may also be worth switching to the laboratory to achieve larger effects, as participants might be more attentive (e.g., De Man et al., 2021). If the effects require social interaction, it might also be useful to combine motor mimicry paradigm and imitation-inhibition task: For example, participants could respond to a number cue by raising their finger on a keyboard while a confederate sits opposite to them acting as the “distractor image” in the background and also raising a finger. The number cue could, for example, be shown between the confederate’s finger on a tablet on which the confederate put their hand, thus the participant would still have to focus on the number cue and the hand of the confederate as in the actual online imitation-inhibition task, but the social appearance and the laboratory environment of motor mimicry would be mixed in. The confederate could be manipulated regarding their acting manner towards the participant, their appearance, their behavior, or their language for the different conditions to achieve a stronger social modulation. Such a combined task would be much more reliable than a standard motor mimicry paradigm and would still capture quantifiable reaction times and error rates. However, it is expensive to perform such a task in the laboratory, especially so when aiming for high statistical power. The same applies to a lesser extent as well to simple imitation-inhibition tasks in the laboratory. Nevertheless, it might work better for some social modulations or provide an alternative workaround for some failed replications with the imitation-inhibition task (e.g., group membership) or to perform the imitation-inhibition task in the laboratory directly.

Additionally, the imitation-inhibition task and its research depend on non-significant results being published. Even if researchers often do not have influence on this, null effects should at least not be ignored because it helps other researchers to refrain from conducting redundant research. Furthermore, it highlights the strengths and weaknesses of the task and enables its use in a more systematic way. Pre-prints may also be worthwhile (Moshontz et al., 2021). This can further decrease the ambiguity of some manipulations.

Importantly, attention should be paid to the finger movement size in the imitation-inhibition task stimuli: the size should be the same regardless of the condition and the finger that is lifted. Particularly, discrepancies might lead to confounding findings. Similarly, it should be common practice to control for spatial compatibility in order to avoid inflating the automatic imitation effect and make it more comparable between studies (Heyes, 2011). It is sufficient to rotate the stimuli by 90° but best would be to observe spatial and imitative compatibility separately (Cracco, Bardi, et al., 2018).

As a final general note, when working with the task, it is important to keep in mind that the task is a reaction time task that was not primarily designed to make social modulations (in difference to motor mimicry designs) but primarily to investigate cognitive phenomena behind covert imitation (Cracco & Brass, 2019). Therefore, researchers should not expect large social effects and keep this fact in mind when creating their hypothesis and theory in order to build their final imitation-inhibition task.

6.6 Conclusion

Within this dissertation I first developed and validated a method to measure automatic imitation in an online environment, namely the online imitation-inhibition task. The online version of the task works as well as its laboratory counterpart und generates strong and robust findings. As there is an ongoing debate in the literature whether automatic imitation can be modulated, particularly socially, it is not only a methodological question as to whether automatic imitation can indeed be modulated but also a question of social relevance, assuming that automatic imitation is a method of measuring covert imitation response tendencies (Brass et al., 2000; Cracco & Brass, 2019). As the online imitation-inhibition task enables research with high-powered samples in an online environment, I could show in sufficient statistical powers experiments, that modulations of automatic imitation are not as universally as previously thought, but that especially bottom-up modulation might work very well, as I found in a *p*-curve analysis.

Thus, the imitation-inhibition task has many strengths and is versatile as long as it is used correctly, which involves following important guidelines (e.g., bottom-up manipulations and high-powered experiments). Keeping in mind that the imitation-inhibition task is only a measurement tool and not a cure-all that responds ideally to every manipulation, social manipulation or question of any kind, the imitation-inhibition task is an adaptive and reliable measurement method with many advantages, that delivers strong results. Although the imitation-inhibition task is probably less universally modulable than previously assumed, many modulations have proven to be very robust (e.g., the animacy effect) and future research should replicate further modulations to strengthen the existing findings.

