Instantiations of the cognitive grounding of linguistically expressed temporal information

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Contents

Summary

Time is a fundamental concept for human behaviour and existence. It is ubiquitous in language, yet, its mental representation is not resolved. Grounded cognition accounts suggest that the mental representation of time as an abstract concept is based on perceptual and motor brain areas. The present dissertation investigated this hypothesis for two instances of grounding of linguistically expressed temporal information. Firstly, the grounding of deictic and sequential time in space by means of the activation of the mental timeline was examined. Secondly, the grounding of duration information via simulation, i.e., the activation of modal representations, upon the processing of expressions containing manner of motion verbs with associated slow or fast movement on a given path was assessed.

This was implemented in three approaches. Firstly, a meta-analysis on the space-time congruency effect was conducted. The results show that the effect is robust when time is task relevant, implying that the mental timeline gets activated when temporal reasoning takes place. Secondly, it was investigated whether the reaction time pattern of faster responses to fast-speed sentences (e.g., *Clara is dashing to the hospital*) compared to slow-speed sentences (e.g., *Clara is limping to the hospital*) in a motion detection task reflects a processing difference that is ascribable to the simulation of the denoted events' duration. The results indicate that this might be the case. However, since the observed pattern asks for an adjustment of the original hypothesis about how the simulation of duration is reflected in reaction times, conclusions cannot be drawn until verification studies have been carried out. Thirdly, it was assessed whether linguistically expressed duration information acts on the internal clock like physical duration, thus drawing on the field of temporal cognition to proceed with questions raised within the grounded cognition debate. Reproduced durations increased with an increase in the duration of the denoted events, which implies that linguistically expressed duration affects perceived duration like physical duration.

Importantly, this dissertation also assessed whether the mentioned effects can be considered instances of automatic activation of modal representations. Yet, task variations indicate that this is not the case. By seeking out contexts in which the above-mentioned grounded cognition effects are reliable, this dissertation contributes to the ongoing project of increasing our understanding of the conditions under which modal representations are activated during language processing.

Zusammenfassung

Zeit ist ein fundamentales Konzept für menschliches Verhalten und menschliche Existenz. Es ist sprachlich allgegenwärtig; dennoch ist dessen mentale Repräsentation nicht geklärt. *Grounded-Cognition*-Ansätze legen nahe, dass die mentale Repräsentation von Zeit als abstraktem Begriff auf perzeptuellen und motorischen Gehirnarealen basiert. In der vorliegenden Dissertation wurde diese Hypothese an zwei Beispielen der Verknüpfung von sprachlichen und nicht-sprachlichen Prozessen bei der Verarbeitung von sprachlich ausgedrückter temporaler Information untersucht: Die Verknüpfung von deiktischer und sequentieller temporaler Information mit räumlichen Erfahrungen wurde anhand der Aktivierung des mentalen Zeitstrahls beleuchtet; die Verknüpfung von Dauerinformation mit nicht-sprachlichen Prozessen wurde anhand von Simulationen, das heißt der Aktivierung von modalen Repräsentationen, untersucht, die bei der Verarbeitung von sprachlichen Äußerungen ausgelöst werden, die Manner-of-motion-Verben mit langsam oder schnell assoziierter Bewegung auf einem gegebenen Weg enthalten.

Dieses Vorhaben wurde in drei Herangehensweisen umgesetzt. Erstens wurde eine Meta-Analyse des Raum-Zeit-Kongruenzeffektes durchgeführt. Die Ergebnisse sprechen dafür, dass der Effekt robust ist, wenn das Konzept Zeit aufgabenrelevant ist, was impliziert, dass der mentale Zeitstrahl aktiviert wird, wenn Schlussfolgerungen über zeitliche Aspekte gezogen werden. Zweitens wurde untersucht, ob das Reaktionszeitmuster von schnelleren Reaktionen bei Sätzen, die schnelle Bewegung ausdrücken (z.B. *Clara prescht zum Krankenhaus*), im Vergleich zu Sätzen, die langsame Bewegung ausdrücken (z.B. *Clara humpelt zum Krankenhaus*), in der Aufgabe, Bewegung in den Sätzen zu erkennen, einen Verarbeitungsunterschied darstellt, der auf die Simulation der Dauer der denotierten Ereignisse zurückführbar ist. Die Ergebnisse deuten an, dass dies der Fall sein könnte. Allerdings erfordert das beobachtete Reaktionszeitmuster eine Anpassung der ursprünglichen Hypothese, wie sich die Simulation von Dauer in Reaktionszeiten widerspiegelt, weswegen Schlussfolgerungen nicht gezogen werden können, solange keine Studien zur Verifikation des Effektes durchgeführt worden sind. Drittens wurde geprüft, ob sprachlich ausgedrückte Dauerinformation die interne Uhr wie physische Dauer beeinflusst, womit auf das Wissenschaftsfeld der Zeitkognition zurückgegriffen wurde, um in Bezug auf Fragen weiterzukommen, die in der *Grounded-Cognition*-Debatte aufgeworfen werden. Die reproduzierten Zeiten nahmen mit einem Anstieg der Dauer der denotierten Ereignisse zu, was dafür spricht, dass sprachlich ausgedrückte Dauer die wahrgenommene Dauer analog zu physischer Dauer beeinflusst.

Darüber hinaus wurde in dieser Arbeit auch untersucht, ob die oben erwähnten Effekte als Fälle von automatischer Aktivierung von modaler Repräsentation betrachtet werden können. Allerdings zeigen Aufgabenabwandlungen, dass dies nicht der Fall ist. Indem Kontexte ausfindig gemacht wurden, in denen die oben erwähnten *Grounded-Cognition*-Effekte zuverlässig auftreten, trägt diese Arbeit zu dem fortlaufenden Projekt bei, unser Verständnis über die Bedingungen zu vergrößern, unter denen modale Repräsentation bei der Sprachverarbeitung aktiviert werden.

Chapter 1

Introduction

In what sense, if any, can time be said to exist?

Aristotle as cited in Gale (1968, p. 1)

The meaning of time:
The ubiquity of a concept and its unresolved mental representation

The interest of humankind in the nature of time can be traced back to questions raised by pre-Socratic philosophers (Michon & Jackson, 1985; Sherover, 1975). Not only due to these historic roots of philosophical debate, but also on account of the pragmatic but existential need of an organism to correlate its behaviour with the temporal course of events in the environment, time is a fundamental concept for human existence (Michon & Jackson, 1985). Even though the Western philosophical debate has moved from early contemplations on the concrete experience of the dynamic continuity of change in the world to relegating time as illusion (Sherover, 1975), as also manifested in the above quoted question raised by Aristotle, it is the former undisputable existential significance of time for human experience, upon which the present dissertation builds its foundation.

As Michon and Jackson point out, "[a]cting five or ten milliseconds too early or too late may demarcate the dividing line between survival and death" (1985, p. 4), which indicates that the tuning of an organism to the temporal contingencies of its environment is essential for its survival. Thus, the creation of temporal order is central to the mental representation of reality, which is also apparent in the ubiquity of time in language: referring to time when describing a situation is inevitable (Madden & Ferretti, 2009). This is because time comes into play via tenses and is thus present in every finite verb phrase. Moreover, even verbs themselves can be distinguished based on their temporal structure or *time schemata*, as suggested by Vendler (1957). He classifies verbs into activities and accomplishments that possess continuous tenses, i.e., they can be used felicitously with present progressive (e.g., *I am drawing a circle*) on the one hand, and states and achievements on the other hand, for which the latter does not hold (e.g., *I am knowing*). The sub-classification of these two groups is also based on the respective temporal structures. While activities (e.g., *running*) go on in time for an indefinite duration in a homogenous way with any part of the process being the same as the whole, this is not true for accomplishments that proceed towards an inherent end and that accordingly take a certain amount of time (e.g., *drawing a circle*). In the group of verbs that do not consist of continuous phases succeeding one another in time, states last for a (short or a long) period of time (e.g., *possessing*), while achievements occur at a single moment in time (e.g., *reaching the summit*).[1] In addition to these cardinal realizations of temporal information in language, there are temporal adverbials (e.g., *always*, *yesterday*) and temporal connectives (e.g., *during*, *until*) to express temporal structure (van Benthem, 1985).

Yet, it is not resolved how the concept of time is represented mentally, or more specifically, how the meaning of temporal information expressed in language is generated (Evans, 2003; Le Poidevin, 2004). Despite its ubiquity in the structuring of experience (Michon & Jackson, 1985), time is an abstract concept as we cannot perceive it. There is neither a sense organ for time nor a decisive physical stimulus (Grondin, 2001). As a consequence, the mental representation of temporal concepts is considered to be grounded in modal, nonsymbolic representations (Santiago et al., 2011). Without the assumption of grounded meaning representations, one would end up at the *symbol grounding problem*, i.e., the question of how symbols can be meaningful if their meaning only comes into play via their relation to other meaningless symbols (Harnad, 1990). Thus, the concept of time, like other abstract concepts, is assumed to be built upon concrete experience with the physical world. The present dissertation assessed to what extent, and in what way, the mental representation of temporal information given in language expressions is grounded in nonsymbolic, modal representations.

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[1] Even though Vendler's impact on linguistics is substantial (Filip, 2019, p. 275), the definition of accomplishments, achievements, activities (i.e., processes in more recent classifications), and states has been extensively discussed, developed, and alternative classifications have been suggested (Maienborn, 2019; for an overview see Filip, 2019).

Time, space, and the mental timeline

One path to follow when trying to understand the grounding of the concept of time is to consider the structural similarity in our thinking and reasoning about space and time. This has been taken as an indication that we conceptualize time in terms of our concrete experience with space, which is also referred to as *the spatial metaphor of time* (Clark, 1973; Lakoff & Johnson, 1983; van Benthem, 1985). The conceptual relatedness between these two domains becomes apparent in natural language, as there is a considerable overlap between spatial and temporal terms (Haspelmath, 1997). For instance, the same prepositions are used to express spatial and temporal reference (e.g., *I drew my brother* ***in*** *the living room* and *I drew my brother* ***in*** *2010*). Yet, even though the grounding of time in space in linguistic expressions is apparent in most languages across the world and thus considered a universal transfer by Haspelmath (1997), it is not given that this transfer also has a cognitive reality when temporal information expressed in language is processed.

One instance that can serve to investigate the cognitive reality of the interaction between temporal and spatial reasoning is the mental timeline. The mental timeline is a linear construal of deictic and sequential time with the present moment (*now*) being mapped onto the spatial deictic centre (*here*) (Núñez & Cooperrider, 2013). While deictic time represents the referring to time (i.e., to some point in the past, in the present, or in the future) relative to the time of utterance, which is the temporal reference point, sequential time stands for a sequencing of events (e.g., *the ceremony precedes the dinner*) with respect to each other (Traugott, 1978). The specific direction and axis of the mental timeline depends on environmental factors and collective worldviews (Núñez & Cooperrider, 2013). The sagittal axis running from the back (past) to the front (future) is the most common construal of time due to the human anatomy and way of moving forward in space (Núñez & Cooperrider, 2013; Núñez & Sweetser, 2006, note, however, that this mapping is reversed for the Aymara of the Andes). Other prevalent realizations of the mental timeline are the one that recruits the lateral axis with the past on the left and the future on the right for most cultures with a left-to-right writing system and the vertical axis running from top to bottom for Mandarin speakers (Bergen & Chan Lau, 2012). When reasoning about temporal sequences people can make use of the mental timeline by ordering the events along its linear trajectory (Eikmeier et al., 2016).

The psychological reality of the mental timeline can be observed behaviourally, such as when people engage with co-speech gestures or are told to arrange cards in temporal order (Bergen & Chan Lau, 2012; Casasanto & Jasmin, 2012; Núñez & Sweetser, 2006). That the mental timeline might also be involved in the processing of sequential and deictic temporal information given in natural language is suggested by several reaction time (RT) studies. In these studies, participants are typically presented with linguistic stimuli referring to the past or the future and asked to respond with two buttons that are arranged along one of the above mentioned axes (e.g., Ulrich & Maienborn, 2010). Importantly, responses are faster when the buttons are arranged congruently to the mental timeline's direction compared to an incongruent arrangement. This behavioural pattern is referred to as the space-time-congruency effect and is explained via the activation of the mental timeline during the processing of stimuli with temporal reference.

Simulation of duration

While the grounding of deictic and sequential time in a linear spatial trajectory becomes apparent in utterances like *In the weeks ahead of us* ... (Lakoff & Johnson, 1983), the grounding of duration, as another type of temporal information, is not as clearly deducible from language use and metaphoric conceptualizations. Duration sometimes is conceptualized as a valuable, but limited resource or money in figurative speech (e.g., *you are wasting your time*, *we run out of time*, *this meeting costs me three hours*; Eikmeier et al., 2016, p. 105). Yet, it is unlikely that the concepts of a limited resource or money are recruited when trying to understand the notion of duration that is expressed in language. Instead, there are first indications that duration is grounded by means of the simulation of duration. Simulation accounts of language comprehension assume that the understanding of situations expressed in language emerges with the aid of the simulating of the described events, i.e., the activation of sensorimotor representations that are also present when actually experiencing similar real events (Barsalou, 1999; Glenberg & Kaschak, 2002). Along these lines, Claus and Kelter (2006) observed longer residual reading times for sentences describing events with long durations (e.g., *For three hours they quarrel about Frank's mother*) compared to sentences describing events with shorter durations (e.g., *For five minutes they quarrel about Frank's mother*). This implies that linguistically expressed duration might be grounded in such a way that the simulation of the described events incorporates the events' durations, and is reflected in reading or reaction times (see Matlock, 2004 for reaction times).

The present dissertation

This dissertation dealt with these two realizations of the grounding of linguistically expressed temporal information: the grounding of deictic and sequential temporal information in space by means of the mental timeline and the grounding of duration information in the simulation of described events with respect to their denoted duration. Its aim was to investigate whether the activation of grounded representations of temporal information during language processing is an automatic process and if not, under which conditions the activation of the corresponding modal representations can be observed.

To clarify the theoretical framework, the theory of grounded cognition and its relation to the term simulation that was brought up with respect to the grounding of duration is discussed shortly in the following section. In the last section of the introduction, the implementation of this dissertation's aims is introduced and the conducted studies, which are reported in Chapters 2 to 5, are outlined briefly. Their results are summarized in more detail in Chapter 6 and discussed with respect to the broader topics they relate to. These include the investigated effects' automaticity, the conditions for the involvement of modal representations in language processing, this dissertation's methodological implications, and the question of replicability, which is a highly relevant topic for the field of grounded cognition.

Grounded cognition

As mentioned prior, the theory of grounded cognition emerged out of the need to provide an answer to the question of how concepts can be meaningful if they are not linked to their referents but are solely defined based on their relation to other meaningless symbols (Harnad, 1990). The latter assumption was established by traditional theories of cognitive science which put forward a computational view of the mind and purported that cognitive operations might be based on the manipulation of amodal and arbitrary symbols (Pylyshyn, 1980). Yet, besides the symbol grounding problem, behavioural data gained from experiments, on for example, the processing of visual mental images suggested the existence of a representational format that has an analogue correspondence to the concepts' referents (Kosslyn, 1981). These findings were complemented by numerous behavioural and neuroscientific studies indicating that cognitive operations – including language processing – are grounded via perceptual and motor brain areas (for a collection of reviews see Cayol & Nazir, 2020; for reviews see e.g., Glenberg & Gallese, 2012; Willems & Casasanto, 2011).

Due to this form of grounding of cognition in the body, the term *embodied cognition* is often used interchangeably with *grounded cognition* (e.g., Knoeferle, 2021). Yet, grounded cognition refers to a wider spectrum of phenomena, as it also takes into account other forms of grounding, for instance the coupling of the brain with the physical or social environment (see Barsalou, 2020 for a collection of literature suggesting these forms of grounding). Moreover, Barsalou (2020) remarks that the term *embodied cognition* is by some researchers incorrectly considered to refer to a necessary involvement of the body in cognitive operations. This comment hints to two central and severe challenges for the field of research. The first one is that the vast interest in grounded cognition has led to the emergence of a wide spectrum of theories and frameworks (see Barsalou, 2016; or Meteyard et al., 2012 for an overview). As a consequence, it has been noted repeatedly that the field lacks a coherent research program and an all-encompassing theory (Ostarek & Huettig, 2019; Zwaan, 2021). A recent special collection edited by Ostarek, Fischer, and Huettig (launched in 2020 in the *Journal of*

Cognition) with the title *The future of embodiment research – Challenges and Opportunities* aims to overcome this issue by working towards a potential unification of the different approaches (Zwaan, 2021).

The second central concern within this field of research is that it threatens to stagnate around the question of whether cognition – or language processing more specifically – necessarily involves the activation of modal representations (e.g., Barsalou, 2016). This question was intensively discussed inter alia due to a lack of reliability of some of the findings (Zwaan, 2021). A prominent example, delineated in detail by Zwaan (2021) in this context, is the *action sentence compatibility effect* (ACE) first reported by Glenberg and Kaschak (2002) who observed that participants were faster to judge a sentence's sensicality when the direction of the response movement matched the direction of the denoted action. More precisely, a sentence with a denoted action direction towards the body such as *Andi delivered the pizza to you* was responded to faster with a movement towards the body than a movement away from the body. Likewise, a sentence that expressed an action with a direction away from the body such as *You delivered the pizza to Andi* was reacted to faster with a movement away from the body than with a movement towards the body. The authors took this as an indication that language understanding is grounded in bodily action by cognitively simulating the actions implied by the sentence. Due to the ACE's prominence despite doubts casted on the reliability of the effect (see e.g., Papesh, 2015), a recent pre-registered multi-lab-study involving 18 labs was designed to conduct a variant of the paradigm of Glenberg and Kaschak (2002) but failed to replicate the ACE (Morey et al., 2022). As Zwaan (2021) outlines, this illustrates the need for developing and finding paradigms that reliably produce an effect such that they can then be extended and varied in a second step to test whether they are only artefacts of the specific methods used. Only if this is not the case inferences can be made beyond the respective paradigm (Zwaan, 2021).

Nonetheless, it is also wrong to state that the field of grounded cognition has reached an impassable dead end. Due to the large number of empirical studies that speak for an engagement of modality-specific brain areas in language processing (i.e., brain structures that are traditionally considered to serve perceptual, affective, and motor processes), many researchers of the field have moved from the question of whether language is grounded to the question of its functional role (Barsalou, 2020; Knoeferle, 2021, p. 2). In this vein, Ostarek and Huettig (2019) stressed the importance of understanding the role of tasks and contexts as they considerably modulate the activation of modal representations during language processing. In developing a methodological approach that might be productive for embodiment research, Zwaan states that it would be an interesting case if a certain operationalisation of a paradigm shows a stable effect while another one does not, "as this might give rise to further theoretical and empirical work" (Zwaan, 2021, p. 7).

In this spirit, a central concern of the present dissertation was to seek out embodiment effects that are reliable across operationalisations and task instructions to contribute to the understanding of the conditions under which modal representations are activated during the processing of temporal information expressed in language. In the following, the terms *grounded* and *embodied cognition* are used interchangeably, however, neither term is intended to imply an involvement of modality-specific representations that is necessary for language processing. When the term *simulation* is used it refers to the activation of these modality-specific, or simply modal representations during language processing, if not stated or discussed otherwise explicitly (see e.g., Chapter 3).

Outline of the conducted studies

The first type of grounding of linguistically expressed temporal information, i.e., the grounding of time in space, was examined in a meta-analysis reported in Chapter 2. This was considered worthwhile because a large amount of RT studies has been published to investigate the space-time association that leads to the activation of the mental timeline. Yet, a wide variety of tasks was employed in these studies and the results are not always consistent. By providing a compilation of the published RT studies on the mental timeline, the meta-analysis was conducted to estimate the size of the effect of the underlying space-time association and the extent of potential publication bias in this field of research.

Moreover, to clarify under which conditions the grounding of time in space becomes evident, the incorporated experiments were sub-classified based on three types of task, i.e., they were grouped into experiments that make time a task-relevant dimension, experiments in which time is task irrelevant, and experiments in which temporal cues are used as primes. This subdivision was carried out because the existing empirical findings suggest that the extent to which time is made relevant in the experimental task has a systematic impact on whether or not the mental timeline is activated (e.g., Ulrich & Maienborn, 2010). More precisely, the mental timeline might only get activated when the concept of time is made salient, for example, by instructing the participants to categorize the temporal reference of the stimulus. The results of the meta-analysis suggest that this assumption can be upheld across multiple studies: experiments in which time is a task-relevant dimension have a mean effect size of $d = 0.46$, while the effect size of experiments in which time is task irrelevant does not significantly deviate from zero. The surprisingly high mean effect size of $d = 0.47$ ($d = 0.36$ after correction for publication bias) for temporal priming studies is discussed in Chapters 2 and 6.

Duration as the second type of linguistically expressed temporal information was investigated with respect to its grounding by examining the processing of sentences and expressions containing manner of motion verbs with associated slow or fast movement (e.g., *Clara is limping to the hospital* vs. *Clara is dashing to the hospital*; in the following referred to as slow-speed and fast-speed sentences, respectively). In contrast to the manipulation of duration via temporal adverbials (e.g., *for three hours* vs. *for five minutes*) in the study by Claus and Kelter (2006) the manipulation of duration as a function of speed and distance is very subtle. Yet, the concept of motion is historically and ontologically closely related to the concept of duration. For instance, early philosophical thinkers such as Plato and Aristotle related the concept of time to visible movement and perception of change (Sherover, 1975). Sherover even states that although these philosophers were “claiming to ask the ontological question ‘what is time?’, each one discussed its essential nature in terms of its experiential connection with motion and argued about which of the two – time or motion – is ontologically prior” (Sherover, 1975, p. 15). That motion might be central to understand the concept of time, or rather duration, can also be inferred from the fact that duration in the model of time in classical physics and in that of Piaget is defined based on the parameters time, speed, and distance (Montangero, 1985, p. 279). Consequently, modelling duration by means of motion at different speed seems a suitable approach to study the grounding of duration information.

The manipulation of duration by means of two levels of speed of motion on a given path is inspired by an early RT study by Wender and Weber (1982). They observed that fast-speed sentences (e.g., *The ball is flying into the goal*) were reacted to faster than slow-speed sentences (e.g., *The ball is rolling into the goal*) when participants were asked to imagine the sentences' content and to decide whether motion was expressed within these sentences. Since the observed RT pattern has an analogue relation to the duration of the described events (fast motion on a given path takes shorter than slow motion on the same path), the finding by Wender and Weber (1982) opens up the question of whether the reported RT pattern reflects a processing difference that is ascribable to the duration of the expressed events. In line with this assumption, Speed and Vigliocco (2014), who investigated the processing of fast-speed and slow-speed sentences in a visual world paradigm, argue that it is the simulation's duration that varies as a function of the speed conditions analogously to the observation of the event in the world.

To evaluate whether the RT effect reported by Wender and Weber (1982) reflects the simulation of the described events' duration, and to gain a better understanding of the underlying processes, their experiment was adopted and developed in Chapters 3 and 4. The first question investigated was whether the effect they reported is dependent on the explicit prompt to imagine the sentences' content. This is because even though both mental imagery and simulation engage overlapping sensorimotor brain areas, they have to be differentiated (Iachini, 2011) since only simulation is considered to be "part-and-parcel of routine cognitive processes" (Zwaan & Pecher, 2012, p. 9), while mental imagery is considered a conscious and resource-consuming process (see also Andres et al., 2015). Consequently, participants in Experiment 1 of Chapter 3 were engaged with the same motion detection task without being asked to imagine the sentences' content. We were able to replicate the speed effect, i.e., fast-speed sentences were reacted to faster than slow-speed sentences despite the changes in the instruction compared to the original study by Wender and Weber (1982). This suggests that the speed effect is not dependent on the prompt to engage with mental imagery and can also emerge upon mere language processing.

The second question investigated was which modality, i.e., motor or visual modality, is responsible for the speed effect by introducing Type of Motion with the levels *human* and *object motion* as manipulation in the stimulus material. Human motion can be simulated both visually and motorically. In contrast, the motion of inanimate objects can only be simulated visually, but not motorically. We hypothesised that, should we observe an effect of speed for object motion sentences, it would consequently only be explicable by the activation of visual, but not motoric representations. The speed effect was thus expected to be present for both types of motion, if it was based on the activation of visual modal representations. In contrast, if the speed effect was ascribable to the activation of motoric modal representations, we expected it to be observable only for human motion sentences, but not for object motion sentences. The results were in accordance with the former: RTs were not modulated by an interaction of Type of Motion and Speed. More precisely, in Experiment 1 of Chapter 3, the speed effect was equally present for both human and object motion sentences. This suggests that visual, rather than motoric, modal representations were activated in the task to detect motion.

The third question investigated was whether the speed effect is automatic. This was tested by asking the participants for a sensicality judgment instead of a motion detection task in Experiment 2 of Chapter 3. We did not observe a speed effect under these conditions, which implies that the activation of modal representations upon the processing of fast-speed and slow-speed sentences as observed in Experiment 1 is modulated by the task and is thus non-automatic. Presumably, the recruited modal representations underlying the speed effect in Experiment 1 and Wender and Weber (1982) facilitate the decisional process when the task is to detect motion.

Finally, to evaluate whether the speed effect, in fact, reflects the simulation of the expressed events’ duration, travelled distance was manipulated in addition to the associated speed in Chapter 4. The task was identical to Experiment 1 of Chapter 3, i.e., participants were asked to detect motion. We assumed that RTs should increase with travelled distance if all parts of the presented sentences were taken into account compositionally to yield nuanced modal representations of the expressed events' temporal structures. The results do not provide a clear-cut answer. Travelled distance was not reflected in RTs in an analogous relation to the denoted events' distance. More specifically, short-distance sentences were reacted to more slowly than long-distance sentences. This finding speaks against the assumption that the speed effect is associated with the simulation of the expressed events' duration. However, post-hoc inspection of the data suggests that different processing strategies might be elicited by the two levels of distance due to different underlying time-scales, since for both levels of distance the items' rated distance was positively correlated with RTs when analysed separately. Different to the outcome of the main analysis, this speaks for a representational format that has an analogue correspondence to physical distance and physical duration within a given time-scale. The results of Chapter 4 thus call for further investigations to validate the observed pattern due to the post-hoc nature of these analyses and considerations.

To assess whether the grounding of linguistically expressed duration can – in addition to reaction times – be apparent in an interference with time perception, a different method was applied in Chapter 5: a duration reproduction task. This type of task was adopted from temporal cognition as a related field of research and entails the advantage of providing an alternative to RT as dependent variable, i.e., reproduced durations (RDs). Firstly, this broadens the perspective, and secondly RDs might be more sensitive to detect a potential activation of modal representations upon the processing of linguistically expressed duration. This is because the perception of the temporal extent of events in our environment is a universal and continuous experience (Matthews & Meck, 2016), which entails the human capability of accurate reproduction of time intervals up to 3 s with small temporal variance (Daikoku et al., 2018). Complementing the findings on the grounding of linguistically expressed duration information gained via the above introduced RT paradigm by implementing a duration reproduction task consequently seems like a promising approach.

Temporal cognition studies have revealed that non-temporal stimulus attributes such as size affect temporal judgments (Matthews & Meck, 2016). Crucial to the present subject of investigation, there are first studies reporting effects of implicit stimulus attributes, such as imagined size, on interval timing (e.g., Birngruber & Ulrich, 2019). Against this background, we tested whether linguistically expressed duration affects RDs analogous to physical duration by making use of the manipulation of duration via speed of motion on a given path as in Chapters 3 and 4. The results of Experiment 1 of Chapter 5 corroborate the notion that linguistically expressed duration can interfere with time perception. More specifically, RDs were higher for longer denoted events, i.e., expressions with slow-speed verbs, than for shorter denoted events, i.e., for expressions with fast-speed verbs. Since this pattern of increased RDs for longer events is also observable for physical duration, the results imply that linguistically expressed duration affects perceived duration analogous to physical duration.

Speed was a highly salient feature in the employed stimulus material in Experiment 1. Nonetheless, RDs were not affected by implicit speed analogous to physical speed. To control whether linguistically expressed speed information affects perceived duration when no duration information is provided, single manner of motion verbs were presented in Experiments 2 and 3 of Chapter 5. While Experiment 2 used a duration reproduction task to detect a potential effect of implicit speed, Experiment 3 was a close replication of a temporal bisection experiment by Zhang et al. (2014). The results of both experiments speak against an effect of implicit speed analogous to physical speed.

In summary, the results of the present dissertation corroborate the assumption that deictic and sequential concepts of time, as well as the mental representation of the duration of events that are denoted by linguistic expressions can involve the activation of modal representations. While the grounding of deictic and sequential concepts of time is apparent in so far as space is utilised to cognitively order events and to reason about the temporal reference of an entity, as suggested by the conducted meta-analysis, the grounding of duration information becomes apparent in RDs, i.e., in an interference with perceived duration. The speed effect, though reliable when the task is to detect motion, is not clearly attributable to the simulation of duration. Nonetheless, it can be considered an effect that is ascribable to the activation of modal representations (a detailed explanation for this notion will be provided in Chapter 3). Yet, not only the manifestation but also the limits of these effects' meaning for cognitive operations becomes apparent in the present dissertation. More specifically, the investigated instances of the cognitive grounding of linguistically expressed temporal information do not seem to reflect automatic processes. This is discussed in more detail in Chapter 6 and brought together with considerations regarding the context-dependency of the observed effects. In addition to the concluding remarks, methodological implications of the present dissertation are outlined in Chapter 6 and this thesis's contribution to the topic of replicability is illustrated.

Chapter 2

The space-time congruency effect: A meta-analysis[2]

Abstract

Several reaction time (RT) studies report faster responses when responses to temporal information are arranged in spatially congruent manner than when this arrangement is incongruent. The resulting space-time congruency effect is commonly attributed to a culturally salient localization of temporal information along a mental timeline (e.g., a mental timeline that runs from left to right). The present study aims to provide a compilation of the published RT studies on this time-space association in order to estimate the size of its effect and the extent of potential publication bias in this field of research. In this meta-analysis, three types of task are distinguished due to hitherto existing empirical findings. These findings suggest that the extent to which time is made relevant to the experimental task has a systematic impact on whether or not the mental timeline is activated. The results of this meta-analysis corroborate these considerations: First, experiments that make time a task-relevant dimension have a mean effect size of $d = 0.46$. Second, in experiments in which time is task irrelevant, the effect size does not significantly deviate from zero. Third, temporal priming studies have a surprisingly high mean effect size of $d = 0.47$, which, however, should be adjusted to $d = 0.36$ due to publication bias.

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[2] This chapter is the published version of the following article: von Sobbe, L., Scheifele, E., Maienborn, C., & Ulrich, R. (2019). The space–time congruency effect: A meta-analysis. *Cognitive Science*, *43*(1), e12709, which has been published in final form at: https://doi.org/10.1111/cogs.12709. Reprinted with permission from the Cognitive Science Society. © 2019 Cognitive Science Society, Inc. All rights reserved. The final published version has only been adapted with respect to minor reformatting required by the present publication.

Introduction

Time cannot be perceived because there is no adequate physical stimulus of time; thus, we seem to rely on our experience of space to frame the otherwise elusive concept of time (Burr et al., 2007; Gentner et al., 2002). Philosophers, linguists, and cognitive psychologists likewise assume that our conceptualization of time is based on our notion of space, since the latter can be traced back to sensorimotor experiences, whereas this is not the case for time (Eikmeier et al., 2016; Núñez & Cooperrider, 2013). The linkage of time as an abstract concept and space as its concrete counterpart is conceived of as the *spatial metaphor of time* (Clark, 1973, p. 50), which becomes apparent in most languages across the world as spatial expressions, for example locative prepositions, are used to express temporal notions (Haspelmath, 1997). Moreover, co-speech gestures suggest that time is organized along a mental timeline (Casasanto & Jasmin, 2012; Núñez & Sweetser, 2006) with time moving from left to right, right to left, back to front, or top to bottom. While the lateral and vertical timelines presumably depend on a language's writing direction (Bergen & Chan Lau, 2012; Fuhrman & Boroditsky, 2007; Ouellet, Santiago, Israeli, et al., 2010), the sagittal timeline is assumed to originate from our spatial orientation in the world, since foreseeable future events are ahead of us due to our experience of usually facing the objects that we are approaching (Lakoff & Johnson, 1980; Núñez & Sweetser, 2006).

To examine the psychological reality of the mental timeline, a considerable number of reaction time (RT) studies have been conducted within the last 15 years (e.g., Torralbo et al., 2006; Ulrich & Maienborn, 2010; Weger & Pratt, 2008; see also Eikmeier et al., 2016 for an overview). On the whole, these studies have revealed a space-time congruency effect; that is, the response to temporal stimuli is faster when the spatial response is consistent with the culturally salient direction of the mental timeline (congruent space-time mapping) than when it is reversed (incongruent condition). Thus, a person who is used to writing and reading from left to right will be faster at responding to past-related stimuli with the left hand as compared to responding with the right hand, whereas future-related stimuli will be responded to faster with the right than with the left hand. This congruency effect can be conceived of as a variation of the traditional spatial stimulus-response compatibility effect, which has been extensively studied in experimental psychology (see Proctor et al., 1992). Moreover, the size of this effect varies in the range of milliseconds so that it seems likely to assume that these RT differences reflect an unconscious, but sound difference in the speed of mental processing (Eikmeier et al., 2016).

However, the space-time congruency effect is not corroborated uniformly by all studies. More precisely, in the majority of experiments, in which the concept of time is not focussed by the task, no facilitation of responses in the congruent space-time mapping has been observed (Maienborn et al., 2015; Sell & Kaschak, 2011; Ulrich et al., 2012; Ulrich & Maienborn, 2010). This raises questions regarding the depth of the process that causes the space-time congruency effect. Instead of automatic sensorimotor activation it might be a facilitated memory access that elicits the congruency effect. According to this memory access account, spatial locations that are associated with past and future work as cues for performing the RT task, when the task explicitly asks participants to respond to the temporal reference of an expression (Maienborn et al., 2015). Thus, the congruent mapping might simply be remembered more easily than the incongruent space-time mapping.

An alternative explanation for the congruency effect’s dependency on the salience of the concept of time is that a coherent working model such as the mapping of time onto space happens only if its efforts are compensated by its benefits (Santiago et al., 2011). Thus, activating a mental timeline will only happen in those situations in which there is a gain to cope with that particular situation and its requirements, which is the case, when the experimental task requires a temporal placement, but which is not the case when the experimental task draws the participant’s attention away from the concept of time.

It is in our interest to assemble the diverging results of different types of task in order to frame their meaning and possible consequences. Another major motivation for providing a compilation of the so far published RT studies by means of a meta-analysis is to assess the size of the space-time congruency effect in those experiments, in which the concept of time is made salient, as there is further substantial variation concerning the design of the conducted studies beyond the salience of time. Under these circumstances a potential publication bias – that is, non-significant or negative results are put into the researcher's file drawer and do not get published – could imply that the actual space-time congruency effect is smaller than portrayed by the published studies and hence might not even significantly deviate from zero (Rosenthal, 1979; Ulrich et al., 2018). We will incorporate the potential publication bias into the estimation of the real effect size in order to examine whether it significantly deviates from zero and, thus, can be considered a sound effect.

For this purpose, a short overview of the designs of the studies that are included in the meta-analysis will be provided in the following section. The studies have shown some variation concerning the temporal and spatial information that are used as a cue for the activation of the mental timeline. In addition, different axes of the mental timeline have been investigated across the studies. Type of temporal and spatial information, as well as direction of the mental timeline or language, however, will not be regarded as moderator variables in the meta-analysis. This is because even though they presumably have a small-sized impact on the size of the congruency effect, the resulting moderated effect sizes should be distributed narrowly around some common mean which will be accounted for adequately by the use of the random-effects model (Borenstein et al., 2009, p. 61). Thus, the coding of corresponding moderator variables is not required. Apart from that, a too fine-grained subdivision of the gathered effect sizes would lead to smaller sample sizes, which would reduce the analysis's power. Such a fine-grained analysis would not provide an additional benefit for our purpose because we are interested in whether the space-time congruency effect is an empirically corroborated and thus acceptable fact.

In contrast, there is good reason to subdivide the studies in two steps. In a first step, the studies will be subdivided with regards to the type of task that is used. In a second step, the axis's origin will be considered. As mentioned above, the way time is made relevant by the task seems to have a major impact on whether or not the appearance of a congruency effect can be expected (e.g., Scheifele et al., 2018; Ulrich & Maienborn, 2010). Thus, the different types of task will be introduced and taken into account as subcategories in the meta-analysis whose method will be outlined in the subsequent section. Consequently, not only an overall effect size for all gained studies will be calculated, but also a separate analysis for each level of the factor *Task*, that is, *time is task relevant*, *time is task irrelevant* and *temporal priming*. Additionally, an analysis of the moderator variable *Axis* with its two levels *lateral/vertical* and *sagittal* within each level of *Task* will be calculated, since the lateral and vertical axes' origin presumably lies in a culture's writing system, whereas the sagittal axis is based on the more profound experience of moving forward. Finally, after comparing the effect sizes of the level *Task* with one another, the level *time is task irrelevant* will be addressed in more detail in a sub-section of the Results Section.

Types of stimuli, responses, axes and task

Three different axes of the mental timeline have been tested in the incorporated RT studies using different temporal and spatial cues in order to elicit its activation. In most studies, temporal information is incorporated by means of the stimulus material, whereas space usually gets activated via response mode. Thus, the stimulus-response mapping serves as the coding of the congruent and incongruent conditions of the mapping of time onto space. The extent to which both dimensions, space and time, are made salient, varies across studies. Especially the salience of the dimension of time has systematically been manipulated by means of different tasks.

Temporal information

To keep the effect sizes of the incorporated studies comparable, our meta-analysis focuses on the processing of deictic and sequential concepts of time. Thus, the analysis will not include studies in which the duration of a stimulus was manipulated for the assessment of the corresponding duration judgements (e.g., Di Bono et al., 2012). The latter line of research focuses on the mechanisms underlying the representation of time intervals and detaches too much from findings regarding the processing of abstract time concepts, which however is of major interest for the purpose of this study. The studies that will be included in the meta-analysis all examine the space-time congruency effect by measuring RT to a stimulus that requires a manual or vocal response that is either congruent or incongruent with the participant's culturally learned and salient mental timeline.

Incorporated are studies that use the following temporal cues as stimulus, prime, or response, presented either visually or auditorily:

- Past- or future-related words or phrases (Aguirre & Santiago, 2017; Bottini et al., 2015; Casasanto & Bottini, 2014; De la Vega et al., 2016; Ding et al., 2015; Eikmeier et al., 2013, Experiment 2; Eikmeier, Alex-Ruf, et al., 2015, Experiment 2; Eikmeier, Hoppe, et al., 2015; Hartmann & Mast, 2012; Kong & You, 2012; Ouellet et al., 2012; Ouellet, Santiago, Funes, et al., 2010; Ouellet, Santiago, Israeli, et al., 2010; Rolke et al., 2013, 2014; Santiago et al., 2007; Torralbo et al., 2006; Weger & Pratt, 2008, Experiment 2);
- Sentences containing temporal information (Eikmeier et al., 2013, Experiment 1; Eikmeier, Alex-Ruf, et al., 2015, Experiment 1; Maienborn et al., 2015; Scheifele et al., 2018; Sell & Kaschak, 2011; Ulrich et al., 2012; Ulrich & Maienborn, 2010); and
- Triplets of pictures showing the progression of an event at which the middle stage represents the reference point for an earlier and a later stage (Boroditsky et al., 2011; Fuhrman et al., 2011; Fuhrman & Boroditsky, 2007) and entities such as buildings, actors, or life events that can be categorized as earlier or later compared to some given reference point (Loeffler et al., 2017; Miles et al., 2011; Walker et al., 2014, 2017; Weger & Pratt, 2008, Experiment 1).

There are two references known to us in which spatial instead of temporal cues were used as stimuli that were combined with temporal instead of spatial responses to assess the space-time congruency effect. First, Eikmeier et al. (2013, Experiment 2) presented sounds that originated in front of or behind the participant to assess the sagittal axis. Second, in Eikmeier, Alex-Ruf et al. (2015, Experiment 2) the sounds were played on the participant's right or left side for examining the lateral axis. In both experiments the participant was asked to respond vocally to the location of the sound’s source either with ‘back’/’front’/’left’/’right’ in the control condition or with ‘past’/’future’ in the experimental condition. All other studies mentioned above used temporal cues as stimulus or prime and spatial information as response but not as stimulus.

Spatial information

In most of the studies considered in our meta-analysis, spatial cues were encoded by requesting a manual response along the axis in question. For testing the lateral axis, participants were asked to respond with a left or right keypress either with their left and right index fingers (e.g., Santiago et al., 2007) or with one finger only in order to cause a movement (e.g., Miles et al., 2011). Similarly, the sagittal axis was tested by corresponding keypresses (further away or close to the body, e.g., Fuhrman et al., 2011; Sell & Kaschak, 2011) or by a slider that was moved forward and backward (e.g., Ulrich et al., 2012). Walker et al. (2017) used mouse presses instead of keypresses as response mode and there are two studies that asked for vocal responses, in which space was encoded semantically through spatial expressions like ‘back’/’front’ or ’left’/’right’ as response to temporal information (Eikmeier et al., 2013, Experiment 1; Eikmeier, Alex-Ruf, et al., 2015, Experiment 1). For the vertical axis, up and down keypresses were used as response keys (e.g., Boroditsky et al., 2011).

There are a few studies that used rather subtle spatial cues. Torralbo et al. (2006) presented pictures of human silhouettes with a speech bubble either in front of or behind the silhouettes’ face containing a time-related word in Spanish. Participants in their first experiment were asked to respond vocally with ‘pasado’ and ‘futuro’ to indicate whether the word was past- or future-related. In this experiment, space was only salient by means of the speech bubble’s position relative to the silhouette. In a likewise subtle manner, Hartmann and Mast (2012) mapped time onto space by moving the participants forward and backward on a motion platform while they were reading words and evaluating their temporal reference. The participants’ bodily movement was task- and response-irrelevant but was still evaluated in relation to the temporal reference of the presented words (Loeffler et al., 2017 used a similar set-up). Similarly, in Walker et al. (2014) spatial information was only implicitly salient. Here, space was not encoded through bodily movement but through the location of the target's auditory presentation: Participants had to respond vocally to categorize the temporal reference of the auditorily presented target irrespective of the sound's source that was arranged either along the lateral or the sagittal axis.

Axes

Three axes, the lateral, the sagittal, and the vertical axis, have been considered in the underlying studies. In most studies, the axis that has been examined is culturally salient for the corresponding group of participants. Only a few studies tested the space-time congruency effect for axes that were not common in that particular culture, usually to control the effect of the experimental condition (e.g., Hartmann & Mast, 2012) or to compare two different cultures (e.g., Boroditsky et al., 2011).[3] Since this applies only to a minority of studies, it is not reasonable to code an axis's cultural salience as a moderator variable. Instead, the issue will be dealt with by excluding those effect sizes from the meta-analysis that are gained by testing culturally irrelevant axes.

However, in order to account for the different origins of the axes as described in the introduction, the lateral and vertical axes will be treated as one level of the factor *Axis*, whereas the sagittal axis will be considered another level of the factor *Axis*. Thus, within each group of *Task*, the level *sagittal* will be compared to the level *lateral/vertical* in order to assess whether the more profound origin of the sagittal axis will also yield a larger mean effect size.

Task

The way time is made salient or relevant for the task has a major influence on the effect size (Scheifele et al., 2018; Ulrich & Maienborn, 2010). Thus, it is of eminent

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[3] Note that there is one study that explicitly examines whether axes that are not culturally salient can become activated by manipulating the reading direction of the experimental stimuli (i.e., by using mirror reversed orthography, vertical downward orthography, and vertical upward orthography). Since the premise of the emergence of the space-time congruency effect in this study by Casasanto and Bottini (2014) is very specific, the results cannot be integrated into the present meta-analysis. Only their standard orthography control group has been incorporated.

importance for the following meta-analysis to distinguish between the different types of task.