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Appendix

P-Curve Disclosure Table

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results
Genschow, Schuler, et al. (2019) ∇	Based on the results of the pilot study, we expected smaller congruency effects in the money priming condition as compared to the control condition.	2 (block order: control first vs. money first) x 2 (congruency: congruent trials vs. incongruent trials) x 2 (priming: money vs. control) mixed design	Two-way interaction	More important for our predictions, however, was the significant interaction between congruency and priming, *F*(1, 145)=5.13, *p* =.025, *dz* =.19, indicating smaller congruency effects in the money priming condition (Δ = 59.35; *SD* = 38.03) than in the control condition (Δ = 64.13; *SD* = 39.32).	*F*(1, 145) = 5.13, *p* = .025
Liepelt & Brass (2010b) ∇	We predicted that, if the direct matching system is tuned to the perception of intentional agents, one should find increased motor priming effects for the leather hand animation when participants believed that it was produced by a human.	2 (group: human vs. wooden) x 2 (congruency: congruent vs. incongruent) mixed design	Two-way interaction	Most importantly, a significant interaction between Group x Congruency was observed, *F*(1, 17) = 10.05, *MSe* = 267,61, *p*< .05, partial η^2 = .37, indicating a significantly larger motor priming effect in the human-hand model group as compared to the wooden-hand model group.	*F*(1, 17) = 10.05, *p* = .006
Leighton et al. (2010) ∇	If social attitudes have a direct and specific effect on mimicry, and if the mimicry that they modulate is not intentional, then participants primed with pro-social words should show a larger automatic imitation effect than those primed with anti-social words, and the neutral group should show an intermediate automatic imitation effect.	2 (congruency: compatible vs. incompatible) x 3 (priming: pro-social vs. anti-social vs. neutral) between-subjects design	Planned contrast	Contrasts revealed that the pro-social group showed a significantly greater automatic imitation effect than the anti-social group (*F*(1,22) = 6.148, *p* < 0.022), and there was a significant linear trend across the three groups (*p* < 0.008), indicating that the prosocial group showed the largest automatic imitation effect, followed by the neutral group, with the anti-social group showing the smallest automatic imitation effect.	*F*(1, 22) = 6.15, *p* = .021
Khemka et al. (2021) ∇	If the modulation of imitation by self-focus is specific to imitation, we should observe an interaction between self-focus condition and imitative, but not spatial, compatibility. If instead it is having its effects on non-imitative processes, we should observe an interaction between self-focus condition and spatial compatibility	2 (imitative compatibility: compatible vs. incompatible) × 2 (spatial compatibility: compatible vs. incompatible) x 2 (focus: self-focus vs. control) within-subjects design	Two-way interaction	Crucially, no interaction was found between experimental condition and imitative compatibility, (*F*(1,57) = 0.23, *p* = .631, η_p^2 = 0.004), or experimental condition and spatial compatibility (*F*(1,57) = 1.22, *p* = .274, η_p^2 = 0.021), suggesting that the self-focus manipulation did not significantly influence imitative or spatial compatibility.	*F*(1, 57) = 0.23, *p* = .631
Genschow et al. (2021): Exp. 2 ∇	In Experiments 2 to 4, we tested the hypothesis that a focus on similarities, as compared with a focus on differences, increases automatic imitation as measured with the imitation-inhibition task (Brass et al., 2000; Brass et al., 2001) —the standard paradigm in the research of automatic imitation. In Experiment 2, participants focused either on similarities or differences between themselves and the other person.	2 (focus: similarities vs. differences) x 2 (congruency: congruent vs. incongruent) within-subjects design	Difference of means	The results indicated a larger congruency effect in the block in which participants focused on similarities (*M* = 105.52; *SD* = 75.20) than in the block in which participants focused on differences (*M* = 67.62; *SD* = 43.45), *t*(26) = 2.96, *p* = .007, *95%CI* [11.54,64.27], *dz* = 0.57 (Fig. 2)	*t*(26) = 2.96, *p* = .007

Genschow et al. (2021): Exp. 3 ∇	In Experiment 3 and 4, we added a neutral condition to the design to exploratorily investigate whether the predicted difference between the similarity and difference focus condition is due to an increase in the similarity focus condition, a decrease in the difference focus condition, or both.	3 (focus: similarities vs. differences vs. neutral focus) x 2 (congruency: congruent vs. incongruent) within-subjects design	Difference of means	As in Experiment 1, the analysis yielded a larger congruency effect in the similarity focus condition (*M* = 95.96; *SD* = 44.26) than in the difference focus condition (*M* = 84.48; *SD* = 46.32), *t*(86) = 2.34, *p* = .021, *dz* = 0.25, *95%CI* [1.74, 21.23] (Fig. 3).	*t*(86) = 2.34, *p* = .021
Genschow et al. (2021): Exp. 4 ∇	Same as above	3 (focus: similarities vs. differences vs. neutral focus) x 2 (congruency: congruent vs. incongruent) within-subjects design	Difference of means	In line with our prediction and the results obtained in Experiment 2 and 3, participants' congruency effect was larger when they focused on similarities (*M* = 93.54; *SD* = 43.02) than when they focused on differences (*M* = 82.52; *SD* = 42.44), *t*(134) = 2.60, *p* = .010, *95%CI* [2.64, 19.40], *dz* = 0.22 (Fig. 4).	*t* (134) = 2.60, *p* = .010
Spengler, Brass, et al. (2010): Exp. 1 ∇	It predicted, first, minimized motor mimicry, expressed as a reduced mean interference effect, under the self-focus condition, and, second, more specifically, that the smaller interference effect is caused by enhanced performance under motoric conflict.	2 (self-focus: mirror vs. no mirror) x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	A repeated-measurements ANOVA of the reaction time data with the factors self-focus (mirror/no mirror) and the factor condition (incongruent/congruent) revealed most importantly a significant interaction effect self-focus by condition (*F*(1, 19) = 6.3, *p* < .02) (Fig. 2A), showing a reduced mean interference effect in the imitation–inhibition task	*F*(1, 19) = 6.3, *p* = .021
Spengler, Brass, et al. (2010): Exp. 2 ∇	Same as above	2 (self-focus: evaluative vs. memory x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	Analysis of the reaction time data with a repeated-measurements ANOVA (factors self-focus (evaluative/memory) and condition (incongruent/congruent)) revealed again a significant interaction (*F*(1, 18) = 4.8, *p* < .04) (Fig. 4A), which indicates a reduced mean interference effect in the self-focus block (Fig. 4B).	*F*(1, 18) = 4.8, *p* = .042
Butler et al. (2016): Exp. 1 ∇	Experiments 1 and 2 will investigate the extent to which facial expressions impact automatic imitation, whereas Experiments 3 and 4 will investigate the extent to which trait information from faces influences automatic imitation.	3 (face type: angry vs. happy vs. neutral) x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	We also entered the RT data into a 3 (face type: angry, happy, neutral) × 2 (congruency: congruent, incongruent) ANOVA, and report the face type by congruency interaction [*F*(2, 52) = 2.97, *p* = 0.060, η_p^2 = 0.102].	*F*(2, 52) = 2.97, *p* = .060
Butler et al. (2016): Exp. 2 ∇	Same as above	4 (face type: angry vs. happy vs. neutral vs. salient neutral) x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	We also entered the RT data into a 2 (face-type) × 2 (congruency) repeated measures ANOVA, and report the face type by congruency interaction [*F*(1, 44) = 6.22, *p* = 0.016 η_p^2= 0.124] for direct comparison to prior work (Rauchbauer et al., 2015).	*F*(1, 44) = 6.22, *p* = .016
Butler et al. (2016): Exp. 3 ∇	Same as above	3 (face type: high agreeable vs. low agreeable vs. neutral) x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	We also entered the RT data into a 2 × 2 ANOVA, and report the face type by congruency interaction [*F*(1, 30) = 1.52, *p* = 0.023, η_p^2 = 0.048].	*F*(1, 30) = 1.52, *p* = .023