Most of the studies that present temporal information as stimulus material ask their participants to categorize the temporal reference of the stimulus. This procedure makes the temporal cues salient for the participants since they are task relevant. The appearance of a space-time congruency effect in these cases is highly predictable as the spatial responses that categorize the temporal reference can be remembered better, when the key-assignment is congruent to the known and culturally transmitted assignment of time onto space. Also making use of a mental timeline as a coherent model to cope with the task is very economical with respect to cognitive capacities. Subsequently, this type of task will be called *time is task relevant*.

However, it is also possible to obscure the object of investigation in order to examine whether the mental timeline gets activated automatically when temporal reference is task irrelevant. For instance, Ulrich and Maienborn (2010) asked their participants to evaluate whether the presented sentence was sensible or not by pressing a left or a right key, respectively. The alignment of the keys was reversed in a second block so that they recorded RTs of both the left and right hand to both past and future-related sensible stimuli. While RTs of the right hand were expected to be shorter for future-related stimuli than for past-related stimuli, the reverse pattern was expected for left hand responses. In this kind of design, the stimulus sentences still have to be processed thoroughly in order to execute the experimental task properly; however, their temporal reference is of no importance to accomplish the task.

Most of the studies assessing the mental timeline's automatic activation by making time task irrelevant fail to find a congruency effect. Thus, this second type of task is referred to as *time is task irrelevant*.[4] Usually a sensicality judgment as exemplified

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[4] Please note that congruency thus is randomised within blocks: For *time is task irrelevant* studies there are congruent and incongruent trials in both blocks, whereas for *time is task relevant* studies congruency is manipulated between blocks: One block contains only congruent responses, the other block only incongruent responses. This methodological difference is a systematic confound when comparing the two levels of *Task*. However, it is not assumed to have a significant influence on the effect size.

above by means of Ulrich and Maienborn (2010) is used in this type of task. There is only one experiment in which a different technique has been implemented to distract the participants' attention from the concept of time: In their Experiment 3, Aguirre and Santiago (2017) asked their participants to judge whether the stimulus expression referred to a real or a potential event. Thus, time was not a task-relevant dimension in their instructional design either.

Additionally, a third category of task will be distinguished in the following analysis. The studies in this third category have used temporal information not as stimulus but as a prime that shows up shortly before the main task has to be carried out. This is the case in Experiment 2A of Weger and Pratt (2008), who presented a prime word with either a prospective or a retrospective cue that was followed by a white circle, which appeared on the right or the left side of the computer screen. Participants were instructed to indicate as fast as possible that they had detected the target by pressing a key that corresponded to the side of the computer screen at which the circle appeared. The space-time congruency effect that arises in this kind of set-up (i.e., responses with the left hand to a target appearing on the left side are faster after seeing a past-related word as compared to a future-related word) shows that the processing of temporal information unconsciously shifts the visual attention along to the mental timeline which causes a facilitation of response velocity (Weger & Pratt, 2008). Since *temporal priming* involves a rather subtle activation of the mental timeline, it will be differentiated from tasks, in which temporal cues explicitly have to be evaluated with respect to their temporal reference.

Method

Sample of studies

We searched for articles using the databases PsycINFO and MEDLINE with the keyword 'mental timeline' and scanned for articles containing terms such as 'space-time congruency effect', 'space-time mapping', 'space time reaction time', 'spatial metaphor of time' and 'space time alignment'. Furthermore, we looked through the reference lists of current articles to find out whether we had overlooked some relevant papers and publications.

We had to exclude all experiments that examined the mental timeline with methods other than RT recordings such as assessing gestures or laying out temporal sequences (Hendricks & Boroditsky, 2015). We only included studies with adult participants because the mental timeline is most probably caused by cultural imprint so needs time to evolve in individuals (Tillman et al., 2015). Of the remaining studies we could only include those that reported statistics with which we could accurately estimate an effect size. This resulted in 30 references. Some of these, which had several sub-experiments, only reported *F*-values in experiments that actually showed a significant congruency effect, so that it was not possible to include all sub-experiments of each paper. Other sub-experiments had a design that tested more than one congruency effect such as Experiment 1 of Fuhrman et al. (2011), in which the lateral, the vertical, and the sagittal axes were tested on English and Mandarin natives, resulting in five estimated effect sizes for one experiment. However, since independence of effect sizes is a precondition for the meta-analysis, the mean of the corresponding effect sizes is taken as an estimate in those cases, in which there is more than one effect size for the same group of participants.[5] Eventually this method led to 62 estimated effect sizes for the 30 incorporated studies on which this meta-analysis is based. We thank Roberto Aguirre, Roberto Bottini, Daniel Casasanto, Verena Eikmeier, Marc Ouellet, Bettina Rolke, and Andrea Sell for providing us with suitable data on their published experiments so that we could incorporate them into the meta-analysis, which we would not have been able to do otherwise. Appendix A contains an overview of all incorporated studies as well as specific information on how we determined the effect size from the statistics of each study.

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[5] For detailed information on how we treat the effect sizes of particular experiments concerning the independence of subgroups see Appendix A.

Coded factors

As outlined above, we analysed *Task* as a factor, of which there are three levels: *time is task relevant*, which applies to 41 out of the 62 estimated effect sizes; *time is task irrelevant*, for which we were only able to gain 10 effect sizes from the underlying references; and as a third level we coded *temporal priming*, which only holds for 11 estimated effect sizes. Within each subgroup of the factor *Task*, we also calculated an analysis of the factor *Axis* with the two levels *lateral/vertical* and *sagittal* to embrace the different axes' origin. We did not differentiate between the lateral and vertical axis, as they are assumed to share a common origin, as outlined above. Since all *temporal priming* studies examine the lateral axis, the factor *Axis* cannot be applied for this level of *Task*. For the level *time is task irrelevant*, a further subdivision into *low* and *high temporal complexity* derives from a theoretical and empirical point of view. This additional subdivision is only carried out because the considered effect sizes vary systematically and can be deduced from theoretical considerations.

We did not code language as a moderator variable since the corresponding differences in effect sizes are expected to be low and can, apart from that, not be derived appropriately from the theoretical background. Additionally, a further subdivision would lead to smaller sample sizes, thus reducing the analyses' power. The gained informative value would not have justified the loss of statistical explanatory power. This is why we also refrained from further clustering into, for example, type of stimulus material.

Effect size analyses

Each effect size was calculated as Cohen's *d*, that is, the standardized difference of mean RTs for the congruent as compared to the incongruent space-time mapping, using the Comprehensive Meta-Analysis (CMA, Version 3.3.070, 2014, Biostat, Englewood, NJ, USA) software. The effect size was defined as positive when the mean RT of the congruent condition was lower than the mean RT of the incongruent condition, which was the case for all of the effect sizes. *d* was calculated from *t*-values combined with the sample size. If only a *F*-value was reported, we calculated the corresponding *t*-value by

$t = \sqrt{F}$. For the majority of studies, congruency was a within-subjects factor. Only four effect sizes are gained from experiments that treat congruency as a between-subjects factor, so d was estimated on the basis of independent groups in these cases, using the sample sizes of the two groups and the t-value for the calculation of d. If two or more effect sizes were gained from the same pool of participants, the mean of the corresponding t-values was taken to calculate d.

We chose the random-effects model for all analyses, because it assumes that the true effect sizes vary across studies but are distributed around some common grand mean, of which the given data represent a random sample (Borenstein et al., 2009). Since the studies included in this meta-analysis have examined different axes on different groups of population, this model is appropriate. Even when clustering the effect sizes into different levels of *Task* or *Axis*, the studies still vary considerably regarding type of language, stimulus material, and response mode. Despite this a-priori assumption, the Q-statistic that tests on heterogeneity and is implemented in CMA will be reported for all analyses since it indicates whether the assumption of a distribution of the true effect sizes around some common mean is justified by the variability of the data. For two analyses of *Task*, namely the estimation of the weighted mean effect size of *time is task irrelevant* and of *temporal priming* studies, the Q-statistic does not support the choice of the random effects model. However, while for the former the results are exactly the same compared to using a fixed-effect model, for the latter, the observed overall variance is largely due to heterogeneity of the true effect sizes as indicated by a high I^2-value. Thus, for both effect size estimations, the use of the random effects model seems appropriate despite a non-significant Q-statistic.[6] Details will be given in each corresponding section. For estimation and correction of publication bias, Duval and Tweedie's (2000) Trim and Fill that is implemented in CMA and that uses the linear (L) estimator has been employed (Borenstein, 2005, p. 203).

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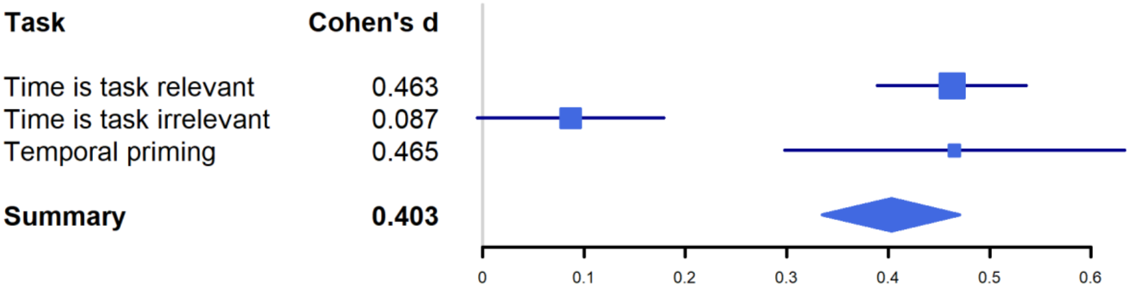
[6] A similar pattern applies to three analyses of the factor *Axis*.

Results

We carried out a preliminary analysis for which the overall weighted mean effect size of the 62 underlying estimated effect sizes is significant ($d = 0.403$, 95% CI = [0.335, 0.470], $p < .0005$). For the preliminary analysis, the Q-statistic supports our choice of the random-effects model since 54.1% of the observed overall variance is due to heterogeneity of the true effect sizes ($I^2 = 54.1$). Thus the data seem to be based on more than one true effect size ($Q(61) = 132.9$, $p < .0005$), which is in line with the random-effects model but not with the fixed-effect model, which assumes one common true effect size (Borenstein et al., 2009, p. 61).

Figure 2.1. ***Forest plot for the different levels of* Task.**



Note. Reprinted from “The space–time congruency effect: A meta-analysis” by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 10. © 2019 Cognitive Science Society, Inc. All rights reserved.

We then calculated separate mean effect sizes for each level of the factor *Task* (see Figure 2.1 for an overview of observed effect sizes and Table 2.1 for an overview of adjusted effect sizes). Furthermore, we decided to examine the publication bias for each level of *Task* separately by the use of distinct funnel plots, since heterogeneity can cause asymmetry of the funnel plots, even in the absence of publication bias (Terrin et al., 2003). An analysis of the moderator variable *Axis* has also been carried out separately for each level of *Task*.

Time is task relevant

The choice of the random-effects model to calculate the weighted mean effect size for the level *time is task relevant* (Figure 2.2), was again supported by the test for heterogeneity, which is significant ($Q(40) = 67.3$, $p = .004$) and 40.5 percent of the observed variation is due to heterogeneity of the true effect sizes ($I^2 = 40.5$). The weighted mean effect size significantly deviates from zero ($d = 0.463$, 95% CI = [0.389, 0.536], $p < .0005$). Using the random-effects model to look for missing studies reveals that there is no publication bias (Figure 2.3). Since no studies are trimmed and filled when applying Duval and Tweedie's trim and fill, the mean effect size stays the same ($d = 0.463$).

Table 2.1. ***Summary of the adjusted mean effect sizes (Cohen's d) after applying Duval and Tweedie's trim and fill.***

Task	Number of estimated effect sizes	Number of effect sizes that are trimmed and filled	Adjusted mean effect size (*d*)	Adjusted 95% CI
Time is task relevant	41	0	0.463	[0.389, 0.536]
Time is task irrelevant	10	3	0.051	[-0.035, 0.137]
Temporal priming	11	3	0.359	[0.179, 0.540]

Note. Adapted only in its format from "The space–time congruency effect: A meta-analysis" by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 11. © 2019 Cognitive Science Society, Inc. All rights reserved.

Time is task relevant – *Lateral/vertical vs. sagittal axis*

Nine out of the 41 effect sizes of *time is task relevant* are yielded by an assessment of the sagittal axis. Further four effect sizes are gained by experiments that test the lateral as well as the sagittal axis (Walker et al., 2014, 2017). In order to guarantee for independence of subgroups, the effect sizes of the lateral axis are removed of these four effect sizes,[7] yielding a total of 13 effect sizes of the level *sagittal*. Three of the 41 effect

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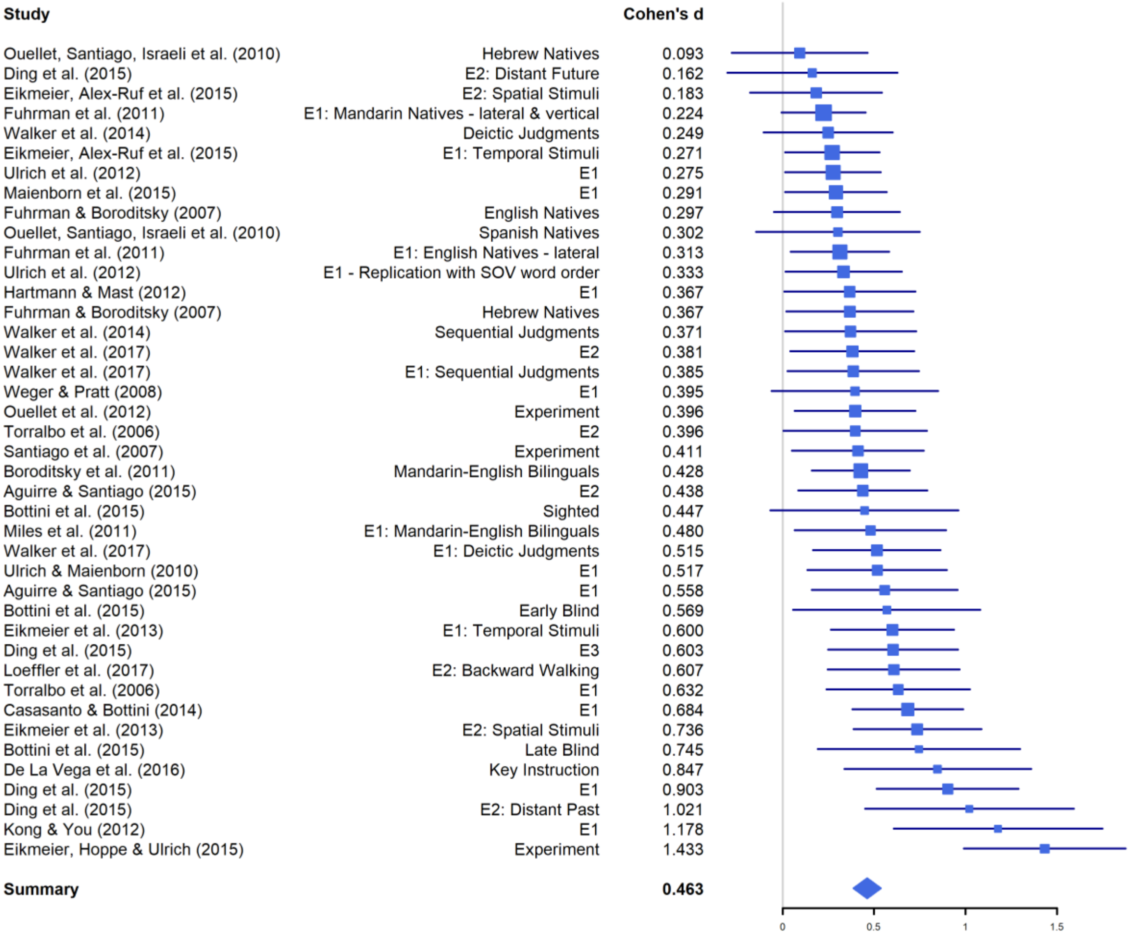
[7] See Appendix A for details. We decided to keep the sagittal effect size and eliminate the lateral effect size of these four effect sizes, since the majority of effect sizes of *time is task relevant* is based on the lateral axis. Thus, for the purpose of statistical evaluation increasing the amount of sagittal effect sizes is preferable.

sizes of *time is task relevant* are based on the testing of the lateral and the vertical axes. The remaining 25 of the 41 effect sizes are all based on the testing of the lateral axis only. Thus, in total there are 28 effect sizes of the level *lateral/vertical*.

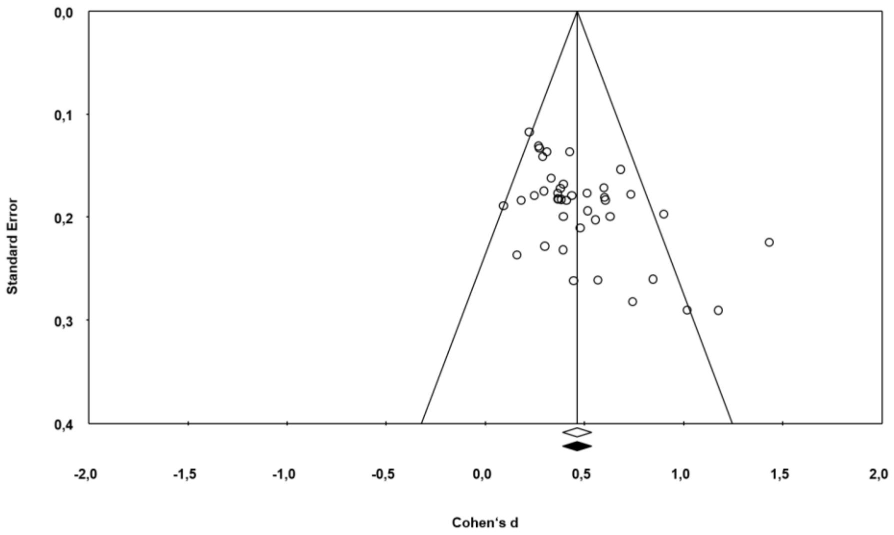
Figure 2.2. ***Forest plot for*** **time is task relevant.**



Note. E = experiment. Reprinted from "The space–time congruency effect: A meta-analysis" by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 11. © 2019 Cognitive Science Society, Inc. All rights reserved.

Both levels of *Axis* are significant. The weighted mean effect size of the level *sagittal* is numerically slightly higher (d = 0.517, 95% CI = [0.362, 0.672], $p < .0005$, $Q(12) = 30.8$, $p = .002$, $I^2 = 61.1$) than the weighted mean effect size of the level *lateral/vertical* (d = 0.437, 95% CI = [0.354, 0.520], $p < .0005$, $Q(27) = 38.3$, $p = .073$, $I^2 = 29.6$)[8]; however, this difference is not significant ($Q(1) = 0.8$, $p = .368$).

Figure 2.3. ***Funnel plot for*** **time is task relevant.**



Note. The circles represent the observed effect sizes. No studies are trimmed and filled when using the random-effects model to look for missing studies. Reprinted from “The space–time congruency effect: A meta-analysis” by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 12. © 2019 Cognitive Science Society, Inc. All rights reserved.

Time is task irrelevant

For the level *time is task irrelevant* (Figure 2.4), the weighted mean effect size is not significant (d = 0.087, 95% CI = [-0.005, 0.179], $p = .063$). We used the random-effects model for the analysis notwithstanding that the test for heterogeneity did not reach

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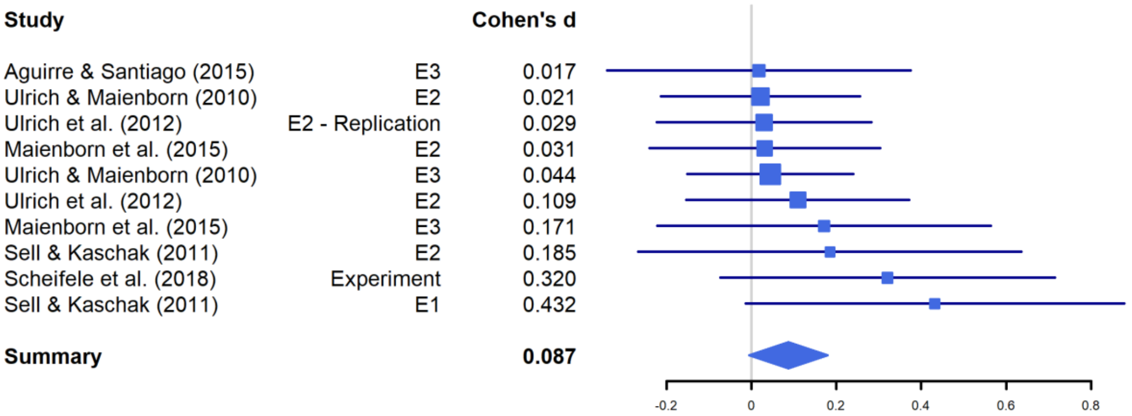
[8] Please note that even though the test for heterogeneity is not significant, I^2 is considerable. Using a fixed-effect model would lead to only slightly different results: d = 0.421, 95% CI = [0.353, 0.488], $p < .0005$.

the level of significance ($Q(9) = 5.0$, $p = .832$, $I^2 = 0.0$).[9] The funnel plot (Figure 2.5) reveals a slight publication bias, suggesting to trim and fill three studies. The estimation of the mean effect size after applying Duval and Tweedie's trim and fill differs only slightly from the observed mean effect size ($d = 0.051$, 95% CI = [-0.035, 0.137]).

Figure 2.4. ***Forest plot for*** **time is task irrelevant**



Note. E = experiment. Reprinted from "The space–time congruency effect: A meta-analysis" by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 13. © 2019 Cognitive Science Society, Inc. All rights reserved.

Time is task irrelevant – *Lateral/vertical vs. sagittal axis*

Six out of the 10 effect sizes that are included in the *time is task irrelevant* group are based on the examination of the sagittal axis. The remaining 4 effect sizes are gained on experiments testing the lateral axis. Interestingly, the weighted mean effect size of *sagittal time is task irrelevant* studies is significant ($d = 0.156$, 95% CI = [0.019, 0.294], $p = .026$, $Q(5) = 3.3$, $p = .661$, $I^2 = 0.0$), whereas the *lateral/vertical time is task irrelevant* studies do not yield a significant weighted mean effect size ($d = 0.032$, 95% CI = [-0.092, 0.155], $p = .617$, $Q(3) = 0.0$, $p = .999$, $I^2 = 0.0$)[10]. However, at the same time the *Q*-statistic

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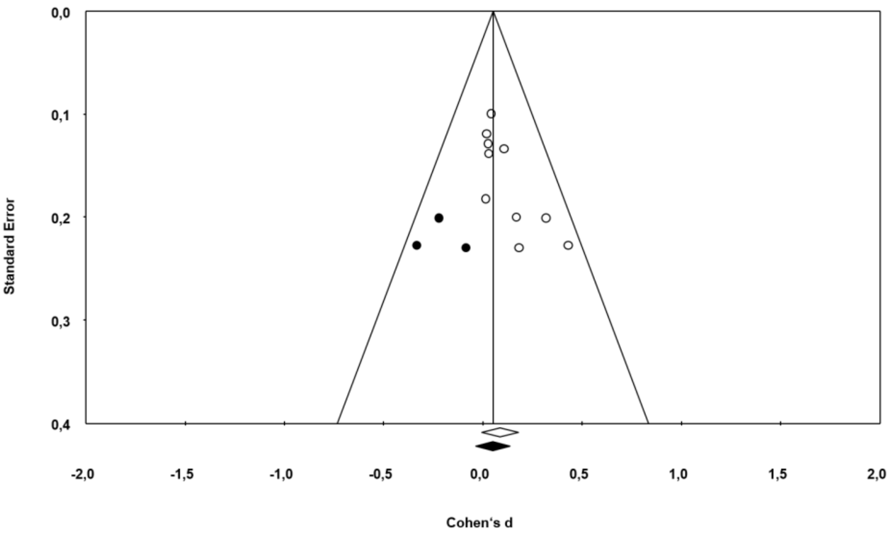
[9] Please note that using the fixed-effect model would yield exactly the same results. The reason for the choice of the random-effects model besides the a-priori assumption outlined above is that a non-significant *p*-value cannot readily be taken as evidence for homogeneity (Borenstein et al., 2009, p. 113).

[10] Again, using the fixed-effect model would yield exactly the same results for both the *lateral/vertical* and the *sagittal* axes.

does not suggest a significant difference between the weighted mean effect sizes of the two levels of *Axis* ($Q(1) = 1.7$, $p = .186$).

Figure 2.5. ***Funnel plot for*** **time is task irrelevant.**

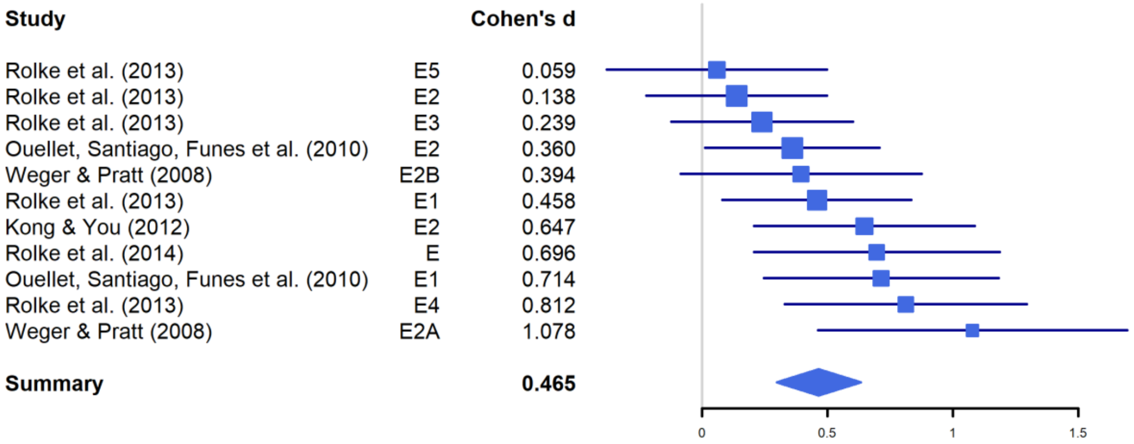


Note. Blank circles represent the observed effect sizes, and filled circles illustrate trimmed and filled studies. Reprinted from "The space–time congruency effect: A meta-analysis" by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 14. © 2019 Cognitive Science Society, Inc. All rights reserved.

Temporal priming

For the *temporal priming* studies the use of the random-effects model again was not utterly supported by the *Q*-statistic ($Q(10) = 16.7$, $p = .082$), however, the proportion of observed variance that can be explained by heterogeneity is high ($I^2 = 40.0$). In addition to the above outlined assumption of a distribution of the true effect sizes around some common mean, the high I^2-value was taken as an indication to still use the random-effects model. The hereby resulting weighted mean effect size (Figure 2.6) significantly deviates from zero ($d = 0.465$, 95% CI = [0.298, 0.633], $p < .0005$).[11] According to the funnel plot (Figure 2.7), there is a slight publication bias, recommending to trim and fill three studies which leads to an adjusted mean effect size of $d = 0.359$ (95% CI = [0.179, 0.540]). All effect sizes that are incorporated in the *temporal priming* studies are based on an examination of the lateral axis. Thus, it is not possible to assess the impact of *Axis* on the weighted mean effect size of *temporal priming*.

Figure 2.6. ***Forest plot for*** **temporal priming.**



Note. E = experiment. Reprinted from "The space–time congruency effect: A meta-analysis" by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 14. © 2019 Cognitive Science Society, Inc. All rights reserved.

Comparing effect sizes of the different levels of *Task*

The estimated mean effect size of *time is task irrelevant* studies significantly deviates from the estimated mean effect size of *time is task relevant* studies ($Q(1) = 39.2$, $p < .0005$) and of *temporal priming* studies ($Q(1) = 15.0$, $p < .0005$). However, the estimated mean effect sizes of *time is task relevant* studies and *temporal priming* studies do not differ significantly ($Q(1) = 0.0$, $p < .978$).

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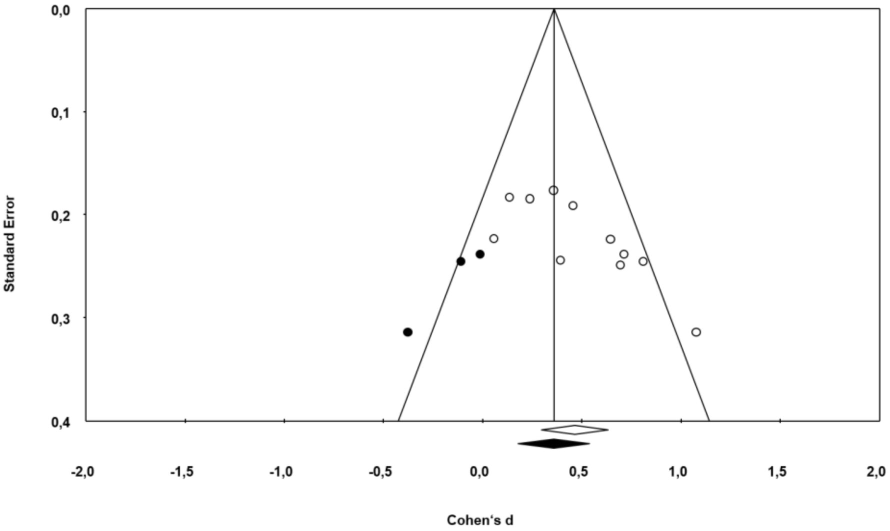
[11] Using the fixed-effect model would yield only slightly different results: $d = 0.442$, 95% CI = [0.314, 0.570], $p < .0005$.

Figure 2.7. ***Funnel plot for*** **temporal priming.**



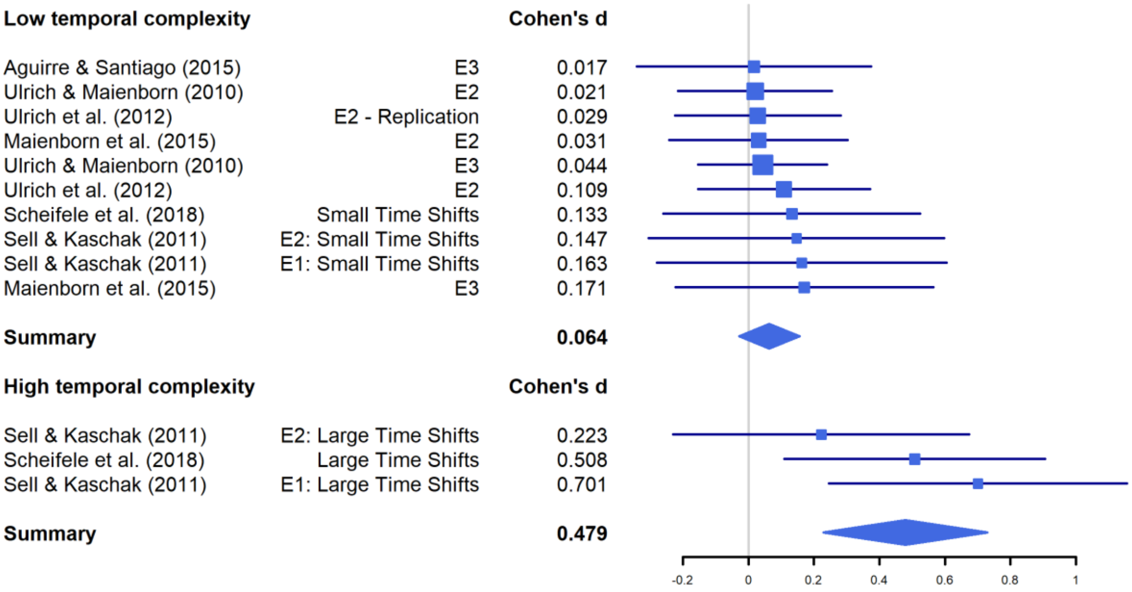
Note. Blank circles represent the observed effect sizes, and filled circles illustrate trimmed and filled studies. Reprinted from "The space–time congruency effect: A meta-analysis" by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 15. © 2019 Cognitive Science Society, Inc. All rights reserved.

Subdividing *time is task irrelevant*

The results of the level *time is task irrelevant* suggest a systematic variation of the effect size depending on the type of temporal stimuli: Single sentences (e.g., The witness remembers the pistol-shot, Maienborn et al., 2015) and discourses with small time shifts (e.g., Jackie is taking a painting class; Tomorrow, she will learn about paintbrushes […], Sell & Kaschak, 2011) show no effect, whereas discourses with large time shifts (e.g., Jackie is taking a painting class; Next month, she will learn about paintbrushes […], Sell & Kaschak, 2011) yield an ample effect. Hence, whether or not the mental timeline is activated automatically seems to depend on whether or not the temporal order information can be processed without using the mental timeline as part of the build-up of a situation model (Scheifele et al., 2018). Only when discourses with larger time shifts are processed, temporal complexity might "get the upper hand" (Scheifele et al., 2018, p. 10), thus eliciting the build-up of a mental situation model to manage the temporal order information: While single sentences usually only refer to one event, multi-sentences discourses with large time shifts refer to several clearly distinguishable and separate events that necessitate a sequencing by means of a localization on the mental timeline.

Accordingly, the subdivision of *time is task irrelevan*t seems to be necessary for stimulus material, in which temporal complexity gets the upper hand (*high temporal complexity*) and stimuli that allow the processing of temporal information without the activation of the mental timeline (*low temporal complexity*). However, subdividing the effect sizes of *time is task irrelevant* into *low* and *high temporal complexity* requires gaining more than one effect size from one experiment, which violates the assumption of independence. Therefore, the following subdivision is only illustrative and the discovered trend has to be corroborated by further future experiments before strong conclusions can be drawn.

Figure 2.8. ***Forest plot for* time is task irrelevant*, subdivided into* low *and* high temporal complexity.**



Note. E = experiment. Reprinted from “The space–time congruency effect: A meta-analysis” by L. von Sobbe, E. Scheifele, C. Maienborn, & R. Ulrich, 2019, *Cognitive Science*, *43*(1), https://doi.org/10.1111/cogs.12709, p. 16. © 2019 Cognitive Science Society, Inc. All rights reserved.

Since *high temporal complexity* as stimulus material is only part of three experiments (Scheifele et al., 2018; Sell & Kaschak, 2011, Experiments 1 and 2), the effect sizes of these three experiments are subdivided into two distinct effect sizes each (one for *low temporal complexity*, i.e., small time shifts in this particular case; one for *high temporal complexity*, i.e., large time shifts). The resulting forest plot (Figure 2.8) for *low temporal complexity* now shows more homogeneity of effect sizes than the overall forest plot (Figure 2.4), while the three effect sizes of *high temporal complexity* are not any longer accounted for as being outliers but seem to depict an activation of the mental timeline that is comparable to the one of *time is task relevant*. The resulting mean effect size deviates from the mean effect size of *low temporal complexity* considerably.

Discussion

Of major interest for the purpose of this meta-analysis is whether the mean effect size of the space-time congruency deviates from zero, even when potential publication bias is accounted for, and what magnitude can be expected. For the default case, when time is a task-relevant dimension, which applies to the majority of effect sizes, our meta-analysis provides a clear answer for these questions. The estimated mean effect size of $d = 0.46$ implies that the space-time congruency effect is a sound effect when time is task relevant, which means that the mental timeline gets activated when temporal reasoning takes place, thereby facilitating responses congruent with the mental timeline's orientation. Beyond that, the mean effect size can be used to ensure adequate power for potential future experiments. More specifically, for a statistical power of .90, at least 41 participants are needed when manipulating the space-time congruency effect within subjects, and at least 81 participants when using a between-subjects design.

Since the aforementioned group of studies (i.e., *time is task relevant*) unifies effect sizes gained from different language populations for different axes of the mental timeline with diverse temporal cues used as stimulus, the random-effects model was used to estimate the publication bias. Using Duval and Tweedie's trim and fill suggests that the obtained overall effect size is not inflated by publication bias. The picture changes slightly, if the fixed-effect model is used for the estimation of the publication bias (suggested adjusted effect size of $d = 0.36$ after the trimming of 11 effect sizes). Indeed, the funnel plot displays some asymmetry that can be detected visually. Hence, assuming that there is no publication bias at all might be a little too optimistic, whereas it is also important to remark that according to the random-effects model a considerable proportion (if not all) of the found asymmetry is due to the remaining variation in experimental set-ups (see also Terrin et al., 2003 for the loose link between asymmetry and publication bias).

One could of course try to further differentiate between the designs of the studies. However, it was not the purpose of this analysis to distinguish between the effect sizes of different language populations or stimulus material, and, in addition, subdividing the level *time is task relevant* would lead to a fragmentation that is no longer statistically meaningful. The only further subdivision that has been carried out is an analysis of the influence of an axis' origin. Even though the sagittal axis with its origin in the fundamental human experience of moving forward in the world (Lakoff & Johnson, 1980; Núñez & Sweetser, 2006) yields a numerically slightly larger weighted mean effect size than the lateral and vertical axes that can presumably be traced back to the cross-culturally more flexible writing and reading direction (Bergen & Chan Lau, 2012; Fuhrman & Boroditsky, 2007; Ouellet, Santiago, Israeli, et al., 2010), the difference between the sagittal and lateral/vertical axes is not significant.

For the second type of task (i.e., *time is task irrelevant*), which, overall, does not significantly deviate from zero, a similar pattern occurs when looking at the vertical and lateral as opposed to the sagittal axis: Even though the weighted mean effect size of the vertical and lateral axes is slightly smaller than the one of the sagittal axis, this difference is not statistically significant. However, while the former does not significantly deviate from zero, the latter does. This suggests a tendency that will have to be investigated by future studies. Specifically, the sagittal axis might be strongly rooted in human experience so that it gets activated as a mental timeline even when time is task irrelevant. Since, however, the sample size is small, no clear conclusions can be drawn so far.

There is yet an alternative explanation for the dispersal of the level *time is task irrelevant*: The discrepant results of its additional subdivision reflect the current discussion about the automatic activation of the mental timeline (Scheifele et al., 2018). Most studies fail to find a space-time congruency effect when time is not a relevant response dimension (Maienborn et al., 2015; Ulrich et al., 2012; Ulrich & Maienborn, 2010). However, Sell and Kaschak (2011) reported an automatically activated mental timeline for discourses containing large time shifts. Since Scheifele et al. (2018) basically replicated Sell and Kaschak's findings, they do not seem to be a false-positive result but indicate that automatic activation of the mental timeline is dependent on the level of temporal complexity (Scheifele et al., 2018). When time shifts are small or only single sentences have to be processed, there is no need for the use of a mental timeline in order to comprehend the temporal information of the linguistic material. However, as the ordering of temporal information gets more demanding, a mental situation model including a mental timeline could be built up to facilitate this process (Scheifele et al., 2018; Zwaan et al., 2001).

This conclusion is also supported by the mean effect sizes gained through the additional subdivision of *time is task irrelevant*: There seems to be no automatic activation of the mental timeline for a low level of temporal complexity (d = 0.06), whereas the effect size of the congruency effect of stimuli with higher temporal complexity (i.e., discourses with large time shifts, d = 0.48) seems to be comparable to the effect size that arises when time is task relevant (d = 0.46). It is important to note, however, that there are not yet enough measured effect sizes to give a reliable evaluation. Future research will have to further identify the exact conditions under which automatic activation takes place and whether the incorporated effect size really is comparable to the one that emerges when time is task relevant.

The weighted mean effect size of the subgroup *temporal priming* is high. This is a surprising result since temporal priming in advance of the execution of a spatial task is meant to make time a task-irrelevant dimension and thus is expected to be rather comparable to the effect size of *time is task irrelevant* (with low temporal complexity), which is close to zero. However, carefully examining the designs of the temporal priming studies reveals that the temporal reference of the primes is in some cases brought into focus by the instruction. For example, Ouellet, Santiago, Funes et al. (2010) asked their participants to remember the temporal reference of the prime. After executing the spatial task (e.g., indicating the appearance of a circle in one of two laterally aligned boxes with a left or right keypress), participants had to answer whether the temporal prime had referred to the past or the future. This final probe question that was carried out in all three experiments of the study makes temporal reference much more salient as compared to a sensicality judgment that is used by most studies that examine automatic activation. Detecting a space-time congruency effect under these circumstances certainly is more likely compared to sensicality judgments where temporal information is irrelevant for performing the task. At least 3 out of the 11 effect sizes of *temporal priming* arise in settings in which the temporal reference of the primes is of essential relevance for the execution of the task (see also Rolke et al., 2013, Experiment 4). This could explain why the mean effect size of *temporal priming* is as large compared to the other two levels of *Task*.

In some of the temporal priming experiments, the priming is more subliminal and can nevertheless positively be said to trigger an automatic activation of the mental timeline. In Experiment 1 of Rolke et al. (2013), for example, a considerable effect size emerged (d = 0.46), even though temporal complexity was very low since single words were used as temporal primes. Here, the need for distinguishing temporal priming studies from studies in which the temporal dimension is task irrelevant becomes apparent: While the priming studies reveal a pronounced space-time congruency effect (adjusted mean effect size: d = 0.36), the *time is task irrelevant* studies do not (adjusted mean effect size: d = 0.05), although both are based on automatic activation. This may be due to the temporal interval given between prime presentation and target task, thus allowing for a build-up of the mental timeline. In the case of sensicality judgments, the responses have to be given much sooner after the processing of the temporal information. This, however, remains in the sphere of speculation and requires further investigation.

Overall, the underlying publication bias is surprisingly low, keeping in mind that the incorporated studies still vary with respect to their specific designs. Thus, our results can only give a coarse estimation. However, it seems relatively safe to expect an effect size somewhere between 0.39 and 0.54 when time is task relevant, an effect size between 0.18 and 0.54 when conducting a temporal priming study, and no effect when time is task irrelevant and temporal complexity of the stimuli is low. The different origins of the axes that are the mental timeline's basis on the contrary do not seem to modulate these effect sizes, although further studies are required to substantiate this conclusion—especially with regard to the *time is task irrelevant* studies. How large exactly the size of the congruency effect is when time is task irrelevant and temporal complexity is high remains an open issue until more studies have been undertaken. For now, at least, the corresponding effect size seems to be comparable to the effect size that appears when time is task relevant. The relatively large confidence interval for the effect size of temporal priming studies indicates that again more studies are needed to refine the picture.

The psychological reality of the mental timeline seems apparent after the outcome of this meta-analysis: Not only is space used to talk about time (Haspelmath, 1997), but space is also cognitively exerted to order events and to reason about the temporal reference of an entity. This corroborates the cognitive existence of the space-time metaphor. Even though the space-time congruency effect of experiments, in which time is a task-relevant dimension, could still be explained by means of the memory account (Eikmeier, Hoppe, et al., 2015; Ulrich & Maienborn, 2010), there still seems to be evidence for an automatic activation of the mental timeline under certain circumstances. Specifically, the mean effect size of temporal priming studies and the trend of studies using high temporal complexity for experiments in which time is a task-irrelevant dimension provide evidence for an automatic activation of the mental timeline. Future studies are required to examine the specific circumstances that facilitate automatic activation in more depth.