Butler et al. (2016): Exp. 4 ∇	Same as above	2 (face type: low vs. high) x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	We also entered the RT data into a 2 face-type × 2 congruency ANOVA, and report the face type by congruency interaction [*F*(1, 51) = 1.27, *p* = 0.265, η_p^2 = 0.024].	*F*(1, 51) = 1.27, *p* = .265
Newey et al. (2019) ∇	Finally, in line with previous findings, we expect that first person, prosocial priming will produce a larger congruency effect than both third person and control conditions.	2 (trial type: congruency: congruent vs. incongruent) x 3 (perspective taking group: first person prosocial priming vs. third person prosocial priming vs. control group) mixed design	Two-way interaction	Crucially, there was no interaction between congruency and group *F*(2,144) =0.943, *p*=.392, η_p^2 =.013 indicating there was no differential effect of priming on congruency between groups.	*F*(2, 144) = 0.94, *p* = .392
Longo & Bertenthal (2009) ∇	If the human bias for mirroring extends to the surface appearance of an actor, automatic imitation of the virtual hand should be reduced or eliminated when attention is directed to its artificiality.	2 (trial compatibility: compatible vs. incompatible)x 2 (instructions: virtualness mentioned vs. not mentioned) x 2 (contrast: only virtual hand presented vs. both virtual and video hands presented) mixed design	Two-way interaction	There was a significant interaction of compatibility and instructions, *F*(1, 116) = 8.32, *p* < 0.005 (see Fig. 2), with significantly less automatic imitation (measured as the difference between RT in the incompatible and compatible conditions) of the virtual hand when its artificiality had been mentioned (4 ms), than when it had not (11 ms).	*F*(1, 116) = 8.32, *p* = .005
Westfal, Koechling, et al. (2024) ∇	Quoted text from pre-registration: We will test the hypothesis that participants primed with pro-social words show a larger automatic imitation effect (i.e., congruency effect) than those primed with anti-social words.	3 (priming: pro-social vs. neutral vs. anti-social) x 2 (trial type: congruent vs. incongruent) mixed design	Two-way interaction	Congruency*condition, *F*(2, 436) = 0.42, *p* = .655, η^2 = .002	*F*(2, 436) = 0.42, *p* = .655
Rauchbauer et al. (2018) ∇	Fourth, in line with Press et al. (2006) we expected no modulation of the automatic imitation effect, as well as baseline adjusted movement inhibition (IIc) and facilitation (FIc) indices due to ethnicity.	2 (group: black vs. white) x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	Further we found a trend effect of Ethnicity with longer reaction times for Black than White hand stimuli (*F*(1, 28) = 3.18, *p* = 0.085, partial η^2 = 0.102; Black: *M*= 514.99, *SE* = 10.769; White: *M*= 512.22, *SE* = 10.51) in the absence of an interaction effect (*F*(2, 56)= 1.554, *p* = 0.220).	*F*(2, 56) = 1.55, *p* = .220
Press et al. (2006): Exp. 1 ∇	If beliefs about animacy exert a top-down influence on functioning of the mirror system, one would expect more effective priming by the human than by the robotic stimuli.	2 (belief about stimulus type: robotic vs. human x 2 (congruency: congruent vs. incongruent) within-subjects design	Two-way interaction	This RT difference was not greater for stimuli believed to be human than for stimuli believed to be robotic. In fact, the RT difference was numerically, but not statistically, greater for stimuli believed to be robotic than for stimuli believed to be human (*F*(1,11) = 1.8, *p* = 0.2).	*F*(1, 11) = 1.8, *p* = .207
Gleibs et al. (2016) ∇	However, the previously reviewed research by Miles etal. [9] suggested that in a cooperative situation, imitation might serve as a means to reduce social distance and to increase interpersonal rapport with an outgroup target, predicting that imitation of an outgroup member will be stronger than imitation of an ingroup member. In sum, we explored how the effects of social group membership and anticipated goals impact on the level of automatic imitation.	2 (group: ingroup vs. outgroup) x 2 (situation: cooperative vs. competitive) within-subjects design	Main effect	When conducting a 2 (Group Affiliation: ingroup, outgroup) by 2 (Situation: cooperative, competitive) ANOVA with imitation effect as the dependent variable we found that only the two-way interaction was significant, *F*(1,95) =5.26, *p* =.024, η^2 =.05. Neither the main effect for group affiliation, (*F*(1,95) =.78, *p* =.38, η^2 =.008), nor for the situation (*F*(1,95) =1.60, *p* = .21, η^2 =.017) were statistically significant.	*F*(1, 95) = 0.78, *p* = .379 *F*(1,95) = 1.60, *p* = .209