Chapter 3

Is rushing always faster than strolling?[12]

Abstract

In the context of the embodied cognition debate, an effect of motion verb associated speed information has previously been detected using eye-tracking, functional magnetic resonance imaging (fMRI), and reaction times (RT). The latter, for instance, was implemented by Wender and Weber (1982), who observed that participants were faster in detecting motion in sentences associated with fast motion compared to sentences associated with slow motion after having formed mental images of the sentences’ content. It remains open whether the reported effects of speed are associated with automatic lexical-semantic retrieval processes or whether they reflect higher top-down cognitive processes. To answer this question, the paradigm by Wender and Weber (1982) was adopted and further elaborated in the present study. In Experiment 1 visualization instructions were eliminated. Additionally, the stimulus material was manipulated in regards to the agent of the described movement (human vs. object motion) in order to determine the representation’s modality (visual vs. motoric). In Experiment 2, the task to detect motion was replaced by the task to judge sensicality. The results suggest that the prompt to perform mental imagery is not a precondition for the engagement of modal representations in this speed of motion paradigm and that the involved representations’ modality is visual rather than motoric. However, the modal representations’ involvement is dependent on the task. They thus do not seem to be part of the invariant semantic representation of manner of motion verbs.

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[12] This chapter is the published version of the following article: von Sobbe, L., Ulrich, R., Gangloff, L., Scheifele, E., & Maienborn, C. (2021). Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs. *Acta Psychologica*, *221*, 103428. https://doi.org/10.1016/j.actpsy.2021.103428 published by Elsevier B.V. under a *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/). The final published version has only been adapted with respect to reformatting required by the present publication. Moreover, a title was created for the appendix (see Appendix B.1).

The data of the speed rating study for object motion sentences and the data of Experiment 1 were collected under my supervision in the context of the unpublished Bachelor thesis Gangloff, L. (2019).

Introduction

Several studies have suggested an involvement of action, emotion, and perception systems in language processing (Horchak et al., 2014). The results of these studies have been explained within the embodied cognition framework, which breaks with the classic cognitive science approach in which cognition is assumed to be based on amodal or symbolic representations (Binder, 2016; Buccino et al., 2016; Dove, 2018; Iachini, 2011). Proponents of embodied cognition, on the contrary, propose that concepts are represented modally (Meteyard et al., 2012). They assume that comprehenders 'simulate' what they read in sentences or discourses (Bergen, 2005; Pecher & Zwaan, 2005) by activating those cortical neurons that are also involved in perception, action, or observation of an action (Buccino et al., 2016; Hauk et al., 2008; Willems et al., 2010).

However, the behavioural effects and sensomotoric activations that are taken as evidence for embodied cognition show substantial variability across studies. Firstly, the observed effects are highly context-dependent (Barsalou, 2016; Binder, 2016; Dove, 2018), which speaks against an automatic engagement of simulation in language processing. Yet, this does not apply to all effects in the context of embodiment as the typical location of a noun's referent (e.g., roof – up vs. root – down) has been shown to be activated automatically (Lachmair et al., 2011; Vogt et al., 2019). Secondly, they seem to be difficult to replicate (Miller et al., 2018; Papesh, 2015; however, see Zwaan & Pecher, 2012 for partially successful replications), and thirdly, effects of interference and also effects of facilitation are observed within the same paradigm (Buccino et al., 2016; Horchak et al., 2014; Zwaan & Pecher, 2012). Thus, there is an ongoing debate on the role of simulation processes for language comprehension. While a strong view of embodied cognition proposes that simulating implied actions is a necessary precondition for comprehending a sentence (Barsalou, 1999; Bergen, 2005; Zwaan & Radvansky, 1998), more moderate accounts assume that modal representations are context-dependent or a by-product of language comprehension (Kaup & Ulrich, 2017; Maienborn et al., 2015; Meteyard et al., 2012). The debate has led to the emergence of a wide spectrum of partially similar, partially largely diverging frameworks that try to account for these diverse findings (Barsalou, 2016; Dove, 2018; Horchak et al., 2014; Mahon, 2015; Ostarek & Huettig, 2019; Ralph et al., 2017). It would outreach the scope of this paper to give a detailed overview of these theories (see Meteyard et al., 2012 for a placement of theories on a continuum from strongly embodied to completely unembodied representations).

The present study aims to contribute to the understanding of one specific object of investigation within this debate. More specifically, the mental representation of speed associated with manner of motion verbs is investigated in the present study by implementing a behavioural paradigm. It contributes to the findings of two eye-tracking studies and one fMRI study that speak for the engagement of modal representations of speed associated with manner of motion verbs, but which do not solve the question of the modal representations' automaticity (Lindsay et al., 2013; Speed & Vigliocco, 2014; van Dam et al., 2017).

Both Speed and Vigliocco (2014) and Lindsay et al. (2013) observed influences of verb-associated speed of motion on eye movements by using a visual-world paradigm. Speed and Vigliocco (2014) observed that participants had longer dwell times on the motion's destination for sentences with verbs associated with slow motion (e.g., *The lion **ambled** to the balloon*; in the following referred to as slow-speed sentences), compared to sentences with verbs denoting fast motion (e.g., *The lion **dashed** to the balloon*; in the following referred to as fast-speed sentences). Lindsay et al. (2013) report longer looking times to the motion's path for slow-speed sentences compared to fast-speed sentences. However, with respect to the motion's destination, Lindsay et al. (2013) recorded longer looking times for fast-speed sentences than for slow-speed sentences, which is in the opposite direction of what Speed and Vigliocco (2014) reported. Despite these discrepancies, which Lindsay et al. (2013) explain with differences in the length of sentences and by means of their analysis (path is not a region of interest in Speed and Vigliocco's (2014) study), the authors of both studies seem to agree that their results imply that the verb-associated speed information is an integral part of simulation. Moreover, both assume that a simulation's duration correlates with the duration of the linguistically expressed event which can be inferred from longer looking times to the path (resp. destination) for slow-speed sentences compared to fast-speed sentences.

The findings of the two eye-tracking studies are complemented by an fMRI study by van Dam et al. (2017). They observed that fast-speed sentences elicited stronger activations in superior and middle occipital regions (i.e., right posterior superior temporal sulcus), while the processing of slow-speed sentences more strongly activated cortical regions that are involved in the representation of actions and action plans (i.e., right primary motor area (M1) and right anterior inferior parietal lobule). The stimulus material of this study was constructed with the same constraints as in the eye-tracking studies, i.e., only the manner of motion verbs were changed within intransitive sentence pairs to manipulate speed (e.g., *The old lady **scurried** across the road* vs. *The old lady **strolled** across the road*).

The three studies are not only comparable with respect to the stimulus material, but also in terms of the tasks that participants had to perform. In the studies by van Dam et al. (2017) and Lindsay et al. (2013), participants were only instructed to read, resp. listen to the experimental sentences.[13] In the study by Speed and Vigliocco (2014) there were some minor attention control tasks such as comprehension questions, mouse-clicking, or verb-verification tasks, yet the authors stress that the visual-world paradigm allows an online recording of simulation during “simply listening and understanding” (Speed & Vigliocco, 2014, p. 369) *without* requiring an additional task. That an effect of verb-associated speed information (in the following referred to as speed effect) was observed in these studies without making the speed of motion explicitly salient via the instruction and without presenting a visual scene in the case of van Dam et al.’s (2017) study suggests that the speed effect might represent an automatic activation of modal representations. Indeed, van Dam et al. (2017) concluded that “representations accessed during comprehension of language that denotes fast and slow motions seem to incorporate information about the speed of motion.” (van Dam et al., 2017, p. 54) Following this line

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[13] The experiment by Lindsay et al. (2013) was split into two parts, one of which involved a mouse-tracking task. However, the part of the experiment that was relevant for the eye-tracking data only involved the task to listen carefully, to look at the screen, and to try to understand what was going to happen.

of argumentation, it might be the grounded lexical-semantic content of manner of motion verbs associated with fast or slow motion (in the following referred to as fast-speed verbs and slow-speed verbs), which leads to the observed differences in neuronal activation patterns.

However, from the perspective of a decompositional semantics approach to the meaning of motion verbs (Bierwisch, 1996, 2011; Lang & Maienborn, 2011; Maienborn, 2017), an automatic activation of modal representations of speed information is not expected. Within the decompositional paradigm, a motion verb such as *walk* or *run* receives a lexical entry along the lines of, e.g., (3.1). Each lexical entry consists of an argument structure (AS) and a semantic form (SF). While AS represents information, which is relevant for syntactic processes, SF represents the respective linguistic knowledge of an expression's meaning, and is thus at the centre of interest for this discussion. According to the semantic representation in (3.1.a), the verb *to walk* denotes a set of dynamic events e, in which an individual x moves along a path w (MOVE(x, w)) in a specific manner of motion (WALK*). The path is further specified by the locative predicate P. Crucially, motion is coded identically in the SF of both verbs by means of the SF component MOVE. Thus, the concept of motion in the propositional representation is equally well accessible for mental processes for all manner of motion verbs. On the contrary, the motion verbs' specific kinematic information, such as speed, is embedded in the idiosyncratic components WALK*(x), and RUN*(x), respectively, and is accessible only in a further processing stage, i.e., the retrieval of conceptual knowledge.

$$\begin{aligned} &\text{(3.1.a) } \textit{walk}\text{:} && \lambda P\ \lambda x\ \lambda e{:}\mathrm{DYN}\ \exists w{:}\mathrm{PATH}\ [e{:}\ \mathrm{MOVE}(x, w)\ \&\ P(w)\ \&\ \mathrm{WALK}^*(x)]\\ &\text{(3.1.b) } \textit{run}\text{:} && \underbrace{\lambda P\ \lambda x\ \lambda e{:}\mathrm{DYN}\ \exists w{:}\mathrm{PATH}}_{\mathrm{AS}}\ \underbrace{[e{:}\ \mathrm{MOVE}(x, w)\ \&\ P(w)\ \&\ \mathrm{RUN}^*(x)]}_{\mathrm{SF}} \end{aligned}$$

Adopted from Maienborn (2017)

According to this view, to retrieve and understand a manner of motion verb's meaning, it would be sufficient to evaluate its lexical entry as in (3.1) without the activation of conceptual (modal) knowledge about the associated kinematic information embedded in the idiosyncratic meaning component. That is, a decompositional semantics account of the meaning of motion verbs does not expect speed effects as observed in the studies by Lindsay et al. (2013), Speed and Vigliocco (2014), and van Dam et al. (2017).

Indeed, there is an alternative explanation for the observed effects. Since sentence presentation in van Dam et al.'s (2017) study was relatively long (2.25 s) it might have encouraged participants to execute mental imagery of the sentences' content (Tomasino et al., 2007). Moreover, the findings in the two eye-tracking studies are not entirely consistent and seem to be dependent inter alia on the amount of time that is available for sentence processing (Speed & Vigliocco, 2014). Consequently, it is conceivable that what the authors of the three studies observed reflects a later cognitive process instead of an automatic lexical-semantic retrieval of grounded information.

In the present study, a reaction time (RT) paradigm originally implemented by Wender and Weber (1982), who also investigated the processing of fast-speed and slow-speed sentences, will be adopted and developed. This is done to shed light on the issue of whether the speed effect reported by the above mentioned studies reflects an automatic activation of modal representations of speed information or whether it stems from a later processing stage. In the study by Wender and Weber (1982), participants were asked to form a mental image of the sentence's content and to respond by pressing a button, if the sentence expressed motion, and by pressing another button, if this was not the case. The sentences contained either fast-speed verbs (*Der Ball fliegt ins Tor* – 'The ball flies into the goal') or slow-speed verbs (*Der Ball rollt ins Tor* – 'The ball rolls into the goal'), or they expressed static scenes (*Der Ball ist im Tor* – 'The ball is in the goal'). Participants were faster in detecting motion for fast-speed sentences compared to slow-speed sentences even though they were naïve with respect to the systematic difference of speed between the sentences.

This pattern in RTs seems to speak for the engagement of modal representations, as can be derived from the decompositions, i.e., from the propositional representations illustrated in (3.1). When drawing on these propositional mental representations to decide whether a sentence expresses motion, it should take the same amount of time for both fast-speed and slow-speed sentences, because the component MOVE is equally well accessible for the verbs of both levels of speed. From a decompositional semantics' perspective, the RT difference between fast-speed and slow-speed sentences reported by Wender and Weber (1982) can thus not be attributed to a process that merely has access to the lexical entries of the manner of motion verbs but must instead involve modal representations.

Yet, it remains an open question whether the activation of modal representations that are assumed to be responsible for the speed effect reported by Wender and Weber (1982) stem from the explicit prompt to engage with mental imagery (Tomasino et al., 2007). Alternatively, the modal representations might be task-driven, such that the salience of motion in the task elicits their activation (Bedny & Caramazza, 2011; Ostarek & Huettig, 2019), or their activation happens automatically upon the processing of fast-speed and slow-speed sentences as suggested by the results of van Dam et al. (2017). To evaluate the role of explicit mental imagery processes for the speed effect, Wender and Weber's (1982) instruction was modified in the present study, such that the explicit request to imagine the sentence's content was eliminated in Experiment 1. However, the main paradigm, i.e., the combination of dynamic with static sentences and the task to detect motion was maintained. In Experiment 2, on the other hand, the task to detect motion was replaced by the task to judge the sentence's sensicality. This task still requires the lexical-semantic retrieval of the involved concepts, such that the speed effect should still be present if modal representations are an invariant component of the semantic representation and thus automatically and necessarily retrieved (Bedny & Caramazza, 2011; Miller et al., 2018; Ulrich et al., 2012).

Another issue that will be dealt with in this study is the modal representations' locus. Slow-speed sentences in van Dam et al.'s (2017) study elicited more activation in motor areas (i.e., M1) than fast-speed sentences, while fast-speed sentences more strongly activated regions involved in visual processing. Yet, their stimulus material did not allow for interpretations concerning the comparison of motion sentences with abstract filler sentences due to differences in syntax between these two types of sentences (van Dam et al., 2017, p. 54). Thus, their results do not enable conclusions concerning the question of whether different levels of speed associated with fast-speed and slow-speed sentences more strongly activate visual or motor representations compared to no-speed sentences.

In an insightful review, Beveridge and Pickering (2013) point out the importance of a spatial context that needs to be established to allow for simulation to take place, since modal representations are inherently perspective-based. Speed and Vigliocco (2014) consider their results being compatible with perceptual as well as action-based simulation. The latter would imply covert motor imitations of the described event. Both types of processes are also conceivable for the speed effect reported by Wender and Weber (1982). Additional to the agent, there is also a potential observer whose perspective can be adopted and simulated. If the agent's perspective were taken, the processing of the experimental sentences could, according to an embodied cognition framework, entail the motoric simulation of the described movement (Andres et al., 2015; Glenberg & Gallese, 2012). One could assume that this happens via simulating the foot being lifted from the ground and then placed back down according to the verb's denoted kinematic pattern. Since this takes longer for slow motion than for fast motion, motoric simulation could explain the observed speed effect. If, however, the observer's perspective were adopted, the simulation would be more perceptual. More specifically, it would contain the visual percept of somebody or something moving either fast or slowly. Recognising that there is movement taking place, as is demanded in this task, would be faster for fast movement, since there is more visually observable spatial change compared to slow movement in a given time interval. Thus, also visual simulation would be able to explain faster responses to the question of whether movement is described in a sentence upon reading fast-speed compared to slow-speed sentences.

However, motoric simulation is only plausible for self-propelled motion, or more specifically, in cases in which the motion can be carried out motorically or at least empathized by the reader. In case of object motion, i.e., externally driven movement (e.g., a ball flying into a goal), this is impossible. Wender and Weber (1982) employed noun phrases that referred to humans as well as common objects as the events' agents. However, they did not specify the amount of each type of noun phrase. This might be due to the fact, that they did not differentiate between human and object movement in their analysis. To distinguish perceptual and motoric simulation, an equal amount of human and object motion sentences was employed in the stimulus material of the present study and taken into account in the analysis. If the speed effect is driven by motoric simulation, it should only be observable for human motion but not for object motion. If, however, the speed effect stems from visual simulation, it should be evident in both types of motion.

Experiment 1

Experiment 1 adopted Wender and Weber's (1982) design. In their study, participants read sentences that described movement and sentences that depicted static scenes and they were instructed to decide rapidly whether the sentence's content contained a movement or not after having imagined the depicted scene. As mentioned above, the main modification in Experiment 1 to their study was to eliminate the explicit request to imagine the sentence's content. A second modification concerned the movement's agent in the stimulus material. Wender and Weber (1982) employed noun phrases with humans as well as common objects as agents, but did not report the respective proportion within the stimulus material. Against the background of perspective taking, as discussed above, the stimulus material of this experiment was equally split into human and object movement. Thirdly, since participants were not instructed to imagine the sentences' content before giving a response, there was a danger of participants giving a response after having read the verb instead of the whole sentence. Since all the sentences were constructed in present tense, the verb always succeeded the noun phrase but preceded the prepositional phrase, which made up the longest part of the sentence (e.g., *Der Traktor brettert über den Acker* – 'The tractor is barrelling across the field'). The verb, though, sufficed as cue for deciding whether the sentence depicted movement or not. Thus, to make RTs interpretable and comparable across items and participants we made sure that all participants had to read the sentences up to the end and to give their response only right afterwards by implementing a Go/NoGo-design (see Procedure for details) in this experiment (see Ulrich & Maienborn, 2010 for an analogous task design and Appendix B.1 for the development of the Go/NoGo-design within this paradigm). One further technical change was made to increase the task flow: Sentence presentation was not self-paced as in Wender and Weber’s (1982) study, but a fixed intertrial interval (ITI) automatically scheduled sentence presentation.

Method

Participants

40 volunteers participated in Experiment 1 (27 female and 13 male) and received reimbursement based on an hourly pay of 8 Euro or course credit. They were native speakers of German and either students of the University of Tübingen or were working in and around Tübingen. The mean age was 25.95 years (*SD* = 7.45). 36 reported being right-handed, the remaining 4 were left-handed. They were naïve with respect to the purpose of investigation. An experimental session lasted about 30 min. They gave written informed consent before the start of the experiment.

A sample size of 40 participants was chosen since we oriented towards the study by Wender and Weber (1982) but wanted to exceed their number of observations. Wender and Weber obtained data from 33 participants and had 20 fast-speed and slow-speed sentences per participant. We increased both number of participants and items; the former to a lesser degree than the latter (the number of fast-speed and slow-speed sentences per participant was 96 in the present experiment).

Apparatus

The experiment was programmed in PsychoPy v1.90.3 and run in a sound-attenuated room. The sentences were presented in the centre of the computer screen in black against a grey background (Arial; scaling factor 0.06). The keys < and - of a standard German keyboard were used as response buttons.

Stimuli

One experimental session consisted of 288 German sentences. German is a satellite-framed or manner-type language, which incorporates manner of motion in the main verb and expresses path information mainly outside the verb, e.g., by means of a directional prepositional phrase (Talmy, 2000). One third of the sentences denoted motion (*The tractor is barrelling across the field*), another one-third of the sentences denoted static scenes (*Charlotte is sleeping on the lawn*), and the rest of the sentences were nonsense sentences (*The emergency car is turning quickly at the sun*). Each of these three types of sentences was equally split into sentences with subjects referring to humans (in the following referred to as human motion) and sentences with subjects referring to common objects (in the following referred to as object motion; see Appendix B.2 for a list of all sentences).

Motion Sentences. All sentences were written in present tense. The denoted events had an agent that was moving (see (3.2) for human and (3.3) for object motion). On behalf of the comparability of RTs, motion sentences were matched as pairs, such that each pair of motion sentences only differed in (the speed of) the motion verb (see (a) for fast-speed verbs and (b) for slow-speed verbs). The sentences had a prepositional phrase that served as the verb's directional adverbial.

(3.2.a) Rebecca sprintet über die Alm.
Rebecca is sprinting across the alpine pasture.
(3.2.b) Rebecca wandert über die Alm.
Rebecca is hiking across the alpine pasture.
(3.3.a) Der Traktor brettert über den Acker.
The tractor is barrelling across the field.
(3.3.b) Der Traktor holpert über den Acker.
The tractor is jolting across the field.

Human Motion. 28 motion verbs were collected that were introspectively pre-classifiable with equal shares into fast-speed and slow-speed verbs. In order to validate the speed manipulation, the 28 motion verbs were presented to 40 participants, who were asked to indicate the verbs' speed on a scale of 1 to 7 (1 = very slow; 7 = very fast). Specifically, they were asked *Wie hoch ist die Fortbewegungsgeschwindigkeit dieses Verbs?* ('How high is this verb's locomotion's velocity?').

The questionnaire was programmed with E-Prime 2.0 Professional; answers were given via mouse click. The verbs were presented in their infinitive form and their order was randomised for each participant. Based on the results of the rating, the pairing of the verbs was arranged such that the difference in speed of each verb pair was maximized. Furthermore, four of the prior 28 verbs were discarded from the final motion verb selection for the benefit of better balancing of frequency. This resulted in 12 motion verbs per speed condition that were matched as pairs. The mean rated speed of fast-speed verbs was $M = 5.97$ ($SD = 0.97$), while the mean rated speed of slow-speed verbs was $M = 2.37$ ($SD = 0.95$). Paired one-tailed *t*-tests were calculated for subjects and for items. Both were significant ($t_1(39) = 42.72$, $p_1 < .001$; $t_2(11) = 10.91$, $p_2 < .001$). Thus, the perceived (i.e., rated) speed of the selected slow-speed compared to the selected fast-speed verbs was significantly lower.

The frequency of the motion verbs was determined using the Leipzig Corpora Collection (2018).[14] While the fast-speed verbs had a mean frequency class of $M = 14.42$ ($SD = 1.62$), the slow-speed verbs had a mean frequency class of $M = 15.50$ ($SD = 2.35$). An independent two-tailed *t*-test was calculated which was not significant, $t(19.52) = 1.31$, $p = .205$. Thus, the frequency of the motion verbs did not differ significantly between the two levels of speed. However, since fast-speed verbs were numerically more frequent than slow-speed verbs, frequency was included in the analysis as a predictor of RTs to account for this potentially confounding variable.

Additionally, the motion verbs' length was compared between conditions via the number of letters. While the fast-speed verbs had an average number of letters of $M = 5.50$ ($SD = 1.24$), the slow-speed verbs were on average longer with $M = 7.33$ ($SD = 1.50$). An independent two-tailed *t*-test revealed that this difference was significant ($t(21.28) = 3.26$, $p = .004$). Consequently, number of letters was also considered a predictor of RTs in the linear mixed-effects model of the main analysis to account for this systematic difference between the fast-speed and slow-speed human manner of motion verbs.

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[14] Leipzig Corpora Collection (2018). German newspaper corpus based on material crawled in 2018. Leipzig Corpora Collection. Dataset. https://corpora.uni-leipzig.de/de?corpusId=deu_newscrawl-public_2018.

Each of the twelve verb pairs was combined with four prepositions (*entlang* – ‘along’; *über* – ‘over/across’; *durch* – ‘through’; *zu* – ‘to’)[15] to increase the amount of stimulus material. This led to a total of 48 pairs of human motion sentences. To avoid repetition for participants, these motion sentences were split into two lists, with counterbalanced levels of speed (*fast* and *slow*), counterbalanced occurrence of each verb, and counterbalanced occurrence of each preposition.

Object Motion. Similarly, verbs that can be used to express object motion were collected and submitted to a speed rating, programmed in PsychoPy v1.90.3, and tested with further 40 participants. Responses were again given via mouse click, and item presentation was randomised for each participant. However, the verbs of motion were not presented isolated but in combination with an object as the agent. This was done because the associated speed for some of the motion verbs is highly dependent on the object that is moving. In addition, since some of the verbs that describe object motion are sound emission verbs that can be coerced to motion verbs in a specific context, they do not sound felicitous with any kind of object, and more importantly, might not be associated with a decisive speed for any potential object. Thus, we wanted to make sure that the objects in combination with the motion verbs we were going to use in the stimulus material were associable with a specific velocity. The participants consequently saw sentences of the form ‘How high is the velocity of an X for an X, when it *verbs*?’. By employing this wording, the velocities for instance of (a) a tractor that jolts and (b) a tractor that barrels, as well as of (c) a jet plane that glides, and (d) a jet plane that whooshes (see (3.4)) were representable on the same 1 to 7 scale (1 = very slow; 7 = very fast) with both, jet plane and tractor, being represented on both sides of the scale depending on the specific verb, irrespective of the fact that a jet plane’s movement is always much higher compared to a tractor’s movement.

(3.4.a) Wie hoch ist die Fortbewegungsgeschwindigkeit eines Traktors für einen Traktor, wenn er holpert?
How high is the velocity of a tractor for a tractor, when it jolts?

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[15] Prepositional phrases with *über* (‘over/across’) and *durch* (‘through’) are ambiguous between a telic and an atelic reading, directional prepositional phrases with *zu* (‘to’) have a telic reading, while *entlang* (‘along’) elicits an atelic reading (Lukassek et al., 2017).

(3.4.b) Wie hoch ist die Fortbewegungsgeschwindigkeit eines Traktors für einen Traktor, wenn er brettert?

How high is the velocity of a tractor for a tractor, when it barrels?

(3.4.c) Wie hoch ist die Fortbewegungsgeschwindigkeit eines Düsenjets für einen Düsenjet, wenn er segelt?

How high is the velocity of a jet plane for a jet plane, when it glides?

(3.4.d) Wie hoch ist die Fortbewegungsgeschwindigkeit eines Düsenjets für einen Düsenjet, wenn er zischt?

How high is the velocity of a jet plane for a jet plane, when it whooshes?

In the rating study, 20 motion verbs were presented in combination with 49 objects to find pairs of motion verbs that were dividable into fast-speed and slow-speed verbs, even when combined with different individual objects. Out of the 122 verb-object-combinations that were presented in the rating study, 8 fast-speed and 8 slow-speed verbs in combination with a total of 48 different objects (6 per verb pair) were selected for the stimulus material. The selected fast-speed verbs in combination with the corresponding objects were rated to have a speed of $M = 5.68$ ($SD = 1.33$), while the selected slow-speed verbs in combination with the same objects were rated to have a speed of $M = 2.45$ ($SD = 1.15$). Paired one-tailed *t*-tests were calculated for subjects and items that were both significant ($t_1(39) = 22.34$, $p_1 < .001$; $t_2(46) = 46.51$, $p_2 < .001$).[16] Thus, it also applies to the object motion verbs in combination with the tested objects that the fast-speed verbs are perceived to denote significantly faster motion than the slow-speed verbs.

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[16] Due to a coding error we unfortunately did not manage to test one of the fast-speed verbs (*brausen* – ‘race’) in combination with one specific object (*Segelboot* – ‘sailing boat’). However, we tested *brausen* with the objects *Yacht* (‘yacht’), *Kreuzfahrtschiff* (‘cruise ship’), *Fischkutter* (‘fishing boat’), *Fähre* (‘ferry’), and *Motorboot* (‘motor boat’), all of which are different types of boats/ships just like *Segelboot*. Since variance of the mean ratings of *brausen* in combination with the named objects is very low ($SD = 0.08$), it seems reasonable to still include *brausen* in the final stimulus material as a verb that describes fast motion of a *Segelboot*.

Table 3.1. ***Human and object motion verb pairs in Experiments 1 and 2.***

	Slow-speed verbs				Fast-speed verbs			
Verbpair	Verb	Rating	Length	Frequency	Verb	Rating	Length	Frequency
	Human motion verb pairs							
1	wandern	3.48	7	12	sprinten	6.80	8	16
2	latschen	2.63	7	18	preschen	6.08	7	16
3	humpeln	1.75	7	18	hasten	5.40	6	17
4	bummeln	2.15	7	15	huschen	4.90	6	16
5	torkeln	1.88	7	18	flitzen	6.28	6	15
6	wanken	1.95	5	16	sausen	6.15	5	14
7	hinken	1.73	5	15	joggen	5.05	5	14
8	schreiten	3.28	9	13	rasen	6.80	4	14
9	spazieren	3.18	8	12	stürmen	6.58	6	13
10	schlendern	2.40	10	14	jagen	6.23	4	12
11	trotten	2.23	7	17	eilen	5.35	4	14
12	schlurfen	1.85	9	18	rennen	6.05	5	12
M		2.37	7.33	15.50		5.97	6.67	14.42
SD		0.63	1.50	2.35		0.65	1.54	1.62
	Object motion verb pairs							
13	segeln	2.91	6	14	zischen	5.66	6	16
14	schweben	2.63	7	13	rauschen	5.52	7	15
15	kriechen	1.50	7	14	rattern	4.23	7	16
16	rollen	2.57	5	11	schießen	6.50	7	11
17	tuckern	2.23	7	17	düsen	5.87	4	16
18	holpern	2.66	7	19	brettern	5.93	8	16
19	gleiten	3.03	7	14	donnern	6.18	7	15
20	treiben	2.08	6	10	brausen	5.55	6	16
M		2.45	6.50	14.00		5.68	6.50	15.12
SD		0.50	0.76	2.93		0.67	1.20	1.73
M (overall)		2.40	7.00	14.90		5.85	5.90	14.70
SD (overall)		0.57	1.30	2.63		0.66	1.29	1.66

Note. *Length* refers to the number of characters of the inflected verb (3^{rd} person singular present tense). *Frequency* refers to the frequency classes obtained from the Leipzig Corpora Collection (2018). *Rating* refers to the speed ratings as described above. Standard deviation of the speed ratings is based on the mean speed ratings. Adapted only in its format from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 6. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

Additionally, the semantic similarity was determined for each object-verb-combination using the cosine-function of the *LSAfun* package in R (Version 3.6.0, 2019) on the de_wiki German cbow space with 400 dimensions (Günther et al., 2014), to control for semantic priming effects of motion verbs by the objects between the two levels of speed. The 48 chosen objects in combination with the fast-speed verbs had a mean cosine value of $M = 0.179$ ($SD = 0.097$), while the same objects in combination with the slow-speed verbs had a mean cosine value of $M = 0.155$ ($SD = 0.097$). An independent two-tailed t-test was calculated which was not significant ($t(94) = 1.21$, $p = .228$). Consequently, semantic priming of the motion verbs by the objects did not systematically differ between the two levels of the factor Speed.

Analogously to the human motion verbs, frequency and the number of letters were determined and tested for the two levels of the factor speed. The fast-speed verbs had a frequency class of $M = 15.12$ ($SD = 1.73$), the slow-speed verbs had a frequency class of $M = 14.00$ ($SD = 2.93$). An independent two-tailed t-test was calculated which was not significant ($t(11.35) = 0.94$, $p = .369$). Thus, just like for the human motion verbs the frequency of the object motion verbs did not differ significantly between the two levels of Speed.

Mean number of letters was identical for the two levels of Speed for object motion verbs (fast-speed verbs: $M = 6.50$, $SD = 1.20$; slow-speed verbs: $M = 6.50$, $SD = 0.76$). Their difference in means was not significant ($t(11.83) = 0.00$, $p > .999$) (see Table 3.1 for an overview of all human and object motion verb pairs including speed rating, frequency class, and number of letters).

Since there were fewer object motion verb pairs than human motion verb pairs (8 vs. 12), object motion verb pairs had to be repeated more often to yield the same amount of 48 experimental sentences. Thus, each of the eight verb pairs was combined with six instead of only four different prepositions (*entlang* – ‘along’; *über* – ‘across’; *durch* –

‘through’; *zu* – ‘to’; and additionally, *in* – ‘into’; *auf* – ‘onto’) to form the entire sentences. Again, in order to avoid repetition for participants, these motion sentences were split into two lists, with counterbalanced levels of speed (*fast* and *slow*), counterbalanced occurrence of each verb, and counterbalanced occurrence of each preposition. Thus, each object motion verb was shown three times to one participant, while each human motion verb was only repeated twice for one participant. However, the sentences in which the repeated motion verbs were embedded, differed for each single presentation of a motion verb with respect to the noun phrase and the prepositional phrase.

Static and Nonsense Sentences. Both static (3.5) and nonsense (3.6) sentences were not matched for length or words since they were not statistically analysed. Just like the motion sentences, they were written in present tense and had a human as agent for human static (3.5.a) and human nonsense sentences (3.6.a) or an object as agent for the object static (3.5.b) and object nonsense sentences (3.6.b).

(3.5.a) Charlotte schläft auf der Wiese.
Charlotte is sleeping on the lawn.
(3.5.b) Das Auto parkt in der Sperrzone.
The car is parking in the restricted zone.
(3.6.a) Olga stampft durch den Artikel.
Olga is trudging through the article.
(3.6.b) Der Rettungswagen wendet zügig an der Sonne.
The emergency car is turning quickly at the sun.

Half of the nonsense sentences were created using motion verbs that were not part of the experimental motion verbs, the other half of the nonsense sentences contained static verbs. Nonsense sentences were created using static and motion verbs in order to avoid that the verb might be used as a cue for the decision whether the sentence was sensible or not. The way the nonsense sentences were designed undermined this strategy. In (3.6.a) for instance, a plausible sentence would be *Olga stampft durch den Garten* (‘Olga is trudging through the garden’). Consequently, only the noun (i.e., ‘article’) reveals that the sentence is not sensible. Thus, to give a correct response, participants had to read the sentences until their end.

Overall, one experimental session consisted of 288 sentences. 96 of these were motion sentences (48 human motion and 48 object motion), 96 were static sentences, and 96 were nonsense sentences, both latter ones with the same equal ratio of humans and objects as agents. Additionally, twelve training sentences were created that contained sentences of all three types of sentences and both types of motion, human and object. These were run as a training block previous to the experiment.

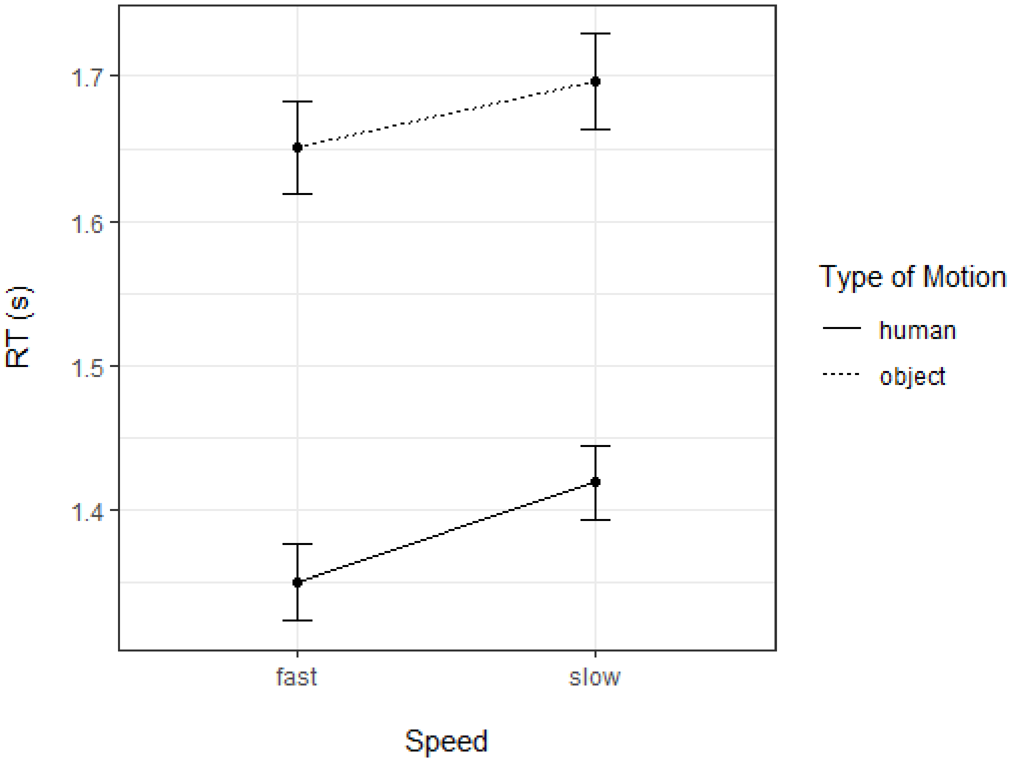
Procedure

Before starting the experiment, participants were given written instructions. They were asked to read the sentences at their own pace and to decide as quickly as possible whether the sentence expressed motion or not, however, only if the sentence was sensical. If the sentence was nonsensical, they had to refrain from responding. This task design is what is referred to by the above mentioned Go/NoGo-design. In cases in which the sentence was sensible and a response was demanded, the participants responded with the index finger of the right hand (key -), when the sentence expressed motion, and with the index finger of the left hand (key <), when this was not the case. The sentence disappeared from the screen as soon as they pressed one of the two buttons, but was presented maximally for a duration of 3.5 s. If the sentence was nonsensical, the participants thus had to wait for 3.5 s until the end of the trial. In case of wrong answers, visual feedback was given. The appearance of the next sentence was not self-paced, but automatic: Subsequent to an ITI of 2 s, the fixation cross automatically appeared for 200 ms, followed by a blank screen presented for 500 ms, after which the next sentence was presented. Half of the participants was randomly assigned to one list of the stimulus material, the other half was assigned to the second list. The presentation order of sentences was randomised. RT was measured from stimulus onset up to the keypress. The factor Speed with the levels *fast* and *slow* and the factor Type of Motion with the levels *human* and *object* were within-subjects factors, whereas Stimulus List was manipulated between-subjects.

Results

Overall accuracy rate for all types of sentences was 93.08% (*SD* = 3.70). Participants correctly refrained from responding in 91.95% (*SD* = 5.78) of the nonsense trials. Sensical sentences were correctly recognised as static or motion sentences in 93.65% (*SD* = 3.93) of sensical trials. Accuracy of *fast* was 96.67% (*SD* = 5.44), accuracy of *slow* was 95.00% (*SD* = 4.47), and accuracy of *static* was 91.46% (*SD* = 4.11).

Figure 3.1. ***Mean RTs in Experiment 1 by Type of Motion and Speed.***



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). Reprinted from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 7. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

Only trials with correct responses from the Speed conditions *fast* and *slow* were submitted to data analysis of RTs. Within this data pool, outliers were identified following a two-step-procedure: Firstly, trials with RTs less than 100 ms (0.00%) were excluded. Secondly, mean RTs for each Subject, in each Type of Motion condition (*human* and *object*), and each Speed condition (*slow* and *fast*) were determined. RTs that deviated more than 2.5 standard deviations from the respective cell's mean, were considered outliers and excluded (2.15% of *fast* and *slow* sentences). Mean RT of *fast* was 1497 ms (*SD* = 470), while mean RT of *slow* was 1555 ms (*SD* = 466) (see Figure 3.1 for mean RTs by Type of Motion and Speed; see Table 3.2 for standardized effect sizes of the differences in means overall and per Type of Motion condition).

Table 3.2. ***Difference scores in Experiment 1 (overall and per Type of Motion) after removal of outliers and incorrect responses.***

	fast	*slow*	Raw differences in means [95% CI]	Cohen's *d* [95% CI]
Overall	1497 (470)	1555 (466)	55.12 [24.75, 85.49]	0.58 [0.24, 0.92]
human motion	1351 (415)	1424 (409)	69.88 [34.92, 104.84]	0.64 [0.30, 0.99]
object motion	1649 (476)	1692 (483)	44.35 [-1.37, 90.08]	0.31 [-0.01, 0.63]

Note. The first two columns represent the raw means (and *SD*s) of RTs (in ms) for the two levels of Speed. The raw differences in means (in ms) and their confidence intervals are obtained by aggregating the data across participants. Adapted only in its format from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 7. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

After outlier removal, data were analysed with a linear mixed-effects model (LMEM) using the *lme4* package (Bates et al., 2015) in R (Version 4.0.4, 2021). The full model included Speed (*fast* vs. *slow*, with *fast* as reference category), Type of Motion (*human* vs. *object*, with *human* as reference category)[17], Length (i.e., number of characters; encoded numerically and mean centred), and Frequency (encoded numerically

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[17] Type of Motion was included as a fixed effect in the model, since object motion sentences are not only longer than human motion sentences (mean number of characters of *human* is 29.9, of *object* motion sentences is 37.4), but also they are designed slightly differently by having definite descriptions (e.g., 'the tractor') compared to proper names (e.g., 'Rebecca') as agents. The difference in number of characters is accounted for by the fixed factor Length, however, systematic variations in RT due to the difference concerning the sentence's subject cannot be explained without including Type of Motion in the LMEM.

and mean centred) as fixed factors to predict RTs. Moreover, an interaction of Speed and Type of Motion was included in the model. Subjects and Items were considered random effects.

The random effects structure was determined by step-wise reducing the most complex random effects structure that also integrated the nesting of subjects into stimulus lists, while keeping the fixed effects structure constant (Barr et al., 2013). The random effects structure was selected based on the Akaike information criterion (AIC). The random effects structure with the lowest AIC value that converged, that did not over-fit the data, and that allowed for χ^2 difference tests included random intercepts for Subjects and Items as well as random slopes for the factor Speed for Items, which yielded following full model: RT ~ Speed * Type of Motion + Length + Frequency + (1 | Subjects) + (Speed | Items).

χ^2 difference tests on the full model in comparison with a reduced model after dropping the respective fixed factor revealed that the factors Length ($\chi^2(1) = 30.40$, $p < .001$), Speed ($\chi^2(1) = 6.97$, $p = .008$), and Type of Motion ($\chi^2(1) = 24.68$, $p < .001$) significantly increased model fit. Neither Frequency ($\chi^2(1) = 0.02$, $p = .876$) nor the interaction of Speed and Type of Motion significantly increased model fit ($\chi^2(1) = 0.04$, $p = .851$). The best-fit model, which was selected based on the AIC value, did not contain Frequency or the interaction of Speed and Type of Motion: RT ~ Speed + Type of Motion + Length + (1 | Subjects) + (Speed | Items). The best-fit model suggests an intercept of 1429.61 ms. The estimated coefficient for the factor Speed (with reference category *fast*) was $\beta = 41.45$ (95% CI [10.94, 71.84], $SE = 15.48$, $t(96.31) = 2.68$, $p = .009$), which implies that RTs for slow-speed sentences were estimated to be 41.45 ms higher than RTs for fast-speed sentences. The estimated coefficient for the factor Type of Motion (with reference category *human*) was $\beta = 165.67$ (95% CI [102.91, 228.96], $SE = 31.89$, $t(99.31) = 5.19$, $p < .001$). This implies higher RTs for object compared to human motion sentences, most probably because object motion sentences carried more semantic content than human motion sentences. More specifically, object motion sentences had a determiner phrase including a noun phrase (e.g., ‘the tractor’) as agents, while human motion sentences had proper names (e.g., ‘Rebecca’) as agents. Proper names have been shown to have a facilitated discourse integration compared to definite descriptions due to their unambiguous reference and potentially inherent definiteness (Burkhardt, 2019). The estimated coefficient for the factor Length was β = 16.99 (95% CI [11.46, 22.45], *SE* = 2.75, *t*(115.86) = 6.17, $p < .001$). *p*-values for the estimated coefficients of the best-fit model were obtained using the Satterthwaite's approximation for degrees of freedom, which is built-in in the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R.

The best-fit model suggests an estimated mean for *fast human motion* of 1430 ms (*SE* = 42.5), for *slow human motion* of 1471 ms (*SE* = 41.9), for *fast object motion* of 1596 ms (*SE* = 42.1), and for *slow object motion* of 1637 ms (*SE* = 42.7). Thus, the estimated coefficient for the factor Speed is 41 ms for both levels of Type of Motion.

To evaluate whether the absence of the interaction of Speed and Type of Motion could be a false negative finding, we conducted a Monte-Carlo simulation to enable a post-hoc power analysis.[18] Of 1000 datasets that were bootstrapped, 74.90% were best fitted by a model that included the interaction of Speed and Type of Motion. For 455 of these, the estimated coefficient for the interaction of Speed and Type of Motion was significant, yielding a statistical power of 60.75% based on an interaction effect of no difference for *object motion* sentences and a 73 ms difference for *human motion* sentences, which is equivalent to the observed effect.