Genschow et al. (2022): Exp. 1 ∇	The goal of the present research was to test the hypothesis that automatic imitation is stronger for in-group members than for out-group members.	3 (hand nationality: Chinese vs. American vs. German) x 2 trial condition: congruent vs. incongruent) design	Difference of means	Automatic imitation of in- and out-group members: To test whether participants imitated members of the in-group more strongly than members of the out-group, we compared participants' congruency effect (i.e., the difference between congruent and incongruent trials) for in- and out-group trials. ***Note:* As the data was only analyzed in a meta-analysis I report here the different effects individually for each experiment.**	t(144) = 0.86, p = .197
Genschow et al. (2022): Exp. 2 ∇	Same as above	Same as above	Difference of means	Same as above	t(120) = 0.18, p = .427
Genschow et al. (2022): Exp. 3 ∇	Same as above	Same as above	Difference of means	Same as above	t(111) = 1.39, p = .084
Genschow et al. (2022): Exp. 4 ∇	Same as above	Same as above	Difference of means	Same as above	t(125) = 0.30, p = .381
Klapper et al. (2014) ∇	Thus, evaluating the interactions in our factorial design between congruency, form, and belief directly addresses our main research question, which is how cues to animacy influence brain circuits of imitative control (Figure 1). Consistent with previous work (Gowen & Poliakoff, 2012; Press, 2011), we expect animacy cues to increase the potency of automatic imitation.	2 (congruency: congruent vs. incongruent x 2 (form: human vs. non-human) x2 (belief: motion capture vs. computer animation) within-subjects design	Two-way interaction	Regarding our top–down hypothesis, the results showed a significant two-way interaction between Congruency and Belief, F(1, 18) = 4.72, p =.04, η_p^2 = .21.	F(1, 18) = 4.72, p = .043
Rauchbauer et al. (2016): Exp. 1 ∇	Our main hypotheses were that mimicry can be flexibly regulated to either reciprocate positive affiliative signals, such as when responding to a smiling interaction partner—or to support appeasement in response to counter-affiliative signals, such as the ones conveyed by an angry and potentially threatening interaction partner.	2 (group-membership: in-group vs. out-group) x 2 (emotional expressions: happy vs. angry) x 3 (congruency: congruent vs. incongruent vs. baseline) within-subjects design	Three-way interaction	More importantly the results revealed a GROUP X EMOTION X CONGRUENCY interaction (F (2,122) = 4.169, p = .018, partial η^2 = .06).	F (2, 122) = 4.17, p = .018
Rauchbauer et al. (2016): Exp. 2 ∇	Same as above	2 (group-membership: in-group vs. out-group) x 2 (emotional expressions: happy vs. angry) x 3 (congruency: congruent vs. incongruent vs. baseline) within-subjects design	Three-way interaction	For the interaction effect of GROUP X EMOTION X CONGRUENCY we observed a trend towards significance (F(2, 120) = 2.25, p = .11, partial η^2 = .04).	F(2, 120) = 2.25, p = .110
Rauchbauer et al. (2016): Exp. 3 ∇	The focus of this analysis was to fortify our hypothesis that the modulation of the mimicry effect between the Personal Threat vs. the No Personal Threat condition was driven by response facilitation.	2 (group: in-group vs. out-group) x 3 (threat: happy vs. angry/personal threat vs. angry/no personal threat) x 2 (congruency: congruent vs. incongruent) within-subjects design	Three-way interaction	Results also revealed a significant interaction effect of THREAT X GROUP X CONGRUENCY (F(1,57) = 6.95, p = .011, partial η^2 = .11)	F(1, 57) = 6.95, p = .011