Accuracy

To evaluate whether the speed effect was also evident in accuracy, we calculated a 2 (Speed) by 2 (Type of Motion) repeated measures ANOVA for all fast-speed and

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[18] 1000 datasets with the observed RTs for human motion sentences but with bootstrapped RTs for object motion sentences were simulated. More specifically, the RTs from the actual data for object motion sentences were randomly assigned without replacement to the conditions *fast* or *slow* to simulate the absence of a speed effect in object motion sentences. For these simulated datasets, the best-fit model was chosen based on AIC analogous to our main analysis between a model that included the interaction and one that did not include the interaction: RT ~ Speed * Type of Motion + Length + (1 | Subjects) + (Speed | Items) and RT ~ Speed + Type of Motion + Length + (1 | Subjects) + (Speed | Items). Statistical power was estimated as the number of models for which the estimated coefficient for the interaction of Speed and Type of Motion was significant according to *p*-values using the Satterthwaite's approximation for degrees of freedom given that the best-fit model included the interaction.

slow-speed sentences (before outlier removal) on accuracy using the *ezANOVA* function in R (Arnhold, 2013). There was a significant main effect of Speed [$F(1, 39) = 6.91$, $p = .012$, $\eta_p^2 = .15$], which reflected that fast-speed sentences had a higher accuracy than slow-speed sentences. The main effect of Type of Motion was also significant [$F(1, 39) = 24.43$, $p < .001$, $\eta_p^2 = .39$]. Human motion sentences had a higher accuracy than object motion sentences (see Table 3.3). Yet, the interaction of Speed and Type of Motion was not significant [$F(1, 39) = 0.68$, $p = .415$, $\eta_p^2 = .02$].

Table 3.3. ***Mean accuracy (and* SD*) in Experiment 1 for Speed by Type of Motion*.**

	fast	*slow*
Overall	96.67 (5.44)	95.00 (4.47)
human motion	98.54 (4.06)	97.50 (3.63)
object motion	94.79 (7.47)	92.50 (8.24)

Note. Adapted only in its format from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 7. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

Discussion

Experiment 1 replicated and extended the results of Wender and Weber (1982) and corroborates the findings of the studies by Lindsay et al. (2013), Speed and Vigliocco (2014), and van Dam et al.'s (2017), who reported a speed effect on eye-movements and on the activation of motor and perception regions. In Experiment 1, slow-speed sentences were reacted to more slowly than fast-speed sentences. This gives an answer to a question raised by Speed and Vigliocco, which is "to what extent […] the simulation of speed [can] be observed when there is no supporting visual scene" (Speed & Vigliocco, 2014, p. 380). The results of Experiment 1 imply that an effect of speed is not restricted to the mapping between sentence meaning and a visual scene but can also be observed in a task setting, in which subjects are asked to decide whether a sentence expresses motion.

The speed effect was observed even though the participants were not instructed to imagine the sentences' content as was done in the study by Wender and Weber (1982). Of course, it is possible that participants were engaged with mental imagery processes, even though not being explicitly asked to do so (Tomasino et al., 2007). However, since they were asked to give a speeded response, the task was more demanding compared to a task such as silent reading as used in van Dam et al.'s (2017) study and it seems possible that this would reduce or suppress imagery processes (see Miller et al., 2018, p. 364 for a similar account). Thus, it seems more likely that the speed effect is not related to conscious mental imagery.

While the RT difference of fast-speed sentences compared to slow-speed sentences in Wender and Weber's (1982) study was 137 ms, the estimated coefficient for the speed manipulation in our experiment was 41 ms and its confidence interval does not contain the RT difference reported by Wender and Weber. This might be due to the elimination of the task to imagine the sentences' content since visualization instructions seem to enhance embodiment effects (Willems et al., 2010). Yet, apart from mental imagery instructions there are a number of differences between the present study and the study by Wender and Weber (see the Introduction of Experiment 1 for details on the differences). Moreover, there is a trend in psychological research of effect sizes from replication studies being smaller than the effect sizes of the original studies (Schäfer & Schwarz, 2019). Thus, a direct comparison of effect sizes is not feasible. Yet, what can be concluded is that the speed effect observed by Wender and Weber (1982) is not dependent on the explicit instruction to engage with mental imagery.

Furthermore, we did not observe an interaction of Speed and Type of Motion. Even though we cannot rule out the possibility of a type 2 error, accuracy reveals a pattern analogous to RTs. While accuracy was higher for fast-speed sentences than for slow-speed sentences, there was no reliable interaction of Speed and Type of Motion on accuracy. More data would be needed to ascertain that there is no true interaction on RTs, yet the results seem to point to a speed effect that is not significantly modulated by Type of Motion. With respect to our hypothesis, this implies that visual rather than motoric modal representations are recruited within this paradigm, since movement in object motion sentences cannot be represented motorically (see the Introduction for details).

The outcome of Experiment 1 indicates that the speed effect observed by Wender and Weber (1982) cannot be attributed to the instruction to imagine the situation described by the motion sentences. Nonetheless, the speed effect might not reflect automatic processes involved in comprehending motion sentences. As mentioned in the introduction, task goals and instructional design play a major role in the elicitation of potential simulation effects (Ostarek & Huettig, 2019). Considering the empirical findings concerning the mental timeline opens up the prospect that the focussing of the concept of motion by the instruction is responsible for the observed speed effect. For the mental timeline it has been shown in various experiments that its activation depends on the salience of the concept of time (Maienborn et al., 2015; Ulrich & Maienborn, 2010; von Sobbe, Scheifele, et al., 2019). Its activation is not automatic and thus not necessary for language understanding. Since this could also apply to the speed effect, the task to detect motion was replaced by the task to judge the sentence’s sensicality in Experiment 2 to assess this hypothesis. This task still requires the lexical-semantic retrieval of the involved concepts, such that the speed effect should still be present if modal representations are an invariant component of the semantic representation and thus automatically and necessarily retrieved (Bedny & Caramazza, 2011; Miller et al., 2018; Ulrich et al., 2012).

Experiment 2

Experiment 2 was conducted with a different task. More specifically, a sensicality judgment task was implemented to examine whether the speed effect observed in Experiment 1 can be attributed to automatic processes when reading motion sentences. Alternatively, it is possible that the speed effect disappears when participants focus their attention on something else other than the concept of motion. Since a semantic analysis is needed to judge a sentence's sensicality, semantic processing in this task design is still encouraged and necessary to perform the task (Miller et al., 2018). If the speed effect is an automatic component of motion sentence processing, such as part of the lexical-semantic retrieval, it should still be observable in Experiment 2.

Method

Participants

Forty new volunteers, who were native speakers of German, were recruited from the same pool of participants and took part under the same compensation conditions. The data of two participants had to be excluded due to an overall error rate that exceeded 25%. Thus, the data of two new participants were collected. Thirty of the participants were female, 10 were male; 34 were right-handed, the rest was left-handed. The mean age was 26.68 years (*SD* = 10.57). Again, they were naïve concerning the object of investigation and one experimental session lasted about 30 min. Before the start of the experiment, they gave written informed consent.

Apparatus and Stimuli

Both, apparatus and stimuli were identical to Experiment 1.

Procedure

The procedure was identical to Experiment 1, except for the critical change in instruction: This time, participants were asked to decide as quickly as possible, whether the sentence was sensible or not. They were asked to respond with the index finger of the right hand (key -) if the sentence was sensible; however, if the sentence was nonsensical they were asked to respond with the index finger of the left hand (key <).

Results

Data analysis was analogous to Experiment 1. Overall accuracy rate was 93.15% (*SD* = 4.51). Accuracy of nonsense sentences was 87.73% (*SD* = 9.82). For the sensical sentences, accuracy of *fast* was 96.09% (*SD* = 3.34), accuracy of *slow* was 95.94% (*SD* = 4.69), and accuracy of *static* was 95.70% (*SD* = 2.91). The same procedure for outlier exclusion was implemented (RTs less than 100 ms: 0.00%; 1.74% of trials were excluded according to the *SD*-criterion). Mean RT of *fast* sentences was 1437 ms (*SD* = 460), while mean RT of *slow* sentences was 1426 ms (*SD* = 453) (see Figure 3.2 for mean RTs by Type of Motion and Speed; see Table 3.4 for standardized effect sizes of the differences in means overall and per Type of Motion condition).

Table 3.4. ***Difference scores in Experiment 2 (overall and per Type of Motion) after removal of outliers and incorrect responses.***

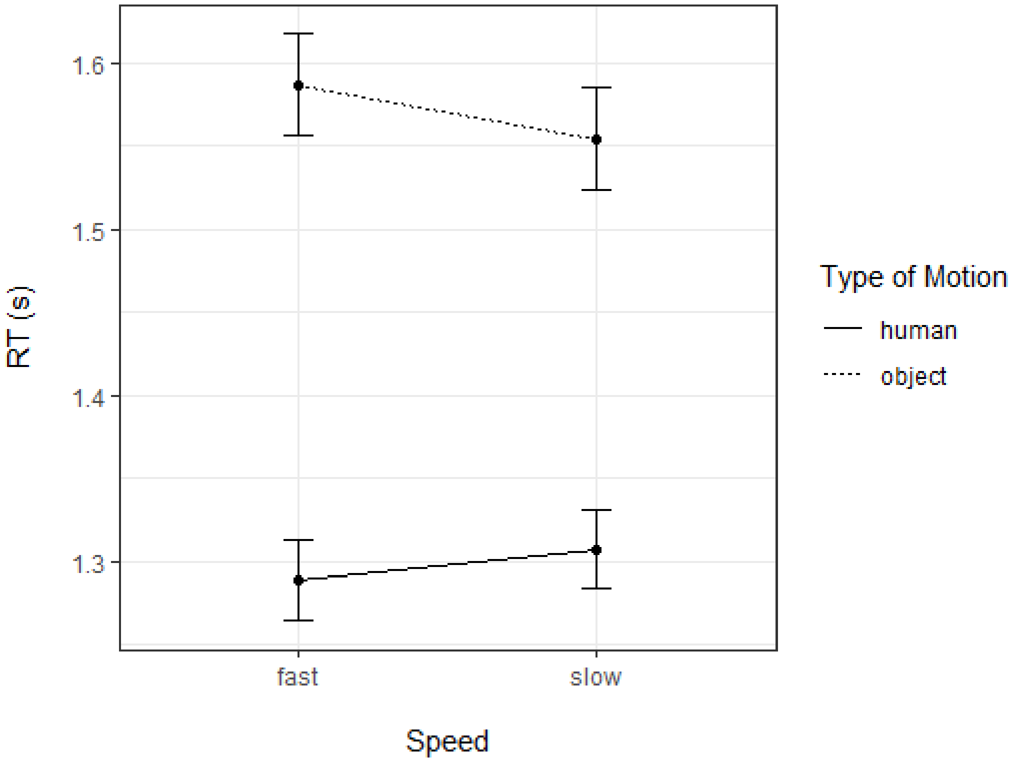
	fast	*slow*	Raw differences in means [95% CI]	Cohen’s *d* [95% CI]
Overall	1437 (460)	1426 (453)	-6.37 [-27.62, 14.88]	-0.10 [-0.41, 0.22]
human motion	1290 (391)	1304 (380)	19.27 [-0.65, 39.20]	0.31 [-0.01, 0.63]
object motion	1590 (475)	1553 (486)	-33.31 [-68.45, 1.84]	-0.30 [-0.63, 0.02]

Note. The first two columns represent the raw means (and *SD*s) of RTs (in ms) for the two levels of Speed. The raw differences in means (in ms) and their confidence intervals are obtained by aggregating the data across participants. Adapted only in its format from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 9. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

The random effects structure with the lowest AIC value included random intercepts for Subjects and Items as well as random slopes for the factor Speed for Items, which yielded following full model: RT ~ Speed * Type of Motion + Length + Frequency + (1 | Subjects) + (Speed | Items).

χ^2 difference tests revealed that like in Experiment 1 the factors Length ($\chi^2(1)$ = 45.88, $p < .001$) and Type of Motion ($\chi^2(1)$ = 16.58, $p < .001$) significantly increased model fit. Again, neither Frequency ($\chi^2(1)$ = 1.97, $p = .161$) nor the interaction of Speed and Type of Motion significantly increased model fit ($\chi^2(1)$ = 0.00, $p = .981$). Yet, different to Experiment 2, there was no significant main effect of the factor Speed ($\chi^2(1)$ = 3.30, $p = .069$). The best-fit model based on AIC did not contain the interaction of Speed and Type of Motion but included all other fixed factors: RT ~ Speed + Type of Motion + Length + Frequency + (1 | Subjects) + (Speed | Items). The best-fit model suggests an intercept of 1386.24 ms. The estimated coefficient for the factor Speed (with *fast* as reference category) of β = -23.88 was not significant (95% CI [-49.81, 1.93], *SE* = 13.20, *t*(93.02) = -1.81, *p* = .074). Note, that the estimated coefficient for Speed was negative. This means that slow-speed sentences were estimated to be reacted to faster than fast-speed sentences. Thus, the speed effect of Experiment 1 was clearly absent in Experiment 2. Moreover, the *p*-value obtained via the Satterthwaite's approximation for degrees of freedom for this model reflects the absolute value of *t*, which does not correspond to the *p*-value associated with the one-sided *t*-test for the factor Speed of our hypothesis (i.e., larger RTs for slow-speed compared to fast-speed sentences). Thus, the actual *p*-value for the estimate of the factor Speed in the best-fit model would be *p* = .963.

Figure 3.2. ***Mean RTs in Experiment 2 by Type of Motion and Speed.***



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). Reprinted from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 9. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

The estimated coefficient for the factor Type of Motion (with *human* as reference category) of $\beta = 126.46$ was significant (95% CI [66.95, 186.84], $SE = 30.33$, $t(100.22) = 4.17$, $p < .001$). Thus, like in Experiment 1, object motion sentences were reacted to more slowly than human motion sentences. The estimated coefficient for the factor Length was $\beta = 20.55$ and significant (95% CI [15.36, 25.63], $SE = 2.57$, $t(119.16) = 8.01$, $p < .001$), while the estimated coefficient for the factor Frequency of $\beta = 5.95$ was not significant (95% CI [1.95, 13.87], $SE = 4.00$, $t(144.22) = 1.49$, $p = .139$).

Table 3.5. ***Mean accuracy (and* SD*) in Experiment 2 for Speed by Type of Motion.***

	fast	*slow*
Overall	96.09 (3.34)	95.94 (4.69)
human motion	98.23 (3.11)	97.92 (4.90)
object motion	93.96 (5.42)	93.96 (6.11)

Note. Adapted only in its format from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 9. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

Accuracy

Analogous to Experiment 1, we assessed whether the observed RT pattern was also reflected in accuracy rates. Thus, we calculated the same 2 (Speed) by 2 (Type of Motion) repeated measures ANOVA for all fast-speed and slow-speed sentences (before outlier removal) on accuracy. There was a significant main effect of Type of Motion [$F(1, 39) = 35.59$, $p < .001$, $\eta_p^2 = .48$], which is due to the fact that like in Experiment 1 human motion sentences had a higher accuracy than object motion sentences (see Table 3.5). Yet, importantly, neither Speed [$F(1, 39) = 0.05$, $p = .826$, $\eta_p^2 < .01$] nor the interaction of Speed and Type of Motion was significant [$F(1, 39) = 0.07$, $p = .800$, $\eta_p^2 < .01$].

Cross-experiment analysis

To assess whether there was a difference between the effect of Speed in Experiment 1 and Experiment 2, we conducted a cross-experiment analysis on RT, in which we included the data of both experiments after outlier removal and analysed both with a LMEM. The full model included Speed (*fast* vs. *slow*, with *fast* as reference category), Type of Motion (*human* vs. *object,* with *human* as reference category), Experiment (*Experiment 1* vs. *Experiment 2,* with *Experiment 1* as reference category), Length (i.e., number of characters; encoded numerically and mean centred), and Frequency (encoded numerically and mean centred) as fixed factors to predict RTs. Moreover, a three-way interaction of Speed, Type of Motion, and Experiment was included in the model. In accordance with our assumption that the speed effect was absent in Experiment 2, while it was present in Experiment 1, we expected an interaction of the factors Speed and Experiment.

The random effects structure was determined analogous to the analyses in Experiments 1 and 2. The random effects structure with the lowest AIC value included random intercepts for Subjects and Items as well as random slopes for the factor Speed for Items, which yielded following full model: RT ~ Speed * Type of Motion * Experiment + Length + Frequency + (1 | Subjects) + (Speed | Items).

χ^2 difference tests revealed that like in Experiments 1 and 2 the factor Length ($\chi^2(1) = 38.71$, $p < .001$) significantly increased model fit, while this was not the case for Frequency ($\chi^2(1) = 0.65$, $p = .421$). Moreover, the three-way interaction of Speed, Type of Motion, and Experiment did not significantly increase model fit ($\chi^2(1) = 0.47$, $p = .494$); neither did the interaction of Speed and Type of Motion ($\chi^2(1) = 0.01$, $p = .938$), nor the interaction of Type of Motion and Experiment ($\chi^2(1) = 0.63$, $p = .427$). Yet, as expected, the interaction of Speed and Experiment significantly increased model fit ($\chi^2(1) = 14.20$, $p < .001$). There was no significant main effect of Speed ($\chi^2(1) = 0.61$, $p = .434$), and no significant main effect of Experiment ($\chi^2(1) = 3.2$, $p = .074$), but Type of Motion ($\chi^2(1) = 24.31$, $p < .001$) had a significant main effect.

The best-fit model based on AIC contained the interaction of Speed and Experiment but did not include any of the other interactions nor Frequency as fixed factor: RT ~ Speed * Experiment + Type of Motion + Length + (1 | Subjects) + (Speed | Items). The best-fit model suggests an intercept of 1438.23 ms. The estimated coefficient for the factor Speed (with reference category *fast*) in the reference category *Experiment 1* was *β* = 40.22 (95% CI [12.29, 68.04], *SE* = 14.22, *t*(219.58) = 2.83, *p* = .005), which is similar to the estimated coefficient for the factor Speed in the analysis of Experiment 1. The factor Experiment (with reference category *Experiment 1*) had an estimated coefficient of *β* = -63.46 (95% CI [-168.49, 41.54], *SE* = 53.58, *t*(81.69) = -1.18, *p* = .240), which implies that compared to Experiment 1, RTs were numerically but not significantly lower in Experiment 2. The factor Type of Motion (with reference category *human*) had an estimated coefficient of *β* = 149.95 (95% CI [91.80, 208.76], *SE* = 29.50, *t*(103.33) = 5.08, *p* < .001). This reflects that in both experiments RTs for object motion sentences were higher compared to RTs for human motion sentences due to their difference in complexity (see Results Section of Experiment 1 for details). The estimated coefficient for the factor Length was *β* = 18.27 (95% CI [13.21, 23.25], *SE* = 2.48, *t*(129.35) = 7.37, *p* < .001), which is comparable to the estimated coefficients of Length in Experiment 1 (*β* = 16.99) and Experiment 2 (*β* = 20.55). Finally and theoretically most crucial, the estimated coefficient for the interaction of Speed and Experiment of *β* = -62.45 was significant (95% CI [-94.92, -29.98], *SE* = 16.56, *t*(6958.82) = -3.77, *p* < .001). This yields an estimate for the factor Speed in Experiment 2 of -22.2 ms, i.e., fast-speed sentences are estimated to be reacted to 22.2 ms more slowly compared to slow-speed sentences in Experiment 2.

Post-hoc pairwise contrasts with the *emmeans* function and package in R using Kenward-Roger approximated degrees of freedom and Tukey-adjusted *p*-values revealed that the estimated difference between *fast* and *slow* was significant in Experiment 1 (estimated difference of 40.2 ms, *SE* = 14.2, *t*(228.1) = 2.83, *p* = .026) but not in Experiment 2 (estimated difference of -22.2 ms, *SE* = 14.2, *t*(226.1) = -1.57, *p* = .400).

Discussion

Experiment 2 did not replicate the speed effect that was observed in Experiment 1. When participants no longer had to detect motion but evaluated the sentences' sensicality, the motion verbs' associated speed no longer produced an RT effect with slow-speed sentences being reacted to more slowly than fast-speed sentences. This was confirmed by a cross-experiment analysis.

Like in Experiment 1, there was no interaction of Speed and Type of Motion. This was corroborated by the analysis on accuracy, which revealed that neither Speed nor the interaction of Speed and Type of Motion significantly modulated accuracy. Crucially, also the cross-experiment analysis supported the assumption that there is no interaction of Speed and Type of Motion in either of the experiments. We do acknowledge that further data is needed to finally confirm this conclusion. Yet, the pattern observed in both RT and accuracy across the two experiments allows for the preliminary conclusion that within this paradigm speed information associated with human motion sentences and speed information associated with object motion sentences is processed equivalently.

The main effect of Type of Motion (i.e., longer RTs for object motion sentences compared to human motion sentences) was consistent across experiments. As mentioned in the Results Section of Experiment 1, object motion sentences had definite descriptions (e.g., ‘the tractor’) compared to proper names (e.g., ‘Rebecca’) as subjects. The definite determiner of the determiner phrase triggers the establishing of a discourse relation which has previously been shown to be reflected in more pronounced negative deflections (i.e., N400) on event-related brain potentials compared to proper names, which might carry a feature of inherent definiteness and thus show a facilitated discourse integration (Burkhardt, 2019; see also Gordon et al., 2001). This might explain the resulting main effect of Type of Motion. Proper names were chosen as agents for human motion sentences to reduce the potential interference of the meaning of the nominal phrase with the rest of the sentence. This brings about a structural difference between these two types of sentences that is not desirable with respect to the stimulus material’s design. However, since the main effect of Type of Motion was consistent across experiments and was also reflected in accuracy rates it seems reasonable to conclude that it is in fact due to the difference in semantic richness, since it is not modulated by the task.

As the cross-experiment analysis reveals, only the speed effect is modulated by the task, which implies that modal representations concerning the motion verbs’ associated speed information most likely are non-automatic. This finding speaks against the assumption that modal speed information is part of the lexical-semantic representation of manner of motion verbs. Please note that our paradigm does not allow us to draw conclusions about modal representations concerning other non-speed information such as motion direction. We cannot exclude that other such information might be represented modally in Experiment 2, since the present paradigm would not be able to detect this.

General Discussion

The results of the present study contribute to the findings concerning the potential engagement of modal representations of speed information in the processing of sentences with manner of motion verbs. In Experiment 1, in which participants were asked to detect motion in fast-speed and slow-speed sentences in contrast to static sentences, an effect of speed was observed on RTs and accuracy such that fast-speed sentences were reacted to faster and more accurately than slow-speed sentences. This pattern however, was not observable in Experiment 2, in which participants were asked to judge the sentences' sensicality. These results suggest that the modal representations of a motion verbs' speed information that are assumed to underlie the speed effect in Experiment 1 are not activated automatically upon the processing of sentences with manner of motion verbs. This finding is specifically interesting with respect to previously observed effects of speed on eye movements (Lindsay et al., 2013; Speed & Vigliocco, 2014) and activations of perceptual and motor brain areas (van Dam et al., 2017), for which it was not clear whether they reflected automatic or higher level, e.g., conscious or top-down modal activations. In the three mentioned studies, the concept of motion was not made specifically salient in the tasks, which suggests an automatic activation of modal representations. Yet, in the eye-tracking studies, participants saw a matching visual scene that might have encouraged the activation of modal representations, while in the fMRI study, participants were given relatively long time intervals (i.e., 2.25 s) to read the sentences which might have encouraged mental imagery processes. In conjunction with the results of the present study, the reported effects of speed seem to reflect a later cognitive process instead of an automatic lexical-semantic retrieval of grounded information. This is in accordance with a decompositional semantics account, which assumes that it is not necessary to activate the specific kinematic pattern of a manner of motion verb to understand its meaning.

Yet, the question of what can be derived from the RT pattern observed in Experiment 1 needs to be assessed with caution. Wender and Weber (1982), whose paradigm was adopted, interpret their results as suggesting that a dynamic mental image is built up from the event described in sentences with motion verbs. Similarly, Speed and Vigliocco (2014) argue that the simulation's duration varies as a function of the speed conditions analogously to the observing of the event in the world. More specifically, they suggest that "the simulation of the meaning of the sentence is *slower* for sentences describing slow motion than sentences describing fast motion" (Speed & Vigliocco, 2014, p. 378, see also p. 369). Thus, it is conceivable that the unfolding of modal representations takes longer for slow-speed sentences compared to fast-speed sentences (Lindsay et al., 2013; Speed & Vigliocco, 2014).

However, some concerns arise with a simulation-based explanation for the speed effect. Firstly, an analogue relationship between the described event and its simulation is not always feasible. For most sentences, consider for example the sentence 'Michelle is darting to the hospital', the event takes too long for a simulation to take the analogue amount of time. With respect to these cases, Lindsay et al. (2013) argue that simulation would need scaling and temporal compression such that a compressed simulation might consist of a starting state, an intermediate state, an end state, and the transitions between them. With respect to the example sentence this would imply that there is a start state, at which Michelle starts to move at some unknown place X, an intermediate state, at which the type of motion becomes relevant (e.g., via a dynamic visual image of Michelle darting across some pavement), and an end state, at which she arrives at the hospital. For both fast-speed and slow-speed sentences in our stimulus material, start and end state would be identical, while the intermediate state would consist of the kinematic pattern of the respective manner of motion verb. Simulating the foot being lifted from the ground and then placed back down according to the verb's denoted kinematic pattern would take more time for slow than for fast movement. Thus, this notion of simulation would still imply a proportional relationship of the simulation's duration to the event's duration and might accordingly best be framed as quasi-analogue. Yet, there is a remark to make concerning the term simulation itself. It is usually used to describe automatic sensomotoric activation as part of a word’s semantic representation (Meteyard et al., 2012), while the results of the present study speak against automatic sensomotoric activation. Consequently, we suggest that speaking of an activation of modal representations is more adequate than using the term simulation.

Different to the authors of the recent studies (Lindsay et al., 2013; Speed & Vigliocco, 2014; van Dam et al., 2017), Wender and Weber (1982) introduce the term mental image which can be understood in the context of Johnson-Laird’s (1980) conception of mental models. This is because different to the recent studies Wender and Weber (1982) explicitly instructed their participants to form a mental image of the sentences’ content. The results of the present study indicate that the effect of speed on RT is not dependent on this explicit instruction to engage with mental imagery. Yet, as noted in the discussion of Experiment 1, this does not rule out that participants might still have consciously imagined the sentences’ content (see Iachini, 2011; Zwaan & Pecher, 2012 for a differentiation of simulation and mental imagery processes). Nonetheless, since the present study investigates the activation of modal representations during language processing, the notion of mental models does not seem fruitful, either.

A third account that is worth mentioning in this context is the assumption of the build-up of a situation model, i.e., the “construction of a mental representation of the state of affairs denoted by the text” (Zwaan, 2016, p. 1028). Zwaan (2016) suggests that situation models integrate abstract mental representations, as well as the retrieval of visual and motor knowledge as part of comprehension. For instance, in a ‘cataphorical’ use of abstract concepts, symbolic representations are used as a placeholder in working memory until they are “subsequently used by the comprehension system to integrate subsequent information so that a situated simulation can be formed” (Zwaan, 2016, p. 1031). Even though Zwaan (2016) thus reintegrates the term ‘simulation’ with its corresponding framework (see also Zwaan, 2008), the spelling out of a situation model’s mechanisms implies that it is to be differentiated from an automatic lexical-semantic simulation (see

Bedny & Caramazza, 2011, p. 86 for a similar interpretation). Situation models thus seem to be a feasible way of framing the observed speed effect, since situation models are not considered to point to a strong version of embodiment (Meteyard et al., 2012).

Yet, there are alternative explanations for the speed effect observed in the present study. For instance, longer events seem to have more diverse associations and a higher semantic and contextual complexity than shorter events as indicated by the results of a free association study and a corpus study (Coll-Florit & Gennari, 2011). This is taken as an explanation by Coll-Florit and Gennari (2011) for their observation of longer processing times for durative states compared to non-durative events. The authors argue that the diverse and distributed nature of the associated knowledge of durative states compared to non-durative events could increase processing costs. However, their stimulus material consisted of states (*to owe 50 euros*) in contrast to events (*to lose 50 euros*), which does not apply to our stimulus material. Duration in the stimulus material of the present study was generated by means of the idiosyncratic component of the lexical entries as outlined in the introduction. The verbs used in the present study all denote events, or rather dynamic eventualities, but not states (Maienborn, 2019). Thus, both fast-speed and slow-speed sentences are comparable to the class of non-durative events but none of them to durative states as in the study by Coll-Florit and Gennari (2011). Even though further norming would be needed to finally exclude this explanation, we consider it unlikely that slow-speed sentences should elicit more diverse associated knowledge than fast-speed sentences due to the semantic similarity between the verbs used in the present study.

A further alternative explanation for the speed effect observed in Experiment 1 is that fast-speed sentences made motion more salient than slow-speed sentences. More specifically, due to the task to detect motion, i.e., the situation-based salience of motion, ‘moving’ stimuli are expected by the comprehender in a top-down manner (Zarcone et al., 2016). Thus, attention is oriented towards and processing facilitated for bottom-up information that has a high salience of motion. This would result in motion being easier to detect or more accessible in fast-speed sentences compared to slow-speed sentences. That participants were not only faster but also more accurate in detecting motion in fast-speed sentences compared to slow-speed sentences in Experiment 1 corroborates the assumption that differences in saliency might be responsible for the observed behavioural pattern.

Importantly though, even if motion detection is faster for fast-speed sentences due to a higher salience of motion in these sentences, this account speaks for an activation of knowledge that is situated beyond the lexical entry of the manner of motion verbs. As illustrated in the introduction, motion is coded identically in the lexical entries of both fast-speed and slow-speed verbs via the semantic component MOVE. Thus, the concept of motion in the propositional representation of all manner of motion verbs is equally well accessible. Using the propositional representation to decide whether a sentence describes motion should consequently take the same amount of time for all manner of motion verbs irrespective of how high the associated velocity of the movement is. Only when kinematic information retrieved from conceptual knowledge via the idiosyncratic meaning component is taken as basis for motion detection, a difference in processing times comes into play.

Following the saliency account, one thus has to assume that the comprehender draws on modal representations to detect motion. Indeed, it is conceivable that the higher visual relatedness between slow motion and static scenes compared to fast motion and static scenes implies a slower motion detection in slow-speed sentences compared to fast-speed sentences (Ratcliff, 1978). Thus, a coarse-grained spatial mental representation that gets saturated with the respective motion parameters (e.g., *A verbs to B*) would serve as basis for detecting motion. More specifically, the comprehender would accumulate evidence for motion across a number of time units from this representation. Due to the larger amount of change per time unit for fast-speed sentences compared to slow-speed sentences, the accumulation of evidence for motion would be faster for fast-speed sentences (see Wender & Weber, 1982 for a similar explanation). Consequently, we do not consider the salience of motion account to be in contradiction with the assumption of the activation of modal representations (see Claus, 2011 for a similar account).

We consider it conceivable that the above mentioned coarse-grained spatial mental representation might be a simplified version of a situation model with a quasi-analogue relation to the described event due to its temporal compression. Yet, we do acknowledge that the exact format of these modal representations remains unknown. Whether the build-up of a simplified situation model and the accumulation of evidence based on the respective mental representations have to be considered two distinct processes or whether they represent two sides of the same coin we cannot deduce. Paradigms that are more decisive are needed to differentiate between processes connected to situation model build-up and processes related to evidence accumulation based on modal representations. Yet, against the background of a decompositional semantics account, it seems feasible that the decision process in Experiment 1 draws on the activation of modal representations.

By differentiating between human and object motion, the present study furthermore assessed the cognitive basis of the presumed modal representations. Speed and Vigliocco (2014) suggest that the speed effect they observed could either be explained via the activation of a visual representation or by means of a covert action imitation (Speed & Vigliocco, 2014, p. 380). In accordance with this, a review article on the mental representation of action verbs suggests that sensorimotor areas that are mainly targeted within the context of action verbs are the visual motion and motor control systems (Bedny & Caramazza, 2011). This is because action verbs' differentiation from other word classes is based on a preponderance of motor and visual-motion features. In the present study, an equal amount of human and object motion sentences was engaged to assess whether the speed effect was based on visual or motor modal processes. Since motor representation is only plausible for human but not for object motion sentences, we hypothesised that the speed effect should be absent for object motion sentences but present for human motion sentences in case of motor representations but present in both types of motion sentences in case of visual representations. The results of Experiment 1 suggest that speed has an effect on RTs (and accuracy) for both types of motion. It thus seems to be based on visual rather than motor representations. This is an interesting finding, since “the motor system […] has received a great deal of attention” (Bedny & Caramazza, 2011, p. 86) with respect to action verb comprehension. Our finding is in line with Miller et al. (2018), whose results speak against modulations of motor event-related potentials as a function of effector-specific information denoted by action verbs.

In accordance with the assumption that neural activity is never epiphenomenal and should thus always reflect some function (Martin, 2016), the results of the present study suggest that the engagement of modal representations in the processing of fast-speed and slow-speed sentences is related to task efficiency (Ostarek & Huettig, 2019). More precisely, activating modal representations by mapping the verb-associated motion patterns onto a coarse-grained mental spatial trajectory (e.g., *A verbs to B*), might help to come to a decision more quickly compared to solving this task amodally. The absence of the speed effect in Experiment 2 suggests that the activation of modal representations of speed information is not part of automatic lexical-semantic retrieval processes. The assumption that modal representations are neither necessary for language understanding nor part of the lexical-semantic representation is in line with moderate accounts of embodied cognition that do not consider sensomotoric representations a precondition for language understanding. The present study extended a prior paradigm of the field and expanded the findings regarding the presumable involvement of modal representations in the processing of sentences containing motion-verb associated speed information as an instance of one specific research subject within the grounded cognition debate. As stated by Ostarek and Huettig (2019) future embodiment research should not only show that effects are present or absent but should also make clear a priori predictions about the processing stages at which interactions arise based on a fully specified complete theory of semantic cognition, which is still work in progress.

Conclusion

The results of this study suggest that sentences with manner of motion verbs can be processed by engaging modal representations. Importantly, these modal representations are non-automatic and not necessary for language comprehension, as they are not activated when a sentence’s sensicality is judged. However, they are involved when participants are instructed to detect motion in a sentence – presumably to facilitate the decisional process. The observable pattern in RT (i.e., slower responses to slow-speed sentences compared to fast-speed sentences when detecting motion) is observable for both human and object motion sentences, which suggests the activation of visual rather than motor representations. The engaged modal representations seem to reflect higher cognitive processes than the mere semantic retrieval of the involved concepts. This speaks against modal representations being part of the core of language processing and is in line with moderate accounts of embodied cognition.

Chapter 4

Duration = Speed × Distance?[19]

Abstract

The speed effect observed in Experiment 1 of Chapter 3 suggests that modal representations are engaged when participants are asked to detect motion in sentences containing manner of motion verbs. The present experiment is designed to test how fine-grained these modal representations are. More specifically, travelled distance (*short* vs. *long*) is manipulated in addition to the speed of motion (*fast* vs. *slow*) to test whether the engaged modal representations incorporate all elements of the sentence or whether they are tailored to the specific task-demands and only take into account speed information. The results do not provide a clear answer to this question as short-distance sentences are reacted to more slowly than long-distance sentences – a pattern opposite to the prediction. A post-hoc explanation might be that different time-scales of the two levels of distance elicit different processing strategies, which leads to the observed effect.

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[19] This chapter is based on data collected under my supervision in the context of the unpublished Bachelor thesis Nieslony, J. (2020). *Über den Einfluss von Geschwindigkeit und Distanz auf die Verarbeitung von Fortbewegungsbeschreibungen*. BA Thesis, University of Tübingen.

Introduction

The studies reported in Chapter 3 investigated the involvement of modal representations of speed information during language processing. The results suggest that the speed effect, which was initially observed by Wender and Weber (1982), is context-dependent. More specifically, participants in the experiments reported in Chapter 3 responded quicker to fast-speed sentences than to slow-speed sentences when the task was to detect motion but not when the task was to judge the sentences' sensicality (see Chapter 3 for details).

Since the semantic component MOVE as part of the propositional representation of manner of motion verbs should be equally well accessible in fast-speed and slow-speed verbs, we concluded that the observation of a speed effect in Experiment 1 of Chapter 3 speaks for the retrieving of conceptual knowledge that is situated beyond the verbs' lexical entry. A feasible assumption is that a coarse-grained spatial mental representation, which gets saturated with the respective kinematic motion information, serves as the basis for detecting motion. More specifically, a simplified version of a situation model might be built up, which temporally compresses the described events to contain a starting state, an intermediate state with the simulation of one kinematic 'standard cycle', and an end state, that is, the movement's destination. By this means, the mental representations of the described events have a proportional relationship with their implied duration. Simulating one kinematic 'standard cycle' takes longer for slow than for fast ways of walking. For example, the sentence *Michelle is reeling to the hospital* describes an event that takes longer than an event described by *Michelle is darting to the hospital*, because the duration of reeling a certain distance is longer than darting the same distance. This is what we referred to by quasi-analogue relation between the build-up of a situation model and the described event's duration in the General Discussion of Chapter 3.

Yet, additionally to the speed information, the distance expressed in the sentences of Experiment 1 in Chapter 3 might also be considered in the build-up of a corresponding situation model since previous studies in the field of embodied cognition have shown an effect of spatial configurations (e.g., Lachmair et al., 2011) and distance, in particular, on processing latencies. For instance, Matlock (2004) reported shorter decision times for answering whether a target fictive motion sentence (e.g., *The road runs through the valley*) was related to a previously read short story after reading a story containing a short-distance travel (e.g., driving 20 miles) compared to a long-distance travel (e.g., driving 100 miles). Matlock argues that this is explicable by the simulation's duration of motion along the previously described and imagined road. Her finding is in line with two earlier studies investigating effects of distance on scanning times of mental visual images based on the representation of perceptually processed images (Denis & Cocude, 1989; Kosslyn et al., 1978) or based on images established from verbal descriptions of spatial configurations (Denis & Cocude, 1989). For both types of mental images, the authors observed an increase in scanning time with increased physical distance.

The paradigm in Experiment 1 of Chapter 3 does not allow to conclude whether only speed is represented modally or whether all elements of the sentence are taken into account to yield a more complex situation model. The present experiment is designed to disentangle these two options to get a clearer picture of how fine-grained the situation model is that serves as a basis to decide whether motion is expressed in a sentence. For this purpose, the task to detect motion is adopted from Experiment 1 of Chapter 3, while the stimulus material is manipulated with respect to the travelled distance (*short* vs. *long*) in addition to the manipulation of speed of motion (*fast* vs. *slow*). If both, speed and spatial information are taken into account when representing the sentences modally to decide whether they express motion, longer distances should be reacted to more slowly than shorter distances. Moreover, there should be an interaction of Speed and Distance, since a difference in speed would produce a larger difference in duration for long distances compared to short distances. If, however, the spatial representation is rather coarse-grained of the form *A verbs to B*, for which the precise characteristics of *B* are not relevant, such that they are not represented modally, no differences in RT are expected with respect to the factor Distance.

Method

Participants

200 native speakers of German (82 female, 115 male, and three, who preferred not to say) were recruited using Prolific (www.prolific.co).[20] They received reimbursement based on an hourly wage of 9 Euro (specifically, they were payed £2.70 for an experimental session of 20 minutes). The data of six participants had to be discarded due to a low accuracy rate (i.e., below or equal to 75%). Six further participants' data replaced their data. The mean age was 29.85 years (*SD* = 10.42). 160 reported being right-handed, 22 reported being left-handed, and 17 that they were ambidextrous. They were naïve concerning the purpose of this investigation.

Apparatus

The experiment was programmed in PsychoPy v2020.1.3 and was run online via Pavlovia.org. The sentences were presented in the centre of the computer screen in black against a grey background (Arial; scaling factor 0.05). The keys 'x' and 'm' were used as response buttons.

Stimuli

One experimental session consisted of 144 German sentences. One-third of the sentences denoted motion (*Clara humpelt zum Krankenhaus* – 'Clara is limping to the hospital'), another one-third of the sentences denoted static scenes (*Hannes sitzt am Ufer* – 'Hannes is sitting at the shore'), and the rest of the sentences were nonsense sentences (*Daniel marschiert den Schnee holzig* – 'Daniel is marching the snow woody').

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[20] The data was anonymised subsequent to collection.

Motion Sentences.

The motion sentences were constructed analogous to the human motion sentences in Chapter 3, i.e., all sentences were written in the present tense and denoted a motion event with a human agent referred to by a proper name. Each sentence had a prepositional phrase (PP) that served as the verb’s directional adverbial. Additional to the manipulation of Speed, Distance was manipulated via the PP. Motion sentences were matched as quadruples, such that each quadruple contained four pairs of motion sentences that either differed for the associated speed of the motion verb (a & c: *slow* vs. b & d: *fast*) or the distance travelled (a & b: *short* vs. c & d: *long*).

(4.1.a) Clara humpelt zum Wandschrank.
Clara is limping to the closet.
(4.1.b) Clara prescht zum Wandschrank.
Clara is dashing to the closet.
(4.1.c) Clara humpelt zum Krankenhaus.
Clara is limping to the hospital.
(4.1.d) Clara prescht zum Krankenhaus.
Clara is dashing to the hospital.

Verb Pairing. 24 manner of motion verbs were selected that had been rated concerning speed (see Table 4.1 for the verb pairing; see Chapter 3, Experiment 1, Section ‘Stimuli’, and Appendix D.1, for details on the speed ratings) and were combined as pairs. The mean rated speed of fast-speed verbs was $M = 6.02$ ($SD = 0.59$), while the mean rated speed of slow-speed verbs was $M = 2.21$ ($SD = 0.49$). An unpaired one-tailed t-test was calculated because the ratings of the manner of motion verbs were extracted from different samples (see Note of Table 4.1 for details). It revealed that the difference in means was significant ($t(21.23) = 17.14$, $p < .001$). Thus, the perceived (i.e., rated) speed of the selected slow-speed compared to the selected fast-speed verbs was significantly lower.

The frequency of the motion verbs was determined using the Leipzig Corpora Collection (2018)[21]. While the fast-speed verbs had a mean frequency class of *M* = 14.75 (*SD* = 1.91), the slow-speed verbs had a mean frequency class of *M* = 16.42 (*SD* = 1.88). An independent two-tailed *t*-test indicated that the difference was significant (*t*(21.99) = 2.15, *p* = .043). Consequently, Verb Frequency was included as predictor of RTs in the linear mixed-effects model of the main analysis to control for this systematic difference between the fast-speed and slow-speed verbs.

Table 4.1. ***Pairing of manner of motion verbs.***

Slow-speed verbs				Fast-speed verbs			
Verb	Rating	Letters	Frequency	Verb	Rating	Letters	Frequency
stiefeln*	2.85	8	19	spurten*	5.66	7	18
latschen	2.63	7	18	huschen	4.90	6	16
humpeln	1.75	7	18	preschen	6.08	7	16
bummeln	2.15	7	15	rennen	6.05	5	12
torkeln	1.88	7	18	sprinten	6.80	8	16
wanken	1.95	5	16	rasen	6.80	4	14
hinken	1.73	5	15	stürmen	6.58	6	13
schreiten	3.28	9	13	flitzen	6.28	6	15
taumeln*	1.86	7	16	eilen	5.35	4	14
schlendern	2.40	10	14	jagen	6.23	4	12
trotten	2.23	7	17	sausen	6.15	5	14
schlurfen	1.85	8	18	hasten	5.40	6	17
M	2.21	7.25	16.42		6.02	5.67	14.75
SD	0.49	1.42	1.88		0.59	1.30	1.91

Note. Rating refers to the mean rated speed on a 7 point Likert scale (1 representing very slow, 7 representing very fast). Ratings of verbs marked with asterisk were collected in the pre-study as described in Appendix D.1; all other ratings are taken from the pre-study as described in Chapter 3. *Letters* stands for the number of characters of the verbs in 3rd person singular present tense. *Frequency* refers to the verbs' frequency class obtained from Leipzig Corpora Collection (2018).