Liepelt et al. (2008): Exp. 4 ∇	If direct matching is modulated by intention attribution, we expect a smaller motor-priming effect for the passive group compared to the intentional group.	2 (group: external source vs. internal intention) x 2 (congruency: congruent vs. incongruent) within-subject design	Two-way interaction	Most importantly, a significant Group x Congruency interaction was observed, F(1, 24) = 7.49, MSE = 212.45, p < .05, partial η^2 = .24, indicating a difference in motor priming between the external source group and the internal- intention group.	F(1, 24) = 7.49, p = .011
Press et al. (2005) ∗	Thus, both hypotheses predict an interaction between stimulus – response compatibility and stimulus type, but only the associative learning hypothesis predicts a reliable effect of compatibility when the stimuli are robotic.	2 (stimulus response compatibility: compatible vs. incompatible) x 2 (stimulus type: human vs. robotic) x2 (stimulus style: naturalistic vs. schematic) within-subjects design	Two-way interaction	This analysis revealed a significant main effect of compatibility, F(1,15) = 21.4, p < 0.001, and a significant stimulus type x compatibility interaction, F(1,15) = 25.1, p < 0.001.	F(1, 15) = 25.1, p = 0.0002
Klapper et al. (2014) ∗	Thus, evaluating the interactions in our factorial design between congruency, form, and belief directly addresses our main research question, which is how cues to animacy influence brain circuits of imitative control (Figure 1). Consistent with previous work (Gowen & Poliakoff, 2012; Press, 2011), we expect animacy cues to increase the potency of automatic imitation.	2 (congruency: congruent vs. incongruent x 2 (form: human vs. non-human) x2 (belief: motion capture vs. computer animation) within-subjects design	Two-way interaction	This analysis showed that the Congruency × Hand interaction was not significant within motion capture blocks (F < 1), but significant within computer animation blocks, F(1, 18) = 14.86, p =.001, η_p^2= .45.	F(1, 18) = 14.86, p =.001
Genschow et al. (2022): Exp. 5 ∗	The goal of the present research was to test the hypothesis that automatic imitation is stronger for in-group members than for out-group members.	2 (congruency: congruent vs. incongruent) x 2 (skin color: black vs. white) x 2 (ethnicity: African American vs. European American) within-subjects design	Difference of means	Automatic imitation of in- and out-group members: To test whether participants imitated members of the in-group more strongly than members of the out-group, we compared participants' congruency effect (i.e., the difference between congruent and incongruent trials) for in- and out-group trials. ***Note:*** **As the data was only analyzed in a meta-analysis I report here the different effects individually for each experiment.**	t(296) = 0.16, p = .437
Genschow et al. (2022): Exp. 6 ∗	Same as above	Same as above	Difference of means	Same as above	t(736) = 0.69, p = .245
Liepelt et al. (2008): Exp. 1 ∗	In the first experiment, we tested whether direct matching as indexed by motor priming is influenced by the goal that is attributed to the observed action. In the present study, goal refers to the inferred end position of a movement.	3 (observed movement: SMC vs. AMC vs. MMC) x 2 (congruency: congruent vs. incongruent) within-subject design	Difference of means	Most important, for the present research question was the comparison of motor-priming effects between the AMC and the MMC. This comparison indicated an increased motor-priming effect in the AMC compared to the MMC, amounting to 25 ms, t(17)=– 4.09,p<.001.	t(17) = – 4.09, p = .0008
Liepelt et al. (2008): Exp. 2 ∗	In line with the findings of Experiment 1, we predicted a larger motor-priming effect in the AMC condition than in the AMC–CO condition.	2 (observed movement: AMC-CO vs. AMC) x 2 (congruency: congruent vs. incongruent) within-subject design	Two-way interaction	The most important finding was a significant interaction of Observed Movement x Congruency, F(1, 16) = 4.80, MSE = 279.21, p < .05, partial η^2 = .23, indicating different motor-priming effects for conditions.	F(1, 16) = 4.80, p = .044
Liepelt et al. (2008): Exp. 3 ∗	In Experiment 3, we aimed to extend these findings with a reversed logic. If perceived intention modulates motor priming, removing the perceived intentionality should decrease the size of the motor-priming effect.	2 (observed movement: SMC vs. PMC) x 2 (congruency: congruent vs. incongruent) within-subject design	Two-way interaction	Most importantly, a significant interaction of Observed Movement x Congruency was observed, F(1, 19) = 4.63, MSE = 344.42, p < .05, partial η^2 = .20, indicating a difference in motor priming between the SMC and the PMC.	F(1, 19) = 4.63, p = .044