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[21] Leipzig Corpora Collection (2018). German newspaper corpus based on material crawled in 2018. Leipzig Corpora Collection. Dataset. https://corpora.uni-leipzig.de/de?corpusId=deu_newscrawl-public_2018.

The motion verbs' length was compared between its two levels via the number of letters of third person singular in the German present tense. While the fast-speed verbs had an average number of letters of *M* = 5.67 (*SD* = 1.30), the slow-speed verbs were on average longer with *M* = 7.25 (*SD* = 1.42). An independent two-tailed *t*-test revealed that this difference again was significant (*t*(21.83) = 2.84, *p* = .009). Consequently, the number of characters was included as a predictor of RTs in the analysis.

PP Pairing. Ninety-six PPs that denoted either a short or a long path (in the following referred to as short-PP and long-PP, respectively) were selected and matched as pairs based on a pre-study, in which participants were asked to rate the denoted distance (see Appendix C.1 for details).

The selected short-PPs were rated to have a distance of *M* = 1.90 (*SD* = 0.48), while the long-PPs were rated to have a distance of *M* = 4.98 (*SD* = 0.49). A paired one-tailed *t*-test revealed that this difference was significant (*t*(47) = 28.19, *p* < .001).

The nouns of the short-PPs had a mean frequency class[22] of *M* = 13.17 (*SD* = 2.75); the nouns of the long-PPs had a mean frequency class of *M* = 12.40 (*SD* = 3.02). This difference was not significant (*t*(93.17) = 1.30, *p* = .197). The average number of characters of short-PPs was *M* = 12.58 (*SD* = 2.86), while the mean number of characters of long-PPs was *M* = 12.85 (*SD* = 3.00). This difference in means again was not significant (*t*(93.77) = 0.45, *p* = .653).

Verb × PP Pairing. The selected 12 verb pairs were each factorially combined with four selected PP pairings to yield 192 sentences in total. In the following, the combination of one verb pair with one PP pair is referred to as Item. The stimulus material thus consisted of 48 Items with two levels of Speed and two levels of Distance each.

The semantic similarity was determined for each verb-noun-combination using the cosine-function of the *LSAfun* package in R (Version 4.0.2, 2020) on the de_wiki German cbow space with 400 dimensions (Günther et al., 2014) to control for semantic priming effects of the nouns of the PPs by the motion verbs. The fast-speed verbs in

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[22] Leipzig Corpora Collection (2018). German mixed corpus based on material from 2018. Leipzig Corpora Collection. Dataset. https://corpora.uni-leipzig.de/de?corpusId=deu_typical-mixed_2018.

combination with the 48 short-PPs had a mean cosine value of $M = 0.153$ ($SD = 0.090$), the slow-speed verbs in combination with the short-PPs had a mean cosine value of $M = 0.165$ ($SD = 0.101$). The fast-speed verbs in combination with the long-PPs had a mean cosine value of $M = 0.107$ ($SD = 0.077$) and the slow-speed verbs in combination with the long-PPs had a mean cosine value of $M = 0.132$ ($SD = 0.105$). A two (Speed) × two (Distance) ANOVA was calculated on the cosine values. While there was no significant interaction of Speed and Distance ($F(1, 188) = 0.22$, $p = .639$) and no significant main effect of Speed ($F(1, 188) = 1.94$, $p = .165$), there was a significant main effect of Distance ($F(1, 188) = 8.52$, $p = .004$), due to a higher cosine value of short-PPs ($M = 0.159$, $SD = 0.095$) compared to long-PPs ($M = 0.119$, $SD = 0.092$). Consequently, semantic similarity indicated by the cosine value of the verb-noun-combinations was incorporated as a predictor of RTs in the main analysis.

Stimulus Lists. To avoid repetition for participants, but to enable a within-subjects design for Items, the motion sentences were split into four stimulus lists such that each participant saw 12 entire Items, i.e., 48 motion sentences with all levels of the interaction of Speed and Distance. The Items were split so that each stimulus list consisted of all verb pairs with two repetitions of each single motion verb and two repetitions of each single noun of the PPs. The stimulus lists did not significantly differ with respect to the number of characters ($F(3, 188) = 0.21$, $p = .891$), to the distance ratings ($F(3, 188) = 0.36$, $p = .937$), to the frequency of the nouns ($F(3, 188) = 0.64$, $p = .592$), and to the cosine values ($F(3, 188) = 0.47$, $p = .705$). Since each verb pair was represented with equal distribution in each stimulus list, frequency of the verbs and speed ratings were identical in each stimulus list.

Static and Nonsense Sentences

Static (4.2) and nonsense (4.3) sentences were adopted with minor changes from the stimulus material of the experiments in Chapter 3 (see Appendix C.2 for the entire stimulus material).

(4.2) Charlotte schläft auf der Wiese.
Charlotte is sleeping on the lawn.

(4.3) Olga stampft durch den Artikel.
Olga is trudging through the article.

Additional, twelve sentences were created as training sentences that consisted of nonsense, static and motion sentences.

Procedure

Before starting the experiment, participants were given written instructions within the PsychoPy-script. Analogous to Experiment 1 in Chapter 3, a Go/NoGo-design was implemented. Specifically, participants were asked to carefully read the sentences and to decide as quickly as possible whether the sentence expressed motion or not, however, only if the sentence was sensical. If the sentence was nonsensical, they had to refrain from responding. In cases in which the sentence was sensible and a response was demanded participants responded with the index finger of the right hand (key m), when the sentence expressed motion, and with the index finger of the left hand (key x), when this was not the case. Trial design was identical to Experiment 1 in Chapter 3.

Participants were randomly assigned to one of the four stimulus lists; yet, stimulus list was counterbalanced with respect to participants (i.e., 50 per stimulus list). The presentation order of sentences was randomised for each participant. RT was measured from stimulus onset up to the keypress. The factor Speed with the levels *fast* and *slow* and the factor Distance with the levels *short* and *long* were within-subjects factors, whereas Stimulus List was manipulated between-subjects.

Twelve training sentences were run as a training block prior to the experiment.

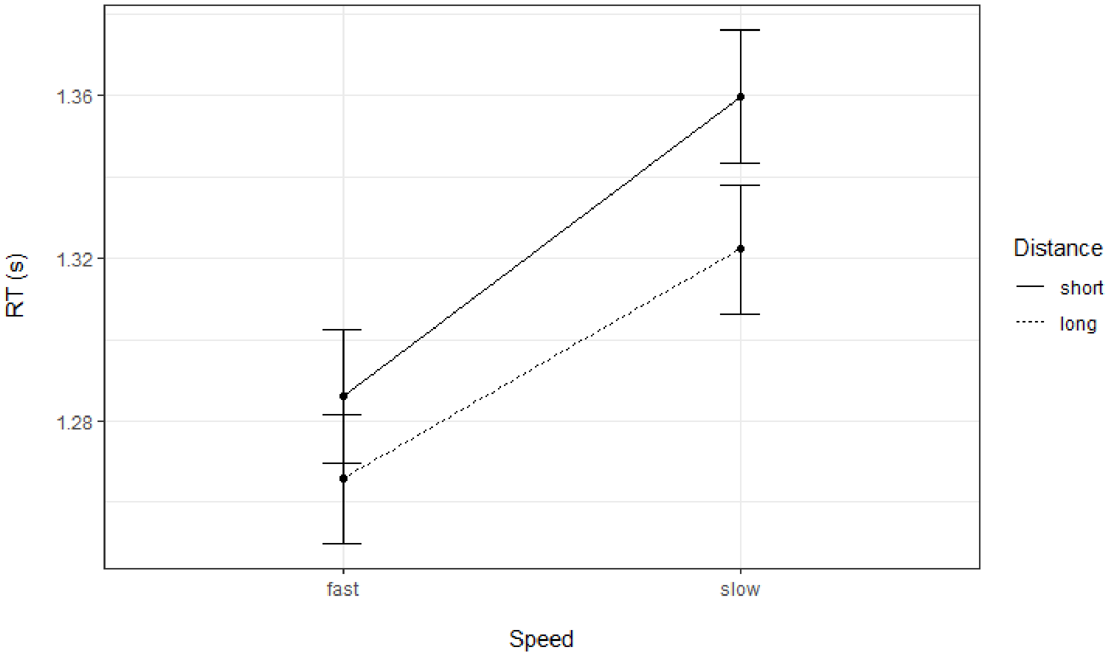
Results

The overall accuracy rate for all types of sentences was 96.55% (*SD* = 3.39). Participants correctly refrained from responding in 94.69% (*SD* = 6.55) of the nonsense trials. Sensical sentences were correctly recognised as static or motion sentences in 97.48% (*SD* = 2.65) of sensical trials. The accuracy of *fast* was 98.19% (*SD* = 3.33), the accuracy of *slow* was

98.27% (*SD* = 3.79), the accuracy of *short* was 98.15% (*SD* = 3.73), and the accuracy of *long* was 98.31% (*SD* = 3.29).

Only trials with correct responses to the motion sentences were submitted to data analysis of RTs. Within this data pool, outliers were identified following a two-step-procedure analogous to the experiments in Chapter 3: Firstly, trials with RTs less than 100 ms (0.00%) were excluded. Secondly, mean RTs for each Subject for each level of the interaction of Speed and Distance were determined. RTs that deviated more than 2.5 standard deviations from the respective cell's mean, were considered outliers and excluded (1.28% of correct motion sentences). The mean RT of *fast* was 1277 ms (*SD* = 417), while the mean RT of *slow* was 1340 ms (*SD* = 437). The mean RT of *short* was 1322 ms (*SD* = 430) and the mean RT of *long* was 1294 ms (*SD* = 426) (see Figure 4.1 for mean RTs by Speed and Distance; see Table 4.2 for standardized effect sizes of the differences in means of Speed overall and per level of Distance).

Figure 4.1. ***Mean RTs as a function of*** **Speed** ***and*** **Distance.**



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008).

After outlier removal the data were analysed with a linear mixed-effects model (LMEM) using the *lme4* package (Bates et al., 2015) in R (Version 4.0.4, 2021). The full model included Speed (*fast* vs. *slow*, with *fast* as reference category), Distance (*short* vs. *long*, with *short* as reference category), Length (i.e., number of characters; encoded numerically and mean centred), Verb Frequency (encoded numerically and mean centred), and Semantic Similarity (i.e., the cosine value of the verb-noun-combinations encoded numerically and mean centred) as fixed factors to predict RTs. Moreover, an interaction of Speed and Distance was included in the model. Subjects and Items, both nested in Stimulus List, were considered random effects.

The random effects structure was determined by step-wise reducing the most complex random effects structure, while keeping the fixed effects structure constant (Barr et al., 2013). The random effects structure was selected based on the Akaike information criterion (AIC). The random effects structure with the lowest AIC value that converged, that did not over-fit the data, and that allowed for χ^2 difference tests included random intercepts for Subjects and Items as well as random slopes for the factor Distance for Subjects and random slopes for the factor Speed for Items, which yielded following full model: RT ~ Speed * Distance + Length + Verb Frequency + Semantic Similarity + (Distance | Subjects) + (Speed | Items).

Table 4.2. ***Difference scores for the two levels of* Speed *overall and per level of* Distance *after removal of outliers and incorrect responses.***

	fast	*slow*	Raw differences in means [95% CI]	Cohen's *d* [95% CI]
Overall	1277 (417)	1340 (437)	65.59 [50.07, 81.11]	0.59 [0.44, 0.74]
short	1286 (414)	1358 (443)	74.22 [49.64, 98.80]	0.42 [0.28, 0.57]
long	1267 (420)	1321 (430)	57.14 [37.02, 77.25]	0.40 [0.25, 0.54]

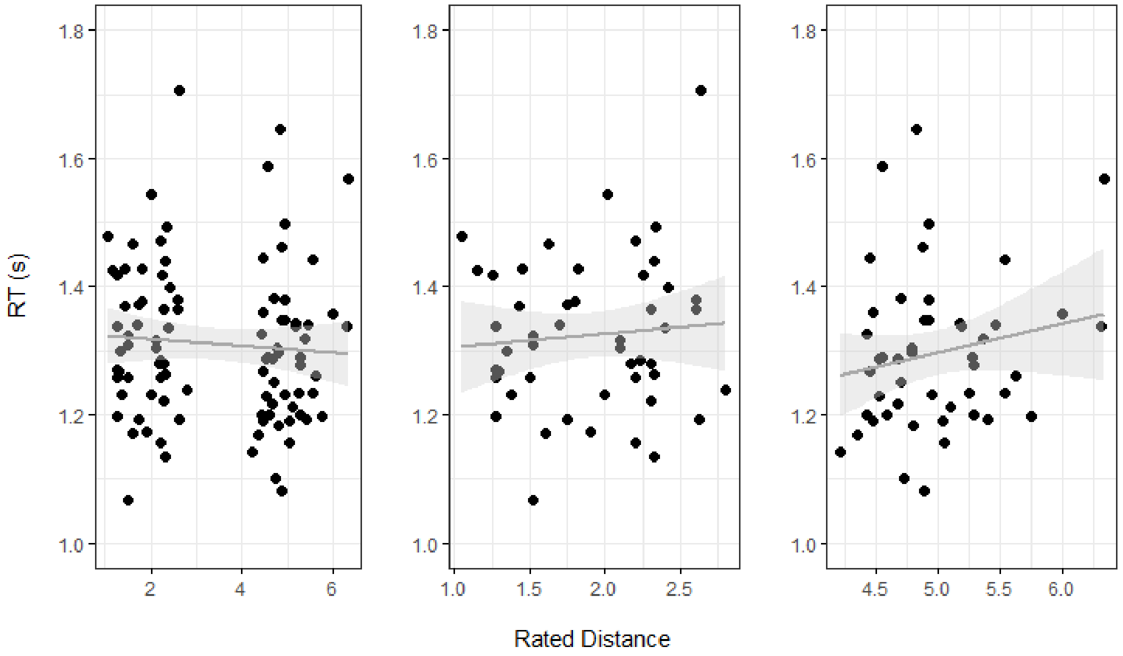
Note. The first two columns represent the raw means (and *SD*s) of RTs (in ms) for the two levels of Speed after outlier removal. The raw differences in means (in ms) and their confidence intervals are obtained by aggregating the data across participants.

Chi-square difference tests on the full model in comparison with a reduced model after dropping the respective fixed factor revealed that the factors Length ($\chi^2(1) = 30.16$, *p* < .001), Semantic Similarity ($\chi^2(1) = 12.12$, *p* < .001), Speed ($\chi^2(1) = 4.60$, *p* = .032), and Distance ($\chi^2(1) = 23.77$, *p* < .001) significantly increased model fit. Neither Verb Frequency ($\chi^2(1) = 0.01$, *p* = .922) nor the interaction of Speed and Distance significantly increased model fit ($\chi^2(1) = 1.10$, *p* = .294). The best-fit model, which was selected based on the AIC value, did not contain Verb Frequency or the interaction of Speed and Distance: RT ~ Speed + Distance + Length + Semantic Similarity + (Distance | Subjects) + (Speed | Items). The best-fit model suggests an intercept of 1311.83 ms. The estimated coefficient for the factor Speed (with reference category *fast*) was *β* = 41.18 (*SE* = 17.67, *t*(50.20) = 2.33, *p* = .024). This implies that RTs for slow-speed sentences were estimated to be 41.18 ms higher than RTs for fast-speed sentences, which is comparable to the estimated coefficient of the factor Speed in Experiment 1 of Chapter 3 with *β* = 41.45. The estimated coefficient for the factor Distance (with reference category *short*) was *β* = -41.60 (*SE* = 8.33, *t*(239.68) = -4.99, *p* < .001). This implies higher RTs for sentences with short-PPs compared to sentences with long-PPs. The estimated coefficient for the factor Length was *β* = 17.79 (*SE* = 2.54, *t*(71.05) = 7.02, *p* < .001), which again is comparable to the estimated coefficient for the factor Length in Experiment 1 of Chapter 3 with *β* = 16.99. Lastly, the estimated coefficient for the factor Semantic Similarity was *β* = -217.48 (*SE* = 58.40, *t*(1934.90) = -3.72, *p* < .001), implying that with an increase in Semantic Similarity, RTs were estimated to decrease. Note that the estimated coefficient is comparatively high because integers are automatically taken as the underlying unit by the model. The estimated coefficient of Semantic Similarity thus represents the change in RTs with a hypothetical step from zero to one of the cosine value.

P-values for the estimated coefficients of the best-fit model were obtained using the Satterthwaite's approximation for degrees of freedom, which is built-in in the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R.

Post-hoc correlations per level of Distance. The estimated negative coefficient for Distance in the LMEM of the main analysis is also reflected in a negative correlation coefficient when RT is correlated with the mean distance ratings obtained in the pre-study (see the left plot in Figure 4.2). For a more fine-grained analysis, this correlation was additionally computed for each level of Distance separately (see the middle and right plot in Figure 4.2). To account for the dependency in the data, two repeated measures correlations were calculated using the *rmcorr* package in R (Bakdash & Marusich, 2017). Interestingly, both analyses suggested a positive and significant correlation of RTs and Rated Distance (short-distance sentences: $r(4451) = .04$, 95% CI [0.01, 0.07], $p = .017$; long-distance sentences: $r(4456) = .07$, 95% CI [0.04, 0.10], $p < .001$).

Figure 4.2. ***Scatter plot of mean RTs as a function of the rated distance of all sentences (left), of only short-distance sentences (centre), and of only long-distance sentences (right).***



Note. RTs are aggregated for each Item and each level of Distance.

Accuracy. Since the speed effect was also evident for accuracy in Experiment 1 of Chapter 3, we conducted a 2 (Speed) by 2 (Distance) repeated measures ANOVA for all fast-speed and slow-speed sentences (before outlier removal) on accuracy using the *ezANOVA* function in R (Arnhold, 2013). Yet, none of the main effects, nor the interaction was significant [Speed: $F(1, 199) = 0.09$, $p = .763$, $\eta_p^2 < .01$; Distance: $F(1, 199) = 0.40$, $p = .526$, $\eta_p^2 < .01$; Speed × Distance: $F(1, 199) = 0.25$, $p = .621$, $\eta_p^2 < .01$].

Discussion

The present experiment was designed to investigate how fine-grained the modal representations are that are elicited by the task to detect motion (see Experiment 1 of Chapter 3 and Wender & Weber, 1982). More specifically, additionally to speed, as already implemented in Experiment 1 of Chapter 3, we manipulated distance to assess whether only speed information is represented modally or whether the travelled distance is also taken into account in the build-up of the respective situation model.

We were able to replicate the speed effect, that is, fast-speed sentences were reacted to faster than slow-speed sentences. The sample size in the present Experiment was increased (N = 200) compared to Experiment 1 of Chapter 3 (N = 40). This gives ample support for the reliability of the speed effect, which seems to be robust not only with respect to the chosen verbs of motion and the resulting verb pairings (see Table 4.1 in comparison with Table 3.1), but also with respect to the specific testing circumstances (on-site vs. online). Thus, the speed effect does not join other embodiment effects that turn out to be difficult to replicate (see e.g., Papesh, 2015 with respect to the ACE).

There was only a speed effect on RTs, but not on accuracy. Accuracy was not modulated by the factor Speed in the present experiment, even though this was the case for Experiment 1 of Chapter 3. However, accuracy was higher in the present experiment compared to Experiment 1 of Chapter 3 (accuracy of sensical sentences 97.48% vs. 93.65%). The lower overall accuracy in Experiment 1 of Chapter 3 compared to the present experiment was mainly driven by the object motion sentences (see Table 3.3). These sentences were semantically more complex (see Chapter 3, Experiment 1, Results Section for details), which might have increased the task's general processing demands, while the high accuracy in the present experiment might reflect a ceiling effect concealing a potential effect of speed on accuracy. Alternatively, the higher sample size of the present study might imply that the effect of Speed on accuracy in Experiment 1 of Chapter 3 might be a false positive finding. A replication of the paradigm with more complex stimulus material – by, for example, employing noun phrases as sentence subject – would be able to resolve this question.

With respect to the added manipulation of distance, the experiment delivers surprising results. Distance does not affect RTs as expected under the assumption that it is represented modally and analogue to physical distance. That there is no analogue relationship between the denoted travelled distances and reaction times is consistent with the idea that it might not be economical or efficient to incorporate distances in the situation models if the task requires to detect motion. Distance might not be a critical determinant for accumulating evidence that helps to discriminate between movement and static scenes. In this vein, Kosslyn et al. (1978, Experiment 3) only observed an effect of distance on RTs when people were instructed to scan the mental images which implies that modal representations of travelled distance might only be activated when they are task relevant. Moreover, Denis and Cocude (1989, p. 305) reported that the positive correlation of time and distance is more pronounced, if the size at which an image should be constructed, is specified. Even though we controlled for the length of the travelled distances expressed by the PPs via a rating study (see Appendix C.1), the absence of a starting point in the stimulus material of the present experiment might have led to an underspecification of the travelled distances, which impeded their modal representations. These arguments would be in line with a null effect of distance, yet, what we observed was not a null-effect, but a significant inverted effect of distance on RT. More specifically, long-distance sentences were reacted to faster than short-distance sentences.

There are three potential post-hoc explanations to account for this unexpected finding of shorter RTs for long-distance sentences compared to short-distance sentences. Firstly, short-distance sentences might be atypical expressions leading to increased processing times. Many of the long-PPs in our stimulus material consisted of nouns that depict buildings or locations that one would walk to under specific but common circumstances and with respective typical purposes. For instance, one would go to the hospital, if one wants to visit a patient or needs health care (other nouns of long-PPs analogous to hospital are *cinema*, *library*, *dentist*, *post office*, etc.). Short-PPs on the other hand consist of nouns that can usually be found inside a household (e.g., *table*, *power socket*, *telephone*, *stove*, etc.). These might not represent very typical paths to travel on. Thus, RTs might be longer for short-distance sentences than for long-distance sentences due to their unprototypicality. Yet semantic similarity between the verbs and the nouns of the PPs was included as predictor for RTs in the LMEM and should cover this confound at least partially. Moreover, the mean cosine value of *short* verb-noun combinations was higher than the mean cosine value of *long* verb-noun combinations. This pattern is in opposite direction to the observed effect of distance on RT. Yet, one could argue that despite a higher semantic similarity for *short* verb-noun combinations they are still not as felicitous utterances as the *long* verb-noun combinations. A rating on the felicitousness of the sentences would provide a more straightforward answer to this issue.

Secondly, the distances expressed by the PPs in our stimulus material might be confounded with the referents' physical size. More specifically, the previously mentioned buildings that are used as nouns of the long-PPs in our stimulus material are large compared to the household objects listed above as part of the short-PPs (see especially *power socket*, *telephone*, or *alarm clock*). The RT pattern of the present experiment would thus be in line with Sereno et al. (2009), who report faster responses to words whose referents are large compared to small referents in a lexical decision task. They explain their results with faster access of stored visual representation of bigger objects, because larger objects contain more low spatial-frequency information, which is transmitted faster through the visual system than smaller objects – when viewed at the same distance (Sereno et al., 2009, p. 1120). However, Kang et al. (2011) failed to replicate the study by Sereno et al. (2009) and instead report a null-result for size in a lexical decision task. Furthermore, Solomon and Barsalou (2004) observed an opposite pattern compared to Sereno et al. (2009): they reported a positive correlation of size and reaction times in a property verification task with highly associated false properties. Thus, given these inconsistent results, the literature does not clearly support the explanation, that longer RTs for short-distance sentences than for long-distance sentences in the present experiment are driven by an effect of referent size, especially since not all short-PPs have nouns with small referents (e.g., *garden* as noun of a short-PP). A rating on the referents' size that could be inserted as additional predictor of RTs in the LMEM would provide more insight for this hypothesis.

A third potential explanation is that different kinds of features are salient in short-distance sentences compared to long-distance sentences. More specifically, the longer implied duration of long-distance sentences might lead to an increase of time-scale that comes along with a characterization of the described events in terms of goals and plans while the mental representations of the shorter events denoted by the short-distance sentences might be more physically characterized (Zacks & Tversky, 2001). Thus, one might hypothesise that the long-distance sentences are represented primarily with respect to the event's goal (e.g., walking to the post office to send off a letter). The mental representation of short-distance sentences, on the other hand, might contain more physical details, fostering the multimodal simulation of the described event (e.g., walking to the telephone due to its ringing, picking up the phone; allowing for auditory, tactile, and visual simulations).

To evaluate whether there might be such a categorical difference in the processing of the two levels of Distance that is not related to the travelled distance, we calculated post-hoc correlations for distance ratings and RTs for both levels of Distance separately. Indeed, RTs were positively correlated with the mean distance ratings within both levels of Distance despite the estimated negative coefficient for Distance in the LMEM. Thus, the observed longer RTs for short-distance compared to long-distance sentences seem to have been driven by a categorical difference between the two levels of Distance that was not related to the travelled distance. Instead, it is feasible that short-distance sentences are represented on a different time-scale compared to long-distance sentences. While the former allows for detailed simulations of the physical characteristics of the denoted events, the latter might be temporally compressed leading to a less physically characterized processing and shorter RTs due to a lower simulation complexity (Alex-Ruf, 2016). This interpretation of the data would support the assumption of detailed and fine-grained modal representations of the described events. It would imply that not only speed but also the travelled distances would be considered in the build-up of the respective situation models that are more complex for short-distance than for long-distance sentences. Even though this interpretation seems plausible, it is built upon post-hoc inspection of the data and thus needs to be confirmed by future studies that specifically test this hypothesis.

In summary, the results of the present experiment suggest that the travelled distances denoted by the sentences that are employed to test whether locomotion is detected faster in fast-speed sentences than in slow-speed sentences is not represented in an analogous relation to the denoted events' distances. Longer distances take longer to travel, yet sentences that denote longer distances are processed faster than sentences that denote shorter distances. This implies that the modal representations underlying the speed effect are task-specific and only incorporate those details of the denoted events that are relevant for task performance. Post-hoc inspection of the results, however, challenges this interpretation by suggesting that the shorter denoted durations of short-distance sentences allow for a more detailed situation model build-up leading to longer RTs compared to long-distance sentences. Moreover, post-hoc correlations imply that within the same time-scale linguistically expressed distance is represented analogously to physical distance in this paradigm. This assumption, however, will have to be verified by future studies. What can be considered a clear conclusion is that the speed effect reported by Wender and Weber (1982) and observed in Experiment 1 of Chapter 3 is reliable across different verb pairs and technical implementations.

Chapter 5

Speed or duration? Effects of implicit stimulus attributes on perceived duration[23]

Abstract

The human ability to keep track of time can be distorted by several non-temporal stimulus aspects such as size or intensity. First studies indicate that not only physical but also implicit stimulus aspects can affect duration estimates. The present study expands these findings by investigating the effects of linguistic expressions including speed and duration information via temporal reproduction (Experiments 1 and 2) and temporal bisection tasks (Experiment 3). In Experiment 1, implicit duration was manipulated by combining verbs that denote slow or fast motion with a path expression (*to stroll to school* vs. *to spurt to school*). Reproduced durations were consistent with an effect of implicit duration but not implicit speed. To control whether implicit speed affects perceived duration when exempted from duration information, single manner of motion verbs were presented in Experiments 2 and 3. The results speak against an effect of implicit speed analogous to physical speed.

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[23] This chapter is the published version of following article: von Sobbe, L., Maienborn, C., Reiber, F., Scheifele, E., and Ulrich, R. (2021). Speed or duration? Effects of implicit stimulus attributes on perceived duration. *Journal of Cognitive Psychology, 33*(8), 877-898. https://doi.org/10.1080/20445911.2021.1950736 published by Taylor & Francis Group. This work is licensed under a *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/). The final published version has only been adapted with respect to reformatting required by the present publication. Moreover, titles were created for the appendices (see Appendix D).

Introduction

Tracing the duration of temporal intervals is an everyday human ability and essential for an organism (Matthews & Meck, 2016; Núñez et al., 2012; Wittmann, 2009). Even though there is no dedicated sense organ for time, humans show stable temporal judgment behaviour such as scalar timing and scale invariance, which speaks for the existence of a specific but yet to be fully understood process that is responsible for the mental representation of physical time (Matthews & Meck, 2016). For instance, in temporal reproduction tasks in which participants are presented with a particular duration and are asked to produce an interval of the same extent, they quite accurately reproduce time intervals up to approximately 3 s (Daikoku et al., 2018). Interestingly, however, various non-temporal aspects of the sensory input affect subjective time, such as stimulus magnitude, intensity, or complexity (Allman et al., 2014; Matthews & Meck, 2016; Wang & Gennari, 2019). A prominent explanation for this phenomenon is the interference of non-temporal stimulus properties with different components of the internal clock (for details on the internal clock model, see Allman et al., 2014; Matthews & Meck, 2016).

Moreover, recent studies have shown that not only physical but also implicit or imagined stimulus aspects affect duration estimates (Birngruber & Ulrich, 2019; Bottini & Casasanto, 2010; Ma et al., 2012). For example, in the study by Birngruber and Ulrich (2019), participants' reproduced durations (RD) were influenced by the imagined size of an animal word's referent. Specifically, participants tended to judge the duration of words longer when the word's referent was a large animal compared to a small one (for an effect of implicit weight and volume, see Ma et al., 2012; for an effect of implied motion, see Yamamoto & Miura, 2012). These findings of analogous effects of mentally imagined to physically present stimulus aspects on perceived durations corroborate the notion that perception and imagination draw on the same brain circuits to establish a depictive representation of the perceived or imagined stimulus (Birngruber & Ulrich, 2019; Moulton & Kosslyn, 2009; Pearson & Kosslyn, 2015).

The present study aims to extend this research and investigates whether dynamic mental images that are induced by linguistic expressions containing manner of motion verbs with different levels of associated speed can affect perceived duration. Against this background, the aim of Experiment 1 was to determine how speed and duration information as implicit stimulus aspects of complex expressions interact and how they contribute to a potential effect on reproduced durations. Experiment 2 was designed to disentangle speed from duration information, again implementing a duration reproduction task. Due to conflicting results with a temporal bisection study by Zhang et al. (2014), Experiment 3 was conducted as a replication of Zhang et al. (2014) to gain a better understanding of the effect of linguistically expressed speed information on perceived duration.

Besides these issues on temporal perception, this research relates to the grounded cognition debate which deals with the question of how and to what extent cognition – including language processing – relies on multimodal representations that come into play via simulations. Such simulations are framed as “the reenactment of perceptual, motor, and introspective states acquired during experience with the world, body, and mind” (Barsalou, 2008, p. 618). Thus, if participants simulate the meaning of expressions that are presented during an interval whose duration has to be estimated, i.e., if the meaning of those expressions were grounded in sensomotoric brain areas in which also the analog physical stimulus is processed, the expressions’ denotations should affect perceived duration similar to their physical counterparts.

Physical speed as a function of distance per time unit has been shown to lead to an overestimation of duration (Brown, 1995; Kanai et al., 2006; Kaneko & Murakami, 2009; Karşılar et al., 2018; Linares & Gorea, 2015; Tomassini et al., 2011; van Rijn, 2014). For instance, Brown (1995) found that higher velocity is perceived to last longer than lower velocity in both duration reproduction and production tasks, using stimulus durations in the supra-second time range, which will in the following be referred to as the dilation effect of physical speed (DEPS). Kanai et al. (2006) replicated Brown’s findings for sub-second stimulus durations. In addition, Tomassini et al.’s (2011) data imply that the DEPS not only applies to visual stimuli but also to the tactile modality. Moreover, Karşilar et al. (2018) replicated the DEPS for biological motion via the visual presentation of an animated, walking stick-figure. Furthermore, the effect has not only been observed for perceived motion but also for self-conducted motion: Time subjectively dilates with an increase in higher running intensity or, more specifically, with an increase in higher ratings of perceived exertion (RPE) in treadmill runs (Hanson & Lee, 2020). Analogous to what has been observed for physical and implicit size, the temporal bisection study by Zhang et al. (2014) indicates that implicit speed acts on perceived duration just like physical speed: the presentation duration of fast-speed verbs and adjectives (e.g., *gallop*) was overestimated compared to that of slow-speed verbs and adjectives (e.g., *limp*), suggesting a dilation effect of implicit speed (DEIS). Thus, an increase in visually, motorically, or tactually perceived speed has been shown to lead to an overestimation of perceived duration which applies to both biological and non-biological motion, and ostensibly also to linguistically encoded speed information, supporting a grounded cognition view on the processing of linguistically expressed speed information.

The dilation effect of both physical and implicit speed can be explained with an information-processing account, which assumes that the duration of an interval is estimated through the number of changes present in a stimulus (Karşılar et al., 2018). Thus, fast motion should be perceived to take longer compared to slow motion since it involves more changes of position in the same unit of time (Kanai et al., 2006). Alternatively, a higher level of arousal when processing fast compared to slow speed might increase the pacemaker rate, which would lead to a larger amount of pulses being counted and to a consequential dilation of perceived duration for fast compared to slow motion (Behm & Carter, 2020; Karşılar et al., 2018; Zhang et al., 2014).

In Experiment 1 of the present study, participants were asked to reproduce the presentation duration of expressions such as *to stroll to school* as opposed to *to spurt to school*, for which they were also told to form mental images. Verb phrases containing a motion verb denoting either fast (e.g., *to spurt*; in the following referred to as fast-speed verbs) or slow locomotion (e.g., *to stroll*; in the following referred to as slow-speed verbs) in combination with a prepositional phrase denoting a path (e.g., *to school*; in the following referred to as path PP) provide the reader with two stimulus aspects. On the one hand, the reader obtains duration information (i.e., fast motion on a given path takes shorter than slow motion on the same path), while on the other hand, the expressions contain the highly salient feature of speed as distinctive property. There are two alternative scenarios for the build-up of the corresponding mental images that the participants were asked to form. Firstly, all items of the expression might be taken into account compositionally to establish a dynamic representation of the described event (Pearson & Kosslyn, 2015). When keeping the path information constant as in these sentences, expressions with slow-speed verbs denote longer events than expressions with fast-speed verbs. We conjecture that this difference in the events’ durations could affect the perceived duration. More precisely, the encoding of these expressions' presentation duration in reference memory for later retrieval in the decisional process might be biased by the duration of the linguistically expressed events. As subjective time increases with physical time (Allman et al., 2014), the perceived duration should be longer for sentences with slow-speed rather than fast-speed verbs.

Alternatively, the mental image might mainly consist of the visual or motoric simulation of the kinematics denoted by the manner of motion verbs. This could be due to the high salience of motion in these expressions and its potential behavioural importance (Matthews & Meck, 2016). During the imagination of the presented expressions, the expressed action might thus overshadow the expressed events’ duration. Consequently, an effect of speed analogous to its physical counterpart should be expected, that is, an overestimation of the presentation duration of fast-speed compared to slow-speed verbs (Zhang et al., 2014), which is thus in contrast to the prediction of the compositional account mentioned in the preceding paragraph.

The design and predictions of Experiments 2 and 3, which investigate the effect of isolated speed information in manner of motion verbs on perceived duration, will be derived in the course of discussion.

Experiment 1

Experiment 1 was designed to distinguish empirically between the two hypotheses established above, that is, the compositional account as opposed to the salience of motion account. For this purpose, expressions such as those mentioned above (*to stroll to school* vs. *to spurt to school*) were presented for varying time intervals, and participants were asked to imagine the expressions’ denotation and then reproduce the duration of the expressions’ physical appearance on the screen. If the speed information was taken into account compositionally in the dynamic mental image, RDs of expressions with fast-speed verbs should be shorter than RDs of expressions with slow-speed verbs due to the duration of the linguistically denoted events. If, however, the mental image was mainly driven by the sensomotoric simulation of the denoted manner of motion with shallow processing of the path information, RDs of expressions with fast-speed verbs are expected to be longer than RDs of expressions with slow-speed verbs. This is derived from the effect of physical speed on perceived duration. The experiment was preregistered on the Open Science Framework (von Sobbe, Ulrich, et al., 2019a).

Method

Participants

Sixty students of the University of Tübingen took part in the experiment and received either payment or course credit for their participation. Their age ranged from 19 to 65 (*M* = 25.28 years) and they were native speakers of German. Due to a predefined exclusion criterion (see the Procedure and Results Sections for details), 22 participants' data had to be replaced. Additionally, one person accidentally participated twice, so her second data set was also replaced. All participants gave written informed consent.

Apparatus and Stimuli

The experiment was programmed in PsychoPy v1.90.3 and presented on two Windows PCs (60 and 100 Hz refresh rate). The arrow down key of a standard German keyboard was used to measure the reproduced duration.

Table 5.1. ***Pairing of manner of motion verbs for Experiments 1 and 2.***

Slow-speed verbs					Fast-speed verbs				
Verb	Rating	Letters	Syllables	Frequency	Verb	Rating	Letters	Syllables	Frequency
hinken	1.74	6	2	15	marschieren	3.89	11	3	13
humpeln	1.78	7	2	18	huschen	4.92	7	2	16
schlurfen*	1.85	9	2	18	joggen*	5.05	6	2	14
taumeln	1.86	7	2	16	eilen	5.33	5	2	14
torkeln	1.92	7	2	18	hasten	5.41	6	2	17
bummeln	2.00	7	2	15	spurten	5.66	7	2	18
wanken	2.00	6	2	16	preschen	5.77	8	2	16
schlendern	2.05	10	2	14	hetzen	5.82	6	2	14
latschen	2.26	8	2	18	rennen	5.85	6	2	12
trotten	2.38	7	2	17	sausen	6.00	6	2	14
spazieren	2.82	9	3	12	jagen	6.09	5	2	12
stiefeln	2.85	8	2	19	stürmen	6.16	7	2	13
schreiten	3.24	9	2	13	flitzen	6.32	7	2	15
gehen	3.34	5	2	6	sprinten	6.36	8	2	16
wandern	3.34	7	2	12	rasen	6.55	5	2	14
M	2.36	7.47	2.07	15.13		5.68	6.67	2.07	14.53
SD	0.59	1.36	0.26	3.40		0.68	1.54	0.26	1.77

Note. *Rating* refers to the mean rated speed on a 7 point Likert scale (1 representing *very slow*, 7 representing *very fast*). Ratings of verbs marked with asterisk were collected in a separate pre-study (see Appendix D.1 for details on the ratings). *Letters* stand for the number of characters of the verbs in the infinitive verb form. *Frequency* refers to the verbs' frequency class obtained from Leipzig Corpora Collection (2018).[24] Adapted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), p. 880. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/). English translations of the verbs are provided in the published version, which can be found online at https://doi.org/10.1080/20445911.2021.1950736.

The stimulus material was built upon thirty German manner of motion verbs denoting either fast or slow human locomotion. The verbs were paired according to a

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[24] Leipzig Corpora Collection (2018). German newspaper corpus based on material crawled in 2018. Leipzig Corpora Collection. Dataset. https://corpora.uni-leipzig.de/de?corpusId=deu_newscrawl-public_2018.

speed rating that was conducted with an independent sample of 40 participants (see Table 5.1 for the verb pairings, and Appendix D.1 for details on the speed rating study).

Each verb pair, for instance *bummeln* (to stroll) and *spurten* (to spurt) was then combined with three different prepositional phrases that denoted a path, on which the motion was carried out (see (5.1) to (5.3)). To minimise the length of the expression, no agent was included and the verbs were in infinitive.

(5.1.a) durchs Museum bummeln (*to stroll through the museum*)
(5.1.b) durchs Museum spurten (*to spurt through the museum*)
(5.2.a) zur Schule bummeln (*to stroll to school*)
(5.2.b) zur Schule spurten (*to spurt to school*)
(5.3.a) zur Haltestelle bummeln (*to stroll to the station*)
(5.3.b) zur Haltestelle spurten (*to spurt to the station*)

To evoke a difference in duration via the motion verbs' speed, the imagination of the same path within each item was necessary. For instance, people should imagine the same path when reading *to stroll to school* as when reading *to spurt to school*. For this purpose, a matching photo of a path was selected for each path PP and presented prior to the duration reproduction task (see Figure 5.1 and the Procedure Section for details). Since the stimulus material was based on fifteen verb pairs that were repeated with three different path PPs each, the stimulus material contained 45 distinct path PPs and matching photos. These were considered items in the statistical analysis (see Appendix D.7 for a list of all stimuli). The expressions were split into three lists such that each list contained all 15 verb pairs, that is, all 30 manner of motion verbs.

Ten additional motion verbs with one path PP each (*springen*, *fallen*, *galoppieren*, *traben*, *schippern*, *segeln*, *fahren*, *düsen*, *brettern*, and *tuckern*) were used for the practice block. All stimuli were presented in white colour against a grey background (Arial; scaling factor 0.07).

Procedure

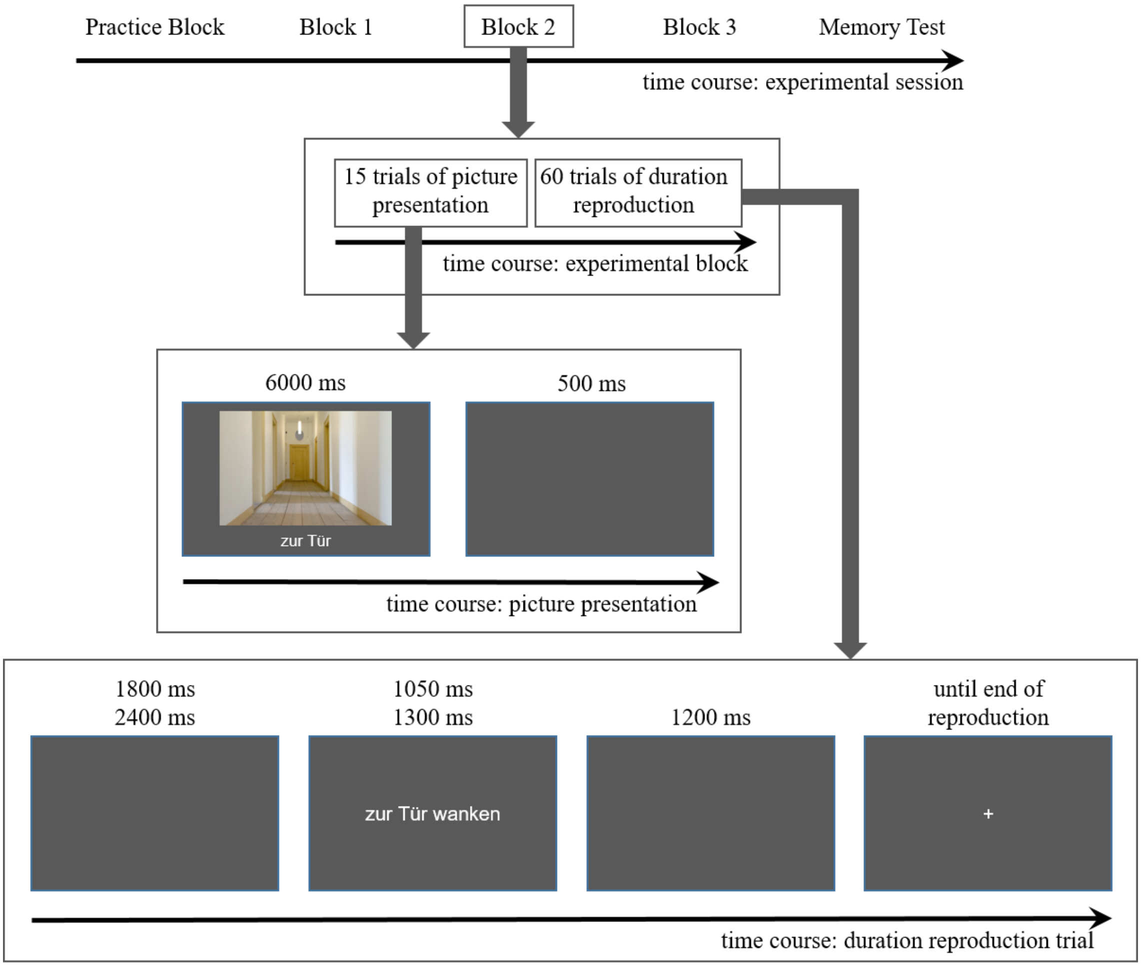
The experiment was run in a sound-attenuated room. One experimental session lasted about 40 min. A session consisted of one practice block, three experimental blocks and a subsequent memory task (see Figure 5.1 for the time course of an experimental session).

Participants were given written instructions prior to the start of the experiment. They were asked to mentally imagine the expressions while performing the duration reproduction task. The necessity to engage with mental imagination was constituted by informing the participants about a memory task that would follow the main experiment. They were told that they would have to recall the verb-path-combinations in the memory task and that vividly imagining the verbs' implied motion along the respective path would help them to perform well in it (Birngruber & Ulrich, 2019). Moreover, participants were instructed to attentively look at the pictures presented prior to the duration reproduction task. They were asked to memorize the pictures in combination with their subtitle, that is, the path PP, and were told that these path expressions would reappear in the subsequent duration reproduction task. The pictures would help them to mentally imagine the expressions that would appear in the later task.

The practice block consisted of an initial presentation of five pictures and 20 subsequent trials of the duration reproduction task. The experimental blocks each consisted of 15 pictures and 60 duration reproduction trials (see Figure 5.1). Each trial of the duration reproduction task started with a blank screen of either 1.8 or 2.4 s. In order to prevent rhythmic response patterns, these two intertrial intervals (ITI) were presented in randomised order (ITI design adopted from Rammsayer & Verner, 2015). Subsequently, the stimulus expression was presented for either 1050 or 1300 ms. These two stimulus durations were employed to ensure that participants were following task instructions and were able to distinguish between the two different stimulus durations. If this was not the case, that is, if the mean RDs of a participant did not increase with stimulus duration, the participant's whole data set was excluded (Birngruber & Ulrich, 2019). The order of stimulus presentation was randomised for each participant. The target stimulus was then replaced by a blank screen with a fixed interstimulus interval (ISI) of 1.2 s after which a white cross appeared at the centre of the screen. The RD started with the appearance of the cross. Participants were asked to press the 'arrow down' key when they felt that the cross was presented as long as the target stimulus had been presented.

As soon as they pressed the key, the next ITI started. Breaks were included after each block and after every 20 trials of one block. Participants could terminate the breaks via key press.

Figure 5.1. ***Time course of the experimental session in Experiment 1.***



Note. Photograph of the corridor by van Erp (2018). Reprinted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), https://doi.org/10.1080/20445911.2021.1950736, p. 881. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/).

In each block, before the beginning of the duration reproduction task, the corresponding pictures that matched the path PPs of that block were presented for 6 s each. The path PP was written underneath the photo; for instance, the photo of a path to a school building was subtitled *to school*. Order of picture presentation was randomised for each participant. Picture presentation was followed by a screen, which informed the participants about the upcoming start of the duration reproduction task (presented for 4 s).

The 60 trials of the duration reproduction task in one experimental block were defined by the factorial combination of 30 expressions (i.e., 15 verb pairs in combination with a path PP) with two different presentation durations (1050 vs. 1300 ms). Each participant was presented with all three stimulus lists (one per block). List order was counterbalanced between participants.

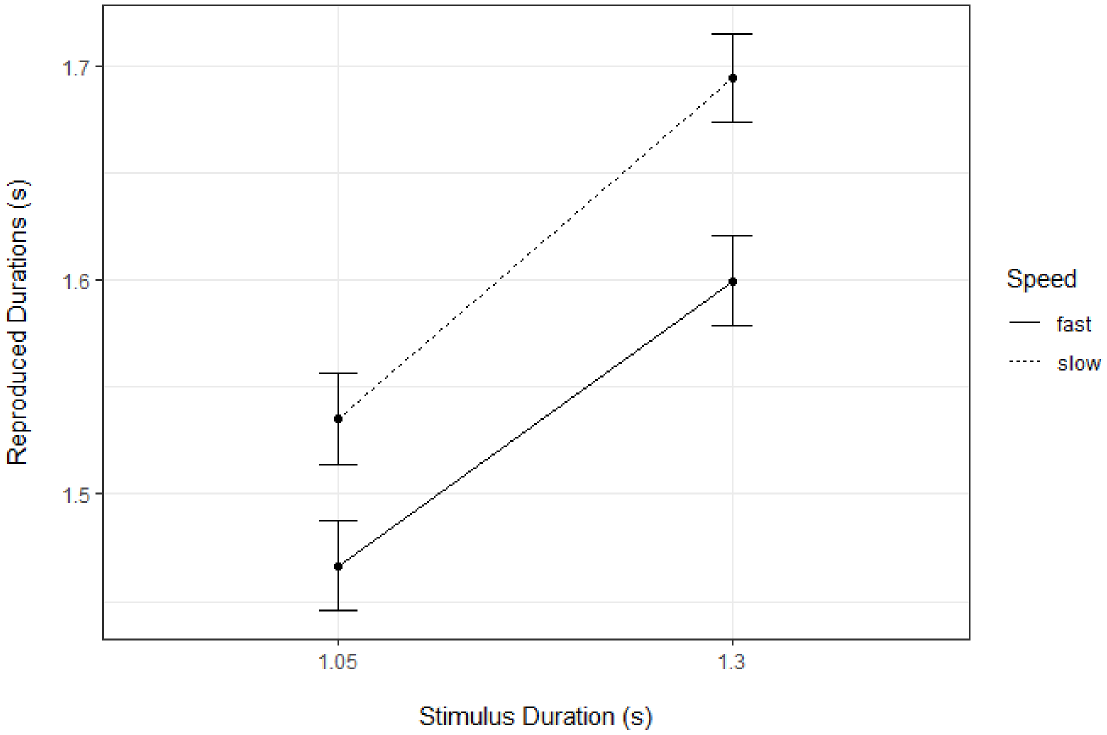
In the memory task, the participants were presented with 32 expressions one after another. They had to indicate via keypress whether these were included in the duration reproduction task or not. Presentation order was randomised for each participant. Sixteen of the presented expressions were extracted from the experimental stimulus material. Eight expressions were expressions in which either the motion verb or the path PP was new and not part of the experimental stimulus material. A further eight expressions were false friends by combining actual verbs of the experimental stimulus material with actual path PPs of the experimental stimulus material but the specific verb-path-PP-combination was not contained in the experimental stimulus material.

Results

Outliers were excluded following a two-step procedure: All trials in which RDs exceeded 5000 ms or fell below 100 ms were excluded. Secondly, for each participant in each stimulus duration and each speed of motion verb condition, RDs that deviated by more than 2.5 standard deviations from the mean of the respective cell were considered outliers and excluded from further analysis. Overall, this led to an exclusion of 5.54% of the data.

Furthermore and according to the pre-defined exclusion criterion (see Procedure), a participants' whole data set was excluded, if the mean RDs of the remaining data of that participant did not increase with stimulus duration. Due to this exclusion criterion, the data of twenty-two participants had to be excluded and were replaced. Mean RDs of the two levels of Speed per Stimulus Duration are plotted in Figure 5.2.

Figure 5.2. ***Mean reproduced durations in Experiment 1 as a function of Speed and Stimulus Duration.***



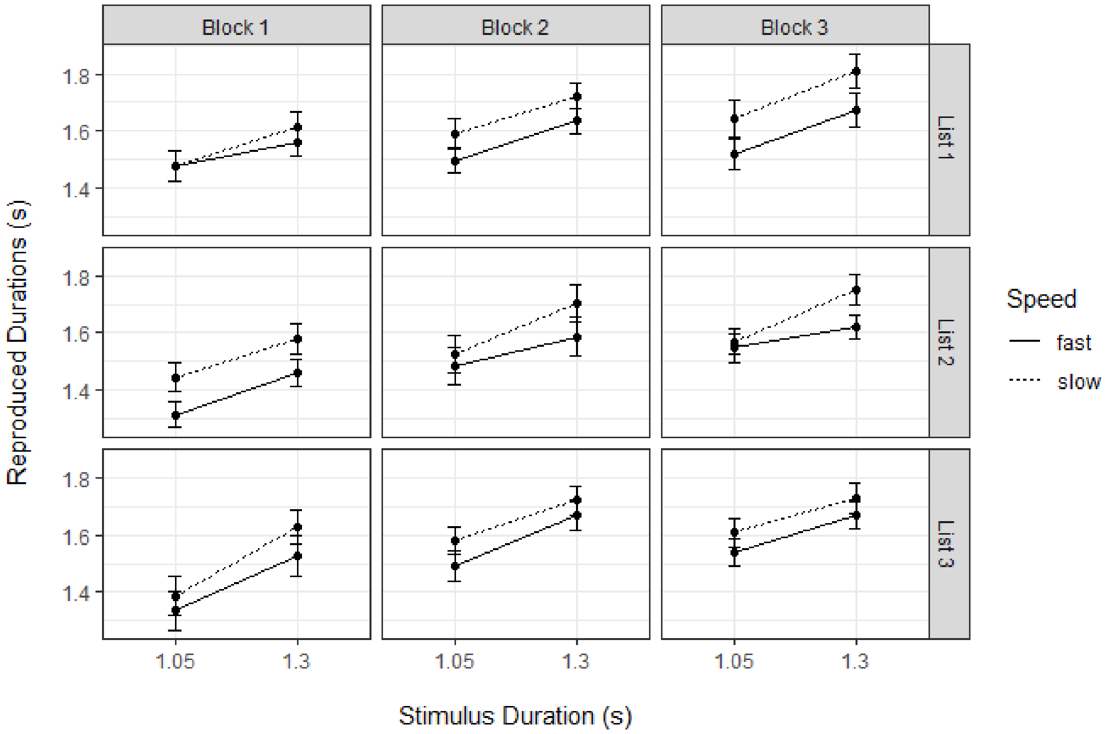
Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). Reprinted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), https://doi.org/10.1080/20445911.2021.1950736, p. 882. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/).

As preregistered, we analysed the remaining data with a linear mixed effects model (LMEM) using the *lme4* package (Bates et al., 2015) in R (Version 4.0.3, 2020) to predict RD as a function of Stimulus Duration (1050 ms and 1300 ms; mean centred), Speed (*fast* and *slow*; with *fast* as reference category), and Number of Characters (mean centred). Number of Characters encodes physical size, which has been shown to affect perceived duration (Birngruber & Ulrich, 2019). Subjects and Items (i.e., path PPs), were considered random factors. Block was not considered as a fixed factor in the preregistration of the analysis, because there was no theoretical foundation for it. However, upon inspection of the data, a trend of longer reproduced durations along with the proceeding of the experiment was observable (Block 1: *M* = 1499 ms (*SD* = 732); Block 2: *M* = 1592 ms (*SD* = 761); Block 3: *M* = 1632 ms (*SD* = 772); see Figure 5.3 for mean RDs of all cells). We thus conducted a sensitivity analysis, which included Block as a fixed factor (see Appendix D.2 for details). This did not alter the results of the preregistered analysis. Moreover, we assessed the effect of Frequency on RDs by including Frequency as a fixed factor in the LMEM (see Appendix D.3 for details), which again did not alter the results of the preregistered main analysis.

The random effects structure was determined by step-wise reducing the most complex random effects structure, while at the same time keeping the fixed effects structure constant (Barr et al., 2013). The Akaike information criterion (AIC) was used as an indicator for the random effects structure selection. The random effects structure with the best AIC value included random intercepts for Subjects and Items (i.e., path PPs), and random slopes for the factor Speed for Subjects. This random effects structure did not only have the best AIC value, but additionally was the most complex random effects structure that converged and did not over-fit the data. This yielded the following full model: RD ~ Stimulus Duration + Speed + Number of Characters + (Speed | Subjects) + (1 | Items).

P-values could not be obtained with χ^2 difference tests on the full model in comparison with reduced models, since one of the reduced models did not converge. Consequently, *p*-values were obtained using the Satterthwaite’s approximation for degrees of freedom as suggested by Luke (2017) which is built-in in the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R (Version 4.0.3, 2020).

Figure 5.3. ***Mean reproduced durations in Experiment 1 as a function of Speed and Stimulus Duration per Block and List.***



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). Reprinted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), https://doi.org/10.1080/20445911.2021.1950736, p. 883. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/).

The full model suggests an intercept of $\beta = 1597.53$ ms. As expected, the estimated coefficient of Stimulus Duration ($\beta = 587.39$, $SE = 35.86$, $t(10030.71) = 16.38$, $p < .001$) was significant, implying that perceived duration increases with physical duration. The estimated coefficient of Number of Characters was also significant ($\beta = 7.18$, $SE = 1.60$, $t(64.12) = 4.49$, $p < .001$) and comparable in size to that of previously estimated coefficients for the number of characters in duration reproduction tasks (7.06, 9.17, and 7.10 ms in the three experiments of Birngruber and Ulrich, 2019). Thus, we were able to replicate the effect of physical size on perceived durations. Interestingly – and theoretically most importantly – the estimated coefficient of Speed with reference category *fast* was positive and significant ($\beta = 94.74$ ms, $SE = 36.79$, $t(53.50) = 2.58$, $p =$

.013), suggesting an increase in perceived duration of expressions with slow-speed verbs compared to expressions with fast-speed verbs.

The coefficient of variation (CV) was calculated for each participant, Stimulus Duration, and both levels of Speed by dividing the standard deviation by the mean of the respective cell. Mean CV for a Stimulus Duration of 1050 ms was 0.25 and mean CV for a Stimulus Duration of 1300 ms was 0.23, which is in accordance with the CV reported by Mioni et al. (2014, Figure 4, Method 1) for a Stimulus Duration of 1 s.

In the memory task, participants' hit rate was 76.67%. The false alarm rate for false friends items was relatively high with 65.21%, while the false alarm rate for items that contained at least either a verb or a context that they had not seen before was much lower (9.58%). These results indicate that participants were doing well in distinguishing between experimental and new items while they were having problems with recalling the correct mappings of verbs and path PPs. Overall, however, the results imply that participants were paying attention to the experimental items.

Discussion

The duration of expressions with slow-speed verbs was reproduced longer than the duration of expressions with fast-speed verbs. This overestimation of expressions with slow-speed verbs implies that the speed information associated with the manner of motion verbs as well as the path on which the motion takes place is taken into account compositionally as part of the mental image of the expressions. Thus, RDs are modulated by the denoted events' durations (i.e., fast motion on a given path takes shorter than slow motion on the same path), suggesting that subjective time increases not only with physical but also with implicit time. Consequently, in the expressions employed in the present experiment in which both speed and duration information is given, implicit speed does not affect perceived duration like physical speed, which would imply a longer perceived duration for fast-speed than for slow-speed verbs. Nonetheless, speed information is taken into account to yield events that vary in duration with respect to speed.

Yet, does implicit speed affect perceived duration like physical speed when it is not presented in complex expressions, but linguistically isolated? Zhang et al.'s (2014) finding corroborates this assumption. They observed that the perceived duration of fast-speed verbs and adjectives (e.g., *gallop*, *rapid*) was overestimated compared to that of slow-speed verbs and adjectives (e.g., *limp*, *gradual*). Yet, this finding is in conflict with Mioni et al. (2015), who investigated the effect of implicit speed using images of vehicles associated with slow or fast speed. They report that the duration of the images of the vehicles associated with fast speed was underestimated compared to the duration of images of vehicles associated with slow speed. However, Mioni et al. (2015) only used two vehicles as stimulus material: A motorbike (representing fast speed) and a bicycle (representing slow speed), both with and without a driver. A potential issue with these items is that there are further differences apart from the associated speed, such as the physical exercise when riding a bike as opposed to driving a mechanically propelled vehicle. This difference might affect duration judgments in addition to the difference in implied speed.

Moreover, by using images instead of linguistic stimuli, they investigated the effect of speed information generated from the associated world knowledge of the pictures rather than from linguistic processing. Thus, the difference in results of Zhang et al. (2014) and Mioni et al. (2015) might simply be due to the fact, that the respective stimulus material targets different cognitive processes.

Since Zhang et al. (2014) only used ten items and did not only investigate the effect of implicit speed in manner of motion verbs but also included adjectives as stimulus material, a follow-up experiment was conducted in this study to provide further data on the effect of implicit speed on perceived duration.

Experiment 2

The motion verbs used in Experiment 1 extracted from the path PPs were employed in Experiment 2. This allowed us to examine whether linguistically encoded speed is sufficient to elicit an effect analogous to physical speed on perceived duration.

Isolating speed information was done by eliminating the path PPs and presenting the manner of motion verbs in their infinitive verb form (e.g., *to walk*; in the following referred to as verbs in infinitive form). Hence, no duration information was given to the participants since both fast and slow motion can be executed for an arbitrary amount of time. Thus, for verbs in infinitive form an effect of implicit speed on perceived duration analogous to the DEPS and the finding by Zhang et al. (2014) was expected, that is, longer perceived duration for fast-speed than for slow-speed verbs.

Furthermore, the motion verbs were inflected in third person singular in present tense with and without a personal pronoun (e.g., *she walks* and *walks*, respectively). This was done to investigate whether inflection modulates the activation of modal representations. The temporal embeddedness given in inflected but not in verbs in infinitive form might elicit the mental creation of a default path on which the motion takes place. With respect to the results of Experiment 1 of the present study, this implies that inflected manner of motion verbs might lead to an effect of implicit duration on perceived duration, since the mental creation of a default path would result in different implicit durations for fast-speed and slow-speed verbs. Analogous to Experiment 1, we expected longer perceived durations for slow-speed than for fast-speed inflected verbs.

The inflected verbs were presented with a personal pronoun (e.g., *she walks*) and without (e.g., *walks*) to disambiguate third person singular from imperative plural since these two verb forms are identical for the German manner of motion verbs used in this study. The disambiguation was implemented since the activation of modal representations can be modulated by verb form, or rather perspective (Beveridge & Pickering, 2013). While imperative elicits a first-person perspective, third person elicits an external perspective (Brunyé et al., 2009). Like Experiment 1, Experiment 2 was preregistered on the Open Science Framework (von Sobbe, Ulrich, et al., 2019b).

Method

Participants

A new sample of sixty students of the University of Tübingen took part in the second experiment and again received either payment or course credit for their participation. Their age ranged from 17 to 60 (M = 23.63 years) and they were all native speakers of German. Due to the predefined exclusion criterion, 11 participants had to be excluded from data analysis and were replaced. All participants gave written informed consent.

Apparatus and Stimuli

The apparatus was identical to Experiment 1.

The stimulus material consisted of the thirty German manner of motion verbs that had been used in Experiment 1. Three different verb form lists were constructed with these verbs, with each verb form list containing the motion verbs in a different verb form. Verb Form List 1 consisted of the verbs in infinitive verb form; Verb Form List 2 contained the verbs inflected in third person singular in present tense in combination with a personal pronoun (half of the verbs were combined with *he*, half of the verbs with *she*). The third verb form list consisted of the verbs inflected in third person singular in present tense without the use of a personal pronoun.

List 1	bummeln	(*to stroll*)
List 2	sie bummelt	(*she strolls*)
List 3	bummelt	(*strolls*)

Seven additional motion verbs (*galoppieren*, *traben*, *schippern*, *fahren*, *düsen*, *brettern*, and *tuckern*) were used for the practice block. As in Experiment 1, all stimuli were presented in white colour against a grey background (Arial; scaling factor 0.07).

Procedure

One experimental session lasted about 45 min. A session consisted of one practice and three experimental blocks, which each consisted of 90 trials of the duration reproduction task. The 90 trials were defined by the factorial combination of one of the verb form lists (15 verb pairs, i.e., 30 expressions) with each expression presented in three different durations (900, 1100 or 1300 ms). The order of verb form list was counterbalanced between participants. ITI, ISI, and the recording of RDs were identical to Experiment 1. The practice block consisted of 21 trials, that is, of seven motion verbs in the three different stimulus durations. Breaks were included analogously to Experiment 1.

Participants were instructed to mentally imagine the motion denoted by the verbs of the duration reproduction task. Again, they were informed about the subsequent memory task and invited to vividly imagine the motion expressed by the verbs in order to score high in it.

In the memory task, the participants were presented with 32 motion verbs in three different verb forms. Sixteen of the presented verbs were part of the experimental stimulus material, the remaining sixteen motion verbs were verbs they had not seen before.

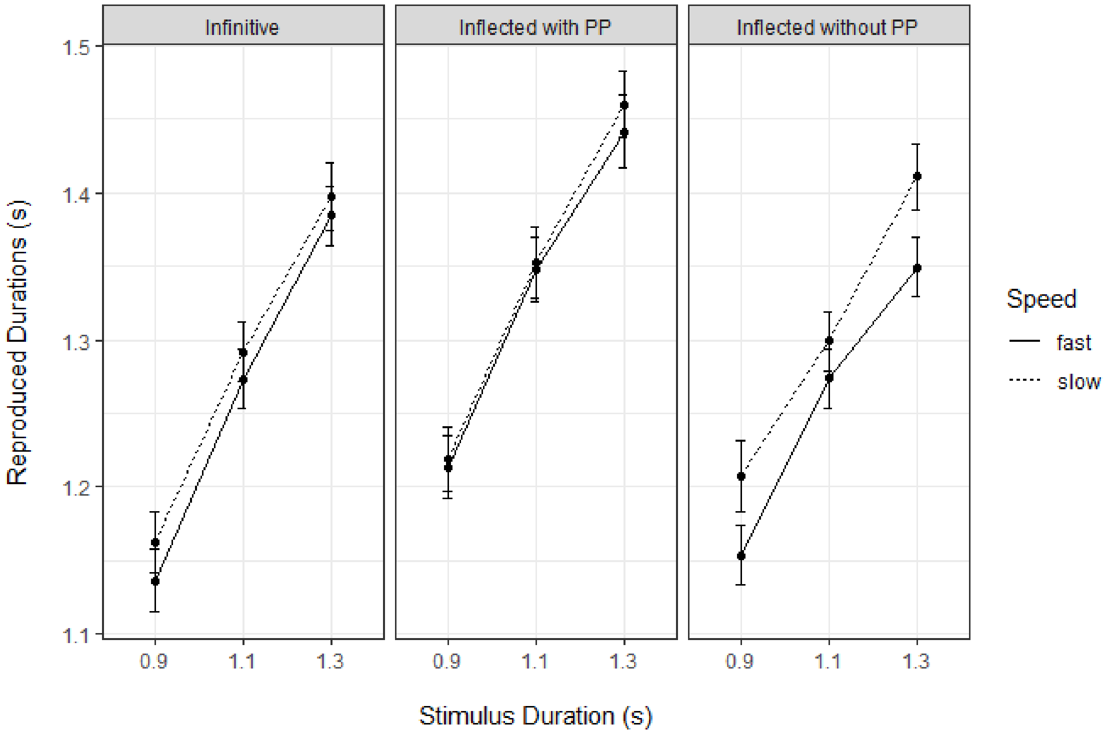
Results

Outliers were excluded following the same two-step procedure with the difference that additionally to Subject, Stimulus Duration, and Speed of Motion Verb, Verb Form List was also treated as a cell in the trimming of the data. Overall, this led to an exclusion of 1.46% of the data.

Furthermore, if the mean RDs of the remaining data of a participant did not strictly increase with the three stimulus durations, the participant’s whole data set was excluded. Consequently, the data of eleven participants had to be excluded and were replaced. Mean

RDs of the two levels of Speed per Stimulus Duration and Verb Form are plotted in Figure 5.4.

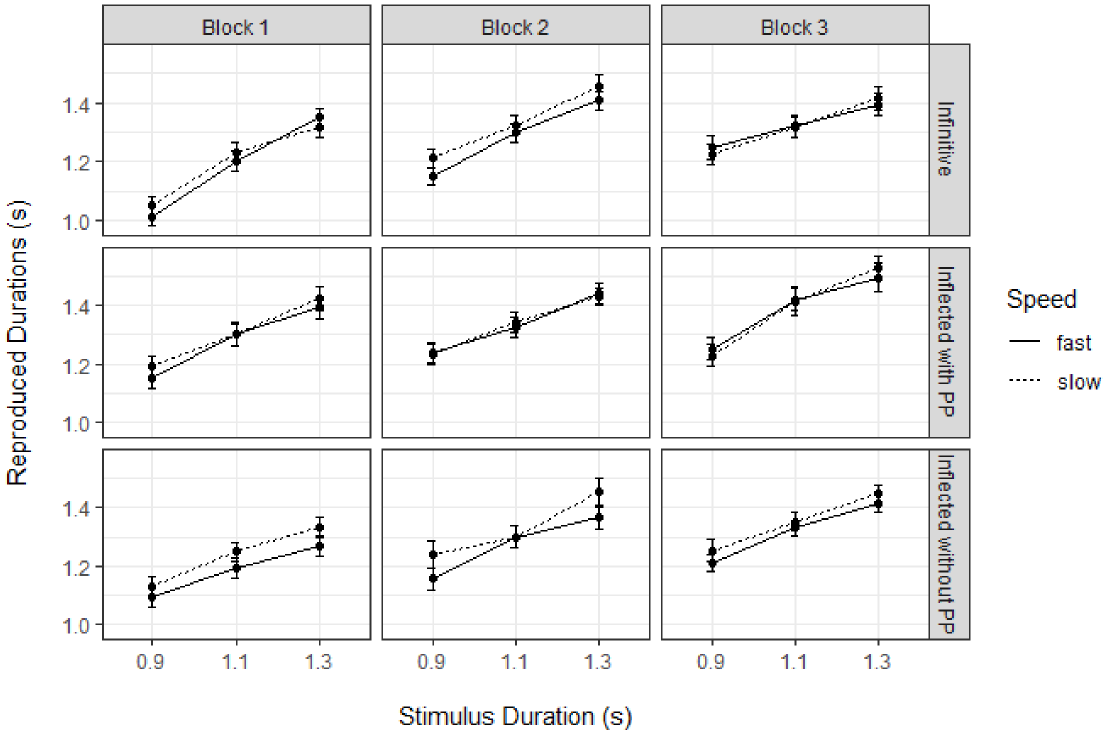
Figure 5.4. ***Mean reproduced durations in Experiment 2 as a function of Speed and Stimulus Duration per Verb Form.***



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). PP stands for the personal pronoun. Reprinted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), https://doi.org/10.1080/20445911.2021.1950736, p. 884. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/).

As preregistered, RD was predicted as a function of Stimulus Duration (900 ms, 1100 ms, and 1300 ms; mean centred), Speed (*fast* and *slow*; with *fast* as reference category), Number of Characters (mean centred), Verb Form (*infinitive*, *inflected with personal pronoun*, and *inflected without personal pronoun*; *infinitive* was used as reference category) and an interaction of Speed and Verb Form. Item numbering was following Experiment 1, such that the pairing of Experiment 1 of fast-speed and slow-speed verbs was adopted, which resulted in 15 Items with two levels of Speed per List. Since only the verb form changed between the lists, the same Item-ID was used for all lists. Thus, each of the 15 Items consisted of two levels of Speed in three levels of Verb Form. Due to the reasons mentioned in the Results Section of Experiment 1, a sensitivity analysis was conducted, which included Block as fixed factor (Block 1: *M* = 1234 (*SD* = 389); Block 2: *M* = 1314 (*SD* = 427); Block 3: *M* = 1348 (*SD* = 437); see Figure 5.5 for RDs of all cells, and Appendix D.4 for the results of the sensitivity analysis). Including Block did not affect the results as compared to the preregistered analysis in a meaningful way.

Figure 5.5. ***Mean reproduced durations in Experiment 2 as a function of Speed and Stimulus Duration per Block and Verb Form.***



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). PP stands for the personal pronoun. Reprinted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), https://doi.org/10.1080/20445911.2021.1950736, p. 887. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/).

The random effects structure was determined as in Experiment 1. The random effects structure with the best AIC value had random intercepts for Subjects and Items, random slopes for the factors Speed and Stimulus Duration for Subjects, and random slopes for the factor Speed for Items. The resulting full model was RD ~ Stimulus Duration + Speed + Number of Characters + Verb Form + Speed * Verb Form + (Speed + Stimulus Duration | Subjects) + (Speed | Items).

Due to the same reasons as outlined in the analysis of Experiment 1, *p*-values were obtained using the Satterthwaite's approximation for degrees of freedom of the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R (Version 4.0.3, 2020).

The full model suggests an intercept of $\beta = 1273.64$ ms. The estimated coefficients of Stimulus Duration ($\beta = 564.00$, $SE = 35.06$, $t(58.87) = 16.09$, $p < .001$) and Number of Characters ($\beta = 7.71$, $SE = 2.75$, $t(29.99) = 2.80$, $p = .009$) were significant, replicating the effect of physical duration and physical size on perceived duration. There was a main effect of the Verb Form *inflected with personal pronoun* ($\beta = 48.60$, $SE = 11.49$, $t(162.19) = 4.23$, $p < .001$), but no main effect of the Verb Form *inflected without personal pronoun* ($\beta = 0.39$, $SE = 8.96$, $t(5632.27) = 0.04$, $p = .965$). Interestingly, neither the effect of Speed (with *fast* as reference category) for the reference Verb Form *infinitive* ($\beta = 13.01$, $SE = 13.07$, $t(33.66) = 1.00$, $p = .326$), nor the interaction of Speed and the Verb Form *inflected with personal pronoun* was significant ($\beta = -12.31$, $SE = 12.32$, $t(15660.86) = -1.00$, $p = .318$). However, the interaction of Speed and the Verb Form *inflected without a personal pronoun* was significant ($\beta = 24.55$, $SE = 12.33$, $t(15460.24) = 1.99$, $p = .046$). Thus, the factor Speed only modulated the Verb Form *inflected without a personal pronoun*.

Speed was not significant for the reference Verb Form *infinitive*. Numerically, however, RDs of slow-speed verbs were not lower – as predicted – but higher than RDs of fast-speed verbs. To rule out the possibility that the result in Experiment 2 is a false negative result with an actual effect that is comparable to the one observed in Experiment 1, a simulation-based post hoc power analysis was performed using the R package *simr* (Green & MacLeod, 2016). Of 1000 datasets simulated from the full model but with a Speed effect of 94.74 ms adopted from Experiment 1, 100% resulted in a significant

Speed effect according to *p*-values using the Satterthwaite's approximation for degrees of freedom. Thus, the power should be sufficient to detect an effect of Speed for isolated verbs in infinitive form, if it was as high as the one for manner of motion verbs in complex expressions (in Experiment 1).

However, the effect of implicit stimulus attributes on perceived duration could be smaller for single words than for complex verb phrases (such as in Experiment 1), but still meaningful. Therefore, the effect of Speed for the Verb Form *inflected without a personal pronoun* could serve as a better source for the effect size one would like to detect with a certain power. To obtain the estimated coefficient for Speed in Verb Form List 3, that is, for inflected verbs without personal pronoun, the LMEM was re-estimated with Verb Form List 3 as reference category. The full model including all coefficients is reported in Appendix D.5. The effect of Speed was significant for the reference Verb Form *inflected without a personal pronoun* (β = 37.56, *SE* = 13.30, *t*(34.06) = 2.82, *p* = .008). This estimate was applied in 1000 simulations from the original model, again using the *simr* package. Of these, 77.50% resulted in a significant Speed effect according to *p*-values using the Satterthwaite's approximation for degrees of freedom. Thus, the power for detecting a Speed effect as high as the one for isolated inflected verbs (in Verb Form List 3) for isolated verbs in infinitive form (in Verb Form List 1) was concluded to be acceptable.

Mean CV was calculated for each Participant, Stimulus Duration, Verb Form, and both levels of Speed by dividing the standard deviation by the mean of the respective cell. Mean CV for a Stimulus Duration of 900 ms was 0.23, mean CV for a Stimulus Duration of 1100 ms was 0.21, and for 1300 ms it was 0.20.

Participants performed well in the memory task. The mean hit rate was 91.88%. The false alarm rate for new items was 5.63%.

Discussion

Even though motion verbs in infinitive form presented without context have a high salience of speed information, RDs did not reflect the pattern that was reported for physical speed, that is, longer RDs for fast than for slow motion (Brown, 1995; Kanai et al., 2006; Kaneko & Murakami, 2009; Karşılar et al., 2018; Linares & Gorea, 2015; Tomassini et al., 2011). We had expected to replicate the findings by Zhang et al. (2014), who observed an effect of implicit speed on perceived durations analogous to physical speed in a temporal bisection task (i.e., a DEIS). However, in the present experiment, the estimated coefficient for the factor Speed was not significant for the manner of motion verbs in infinitive form. Numerically, however, slow-speed verbs were reproduced longer, not shorter than fast-speed verbs, which does not correspond to the predicted pattern, but is reminiscent of the effect observed in Experiment 1.

To assess whether the effect of Speed on the perceived duration for verbs in infinitive form in fact behaved as observed by Mioni et al. (2015), that is, longer RDs for slow-speed than for fast-speed verbs, we conducted two post-hoc simulation based power analyses, one in which the effect of the factor Speed was assumed to be equivalent to the one observed in Experiment 1 and one in which it was assumed to be equivalent to the one observed for single manner of motion verbs inflected without personal pronoun. The second power analysis was conducted, since richer stimuli such as complex expressions might elicit deeper semantic processing compared to isolated words (Bedny & Caramazza, 2011; Miller et al., 2018), such that the effect of Speed for the Verb Form *inflected without a personal pronoun* might be a better estimate for the actual effect size. Yet, both power analyses indicated that the power was high enough to consider a false negative result as unlikely.

Inflected verb forms of the manner of motion verbs were instead predicted to have an effect on perceived duration analogous to Experiment 1. This was based on the assumption that the temporal saturation due to the inflection of the verbs might elicit the mental creation of a default path on which the motion takes place. As a consequence, inflected motion verbs would provide duration information, with longer events for slow-speed verbs than for fast-speed verbs. Thus, the factor Speed was expected to affect RDs analogously to Experiment 1: Just like physical duration, implicit duration should increase RDs. While RDs of inflected motion verbs presented without personal pronoun behaved in the predicted way, this was not the case for RDs of inflected motion verbs presented with a personal pronoun. Accordingly, temporal saturation cannot be considered the cause of the effect on RDs for the inflected motion verbs presented without a personal pronoun. The contribution of verb form for the establishment of a representation of the temporal structure of a linguistically expressed event remains to be investigated by future studies.

Personal pronouns were included to disambiguate third person singular from imperative plural, since these two inflections are identical for the German manner of motion verbs used as stimuli in this study. There was a significant main effect of the Verb Form *inflected with personal pronoun* most likely stemming from the fact that the personal pronouns systematically increased the length of all items compared to the other two Verb Forms. Moreover, inflected verbs with personal pronoun can be considered sentential units consisting of a pronominal subject and a predicate, while inflected verbs without personal pronouns and verbs in infinitive form are lexical units. This difference in complexity might be another reason for the significant main effect of the Verb Form *inflected with personal pronoun*.

Overall, the results suggest, that the dilation effect of speed only holds for physical, but not for implicit speed and is thus in conflict with the study by Zhang et al. (2014). With respect to the inflected manner of motion verbs without personal pronoun, the results of Experiment 2 corroborate the findings of Mioni et al. (2015), who observed an overestimation of the presentation duration of pictures associated with slow speed compared to pictures associated with fast speed.

Experiment 3

The results of Experiment 2 are in conflict with the results of Zhang et al. (2014). However, there are two differences between Experiment 2 and their study. Firstly, the stimulus material they used was slightly different since they did employ a smaller amount of stimuli and a wider range, that is, they used manner of motion verbs and adjectives conveying the two levels of speed, fast and slow. Secondly, they implemented a temporal bisection task, while participants in our experiment were asked to reproduce the duration of the presented expressions. To assess whether the difference in results can be attributed to the temporal tasks chosen, we replicated the temporal bisection task used by Zhang et al. (2014). In their study, participants were instructed to learn a short (i.e., 400 ms) and a long (i.e., 1200 ms) standard duration and were subsequently asked to indicate whether the presentation duration of fast-speed and slow-speed verbs and adjectives was closer to the short or the long standard duration. We included a larger amount of fast-speed and slow-speed verbs, but did not include adjectives to generate a more homogenous but larger group of items. We followed their experimental design as closely as possible. The experiment was preregistered on the Open Science Framework (von Sobbe, Reiber, et al., 2021).

In temporal bisection studies, the proportions of ‘long’ responses are taken as a measure of the perceived duration. Moreover, the temporal bisection point (TBP), just noticeable difference (JND) and Weber fraction can be estimated with help of logistic psychometric functions that are fitted for the proportions of ‘long’ responses. TBP, also referred to as the point of indifference (Maricq et al., 1981), indicates the duration at which participants are equally likely to give a ‘short’ or a ‘long’ response (Kopec & Brody, 2010). The TBP is the 50% point of the logistic psychometric function (Zhang et al., 2014). A lower TBP implies an overestimation of duration, since the participants judge the comparison durations as being ‘long’ earlier compared to a higher TBP. JND and Weber fraction are measures of the participants’ temporal discriminability. JND refers to the smallest duration that leads to a change in a participant’s behaviour (Kopec & Brody, 2010). It is calculated as the half difference between the durations at which 25%

and 75% ‘long’ responses are given, which are retrieved from the logistic psychometric function (Zhang et al., 2014). JND and Weber fraction, that is, JND divided by TBP, indicate the steepness of the psychometric function. A steeper psychometric function indicates good discriminability and results in a lower Weber fraction, while a higher Weber fraction indicates a more gradual psychometric function and a poorer discriminability (Kopec & Brody, 2010).

In the study by Zhang et al. (2014), participants overestimated the presentation duration of fast-speed verbs and adjectives compared to that of slow-speed verbs and adjectives. Firstly, this was detectable in a higher proportion of ‘long’ responses for fast-speed compared to slow-speed verbs and adjectives at a duration of 800 ms (but not at durations of 400, 600, 1000, and 1200 ms). Secondly, this overestimation of fast-speed verbs and adjectives was apparent in a lower TBP for fast-speed verbs and adjectives compared to the TBP of slow-speed verbs and adjectives. JND and Weber fraction were not affected by the level of Speed.

Method

Participants

We raised the number of participants from 32 in the study by Zhang et al. (2014) to 500 in the replication to increase statistical power. Due to a pre-defined exclusion criterion (see Procedure for details), we had to discard the data of 46 participants. Seven more participants were excluded since these participants’ response pattern revealed that they did not follow or understand the task instructions properly.[25] These fifty three participants’ data was replaced to reach the pre-determined sample size. Participants were native speakers of German (207 female, 289 male, and 4 with diverse gender) and

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[25] More specifically, three of these participants did not distinguish between the two levels of standard duration but between the two levels of Speed. The other four participants answered randomly without any clear pattern such that they were presumably simply clicking through the experiment. These participants were spotted since the TBPs of their fitted logistic functions were outside the range of the comparison durations. Since this additional exclusion of participants was not part of the pre-registered exclusion procedure, we conducted a sensitivity analysis including these participants, which did not change the results in a meaningful way (see Appendix D.6).

recruited using Prolific (www.prolific.co). They received reimbursement based on an hourly wage of 9 Euro (specifically, they were payed £1.10 for an experimental session of 8 min). The mean age was 28.73 years (*SD* = 8.68). 443 reported being right-handed, 46 were left-handed, and the remaining 11 reported being ambidextrous. They were naïve with respect to the purpose of investigation. All participants gave informed consent.

Table 5.2. ***Manner of motion verbs for Experiment 3.***

Slow-speed verbs					Fast-speed verbs				
Verb	Rating	Letters	Syllables	Frequency	Verb	Rating	Letters	Syllables	Frequency
gehen	3.34	5	2	6	rasen	6.55	5	2	14
hinken	1.74	6	2	15	hasten	5.41	6	2	17
wanken	2.00	6	2	16	rennen	5.85	6	2	12
tapsen	2.00	6	2	18	hetzen	5.82	6	2	14
humpeln	1.78	7	2	18	huschen	4.92	7	2	16
taumeln	1.87	7	2	16	spurten	5.66	7	2	18
torkeln	1.92	7	2	18	brausen	5.67	7	2	16
bummeln	2.00	7	2	15	stürmen	6.16	7	2	13
trotten	2.38	7	2	17	flitzen	6.32	7	2	15
latschen	2.26	8	2	18	preschen	5.77	8	2	16
stiefeln	2.85	8	2	19	sprinten	6.36	8	2	16
M	2.20	6.73	2.00	16.00		5.86	6.73	2.00	15.18
SD	0.50	0.90	0.00	3.58		0.47	0.90	0.00	1.78

Note. *Rating* refers to the mean rated speed on a 7 point Likert scale (1 representing *very slow*, 7 representing *very fast*). *Letters* stand for the number of characters of the verbs in the infinitive verb form. *Frequency* refers to the verbs' frequency class obtained from Leipzig Corpora Collection (2018). Adapted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), p. 891. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/). English translations of the verbs are provided in the published version, which can be found online at https://doi.org/10.1080/20445911.2021.1950736.

Apparatus and Stimuli

The experiment was programmed in PsychoPy v2020.1.3 and was run online via Pavlovia.org. The standard visual stimuli for the training session were white filled squares presented against a black background in the centre of the screen (scaling factor 0.1). Comparison stimuli were 11 slow-speed and 11 fast-speed verbs in infinitive form (see Table 5.2) that were selected from the rating study conducted for Experiment 1 (see Appendix D.1 for details). The verbs in the two levels of Speed did not differ in number of characters or number of syllables. Moreover, the frequency was not significantly different for the two levels of Speed ($t(14.66) = 0.68$, $p = .508$). The words were presented in the centre of the computer screen in white against a black background (Arial; scaling factor 0.04). The keys ‘d’ and ‘k’ were used as response buttons.

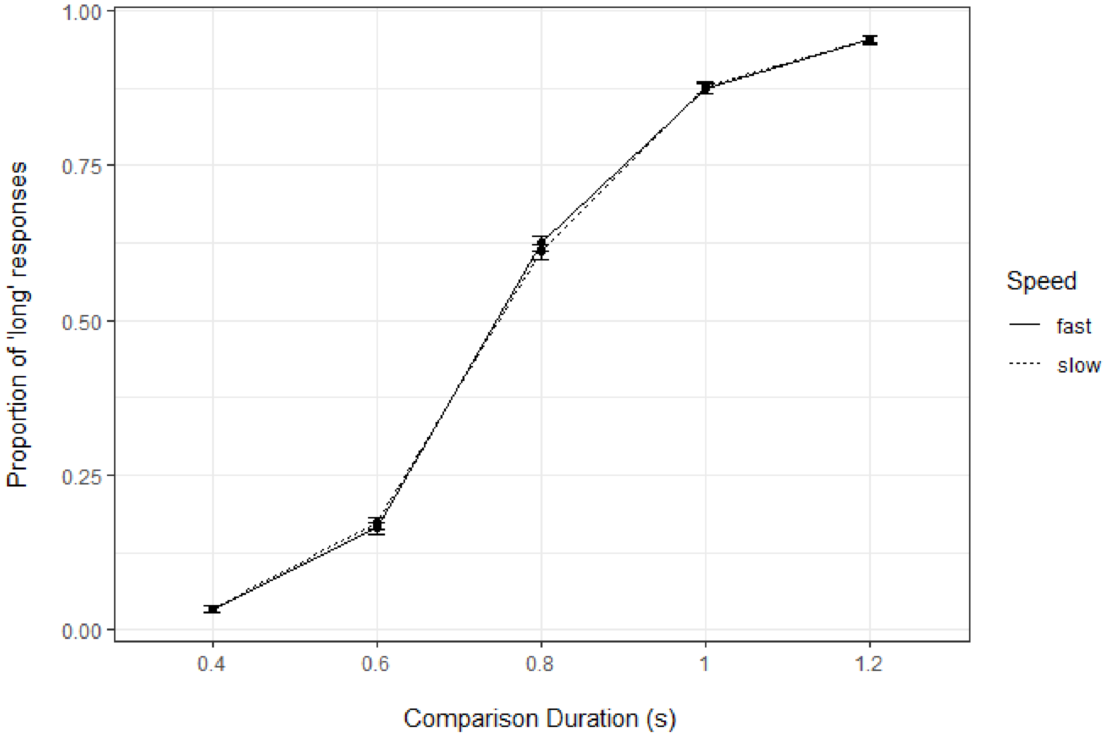
Procedure

Just like Zhang et al. (2014) we had the two factors Speed (*fast* vs. *slow*) and Comparison Duration (400, 600, 800, 1000, and 1200 ms) in a within-subjects design. In the study by Zhang et al. (2014) all stimulus words consisted of two Chinese characters. In our stimulus material, the number of characters was identical for the two levels of Speed. Yet, there was equal variation in the number of characters within each level of Speed. More specifically, both levels of Speed consisted of the same amount of verbs with five to eight characters (see Table 5.2 for details). However, in a temporal bisection study by Karşılar and Balcı (2019), no effect of physical stimulus size on perceived duration was observed when the stimuli contained or implied symbolic meaning. Since this also applies to the stimuli of the present experiment, no modulation of perceived duration by the number of characters was expected.