Cracco, Genschow, et al. (2018): Exp. 1 ∗	If automatic imitation is sensitive to social norms, then the congruency effect should be weaker when an antisocial gesture is observed than when a prosocial gesture is observed.	2 (gesture: antisocial vs. prosocial) x 2 (congruency: congruent vs. incongruent) within-subject design with reaction time as dependent variable	Two-way interaction	As predicted, there was also a significant observed gesture x congruency interaction, $F(1,36) = 7.41$, $p = .010$, $dz = 0.45$, *CI 95%* = [0.11, 0.78], which indicated that the congruency effect was larger when the prosocial gesture was observed, $t(36) = 5.40$, $p < .001$, $dz = 0.89$, *CI 95%* = [0.50, 1.26], than when the antisocial gesture was observed, $t(36) = 1.91$, $p = 0.064$, $dz = 0.31$, *CI 95%* = [-0.02, 0.64].	$F(1, 36) = 7.41$, $p = .010$
Cracco, Genschow, et al. (2018): Exp. 2 ∗	Same as above	2 (context: antisocial vs. prosocial) x 2 (gesture: antisocial vs. prosocial) x 2 (congruency: congruent vs. incongruent) within-subject design	Two-way interaction	In line with Experiment 1, we also found a significant observed gesture x congruency interaction, $F(1,117) = 13.67$, $p < .001$, $dz = 0.34$, *CI 95%* = [0.15, 0.53], which indicated that the congruency effect was larger when a prosocial gesture was observed, $t(117) = 8.07$, $p < .001$, $dz = 0.74$, *CI 95%* = [0.54, 0.95], than when an antisocial gesture was observed, $t(117) = 5.23$, $p < .001$, $dz = 0.48$, *CI 95%* = [0.29, 0.67].	$F(1, 117) = 13.67$, $p = .0003$
Cracco, Genschow, et al. (2018): Exp. 3 ∗	Same as above	2 (context: antisocial vs. prosocial) x 2 (gesture: antisocial vs. prosocial) x 2 (congruency: congruent vs. incongruent) within-subject design	Two-way interaction	As predicted, the observed gesture x congruency interaction was also significant, $F(1, 117) = 8.83$, $p = .004$, $dz = 0.27$, *CI 95%* = [0.09, 0.46], showing that the congruency effect was stronger when a prosocial gesture was observed, $t(117) = 8.45$, $p < .001$, $dz = 0.78$, *CI 95%* = [0.57, 0.98], than when an antisocial gesture was observed, $t(117) = 5.78$, $p < .001$, $dz = 0.53$, *CI 95%* = [0.34, 0.72].	$F(1, 117) = 8.83$, $p = .004$
Trilla et al. (2020) ∗	To sum up, the current study aimed to (1) re-test the influence of direct and averted gaze on automatic imitation using a task that disentangled imitative and spatial compatibility, and (2) examine whether autistic traits modulate the influence of gaze on compatibility effects.	2 (imitative compatibility: compatible vs. incompatible) x 2 (gaze: directed vs. averted) x 2 (spatial compatibility: compatible vs. incompatible) within-subject design	Three-way interaction	However, AQ significantly modulated the effect of gaze on imitative compatibility, $b = -1.29$, *95% CI* = [−2.28, −0.31], $SE = 0.50$, $t(59) = -2.59$, $p = 0.01$.	$t = -2.59$, $p = .012$
Westfal et al. (2023) ∗	As several previous experiments on the animacy effect (e.g., Cracco et al., 2015; Klapper et al., 2014; Press et al., 2006) confounded animacy with the size of finger movements, the question arises whether the animacy effect can still be detected if the stimuli control for movement size.	2 (trial condition: congruent vs. incongruent) x 3 (hand: human vs. robotic vs. robotic corrected) within-subjects design	Difference of means	The congruency effect was stronger for the human hand ($M = 71.58$, $SD = 43.44$) than for the robotic hand ($M = 46.54$, $SD = 45.76$), $t(279) = 10.69$, $p < .001$, $d_z = 0.64$, *CI 95%* = [0.51, 0.77]. The congruency effect was also stronger for the human hand ($M = 71.58$, $SD = 43.44$) than for the robotic corrected hand ($M = 43.77$, $SD = 41.60$), $t(279) = 12.21$, $p < .001$, $d_z = 0.73$, *CI 95%* = [0.60, 0.86].	$t(279) = 10.69$, $p < .00001$ $t(279) = 12.21$, $p < .00001$
Westfal, Cracco, et al. (2022) ∗	Specifically, we investigated whether automatic imitation is different for robotic as compared to human hands. In laboratory settings, typically, a smaller automatic imitation effect is detected for robotic hands compared to human hands (Bird et al., 2007b; Chaminade & Cheng, 2009; Kilner et al., 2003, 2003; Press et al., 2005, 2006).	2 (trial condition: congruent vs. incongruent) x 2 (hand: human vs. robotic) within-subjects design	Two-way interaction	More importantly, the interaction was significant as well: This means that the congruency effect was stronger for human hands ($M = 45.14$, $SD = 37.32$) than robotic hands ($M = 34.26$, $SD = 40.42$), $F(1, 103) = 7.87$, $p = .006$, $\eta_p^2 = .07$ (see Figure 11).	$F(1, 103) = 7.87$, $p = .006$

Bird, Leighton, et al. (2007) ∗	Second, an ‘animacy effect’: the automatic imitation effect is greater when the observed action is performed by a human effector than when it is performed by a human-like mechanical device, or ‘robot’ (Kilner et al. 2003; Press et al. 2005).	2 (trial condition: congruent vs. incongruent) x 2 (hand: human vs. robotic) x 2 (autism: ASD vs. control group) mixed design	Two-way interaction	The interaction between trial type and stimulus type was also significant (‘animacy bias’, $F(1,30) = 29.6$, $p < 0.001$, $\eta_p^2 = .50$).	$F(1,30) = 29.6$, $p = 0.00001$
Chiavarino et al. (2013) ∗	Based on the previously reviewed literature, we assumed that goal-directed actions would primarily elicit a coding based on the goal of the action, whereas actions devoid of a goal would automatically trigger movement coding.	2 (trial condition: congruent vs. incongruent) x 2 (movement: goal-directed vs. goal-less) x 2 (presentation type: blocked vs. random) x 2 (task: automatic imitation vs. spatial compatibility) mixed design	Two-way interaction	More importantly, there was an Action Type · Compatibility interaction, $F(1, 46) = 5.4$, $p =.025$,g2 = .11, revealing that the effect of compatibility was stronger for the goal-less than for the goal-directed actions.	$F(1, 46) = 5.4$, $p = .025$
Haffey et al. (2013) ∗	We predicted that trait empathy will be positively associated with the extent of automatic mimicry for rewarding human hands compared to non-rewarding human hands	2 (compatibility: congruent vs. incongruent) x 2 (sociality: human vs. robotic) x 2 (reward: high vs. low) within-subjects design	Three-way interaction	This showed that there was no Sociality × Reward × Compatibility interaction ($F(1,35) = 0.215$, $p = 0.646$;	$F(1,35) = 0.22$, $p = .646$
Press et al. (2007) ∗	Therefore, compared with group IT, group CT should show a smaller human bias at post-test than at pre-test.	2 (stimuli: human vs. robotic) x 2 (group: CT vs. IT) x 2 (session: pre-test vs. post-test) mixed design	Two-way interaction	Simple effects analyses comparing effects of compatibility with human and robotic stimuli showed that there was a significant effect of animacy in both training groups at pre-test (group CT: $F(1,7)=41.4$, $p<0.001$; group IT: $F(1,7)=18.8$, $p<0.005$).	$F(1 ,7) = 41.4$, $p = .0004$ $F(1, 7)= 18.8$, $p = .003$
Press et al. (2006): Exp. 2 ∗	In this factorial design, an effect of stimulus type would indicate bottom-up processing.	2 (stimulus type: robotic vs. human x 2 (congruency: congruent vs. incongruent) x 2 (belief: human vs. robotic) within-subjects design	Two-way interaction	There was a main effect of compatibility ($F(1,22) = 20.7$, $p < 0.001$) and a genuine stimulus type–compatibility interaction ($F(1,22) = 4.5$, $p < 0.05$).	$F(1, 22) = 4.5$, $p = 0.045$
Longo et al. (2008): Exp. 1 ∗	If the actions are coded in terms of goals, comparable levels of automatic imitation should be elicited from both types of action, because the goals are the same.	2 (compatibility: compatible vs. incompatible) x 2 (movement: possible vs. impossible) within-subjects design with reaction time as dependent variable	Two-way interaction	RT was comparable with possible (315.85 ms) and impossible (317.76) movements, $F(1, 23) = 0.67$, ns, $\eta_p^2 = .028$, and there was no significant interaction between movement and compatibility, $F(1, 23) = 0.04$, ns, $\eta_p^2 = .002$.	$F(1, 23) = 0.04$, $p = .843$
Longo et al. (2008): Exp. 2 ∗	Same as above	Same as above	Two-way interaction	In contrast to Experiment 1, there was a significant interaction between compatibility and movement, $F(1, 23) = 7.75$, $p = .01$, $\eta_p^2 = .252$, indicating that the amount of automatic imitation was modulated by whether a possible or impossible action was observed.	$F(1, 23) = 7.75$, $p = .011$
Longo et al. (2008): Exp. 3 ∗	Same as above	Same as above	Two-way interaction	Like Experiment 1, and unlike Experiment 2, there was no interaction between movement and compatibility, $F(1, 23) = 0.09$, ns, $\eta_p^2 = .004$.	$F(1, 23) = 0.09$, $p = .767$

∇ = top-down manipulation, ∗ = bottom-up manipulation

Note. Background image modification was not counted as bottom-up if the manipulation did not involve the hand itself, thus as only the belief was manipulated and not the motor input, it was counted as top-down manipulation.