One experimental session consisted of a training session and a test session. Prior to the training session, participants were told to keep in mind a short (400 ms) and a long (1200 ms) presentation duration of a white square, i.e., the visual standard stimulus. Both durations were presented to them once for the purpose of acquaintance. In the training session, participants were asked to discriminate these two durations and to give their response with one of the two response buttons, ‘d’ (i.e., ‘short duration’) and ‘k’ (i.e., ‘long duration’) with the index finger of the left and right hand, respectively. The training session consisted of the white square presented for the short (400 ms) or the long (1200 ms) duration, with each duration being repeated five times in randomised order.

Subsequent to the presentation of the square, a red exclamation mark ('!') appeared on the screen, which prompted the participants to give a response. The ITI was 1 s. If participants failed to reach an accuracy of at least 80% in the training session, this was taken as an indication that they did not understand the instruction. Their data was thus excluded from the analysis.[26]

Figure 5.6. ***Mean proportion of 'long' responses in Experiment 3 as a function of Speed and Comparison Duration.***



Note. Error bars represent 95% confidence intervals of the within-subject standard error (Morey, 2008). Reprinted from "Speed or duration? Effects of implicit stimulus attributes on perceived duration" by L. von Sobbe, C. Maienborn, F. Reiber, E. Scheifele, and R. Ulrich, 2021, *Journal of Cognitive Psychology*, *33*(8), https://doi.org/10.1080/20445911.2021.1950736, p. 892. Creative Commons Attribution 4.0 International License (CC BY) (https://creativecommons.org/licenses/by/4.0/).

Before the test session, the participants were told that they were now going to see words instead of a square and that their task was to indicate via keypress ('d' and 'k')

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[26] Note that accuracy in the training session of all participants in Zhang et al. (2014) was 100%.

whether the presentation duration of the word was closer to the short or the long standard duration. They initiated the test session with the space bar. Like in the training session, a red exclamation mark appeared subsequent to the stimulus as a prompt to respond. ITI was identical to the training session. Participants were asked to focus the centre of the screen throughout the experiment. One test session consisted of 110 trials (i.e., 22 verbs × 5 comparison durations) that were presented in randomised order.

Results

Mean accuracy in the training session was 92.21% (*SD* = 17.02).

Analogous to Zhang et al. (2014), the proportions of 'long' responses were calculated for each level of Speed and each Subject (see Figure 5.6). TBP, JND, and Weber fraction were calculated as outlined in Zhang et al. (2014), that is, based on logistic psychometric functions fitted for 'long' responses for each level of Speed against the comparison durations for each participant using the *quickpsy* function in R (Linares & López-Moliner, 2016). Repeated-measures ANOVAs were calculated on the proportions of 'long' responses, TBP, JND, and Weber fraction using the *ezANOVA* function in R (Arnhold, 2013).

On the proportions of 'long' responses, a 2 (Speed) x 5 (Comparison Duration) repeated-measures ANOVA was performed. Like in the study by Zhang et al. (2014) the main effect of Comparison Duration was significant [$F(4, 1996) = 6033.77$, $p < .001$, $\eta_p^2 = .92$, *p*-value was Greenhouse-Geisser adjusted]. However, unlike in the study by Zhang et al. (2014), neither the main effect of Speed [$F(1, 499) = 0.06$, $p = .801$, $\eta_p^2 < .01$], nor the interaction of Speed and Comparison Duration [$F(4, 1996) = 2.04$, $p = .104$, $\eta_p^2 < .01$, *p*-value was Greenhouse-Geisser adjusted] was significant.

Different to Zhang et al. (2014), who reported a significant effect of Speed on TBPs, the three one-way ANOVAs calculated on TBP, JND, and Weber fraction in the present study indicated that none of these measures was influenced by Speed. More specifically, the mean TBP for fast-speed verbs was 771 ms (*SD* = 101), while the mean

TBP for slow-speed verbs was 772 ms (*SD* = 102) [$F(1, 499) = 0.16$, $p = .690$, $\eta_p^2 < .01$]. Mean JND for fast-speed verbs was 110 ms (*SD* = 69); mean JND for slow-speed verbs was 111 ms (*SD* = 68) [$F(1, 499) = 0.34$, $p = .558$, $\eta_p^2 < .01$]. Weber fraction for fast-speed verbs was M = 0.14 (*SD* = 0.10); Weber fraction for slow-speed verbs was M = 0.14 (*SD* = 0.09) [$F(1, 499) = 0.02$, $p = .899$, $\eta_p^2 < .01$].

Discussion

Unlike Zhang et al. (2014), neither the proportions of 'long' responses nor the TBP were modulated by the level of speed associated with the manner of motion verbs. Despite the increased power in the current experiment (500 participants compared to 32 in the study by Zhang et al., 2014), we could not replicate the original findings. Moreover, mean TBP and Weber fraction in the present experiment broadly adhere with previous temporal bisection studies (Kopec & Brody, 2010; Wearden & Lejeune, 2008). Thus, participants seem to have performed the task as required.

The results of Experiment 3 are in line with the results of Experiment 2 despite the use of a different temporal task. The associated level of speed of verbs in infinitive form does not affect perceived duration like physical speed, for which an overestimation of fast compared to slow motion was observed (Brown, 1995; Kanai et al., 2006; Kaneko & Murakami, 2009; Karşılar et al., 2018; Linares & Gorea, 2015; Tomassini et al., 2011). Thus, we assume that the finding by Zhang et al. (2014) might either reflect a false positive finding, or the effect they observed was driven by specific items of their stimulus material and thus would not generalise to the present stimulus material.

General Discussion

By implementing both duration reproduction tasks (Experiments 1 and 2) and a temporal bisection task (Experiment 3), the present study investigated whether mental images elicited by complex linguistic stimuli affect perceived duration and, if so, how different aspects of the expressions contribute while doing so. More precisely, in Experiment 1 we presented expressions that denoted short and long events yielded by two levels of speed associated with manner of motion verbs while keeping the path constant. Therefore, the expressions contained information about two competing attributes that both affect perceived duration when they are physically present: duration and speed (Matthews & Meck, 2016). Previous studies have shown that an increase in physical duration and faster physical speed both lead to an increase in RDs (Brown, 1995). In the expressions employed in Experiment 1, fast-speed verbs implied a shorter event than slow-speed verbs, since covering a certain path with a slow movement takes longer than covering the same path with a fast movement. Thus, depending on which source of information was more salient for the generation of the mental image, different patterns of RDs were expected.

Indeed, RDs were modulated by the linguistic expressions in Experiment 1, such that expressions with fast-speed verbs had shorter reproduction times than expressions with slow-speed verbs. This pattern of results corresponds to an effect that is attributable to implicit duration information, but not to implicit speed information, for which – analogous to physical speed – longer RDs for expressions with fast-speed than slow-speed verbs were predicted (see Wang & Gennari, 2019, for converging results on language-mediated temporal memory distortions).

In Experiment 2, we presented only the manner of motion verbs of Experiment 1 to control whether implicit speed in fact has an analogous effect to physical speed when it is not competing with duration information. However, the speed information given in isolated manner of motion verbs in infinitive form did not modulate RDs. Since this finding is in conflict with the study by Zhang et al. (2014), we replicated their temporal bisection task in Experiment 3 to assess whether the difference in results was due to the temporal tasks chosen. Yet, the replication of the study by Zhang et al. (2014) was unsuccessful. The associated speed of verbs in infinitive form did not modulate perceived duration, irrespective of the task that was implemented.

Beyond the assessment of an effect of implicit speed on perceived duration, this finding is valuable for methodological reasons. Even though temporal bisection and temporal reproduction tasks have been employed in distinct studies that investigate the same non-temporal stimulus attribute effect (see Cai & Wang, 2014 for a temporal reproduction task; see Oliveri et al., 2008 for a temporal bisection task to investigate the effect of numerical magnitude on perceived duration), there is a lack of a direct comparison in the literature between the two tasks when assessing the effects of non-temporal stimulus attributes on perceived duration. The results of Experiment 2 and 3 suggest that both tasks can be used interchangeably without producing different results.

Mioni et al. (2015), who also implemented a temporal bisection task when investigating the effect of images of vehicles associated with either fast or slow speed on perceived duration, observed an opposite pattern to Zhang et al.'s (2014) study, that is, an underestimation of fast compared to slow speed. This is in line with the pattern of RDs for manner of motion verbs inflected in third person singular without personal pronoun in Experiment 2 of this study. Interestingly, we observed a null-effect for manner of motion verbs inflected in third person singular *with* personal pronoun, analogous to the verbs in infinitive form in Experiment 2. One might hypothesise that the inflected verbs without personal pronouns were confused with imperative plural by the participants due to their identical phenotype. Since imperative has a high action relevance and directly addresses the reader, this might be the reason why the mental image was strong enough to yield an effect of implicit speed on RDs. Future studies will have to investigate whether there is a systematic difference in the depth of mental images elicited by third person versus imperative verb phrases.

However, it remains unanswered, why implicit speed does not modulate RDs like physical speed, for which an overestimation of fast compared to slow speed has been reported (Brown, 1995; Kanai et al., 2006; Kaneko & Murakami, 2009; Karşılar et al., 2018; Linares & Gorea, 2015; Tomassini et al., 2011). In case of the complex expressions in Experiment 1, the participants are faced with linguistically expressed events. According to Zacks and Tversky, events are "a segment of time at a given location that is conceived by an observer to have a beginning and an end" (Zacks & Tversky, 2001) and are recognised on the basis of their temporal structure. Thus, temporal information is essential for the perceiving, thinking, and talking about events (Zwaan & Radvansky, 1998). This offers a straightforward explanation why RDs in Experiment 1 were modulated according to duration, but not speed information: The expressions are processed as a particular instance of a motion event and are encoded in working memory indexed according to their temporal information (Zwaan & Radvansky, 1998).

With respect to the manner of motion verbs in infinitive form in Experiments 2 and 3, on the contrary, linguistically speaking participants are faced with atelic processes but not events. However, the mental imagination of atelic motion without a particular bounding in space and time is challenging, if not impossible. This might be the reason why an effect of linguistically expressed speed analogous to physical speed on perceived duration could not be traced. It might simply be too difficult to imagine the denoted motion without the mental generation of a default path on which the motion takes place, which then again produces duration information. This would also explain the pattern of RDs observed for inflected manner of motion verbs without personal pronoun in Experiment 2, that is, longer RDs for slow-speed than for fast-speed verbs, a pattern that is in accordance with inferred duration.

Is it possible that the results of the present study are influenced by the understanding of the experimental aims by the participants? Indeed, some participants identified the object of investigation as indicated by a follow-up survey subsequent to the experimental sessions in Experiments 1 and 2, while other participants assumed our cover story to be true, which was designed to foster the engagement with imagery processes (see the Procedure Section of Experiment 1 for details). Yet, even though some participants in both experiments assumed that we were investigating an effect of associated speed on perceived duration, the outcome of the two experiments are not identical. Thus, we consider it unlikely that the results are only explicable by means of a conscious manipulation by the participants. However, we acknowledge that this is a potential draw-back of our paradigm, which could be addressed better in future studies, for example, by including filler items to mask the purpose of the study altogether.

Against the background of grounded cognition, our findings provide mixed results. The mental representation of speed expressed via single manner of motion verbs in infinitive form is not as analogous to the mental representation of physical speed as suggested by strong versions of embodied cognition, since it does not affect perceived duration as physical speed does. Linguistically expressed duration, on the other hand, affects RDs analogous to actual duration. The results of Experiment 1 on more complex phrases in conjunction with the results of Experiments 2 and 3 on single words corroborate the assumption that richer linguistic stimuli elicit deeper semantic processing and are thus more likely to produce activation of the associated modal representations (Bedny & Caramazza, 2011; Miller et al., 2018), since only complex expressions, but not verbs in infinitive form had an effect on perceived duration. Moreover, even though the kinematic information is specifically salient in the expressions of Experiment 1, the modal representations are not reduced to the corresponding motoric simulation, but also contain the compositional integration of the path. Thus, the underlying simulations reproduce the described events in their temporal structure, that is, longer events for slow motion than for fast motion when the path is kept constant, which yields nuanced representations of the events' durations denoted by the linguistic expressions.

In summary, our results suggest that complex linguistic expressions have the potential to act on the internal clock analogous to the physical counterparts that are denoted by these expressions. More specifically, the duration of the events denoted by the present study's expressions affect RDs analogous to physical duration, that is, longer RDs for longer events compared to shorter events. Even though the modulation of duration depends on the underlying speed information, linguistically expressed speed does not affect RDs like physical speed. This might be related to the difficulty of imagining speed without being bound in time and space and detaching speed from duration information.

Chapter 6

Summary and conclusions

The involvement of modal representations in the processing of temporal information given in linguistic expressions was investigated in the present dissertation in three approaches. Firstly, the activation of the mental timeline during the processing of deictic and sequential temporal information as a function of the saliency of the concept of time was assessed across multiple studies in a meta-analysis in Chapter 2. Secondly, it was investigated whether the speed effect initially observed by Wender and Weber (1982) reflects an effect that is attributable to the simulation of the described events' duration in Chapters 3 and 4. Thirdly, reproduced durations were used as a measure to test whether implicit duration information given in linguistic expressions affects perceived duration analogously to actual duration in Chapter 5. The results, which will briefly be summarized in the following section, give insights with respect to the automaticity of modal representations in language processing and the conditions under which an activation occurs. Furthermore, methodological implications can be drawn from the present dissertation and the question of replicability is addressed at various instances. In the following, the results of Chapters 2 to 5 will be discussed with respect to these key points.

Summary

The results of the meta-analysis in Chapter 2 suggest that the mental timeline can be considered to have a cognitive reality in experiments in which time is task relevant with a mean effect size of $d = 0.46$. This implies that space is used to order events and to reason about the temporal reference of an entity. Yet, importantly, the mental timeline is not activated in tasks in which time is not task relevant, which speaks against an automatic activation of spatial associations upon the processing of linguistically expressed sequential and deictic temporal information. The implication of the surprisingly high mean effect size of $d = 0.47$ for temporal priming studies (i.e., $d = 0.36$ after adjustment for publication bias) will be discussed below.

The results of Chapters 3 and 4 indicate that the speed effect that was initially reported by Wender and Weber (1982) is replicable – even without the prompt to engage with mental imagery. More specifically, when participants were instructed to detect motion in a sentence, slower responses to slow-speed sentences compared to fast-speed sentences were observed for human as well as for object motion sentences and for different verb pairs and technical implementations. However, analogous to what was observed in the meta-analysis in Chapter 2, the speed effect was absent in a sensicality judgement task, speaking for a task-dependent, non-automatic activation of modal representations, which is not necessary for language processing.

Unfortunately, the question of whether the speed effect reflects an effect that is attributable to the simulation of the described events' duration could not be answered unambiguously by the results of Chapter 4. More specifically, the denoted travelled distances were not reflected in reaction times (RTs) as expected, i.e., longer RTs for long-distance sentences than short-distance sentences. Yet, we did not observe a null-effect for Distance. Instead, RTs were in fact significantly shorter for long-distance sentences than for short-distance sentences. This allows for the post-hoc consideration that there might be a systematic difference in the processing of short-distance and long-distance sentences due to the different timescales they entail. More precisely, the shorter denoted durations of short-distance sentences might allow for a more detailed situation model build-up leading to longer RTs compared to long-distance sentences. Post-hoc analyses of the data furthermore indicate that within each timescale RT increases with an increase of the denoted travelled distance as rated in a pre-study. Taken together, this indicates that duration might be taken into account in the modal representations, yet in a more complex and interactive way than initially expected. This assumption, however, would have to be verified by future experiments due to the post-hoc nature of these considerations and analyses.

The attempt of Chapter 5 to test whether implicit duration information affects perceived duration analogously to actual duration turned out to be fruitful. More precisely, reproduced durations were affected in such a way that they increased with an increase in implied duration. This suggests that complex linguistic expressions can act on the internal clock analogous to the physical counterparts of these expressions' referents. Interestingly, linguistically expressed speed did not affect perceived duration like physical speed even though the manipulation of the factor Speed was responsible for the observed effect of duration in Experiment 1 of Chapter 5. This was indicated by the results of two experiments implementing two different temporal judgment tasks, i.e., a duration reproduction task and a temporal bisection task (Experiments 2 and 3 of Chapter 5). The absence of an effect of Speed could either be due to the lower linguistic complexity of the stimulus material used for testing for an effect of Speed or might be related to the difficulty of mentally detaching speed from duration information and imagining speed without being bound in time and space.

In the following, these results will be discussed in more detail within the context of the broader topics they are associated with.

Automaticity

The view that modal representations are not necessary for language processing, for example, because they are not essentially needed to recognise or understand words (for a summary of arguments see Mahon, 2015) is becoming more prevalent in the literature (Barsalou, 2020). Yet, even if modal representations were not indispensable for conceptual processing, it is possible that they are activated automatically. For instance, language-space associations are activated even in tasks in which it is not relevant to process the meaning of implicit location words, i.e., words whose referents are associated with the upper or lower space (Lachmair et al., 2011). This suggests that there is an automatic (re)activation of the typical locations in which the presented nouns' referents appear (Vogt et al., 2019). Yet importantly, it is not a given that automatic reactivations of experiential traces during language processing are functionally relevant for comprehension (see Strozyk et al., 2019 for counterevidence). An automatic activation of modal representations during language processing that does not represent an integral part of concept representation could, for instance, be explained by spreading activation from central cognitive representations to input and output representations (Mahon, 2015).

In the present dissertation, the question of automaticity was dealt with in Chapters 2 and 3 by assessing whether the space-time congruency effect can be assumed to be present in tasks in which the concept of time is a task-irrelevant dimension and by investigating whether the speed effect of Experiment 1 in Chapter 3 can be observed in a sensicality judgment task. Yet, for both effects an automatic activation is not supported by the results of the respective studies. The estimated effect size of the space-time congruency effect does not significantly deviate from zero when time is not a task-relevant dimension (Chapter 2). Similarly, faster responses to fast-speed sentences than to slow-speed sentences were not observed when the participants were asked to judge the sentences' sensicality instead of deciding whether the sentences expressed motion (Chapter 3). This finding gives support to the assumption that the activation of modal representations is modulated by context and task (Lebois et al., 2015b, 2015a). The pattern portrayed by the results of the meta-analysis and Experiments 1 and 2 of Chapter 3 is in accordance with a salience-of-stimulus-dimension account (Dudschig & Kaup, 2017). When a stimulus dimension is made salient (i.e., time or motion, respectively) a corresponding effect can be observed, however it vanishes when this dimension is not made salient by the task (Lebois et al., 2015a).

Yet, there are three observations that point to a different direction, i.e., that modal representations of temporal information might be activated automatically. Firstly, the weighted mean effect size of the space-time-congruency effect in temporal priming studies is considerably high – even after correction for publication bias. This finding is surprising since priming prior to the execution of a spatial task should make time a task-irrelevant dimension. As discussed in Chapter 2, the high weighted mean effect size of temporal priming studies might be due to secondary tasks in which time is made salient, thus diminishing the notion that time really is task irrelevant. Alternatively, it might be related to the longer time period given between prime and response in temporal priming studies compared to stimulus and response in experiments of the category *time is task irrelevant*, thus fostering the build-up of the mental timeline. However, since there are too few temporal priming studies in which time is not put into focus by a secondary task, the weighted mean effect size for temporal priming studies has to be treated with caution, which is also suggested by its relatively large confidence interval.

Secondly, a subdivision of the studies of the category *time is task irrelevant* in Chapter 2 suggests that the mental timeline might be activated automatically in experiments in which time is not made salient by the task given that temporal complexity is high. We hypothesised that the mental timeline in these cases serves the purpose to facilitate the ordering of complex temporal information. The idea that richer linguistic stimuli might foster the involvement of modal representations in language processing is also discussed in the context of action verb processing (Bedny & Caramazza, 2011). However, it is not clear whether one can really speak of automatic activation of modal representations in a strict sense when it is due to rich linguistic contexts. This is because rich linguistic contexts most likely elicit the build-up of a situation model (see General Discussion of Chapter 3 for details) in which “situated simulations” (Zwaan, 2016, p. 1031) can be formed. Importantly though, situation models are assumed to be part of a cognitive process higher than lexical-semantic retrieval (Meteyard et al., 2012, p. 795; Zwaan, 2016). Thus, modal representations or situated simulations elicited as part of a situation model build-up would *succeed* the automatic lexical-semantic retrieval of word meaning (Zwaan, 2016). Consequently, even though the task does not make the concept of time salient in the above mentioned subdivision of *time is task irrelevant* studies, the activation of the mental timeline does not appear to be automatic in a strict sense, but related to the ordering of complex temporal information with help from the mental timeline.

These considerations are not in line with the results of a recent RT study by Grasso et al. (2021), in which participants were presented with verbs and pseudo-verbs that were conjugated to past-tense or future-tense and were asked to decide whether the stimulus was a word or not by giving a response along the lateral axis. Consequently, time was not a task-relevant dimension in this study. Since they observed a space-time congruency effect across three experiments as long as the response required a movement but not if the response required a key press without movement, they concluded that movement is a key factor for the activation of the mental timeline. Moreover, they argue that their results provide evidence for an automatic activation of the mental timeline, even though the stimuli do not have a high temporal complexity. Yet, in two of their experiments, they used the primes ‘yesterday’ to precede past-tense words and ‘tomorrow’ to precede future-tense words.[27] The authors used these primes to reinforce the salience of temporal information. Thus, the observed congruency effect might reflect an activation of the mental timeline that is elicited by the high salience of temporal information induced by the primes on a cognitive higher level instead of being automatic. In Experiment 3, the authors acknowledge this issue by not showing any primes to the participants. Even though they replicate the space-time congruency effect under these conditions, caution should be taken in regards to their conclusion that the phenomenon they observe reflects an automatic activation. Firstly, the authors themselves state that “movement might be a key component underlying the activation of the mental timeline” (Grasso et al., 2021, p. 11) implying that the activation is elicited by a factor that is not connected to the lexical-semantic representation, i.e., the movement. Secondly, the stimulus material they used does not comprise filler items that, for example, could be conjugated to present tense. Moreover, even the pseudo-words, which they needed for the implementation of the lexical decision task, were conjugated to past-tense or future-tense. Thus, a strong polarity between past-tense and future-tense is opened up solely by the stimulus material, which, in combination with a movement along the left-right axis, might elicit the mapping of the two-dimensional trajectory resulting from the past-future-polarity onto the spatial left-right trajectory, which is made salient by the movement. Consequently, the results of Grasso et al. (2021) can be explained without recourse to the assumption of an automatic activation. Nonetheless, they provide valuable insights with respect to the factors that contribute to an activation of the mental timeline. Taken together, this debate demonstrates the high topicality of the issue and elucidates the need for follow-up investigations.

Thirdly, the post-hoc analysis in Chapter 4 hints to the possibility that linguistically expressed distance was reflected in RTs since for both, short-distance and long-distance sentences, there was a positive correlation between rated distance and RT when analysed separately. Distance, however, was not put into focus by the task as

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[27] The primes, which were always congruent with the verb’s tense, preceded all targets in Experiment 1 and only half of the targets in Experiment 2.

participants were only asked to detect motion in the presented sentences. Consequently, a stimulus attribute that was not made salient by the task appears to have been represented modally automatically in the investigated paradigm of Chapter 4. However, similar to the pattern discussed in the preceding paragraph with respect to complex temporal information, it might be the situation model build-up that is elicited by the task to detect motion which is also responsible for the activation of modal representation of other stimulus aspects, even though they are not focused by the task. It would be fruitful to combine a sensicality judgment task with the stimulus material used in Chapter 4 to investigate whether the same pattern emerges even if the task setting does not foster the activation of modal representations. Only then can automatic modal representation of linguistically expressed distance be discussed with firm resolution. Moreover, these considerations are based on post-hoc analyses, such that further experiments are needed to clarify the picture.

Taken together, the results of the present dissertation do not speak for an automatic activation of modal representations of temporal information in linguistic expressions. Instances that appear like automatic activation on first glance most likely are connected to other factors that elicit the build-up of a situation model, which is situated at a higher cognitive level and needs to be differentiated from automatically activated modality-specific representations.

Conditions for the involvement of modal representations

As indicated in the preceding section, the particular findings of the present dissertation that speak for an automatic activation of modal representations on first glance most likely are related to other factors that seem to elicit the build-up of a situation model. Importantly, situation models are assumed to be situated at a cognitive higher level than the automatic lexical-semantic retrieval of concept representations (Zwaan, 2016). Next to the salience of a stimulus attribute via the task, which was investigated in most depth in the present dissertation, other factors include the complexity of the stimuli, the time that is available for conceptual processing (see Speed & Vigliocco, 2014 for a similar account), and tasks that elicit modal representations with respect to other stimulus attributes, as outlined in the preceding section. Since these factors increase the information density and general complexity of processing compared to the mere retrieval of concept representation, it seems likely that the grounded cognition effects observed in the context of the present dissertation are not part of automatic lexical-semantic retrieval processes. Instead, the respective modal representations might be embedded in situation models, which could, for example, be built up to structure the described events in an efficient way or to increase task performance.

The notion that stimulus complexity increases the likelihood for the activation of modal representations is also corroborated by the results of Chapter 5, since complex expressions (Experiment 1) had an effect on perceived duration, but not verbs in infinitive form (Experiments 2 and 3). It has been argued that deeper semantic processing is more likely to activate associated modal representations (Bedny & Caramazza, 2011; Johnson-Laird, 1980; Louwerse, 2011; Louwerse & Jeuniaux, 2008, 2010), while symbolic or amodal processing is prevalent in early processing and in shallow understanding (Houghton & Klin, 2020, p. 11). For more complex or richer linguistic stimuli, on the other hand, the build-up of a situation model might become essential to construct an appropriate representation of the state of affairs denoted by the expressions (Zwaan & Radvansky, 1998). Both, the subdivision of the mental timeline studies of the category *time is task irrelevant* (see previous section on automaticity) as well as the results of Chapter 5 lend support to the assumption that richer linguistic stimuli are more likely to activate modal representations.

Sensicality judgment tasks, on the contrary, do not seem to elicit the activation of modal representations, since both the space-time congruency effect (Chapter 2) and the speed effect (Chapters 3 and 4) vanish when participants evaluate the presented sentences with respect to their sensicality. The assumption that neural activity is never epiphenomenal and should thus always reflect some function (Martin, 2016), suggests that the engagement of modal representations does not facilitate sensicality judgments while it might speed up evidence accumulation for judgments concerning the temporal reference of an item or the presence of expressed motion. Further, this implies that tasks that exclusively target the compositional integration of the lexical-semantic representations of all elements of the presented expressions, such as a sensicality judgment task, are completed well without recourse to modal representations. This again corroborates the notion that modal representations are not an integral part of the lexical-semantic representation of processed words and expressions.

Another factor that is related to the question of preconditions for the activation of modal representations is the explicit prompt to engage with mental imagery. Yet, the results of the present dissertation indicate that temporal judgment and motion detection tasks seem to be sufficient to activate modal representations and are not dependent on the instruction to mentally imagine the denoted referents. The role of explicit prompts to engage with mental imagery for the activation of modal representations was specifically investigated in Chapter 3 by conceptually replicating the study by Wender and Weber (1982), but eliminating the mental imagery instruction. Since we were able to replicate the speed effect under these conditions and since participants in the mental timeline studies generally are not instructed to mentally imagine the presented stimuli’s referents, explicit mental imagery does not seem to be a precondition for the involvement of modal representations in language processing in these dual-choice judgment tasks. This finding is particularly interesting, since the design of the two eye-tracking studies and the fMRI study (Lindsay et al., 2013; Speed & Vigliocco, 2014; van Dam et al., 2017) that also investigated an effect of the associated speed of fast-speed and slow-speed sentences allowed for the interpretation that the effect they observed might be driven by the execution of conscious mental imagery (see Introduction of Chapter 3 for details). Even though it is possible that participants in Experiment 1 of Chapter 3 consciously imagined the sentences’ content, they were asked to respond as fast as possible after reading the sentences at their own pace such that extensive conscious mental imagery can be expected to be rather reduced (Miller et al., 2018, p. 364; Zwaan, 2009, p. 1143).

In Experiments 1 and 2 of Chapter 5, on the contrary, we explicitly asked participants to imagine the described motion. This was done because perceived duration has barely been used as a measure to investigate effects of grounded cognition. Thus, it was not clear to what extent – if at all – complex linguistic expressions were able to affect duration judgments. Since we were specifically interested in the differentiation of implicit duration and speed information as a potential predictor for perceived duration, we used the instruction to engage with mental imagery as an assurance for the activation of modal representations. As both unconscious simulations that accompany language processing and conscious mental imagery articulate their content via sensorimotor areas (Iachini, 2011), explicit mental imagery constitutes a low-threshold possibility to get a first insight into differences concerning the content of the activated representations. Thus, even though the two processes, i.e., simulation and mental imagery, have to be differentiated with respect to other aspects such as short- vs. long-term memory, amount of detail and specificity, as well as consciousness and automaticity (Iachini, 2011), the use of mental imagery instructions in Chapter 5 allows for first insights into the characteristics of potential simulations. However, it shall not be concealed that follow-up experiments are needed in which participants are not instructed to imagine the denoted movements to investigate whether the same pattern emerges during mere language processing, i.e., that implicit duration, but not implicit speed affects perceived duration analogously to actual duration and physical speed, respectively.

To sum up, the results of the present dissertation lend support to the assumption that the salience of a certain stimulus attribute is a critical factor for the engagement of modal representations in language processing. Activated modal representations can thus be assumed to serve a cognitive purpose such as performing in a task efficiently. Consequently, the effects observed in the context of the present dissertation most likely cannot be considered artefacts of automatic cascading activation upon concept retrieval. Instead, they seem to be mediated by higher cognitive processes, which is also corroborated by the tendency that they can increasingly be observed in the context of higher temporal or linguistic complexity.

Methodological implications

As mentioned in the preceding paragraph, the fields of temporal cognition and grounded cognition have not been combined often. The first studies that investigated the effect of attributes of words' referents on perceived duration used single words as their stimulus material (e.g., Birngruber & Ulrich, 2019; Bottini & Casasanto, 2010; Ma et al., 2012; Zhang et al., 2014). In Experiment 1 of Chapter 5, we were able to show that not only implicit stimulus attributes of single words but also of more complex expressions can affect perceived duration. More specifically, the results of Experiment 1 in Chapter 5 suggest that there is a compositional representation of the expressions' referents since speed information associated with manner of motion verbs as well as the path on which the motion takes place is taken into account to yield an effect of implicit duration on perceived duration. Consequently, Chapter 5 creates a positive methodological outlook for the investigation of the grounding of linguistically expressed concepts in sensomotoric brain areas, as it shows that reproduced duration is a sensitive measure that can be used to detect the activation of modal representations.

This provides the field of grounded cognition with a method in addition to reaction time paradigms that are commonly implemented to test hypotheses behaviourally (Ostarek & Huettig, 2019). As outlined in the previous section, it would be important to modify the duration reproduction task in Chapter 5 such that participants are not explicitly asked to imagine the expressions' content. Against the background of the speed effect that was investigated in Chapters 3 and 4, which is observable independent of the task to engage with mental imagery, it seems worthwhile to assess whether the same applies to the duration effect observed in Chapter 5.

Replicability

A central concern of the present dissertation was to seek out grounded cognition effects that are reliable across operationalisations and task instructions as there is a problem of replicability in the field of grounded cognition (Ostarek & Huettig, 2019). Yet, it shall be noted that the so called replication crisis does not only apply to this specific field of research, but also to both research in Psychology more generally and to other disciplines as well (Zwaan et al., 2018). Reasons for the replication crisis may be questionable research practices due to a pressure experienced by researchers to publish statistically significant results (Zwaan et al., 2018), yet Ulrich and Miller (2020) show that it is the base rate of true effects that crucially determines the replicability rate within a field of research. Irrespective of the origin of the difficulty of replicating grounded cognition effects, it is important to identify reliable effects to be able to increase the base rate of true effects that then allow to deduce testable hypotheses to finally achieve scientific progress (Ostarek & Huettig, 2019; Ulrich & Miller, 2020; Zwaan, 2021).

The question of replicability is reflected in each chapter of the present dissertation. Firstly, a meta-analysis, as conducted in Chapter 2, specifically allows for the estimation of potential publication bias. Publication bias refers to the phenomenon that non-significant results are put into a researcher's file drawer and do not get published (Rosenthal, 1979; Ulrich et al., 2018). The results of Chapter 2 suggest that publication bias concerning the space-time congruency effect is small. Only for temporal priming studies and for experiments of the category *time is task irrelevant*, the estimated real effect size had to be adjusted slightly due to publication bias. However, publication bias was not suggested for the group of experiments in which time is task relevant. This implies that the space-time congruency effect reflects a stable effect that allows for extensions and variations of the original paradigm giving rise to further empirical and theoretical work (Zwaan, 2021). An example of this is provided by the recent study by Grasso et al. (2021), which was intensively discussed above with respect to the question of automaticity.

Secondly, the question of reliability was addressed by explicitly replicating previously conducted experiments in Chapters 3, 4, and 5. More specifically, Chapters 3 and 4 are based on the original finding of a speed effect by Wender and Weber (1982) as outlined in the Summary Section. Since we modified the task instruction, one cannot speak of a direct replication. Instead, we conducted conceptual replications, i.e., extensions to a previously used method (Zwaan, 2021). Zwaan (2021) stresses that conceptual replications are crucial to rule out the possibility that an effect is an artefact of the specific method used. The results of Experiment 1 in Chapter 3 and the experiment reported in Chapter 4 lend support to the assumption that this does not apply to the speed effect. More precisely, we were able to replicate the speed effect even when varying the instruction, using slightly different stimulus material, various pools of participants, and additional manipulations (see the Discussion Section of Chapter 4 for details).

Zwaan suggests that it would be an interesting case “if a certain category of operationalizations shows the effect while another does not” (Zwaan, 2021, p. 7). This indeed is the case for Experiment 2 of Chapter 3. The speed effect is not observable when participants are asked to judge the sentences’ sensicality. As discussed in the section on automaticity and conditions for the involvement of modal representations, this pattern indeed gives rise to theoretical considerations, as Zwaan (2021) proposes.

A further, rather loose conceptual replication was conducted in Experiment 2 of Chapter 5 of the original study by Zhang et al. (2014). This was done by not only varying the stimulus material but also the temporal task, which even entailed different dependent variables: while Zhang et al. (2014) used a temporal bisection task to investigate an effect of implicit speed on perceived duration, we conducted a duration reproduction task. Since the results of Experiment 2 in Chapter 5 were in conflict with the findings by Zhang et al. (2014) we moved towards a more direct replication in Experiment 3 that was identical to the study by Zhang et al. except for the stimuli’s language (German instead of Chinese) and the restriction of the stimulus material to manner of motion verbs. Even though conceptual replications are important to be able to draw inferences beyond the specific paradigm that is implemented, as outlined above, direct replications nonetheless provide a valuable contribution, since there is a bias against the null hypothesis for conceptual replications (Zwaan, 2021). This is apparent in Chapter 5: if we had not conducted the more direct replication in Experiment 3, we might have been misled to conclude that reproduced durations are not sensitive enough to detect a potential effect of implicit speed on perceived duration while being inclined to assume that the finding by Zhang et al. (2014) is reliable. However, as shown by the more direct replication in Experiment 3, it is unlikely that implicit speed affects perceived duration like physical speed.

Conclusion

In summary, a combination of conceptual replications with a potential recourse to more direct replications, as well as the use of a meta-analysis, turned out to be productive to progress in our understanding of the grounding of linguistically expressed temporal information. The results of the present dissertation suggest that deictic and sequential concepts of time, as well as duration information inferable from linguistic expressions, can be grounded in modal representations. More specifically, space seems to be cognitively exerted to sequentially order events and to reason about the temporal reference of an entity. However, the activation of the mental timeline seems to be non-automatic as it is only present in experiments in which time is made salient by the task. Temporal priming studies reflect an interesting borderline case of experiments in which time is not made specifically salient, yet the estimated real effect size is surprisingly high. Even though numerous studies have been conducted investigating the activation of the mental timeline, still more experiments are needed to clarify the role of temporal complexity for the mental timeline's activation and to specify the factors that contribute to the mental timeline's activation in temporal priming studies. This is another conclusion that has been gained with help of the meta-analysis of the present dissertation and that is already put into practice by follow-up studies (see Grasso et al., 2021).

The need for further empirical investigation became apparent also with respect to the grounding of duration information inferable from linguistic expressions. The speed effect that was first reported by Wender and Weber (1982) is a reliable effect, yet it is not entirely clear whether it reflects an effect that is attributable to the simulation of the described events' duration. Post-hoc inspection of the data in Chapter 4 suggests that more than just the speed information might be represented modally when the task is to detect motion in sentences, however, further experiments are needed to confirm this assumption. Like the mental timeline, the speed effect is modulated by the task; more specifically, it is dependent on the salience of the concept of motion.

The attempt to combine the field of temporal cognition with questions raised within the grounded cognition debate turned out to be productive. Linguistically expressed duration information seems to act on the internal clock like physical duration: reproduced durations were increased for longer events compared to shorter events denoted by complex linguistic expressions. However, since participants were asked to mentally imagine the linguistic expressions' referents, follow-up studies are needed to test whether an effect of linguistically expressed duration on perceived duration can be elicited by mere language processing and whether it is automatic.

Clearly, the present dissertation can only provide a fragmentary prospect and exemplary approach to the investigation of the grounding of linguistically expressed temporal information. Time can be expressed in language via numerous other ways and the present dissertation has focused on a specific and subtle manipulation of duration information, i.e., the constitution of different levels of duration via the combination of different levels of speed on a given path. Even though more explicit duration manipulations remain a yet to be investigated source of linguistically expressed temporal information, the present dissertation allows for valuable insights concerning the to be questioned automatic activation of modal representations in the investigated paradigms, the conditions under which an activation occurs and the reliability of the effects under discussion. Given the vast importance of temporal relations for correlating one's own behaviour with the events in the environment, it is surprising that grounded representations are not more prevailing in the processing of temporal information in linguistic expressions. In sum, the observed context-dependency of the effects investigated in the present dissertation on the one hand is indicative of the limited role that modal representations play for language processing. On the other hand, the robustness of the investigated effects in some of the here examined contexts speaks for the modal representations' contribution to task performance in those tasks, in which they are recruited.

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Appendix

Appendix A (refers to Chapter 2)

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Torralbo et al. (2006)	Flexible Conceptual Projection of Time Onto Spatial Frames of Reference	Experiment 1	Images of human head silhouette in side view & speech bubble in front of/behind silhouette containing past/future words	Is the person thinking of the past or the future?	time is task relevant	vocal ("pasado"/ "futuro")	past word - speech bubble behind silhouette; future word - speech bubble in front of silhouette	sagittal
		Experiment 2	Images of human head silhouette in side view & speech bubble in front of/behind silhouette containing past/future words	Is the person thinking of the past or the future?	time is task relevant	left/right keypress	past word - word appears on the left side of the silhouette/left keypress; future word - word appears on the right side of the silhouette/right keypress	lateral
Fuhrman & Boroditsky (2007)	Mental Time-Lines Follow Writing Direction : Comparing English and Hebrew Speakers	English Natives	Triplets of Pictures (early, middle & late picture of a situation); short as well as long time intervals. Prime: middle picture, target: earlier or later picture	Does the event depicted in the second picture happen earlier or later than the event in the first picture?	time is task relevant	left/right keypress	earlier picture - left keypress; later picture - right keypress	lateral
		Hebrew Natives	Triplets of Pictures (early, middle & late picture of a situation); short as well as long time intervals. Prime: middle picture, target: earlier or later picture	Does the event depicted in the second picture happen earlier or later than the event in the first picture?	time is task relevant	left/right keypress	earlier picture - right keypress; later picture - left keypress	lateral
Santiago et al. (2007)	Time (also) flies from left to right	Experiment	words (past & future); presentation on right or left side of the screen	Does the word refer to the past or the future?	time is task relevant	left/right keypress	past word - left keypress/word appears on the left side; future word - right keypress/word appears on the right side	lateral
Weger & Pratt (2008)	Time flies like an arrow: Space-time compatibility effects suggest the use of a mental timeline	Experiment 1	Actors of the early and late 20th century	Did the actor become popular before or after you were born?	time is task relevant	left/right keypress	actor popular before date of birth - left keypress; actor popular after date of birth - right keypress	lateral
		Experiment 2A	Prime: words (past & future); Target: white circles (right/left side of screen)	Indicate the location of the target with your corresponding index finger.	temporal priming	left/right keypress	past word - target appears on the left side & left keypress; future word - target appears on the right side & right keypress	lateral
		Experiment 2B	Prime: words (past & future); Target: white circles (right/left side of screen)	Indicate that you have detected the target by pressing the space bar with the index finger of your right hand.	temporal priming	one-finger keypress	past word - target appears on the left side & one-finger keypress; future word - target appears on the right & one-finger keypress	lateral

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Torralbo et al. (2006)	within-subjects	30	Spanish	$F1(1,29) = 11.963, p < .005$	3.4588	The F-value of the non significant Left-Right congruency effect is not reported (Torralbo 2006: 750), so cannot be considered in the meta-analysis.
	within-subjects	27	Spanish	Left-Right Congruency: $F1(1,26) = 8.16, p < .01$; Response Congruency: $F1(1,26) = 1.596, p = .21$	Left-Right Congruency: 2.8566; Response Congruency: 1.2633; Mean = 2.05995	The F-value of the non significant Front-Back congruency effect is not reported (Torralbo 2006: 753), so cannot be considered in the meta-analysis. For guaranteeing independence of subgroups, we took the mean of response and left-right congruency t-Values for an estimated overall effect size.
Fuhrman & Boroditsky (2007)	within-subjects	34	English	planned paired t-test: $t(33) = 1.73, p < .05$ (one-tailed)	1.73	
	within-subjects	34	Hebrew	planned paired t-test: $t(33) = 2.14, p < .02$ (one-tailed)	2.14	
Santiago et al. (2007)	within-subjects	32	Spanish	Meaning x Key: $F(1,31) = 9.14, p< .01$; Meaning x Screen Position: $F(1,31) = 2.63, p = .11$	Meaning x Key: 3.0232; Meaning x Screen Position: 1.6217; Mean = 2.3225	For guaranteeing independence of subgroups, we took the mean of Meaning x Key and Meaning x Screen Position t-Values for an estimated overall effect size.
Weger & Pratt (2008)	within-subjects	20	English	Age of actor x responding hand: $F1(1,19) = 3.12, p = .09$	1.7664	
	within-subjects	16	English	Cue type x Target side: $F(1,15) = 18.6, p < .01$	4.3128	Due to semantic satiation, post hoc tests were calculated by the authors for each half of the experiment. For the sake of simplicity, the overall interaction is used for the meta-analysis.
	within-subjects	18	English	Cue direction x Target side: $F(1,17) = 2.8, p = .11$	1.6733	Due to semantic satiation, post hoc tests were calculated by the authors for each half of the experiment. For the sake of simplicity, the overall interaction is used for the meta-analysis.

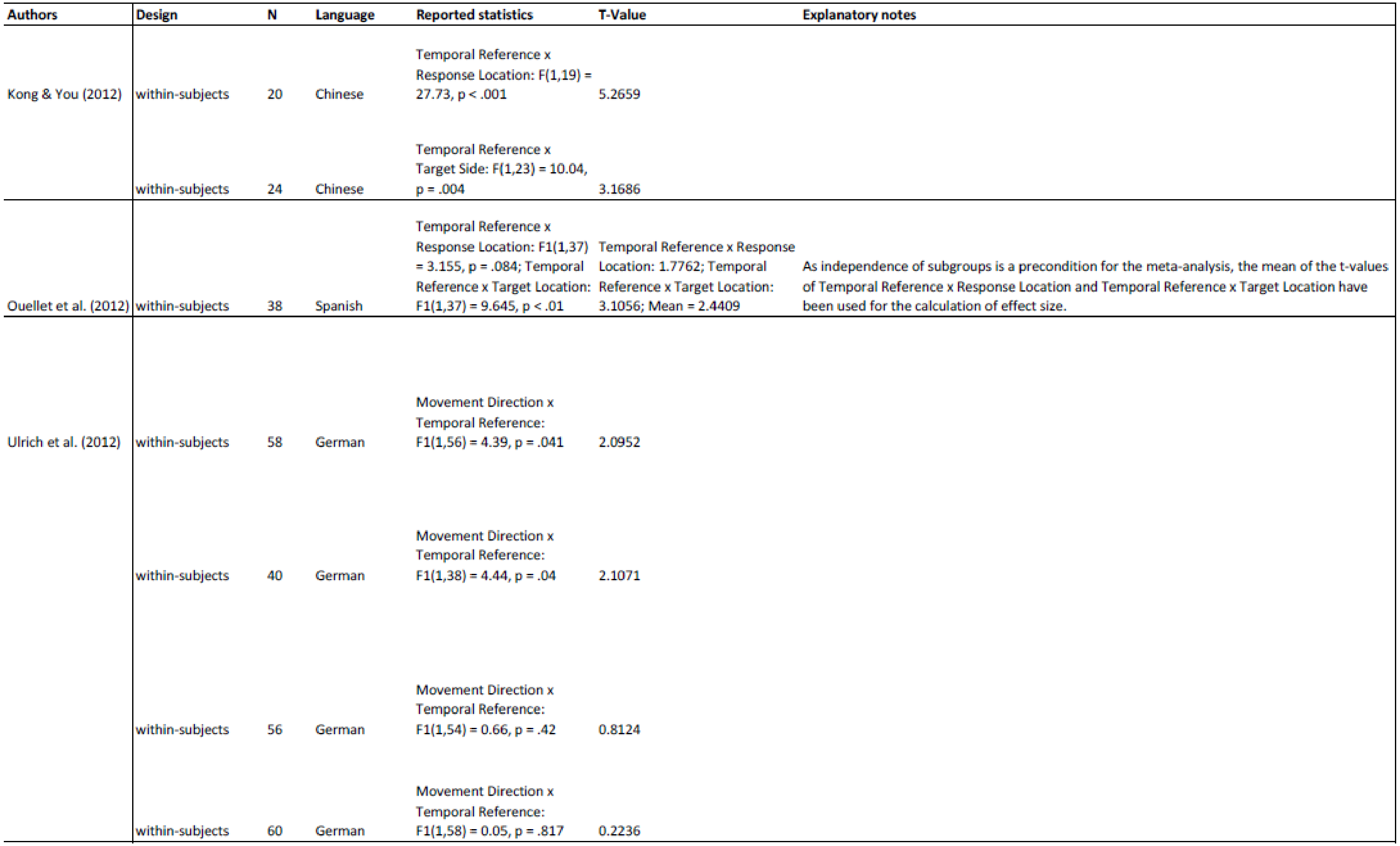
Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Ouellet, Santiago, Israeli et al. (2010)	Is the future the right time?	Spanish Natives	Words (future & past); auditive presentation on the left or right ear	Does the word refer to the past or the future?	time is task relevant	left/right keypress	past word - left keypress/auditive presentation on the left ear; future word - right keypress/auditive presentation on the right ear	lateral
		Hebrew Natives	Words (future & past); auditive presentation on the left or right ear	Does the word refer to the past or the future?	time is task relevant	left/right keypress	past word - right keypress/auditive presentation on the right ear; future word - left keypress/auditive presentation on the left ear	lateral
Ouellet, Santiago, Funes et al. (2010)	Thinking About the Future Moves Attention to the Right	Experiment 1	Prime: words (past & future), Target: white circle appearing in right or left box	Remember the word's temporal reference. Indicate, whether the circle appears in the left or right box by pressing the matching key ('z'/'m').	temporal priming	left/right keypress	past word - left keypress; future word - right keypress	lateral
		Experiment 2	Prime: words (past & future), Target: arrow appearing on left or right side, pointing to the left or the right (spatial Stroop task)	Remember the word's temporal reference. Indicate, the pointing direction of the arrow by responding with the matching key.	temporal priming	left/right keypress	past word - left keypress/target appears on the left side; future word - right keypress/target appears on the right side	lateral
Ulrich & Maienborn (2010)	Left-right coding of past and future in language: The mental timeline during sentence processing	Experiment 1	Sentences (past & future)	Indicate the temporal reference of the sentence. Refrain from responding when the sentence's content is nonsensical.	time is task relevant	left/right keypress	past sentence - left keypress; future sentence - right keypress	lateral
		Experiment 2	Sentences (past & future)	Indicate, whether the sentence makes sense or not.	time is task irrelevant	left/right keypress	past sentence - left keypress; future sentence - right keypress	lateral
		Experiment 3	Sentences (past & future); temporal cues at the end of the sentence	Indicate, whether the sentence makes sense or not.	time is task irrelevant	left/right keypress	past sentence - left keypress; future sentence - right keypress	lateral

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Ouellet, Santiago, Israeli et al. (2010)	within-subjects	20	Spanish	Separate ANOVAs: Temporal Reference x Response Location: F1(1,19) = 6.918, p = .0165; Temporal Reference x Target Location: F1(1,19) = 0.00524, p = .943	Temporal Reference x Response Location: 2.6302, Temporal Reference x Target Location: 0.07239; Mean = 1.3513	We thank Marc Ouellet for letting us know the results of the separate ANOVAs; they are not reported in the article. For guaranteeing independence of subgroups, we took the mean of Temporal Reference x Response Location and Temporal Reference x Target Location t-Values for an estimated overall effect size of the Spanish Group.
	within-subjects	28	Hebrew	Separate ANOVAs: Temporal Reference x Response Location: F1(1,27) = 0.788, p = .383; Temporal Reference x Target Location: F1(1,27) = 0.01001, p = .921	Temporal Reference x Response Location: 0.8877, Temporal Reference x Target Location: 0.1000; Mean = 0.4939	We thank Marc Ouellet for letting us know the results of the separate ANOVAs; they are not reported in the article. For guaranteeing independence of subgroups, we took the mean of Temporal Reference x Response Location and Temporal Reference x Target Location t-Values for an estimated overall effect size of the Hebrew Group.
Ouellet, Santiago, Funes et al. (2010)	within-subjects	22	Spanish	Temporal Reference x Target Location: F1(1,21) = 11.22, p < .01	3.3496	
	within-subjects	34	Spanish	Temporal Reference x Response Side: F1(1,33) = 4.56, p < .05, Temporal Reference x Target Location: F1(1,33) = 4.24, p < .05	Temporal Reference x Response Side: 2.1354, Temporal Reference x Target Location: 2.0591, Mean = 2.0973	For guaranteeing independence of subgroups, we took the mean of Temporal Reference x Response Side and Temporal Reference x Target Location t-Values for an estimated overall effect size.
Ulrich & Maienborn (2010)	within-subjects	30	German	Hand x Temporal Refernce: F(1,28) = 8.03, p < .01	2.8337	
	within-subjects	70	German	Response Hand x Temporal Reference: F(1,68) = 0.03, p = .866	0.1732	
	within-subjects	100	German	Response Hand x Temporal Reference: F(1,96) = 0.19, p = .668	0.4359	

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Boroditsky et al. (2011)	Do English and Mandarin speakers think about time differently?	Mandarin-English Bilinguals	Triplets of Pictures (early, middle & late). Prime: middle picture, target: earlier or later picture	Does the second picture show an earlier or later time than the first picture?	time is task relevant	left/right keypress; up/down keypress	earlier picture - left/up keypress; later picture - right/down keypress	lateral & vertical
Fuhrman et al. (2011)	How Linguistic and Cultural Forces Shape Conceptions of Time: English and Mandarin Time in 3D	Experiment 1: English Natives - lateral	Triplets of Pictures (early, middle & late of a situation); short and long intervals. Prime: middle picture, Target: earlier or later picture	Does the second picture show an earlier or later time than the first picture?	time is task relevant	left/right keypress	earlier picture - left keypress; later picture - right keypress	lateral
		Experiment 1: Mandarin Natives - lateral & vertical	Triplets of Pictures (early, middle & late of a situation); short and long intervals. Prime: middle picture, Target: earlier or later picture	Does the second picture show an earlier or later time than the first picture?	time is task relevant	left/right keypress; up- and down keypress	earlier picture - left/up keypress; later picture - right/down keypress	lateral & vertical
Miles et al. (2011)	Can a mind have two time lines? Exploring space-time mapping in Mandarin and English speakers	Experiment 1: Mandarin-English Bilinguals	Pictures of past or future buildings & cities	Categorize the depicted building/city as being from the past or future.	time is task relevant	left/right keypress; up- and down keypress (both with one finger only)	past building/city - left/up keypress; future building/city - right/down keypress	lateral & vertical
Sell & Kaschak (2011)	Processing time shifts affects the execution of motor responses	Experiment 1	three-sentences texts with small and large time shifts (sentence-by-sentence presentation)	Determine, whether the sentence is sensible or not.	time is task irrelevant	keypress (forwards/backwards; with movement)	past word - backwards movement; future word - forwards movement	sagittal
		Experiment 2	three-sentences texts with small and large time shifts (sentence-by-sentence presentation)	Determine, whether the sentence is sensible or not.	time is task irrelevant	keypress (front/back; without movement)	past word - keypress close to body; future word - keypress far from body	sagittal
Hartmann & Mast (2012)	Moving along the mental time line influences the processing of future related words	Experiment 1	Words/phrases (past & future); passive displacement forwards and backwards by means of a motion platform during task performance	Categorize the verbal stimulus to the concepts of future or past.	time is task relevant	left/right keypress (assignment of past and future as a between-subjects factor)	past stimulus - backwards movement; future stimulus - forwards movement	sagittal

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Boroditsky et al. (2011)	within-subjects	58	Mandarin	Main effect of canonicality: F1(1,54) = 10.6, p < .01	3.2558	English Natives also show a main effect of canonicality (F1(1,109) = 10.3, p < .01). However, there is also a canonicality by axis interaction, indicating that English speakers showed a lateral but not a vertical congruency effect (Boroditsky et al. 2011: 126). Thus, the resulting effect size would also be generated by an axis, that is culturally not salient for the language population (i.e. vertical). Since we only incorporate effect sizes of culturally salient axes in the meta-analysis, we did not include the English speakers' effect size of this experiment.
Fuhrman et al. (2011)	within-subjects	56	English	Planned paired t-test for English speakers: t(56) = 2.34, p < .05	2.34	Since the vertical axis (t(56) = 0.03, p = .98) is not culturally salient for English Natives, the resulting effect size is not included in the meta-analysis. For the sagittal axis, no planned t-test is reported for English speakers; but only the main effect of key mapping for both languages. Since independence of subgroups is needed for the meta-analysis, the result of the sagittal axis thus cannot be integrated.
	within-subjects	74	Mandarin	Planned paired t-test for Mandarin speakers on the lateral axis: t(74) = 1.15, p < .15; Planned paired t-test for M. speakers on the vertical axis: t(74) = 2.70, p < .01	lateral: 1.15, vertical: 2.70, Mean = 1.9250	For the sagittal axis, no planned t-test is reported for Mandarin speakers; but only the main effect of key mapping for both languages. Since independence of subgroups is needed for the meta-analysis, the result of the sagittal axis thus cannot be integrated. Also, in order to guarantee independence of subgroups, we took the mean of t-values for the lateral and vertical axes of Mandarin speakers to calculate the effect size. Since Experiment 2 uses a pointing task, that does not allow the recording of reaction times, Experiment 2 is not incorporated into the meta-analysis.
Miles et al. (2011)	within-subjects	25	Mandarin-English	Separate ANOVA for the Mandarin-English bilingual group: main effect of trial type: F(1,24) = 5.76, p < .05	2.4	As there is no interaction of axis x trial type, the effect size for both axes (lateral & vertical) is similar. For English monolinguals, only the effect size of the lateral axis would be of relevance for the meta-analysis (see comments above), however, the size of it is not reported, so English speakers of Experiment 1 cannot be included in the meta-analysis. Since there are not reaction time recordings for Experiment 2, it cannot be incorporated into the meta-analysis either.
Sell & Kaschak (2011)	between-subjects (factor response location: towards/away)	79	English	follow-up ANOVA for large time shifts: F(1,77) = 9.69, p = .003; follow-up ANOVA for small time shifts: F(1,77) = 0.529, p = .471	Large time shifts: 3.1129; Small time shifts: 0.7253; Mean = 1.9191	We thank Andrea Sell for letting us know the result of the separate ANOVA for small time shifts. Also, she informed us, that there were 40 subjects assigned to the away-group and the remaining 39 participants were assigned to the towards-group. To account for independence of subgroups in the meta-analysis, the mean of the t-values of large and small time shifts has been used for calculating the effect size.
	between-subjects (factor response location: towards/away)	76	English	follow-up ANOVA for large time shifts: F(1,74) = 0.939, p = .336; follow-up ANOVA for small time shifts: F(1,74) = 0.408, p = .525	Large time shifts: 0.9690; Small time shifts: 0.6387; Mean = 0.8039	We thank Andrea Sell for letting us know the result of the separate ANOVA for large and small time shifts. Also, she informed us, that there were 40 subjects assigned to the away-group and the remaining 36 participants were assigned to the towards-group. To account for independence of subgroups in the meta-analysis, the mean of the t-values of large and small time shifts has been used for calculating the effect size.
Hartmann & Mast (2012)	within-subjects	32	German	Motion direction x stimulus category: F(1,31) = 4.30, p = .047	2.0736	Since Experiment 2 evaluates an axis that is not culturally salient (vertical axis), the results are not included in the meta-analysis. The authors use Experiment 2 as a control condition (Hartmann & Mast 2012: 1559). Native Language of participants is probably German. It is not reported, but both authors are associated with the University of Bern.

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Kong & You (2012)	Space-time compatibility effects in the auditory modality	Experiment 1	Words (past & future); auditive presentation	Discriminate, whether the word refers to the past or the future.	time is task relevant	left/right keypress	past word - left keypress; future word - right keypress	lateral
		Experiment 2	Prime: words (past & future), auditive presentation; Target: sounds on right or left ear	Press the space bar with the index finger of your right hand as soon as you detect a target.	temporal priming	one-finger keypress	past word - target on left ear; future word - target on right ear	lateral
Ouellet et al. (2012)	A multisensory interaction effect in the conceptual realm of time	Experiment	Words (past & future); auditive presentation on left or right loudspeaker	Discriminate, whether the word refers to the past or the future.	time is task relevant	left/right keypress	past word - left keypress; future word - right keypress	lateral
Ulrich et al. (2012)	With the past behind and the future ahead: Back-to-front representation of past and future sentences	Experiment 1	Sentences (past & future)	Indicate the temporal reference of the sentence. Refrain from responding when the sentence's content is nonsensical.	time is task relevant	slider (forwards/backwards)	past sentence - movement towards body; future sentence - movement away from body (forwards)	sagittal
		Experiment 1 - Replication with SOV word order	Sentences (past & future)	Indicate the temporal reference of the sentence. Refrain from responding when the sentence's content is nonsensical.	time is task relevant	slider (forwards/backwards)	past sentence - movement towards body; future sentence - movement away from body (forwards)	sagittal
		Experiment 2	Sentences (past & future)	Indicate, whether the sentence makes sense or not. Subsequent to the main task: Indicate the temporal reference of the sentence.	time is task irrelevant	slider (forwards/backwards)	past sentence - movement towards body; future sentence - movement away from body (forwards)	sagittal
		Experiment 2 - Replication without secondary task	Sentences (past & future)	Indicate, whether the sentence makes sense or not.	time is task irrelevant	slider (forwards/backwards)	past sentence - movement towards body; future sentence - movement away from body (forwards)	sagittal



Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Kong & You (2012)	within-subjects	20	Chinese	Temporal Reference x Response Location: F(1,19) = 27.73, p < .001	5.2659	
	within-subjects	24	Chinese	Temporal Reference x Target Side: F(1,23) = 10.04, p = .004	3.1686	
Ouellet et al. (2012)	within-subjects	38	Spanish	Temporal Reference x Response Location: F1(1,37) = 3.155, p = .084; Temporal Reference x Target Location: F1(1,37) = 9.645, p < .01	Temporal Reference x Response Location: 1.7762; Temporal Reference x Target Location: 3.1056; Mean = 2.4409	As independence of subgroups is a precondition for the meta-analysis, the mean of the t-values of Temporal Reference x Response Location and Temporal Reference x Target Location have been used for the calculation of effect size.
Ulrich et al. (2012)	within-subjects	58	German	Movement Direction x Temporal Reference: F1(1,56) = 4.39, p = .041	2.0952	
	within-subjects	40	German	Movement Direction x Temporal Reference: F1(1,38) = 4.44, p = .04	2.1071	
	within-subjects	56	German	Movement Direction x Temporal Reference: F1(1,54) = 0.66, p = .42	0.8124	
	within-subjects	60	German	Movement Direction x Temporal Reference: F1(1,58) = 0.05, p = .817	0.2236	

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Eikmeier et al. (2013)	Dimensional overlap between time and space	Experiment 1: Temporal Stimuli	Sentences (past & future)	Indicate whether the presented sentence refers to the past or the future. Refrain from responding when the sentence's content is nonsensical.	time is task relevant	vocal	past sentence - "hinten"; future sentence - "vorne"	sagittal
		Experiment 2: Spatial Stimuli	Sounds (location: in front of/behind participant)	Respond to the location of the sound.	time is task relevant	vocal	Sound from behind - "Vergangenheit"; Sound from in front of the participant - "Zukunft"	sagittal
Rolke et al. (2013)	Priming the mental time-line: Effects of modality and processing mode	Experiment 1	Prime: words (past/future/filler), presented visually; Target: pink or yellow square, presented at center of the screen	Indicate by keypress, whether the color of the square is pink or yellow.	temporal priming	left/right keypress	past word - left keypress; future word - right keypress	lateral
		Experiment 2	Prime: words (past/future/filler), presented auditorily; Target: pink or yellow square, presented at center of the screen	Indicate by keypress, whether the color of the square is pink or yellow.	temporal priming	left/right keypress	past word - left keypress; future word - right keypress	lateral
		Experiment 3	Prime: words (past/future/filler), presented auditorily; Target: pink or yellow square, presented at center of the screen	Indicate by keypress, whether the color of the square is pink or yellow. Refrain from responding when a non-word preceded the square.	temporal priming	left/right keypress	past word - left keypress; future word - right keypress	lateral
		Experiment 4	Prime: words (past/future/filler), presented auditorily; Target: pink or yellow square, presented at center of the screen	Indicate by keypress, whether the color of the square is pink or yellow. Refrain from responding when a word referring to the present preceded the square.	temporal priming	left/right keypress	past word - left keypress; future word - right keypress	lateral
		Experiment 5	Prime: words (past/future; without valence filler), presented auditorily; Target: pink or yellow square, presented at center of the screen	Indicate by keypress, whether the color of the square is pink or yellow.	temporal priming	left/right keypress	past word - left keypress; future word - right keypress	lateral

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Eikmeier et al. (2013)	within-subjects	40	German	Main effect of congruency: F(1,38) = 14.4, p < .001	3.7947	The main effect of congruency subsumes the congruency effect of the experimental and the control group. Even though the control group (responding to the question not with "vorne"/"hinten" but with "Zukunft"/"Vergangenheit") does not import a space-time-congruency effect, since the dimension of space is not present in this set-up, the congruency effect is not modulated by group (Eikmeier et al. 2013: 1122). Thus the resulting effect size of both groups together can be taken as an estimation of the effect size of only one group (i.e. the experimental group).
	within-subjects	40	German	Main effect of congruency: F(1,38) = 21.7, p < .001	4.6583	Again, the main effect of congruency subsumes the congruency effect of experimental and control group. For the same reasons as in Experiment 1, the resulting effect size can still be taken as an appropriate estimation of only the experimental group.
Rolke et al. (2013)	within-subjects	30	German	Temporal Reference x Response Hand: F1(1,29) = 6.3, p = .02	2.50998	
	within-subjects	30	German	Temporal Reference x Response Hand: F1(1,29) = 0.57, p = .4548	0.755	We thank Bettina Rolke for letting us know the exact F-value of the interaction of temporal reference x response hand.
	within-subjects	30	German	Temporal Reference x Response Hand: F1(1,29) = 1.72, p = .20	1.3115	
	within-subjects	22	German	Temporal Reference x Response Hand: F1(1,21) = 14.5, p = .001	3.8079	
	within-subjects	20	German	Temporal Reference x Response Hand: F1(1,19) = 0.07, p = .7920	0.2646	Again, we thank Bettina Rolke for letting us have the exact F-value of the interaction of the two factors.

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Casasanto & Bottini (2014)	Mirror reading can reverse the flow of time	Experiment 1	Three-word temporal phrases (past & future)	Indicate, whether the phrase refers to the past or the future.	time is task relevant	left/right keypress (with index finger of dominant hand)	past phrase - left keypress; future phrase - right keypress	lateral
Rolke et al. (2014)	Crossed hands stay on the time-line	Experiment	Prime: words (past & future), Target: rectangle appearing on the right or left side of the screen	Indicate, at what side of the screen the rectangle appears with a corresponding keypress.	temporal priming	left/right keypress (with crossed & uncrossed hands)	past word - left keypress; future word - right keypress	lateral
Walker et al. (2014)	Disentangling Spatial Metaphors for Time Using Non-spatial Responses and Auditory Stimuli	Deictic Judgments	Life events (past & future; pronoun 'your'), auditorily presented from front or behind/left or right	When does the event take place? (In your past or your future?)	time is task relevant	vocal response ("past"/"future")	past life event - back/left presentation; future life event - front/right presentation	sagittal & lateral
		Sequential Judgments	Life events (earlier & later compared to a reference event; pronoun 'her'), auditorily presented from front or behind/left or right	Does the second event occur earlier or later compared to the first event?	time is task relevant	vocal response ("earlier"/"later")	earlier event - front/left presentation; later event - back/right presentation	sagittal & lateral
Aguirre & Santiago (2015)	Do potential past and future events activate the Lateral Mental Timeline?	Experiment 1	Phrases (past & future) - real & potential	Does the phrase refer to the past or the future?	time is task relevant	left/right keypress	past phrase - left keypress; future phrase - right keypress	lateral
		Experiment 2	Phrases (past & future) - only potential	Does the phrase refer to the past or the future?	time is task relevant	left/right keypress	past phrase - left keypress; future phrase - right keypress	lateral
		Experiment 3	Phrases (past & future) - real & potential	Judge, whether the expression refers to a real or a potential event.	time is task irrelevant	left/right keypress	past phrase - left keypress; future phrase - right keypress	lateral
Bottini et al. (2015)	Space and time in the sighted and blind	Sighted	words (past & future), orally presented	Does the word refer to the past or the future?	time is task relevant	left/right keypress (crossed & uncrossed hands)	past word - left keypress; future word - right keypress	lateral

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Casasanto & Bottini (2014)	within-subjects	52	Dutch	Congruency: F1(1,51) = 24.33, p < .001	4.933	We thank Daniel Casasanto and Roberto Bottini for telling us the corresponding F-value with which we were able to calculate Cohen's d. Since all other space-time congruency effects that are reported in their paper are based on axes that are not culturally salient, they are not included in the meta-analysis even though it is the purpose of their paper to assess, whether a culturally non-salient axis can be used for the mental timeline when supported by context, or task respectively. However, the in their study emerging mental timelines and their resulting congruency effects are still phenomenologically different to the other incorporated effect sizes of the meta-analysis. Thus, only Experiment 1, standard orthography, can be incorporated in the analysis.
Rolke et al. (2014)	within-subjects	20	German	Temporal Reference x Response Key Position: F1(1,19) = 9.7, p = .01	3.1145	This space-time congruency effect is not modulated by response condition (crossed/uncrossed hands) and not by SOA (Rolke et al. 2014: 135).
Walker et al. (2014)	within-subjects	32	English	Sagittal axis: Temporal Reference x Location: F1(1,31) = 0.5597, p = .46; Lateral axis: Temporal Reference x Location: F1(1,31) = 4.28, p = .047	Sagittal axis: 0.7481, Lateral axis: 2.0688, Mean = 1.4085	For the sagittal axis, the F-value of the interaction was not reported. However, we determined it by using the reported p-value and the sample size. Since independence of subgroups is a precondition for the meta-analysis, we took the mean of the t-values of the sagittal and lateral axis to generate Cohen's d.
	within-subjects	32	English	Sagittal axis: Temporal Reference x Location: F1(1,31) = 5.42, p = .027; Lateral axis: Temporal Reference x Location: F1(1,31) = 3.51, p = .07	Sagittal axis: 2.3281, Lateral axis: 1.8735, Mean = 2.1008	Since independence of subgroups is a precondition for the meta-analysis, we took the mean of the t-values of the sagittal and lateral axis to generate Cohen's d. For sequential judgments on the sagittal axis the assignment is reversed: earlier events are responded to faster when they originate in the front than behind the body, whereas later events are responded to faster when presented behind the body compared to in front of the body. Assuming a different assignment still makes the effect size positive, since it is assumed that congruent trials are faster than incongruent. Thus the effect size is still categorized as being positive.
Aguirre & Santiago (2015)	within-subjects	28	Spanish	Time x Response Side: F(1,27) = 8.71, p = .006	2.9513	
	within-subjects	34	Spanish	Time x Response Side: F(1,33) = 6.53, p = .02	2.5554	
	within-subjects	30	Spanish	Time x Response Side: F(1,29) = 0.009, p = .927	0.0949	We thank Roberto Aguirre for letting us know the exact F-value of the Time x Response Side interaction.
Bottini et al. (2015)	within-subjects	16	Italian	Main effect of Congruity: t(15) = 1.7861, p = .04715	1.7861	Since posture does not modulate the congruity effect (Bottini et al. 2015: 70), a t-value is calculated, aggregating both conditions (crossed & uncrossed hands). We thank Roberto Bottini for providing us with the raw data of the experiment.

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Bottini et al. (2015)	Space and time in the sighted and blind	Early Blind	words (past & future), orally presented	Does the word refer to the past or the future?	time is task relevant	left/right keypress (crossed & uncrossed hands)	past word - left keypress; future word - right keypress	lateral
		Late Blind	words (past & future), orally presented	Does the word refer to the past or the future?	time is task relevant	left/right keypress (crossed & uncrossed hands)	past word - left keypress; future word - right keypress	lateral
Ding et al. (2015)	Are past and future symmetric in mental time line?	Experiment 1	Words (near past (yesterday) & near future (tomorrow))	Indicate, whether the time of word was earlier or later than yesterday/tomorrow noon.	time is task relevant	left/right keypress	earlier word - left keypress; later word - right keypress	lateral
		Experiment 2: Distant Past	Words (distant past (last year))	Indicate, whether the time of word was earlier or later than July of last year.	time is task relevant	left/right keypress	earlier word - left keypress; later word - right keypress	lateral
		Experiment 2: Distant Future	Words (distant future (next year))	Indicate, whether the time of word was earlier or later than July of next year.	time is task relevant	left/right keypress	earlier word - left keypress; later word - right keypress	lateral
		Experiment 3	Words (near/distant past/future)	Judge, whether the time of word was earlier or later than present.	time is task relevant	left/right keypress	past word - left keypress; future word - right keypress	lateral
Eikmeier, Hoppe & Ulrich (2015)	Response mode does not modulate the space-time congruency effect: Evidence for a space-time mapping at a conceptual level	Experiment	Words (past & future) presented auditorily	Does the word refer to the past or the future?	time is task relevant	slider (forwards/backwards) & vocal	past word - movement towards body/"hinten"; future word - movement forwards/"vorne"	sagittal
Eikmeier, Alex-Ruf et al. (2015)	How strongly linked are mental time and space along the left-right axis?	Experiment 1: Temporal Stimuli	Sentences (past & future)	Does the sentence refer to the past or the future? Refrain from responding when the sentence is nonsensical.	time is task relevant	vocal	past sentence - "links"; future sentence - "rechts"	lateral

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Bottini et al. (2015)	within-subjects	17	Italian	Main effect of Congruity: t(16) = 2.3464, p .01608	2.3464	Since posture does not modulate the congruity effect (Bottini et al. 2015: 71), a t-value is calculated, aggregating both conditions (crossed & uncrossed hands). We thank Roberto Bottini for providing us with the raw data of the experiment.
	within-subjects	16	Italian	Main effect of Congruity: t(15) = 2.9783, p = .004689	2.9783	Since posture does not modulate the congruity effect (Bottini et al. 2015: 70), a t-value is calculated, aggregating both conditions (crossed & uncrossed hands). We thank Roberto Bottini for providing us with the raw data of the experiment.
Ding et al. (2015)	within-subjects	36	Chinese	Main effect of Response Congruence: F(1,34) = 29.33, p < .001	5.4157	Response Congruence was not modulated by Type of Time Words (Ding et al. 2015: 3), so it's not necessary to generate two different effect sizes for each Group of Type of Time Words (between-subjects factor).
	within-subjects	18	Chinese	F(1,34) = 18.76, p < .001	4.3313	There is an overall F-value for both groups (distant past and distant future). However, separate analyses show, that the effect only is significant for distant past, but not distant future. That's why separate effect sizes are generated here. Since Type of time words is a between-subjects factor, the two effect sizes are independent, thus meeting the precondition of independence for the meta-analysis.
	within-subjects	18	Chinese	F(1,34) = 0.47, p = .49	0.6856	
	within-subjects	36	Chinese	Main effect of response congruence: F(1,34) = 13.09, p < .001	3.618	Since there is no significant interaction of temporal distance and response congruence (Ding et al. 2015: 4), it is not necessary to generate two different effect sizes for each group of participants (near and far temporal distance). Experiment 4 cannot be incorporated, since the F-value of the interaction of type of time words and response key is only reported as being non-significant, but no numeric value is given.
Eikmeier, Hoppe & Ulrich (2015)	within-subjects	40	German	F1(1,39) = 82.16, p < .001	9.0642	We thank Verena Eikmeier for letting us know the F1-value, since only the min F'-value is reported in the paper.
Eikmeier, Alex-Ruf et al. (2015)	within-subjects	60	German	Additional two-sided t-tests: experimental group: congruency effect: t(59)= 2.1, p = .04	2.1	There are 60 participants in each group (experimental & control). However the congruency effect is modulated by group, which is why separate t-tests have been calculated for each group. Since the control group doesn't have a spatial dimension (only temporal cues), it cannot be considered as an effect size of a space-time congruency effect and is not incorporated into the meta-analysis.

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Eikmeier, Alex-Ruf et al. (2015)	How strongly linked are mental time and space along the left-right axis?	Experiment 2: Spatial Stimuli	Sounds (location: left or right of participant)	Do the sounds originate on your left or your right side?	time is task relevant	vocal	Sound on the left side - "Vergangenheit"; Sound on the right side - "Zukunft"	lateral
Maienborn et al. (2015)	Do we map remembrances to the left/back and expectations to the right/front of a mental timeline? Space-time congruency effects with retrospective and prospective verbs	Experiment 1	Sentences (retrospective & prospective verbs)	Does the sentence refer to the past or the future? Refrain from responding when the sentence is nonsensical.	time is task relevant	left/right keypress	past sentence - left keypress; future sentence - right keypress	lateral
		Experiment 2	Sentences (retrospective & prospective verbs)	Indicate, whether the sentence makes sense or not.	time is task irrelevant	left/right keypress	past sentence - left keypress; future sentence - right keypress	lateral
		Experiment 3	Sentences (retrospective & prospective verbs)	Indicate, whether the sentence makes sense or not.	time is task irrelevant	keypress (forwards/backwards movement)	past sentence - movement towards body; future sentence - movement away from body (forwards)	sagittal
De La Vega et al. (2016)	The Mental Timeline in a Crossed-Hands Paradigm: A Matter of Instruction	Key Instruction	Words (past & future)	Does the word refer to the past or the future?	time is task relevant	left/right keypress (crossed & uncrossed hands)	past word - left (side) keypress; future word - right (side) keypress	lateral
Loeffler et al. (2017)	Walking back to the future: The impact of walking backward and forward on spatial and temporal concepts	Experiment 2: Backward Walking	Encoding Phase: The participants learn foods, that a fictive personality liked 10 years ago or will like 10 years in the future. Recognition-Test Phase: backward movement, forward movement, standing. During which: auditorily presented foods that have to be allocated as learned in the encoding phase.	Indicate, whether the item belongs to the past or the future.	time is task relevant	vocal	past item - backward walking	sagittal

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Eikmeier, Alex-Ruf et al. (2015)	within-subjects	30	German	Additional two-sided t-tests: experimental group: congruency effect: t(29)= 1.0, p = .32	1	There are 30 participants in each group (experimental & control). The congruency effect is modulated by group, which is why separate t-tests have been calculated for each group. The congruency effect only is significant for the control group, but since the control group doesn't have a temporal dimension (only spatial cues), it cannot be considered as an effect size of a space-time congruency effect and is not incorporated into the meta-analysis.
Maienborn et al. (2015)	within-subjects	52	German	Temporal Reference x Hand: F1(1,48) = 4.40, p = .041	2.0976	
	within-subjects	52	German	Temporal Reference x Hand: F1(1,48) = 0.05, p = .826	0.2236	
	between-subjects (Groups of Response Direction: toward/away)	100	German	Temporal Reference x Response Direction: F1(1,98) = 0.73, p = .395	0.8544	There are 50 partcipants in each group of response direction (50: toward; 50: away).
De La Vega et al. (2016)	within-subjects	20	German	Temporal Reference x Response side: F1(1,38) = 14.36, p = .001	3.7895	Since the exact F-value of the interaction of time and side for the hand-instruction-group is not reported, it cannot be incorporated into the meta-analysis.
Loeffler et al. (2017)	within-subjects	35	German	Congruency effect for backward walking: t(35) = 3.59, p = .001	3.59	Experiment 1 tests the influence of walking on spatial concepts. Since there is no space-time congruency effect, Experiment 1 is not incorporated into the meta-analysis. During forward walking the response times to past-related and future-related stimuli did not differ (Loeffler et al. 2017: 352). No t-value reported for forward movement; thus, it cannot be considered in the meta-analysis.

Authors	Title	Experiment	Stimulus/Material	Instruction	Task	Response Mode	Congruence	Axis
Walker et al. (2017)	The spatial alignment of time: Differences in alignment of deictic and sequence time along the sagittal and lateral axes	Experiment 1: Deictic Judgments	Life events (past & future; pronoun 'your'), visually presented	When does the event take place? (In your past or your future?)	time is task relevant	mousepress (in front of body/behind body or left/right side of body)	past event - mousepress behind/left of body; future event - mousepress in front of/right of body	sagittal & lateral
		Experiment 1: Sequential Judgments	Life events (earlier & later compared to a reference event; pronoun 'her'), visually presented	Does the second event occur earlier or later compared to the first event?	time is task relevant	mousepress (in front of body/behind body or left/right side of body)	earlier event - mousepress in front of/on left side of body; later event - mousepress behind/on right side of body	sagittal & lateral
		Experiment 2	Life events (earlier & later compared to a reference event), visually presented	Does the second event occur earlier or later compared to the first event?	time is task relevant	mousepress (in front of body/behind body)	earlier event - mousepress in front of body; later event - mousepress behind body	sagittal
Scheifele et al. (2018)	A Replication of 'Processing time shifts affects the execution of motor responses' (Sell & Kaschak, 2011; Experiment 1)	Experiment	three-sentences texts with small and large time shifts (sentence-by-sentence presentation)	Determine, whether the sentence is sensible or not.	time is task irrelevant	keypress (forwards/backwards; with movement)	past word - backwards movement; future word - forwards movement	sagittal

Authors	Design	N	Language	Reported statistics	T-Value	Explanatory notes
Walker et al. (2017)	within-subjects	36	English	Sagittal axis: Temporal Reference x Location: F1(1,35) = 16.82, p < .001, Lateral axis: Temporal Reference x Location: F1(1,35) = 4.31, p = .045	Sagittal axis: 4.1012, Lateral axis: 2.0761; Mean = 3.0887	Since independence of subgroups is a precondition for the meta-analysis, we took the mean of the t-values of the sagittal and lateral axis to generate Cohen's d.
	within-subjects	32	English	Sagittal axis: Temporal Reference x Location: F1(1,31) = 4.73, p = .037; Lateral axis: Temporal Reference x Location: F1(1,31) = 4.77, p = .037	Sagittal axis: 2.1749, Lateral axis: 2.1840; Mean = 2.1795	Since independence of subgroups is a precondition for the meta-analysis, we took the mean of the t-values of the sagittal and lateral axis to generate Cohen's d. For sequential judgments on the sagittal axis the assignment is reversed: earlier events are responded to faster by mousepress in front of the body than behind the body, whereas later events are responded to faster by mousepress behind the body compared to in front of the body. Assuming a different assignment still makes the effect size positive, since it is assumed that congruent trials are faster than incongruent. Thus the effect size is still categorized as being positive.
	within-subjects	36	English	Temporal Reference x Response Location: F1(1,34) = 5.21, p = .029	2.2825	The interaction is modulated by pronoun: The effect only gets significant for the pronoun 'her'. However, to keep the subgroups of the meta-analysis independent, we could not split up the effect sizes for each pronoun. This is why we took the overall F-value (the F-value of the separate analysis of the pronoun 'your' is not reported).
Scheifele et al. (2018)	between-subjects (factor response location: towards/away)	100	German	follow-up ANOVA for large time shifts: F(1, 98) = 6.45, p = .013, follow-up ANOVA for small time shifts: F(1, 98) = 0.44, p = .509	Large time shifts: 2.5397; Small time shifts: 0.6633; Mean = 1.6015	There were 50 subjects assigned to each group of response location (towards: 50, away: 50). To account for independence of subgroups in the meta-analysis, the mean of the t-values of large and small time shifts has been used for calculating the effect size. We didn't use the reported F-value of Direction x Location for the estimation of effect size, in order to keep the method the same as in Sell & Kaschak (2011), of which we only have the F-values of the separate analyses.

Appendix B (refers to Chapter 3)

B.1 Pre-studies

Preceding the study reported in Chapter 3, we conducted four experiments based on the paradigm by Wender and Weber (1982), in which we developed an operative task design that was then used in the present study. In the interest of completeness, we will briefly report the development within this Appendix (see Table B.1.1 for the mean difference scores). All four Experiments only included human motion sentences.

In Pre-Experiment 1, we started with a design relatively close to Wender and Weber's (1982) design. More specifically, sentence presentation was self-paced (i.e., participants pressed the space-bar to proceed to the next sentence). Like in Experiment 1 of Chapter 3, participants were asked to indicate whether the sentences expressed motion. The stimulus material consisted of fast-speed and slow-speed sentences, and of static sentences. To ensure that participants were reading the whole sentence before giving a response, we announced that there would be a recognition test on the experimental sentences subsequent to the RT task.

Pre-Experiment 2 was designed analogous to Experiment 2 of Chapter 3. More specifically, participants were asked to make a sensicality judgment. For this purpose, nonsense sentences were created. Static sentences were not part of the stimulus material. Slight changes in the fast-speed and slow-speed sentences between Pre-Experiment 1 and 2 were undertaken to reduce the range of frequency between the two levels of speed. There was no announcement of a recognition test since the differentiation of sensical and nonsense sentences required the reading of the entire sentences. Sentence presentation was self-paced like in Pre-Experiment 1.

To evaluate whether the announcement of the memory task in Pre-Experiment 1 inhibited the natural reading and reaction process and to assess whether the changes in stimulus material led to behavioural differences between Pre-Experiment 1 and 2, we conducted Pre-Experiment 3. For this purpose, the fast-speed and slow-speed sentences of Pre-Experiment 2 were employed. Participants were instructed to detect motion, yet there was no announcement of a memory task prior to the start of the RT experiment. Additionally, sentence presentation was ended automatically after a presentation of 3.5 s to increase the task flow. In a follow-up questionnaire, the majority of participants reported to have read only the verb in order to decide whether a sentence described motion. This was reflected in mean RTs that were much lower compared to the other studies.

Pre-Experiment 4 was split into two experiments, 4.1 and 4.2. Experiment order was counterbalanced between participants. Pre-Experiment 4.1 employed a Go/NoGo-Design (as described in the Procedure Section of Experiment 1 of Chapter 3) and consisted of the fast-speed and slow-speed sentences of Pre-Experiment 2. Thus, we could ensure that participants were reading the sentences to the end without announcing a memory test that might impede intuitive responding.

Pre-Experiment 4.2 consisted only of verbs and not of entire sentences. The verbs were taken from the stimulus material of Pre-Experiment 2 and were presented both as uninflected verbs and verbs inflected in third person singular in present tense. The instruction was to indicate, whether the verbs expressed motion.

Table B.1.1. ***Difference scores in the Pre-Experiments (PE) of Chapter 3 after removal of outliers and incorrect responses.***

	fast	*slow*	Raw differences in means [95% CI]	Cohen's *d* [95% CI]
PE 1	1289 (502)	1333 (530)	52.08 [14.33, 89.83]	0.44 [0.12, 0.77]
PE 2	1602 (546)	1572 (502)	-40.11 [-71.86, -8.37]	-0.40 [-0.73, -0.08]
PE 3	940 (350)	934 (308)	-8.21 [-39.72, 23.29]	-0.08 [-0.40, 0.23]
PE 4.1	1393 (452)	1444 (494)	46.66 [13.29, 80.02]	0.45 [0.12, 0.78]
PE 4.2	798 (288)	830 (286)	29.07 [-3.12, 61.27]	0.29 [-0.03, 0.61]
4.2 uninflected	815 (323)	816 (276)	0.16 [-41.68, 42.00]	0.00 [-0.31, 0.32]
4.2 inflected	781 (249)	843 (296)	59.23 [21.48, 96.97]	0.50 [0.17, 0.84]

Note. Same criteria for outliers were applied as described for Experiment 1 in Chapter 3. The first two columns represent the raw means (and *SD*s) of RTs (in ms) for the two levels of Speed. The raw differences in means (in ms) and their confidence intervals are obtained by aggregating the data across participants. Adapted in its format from "Is rushing always faster than strolling? A reaction time study on the processing of sentences containing manner of motion verbs" by L. von Sobbe, R. Ulrich, L. Gangloff, E. Scheifele, & C. Maienborn, 2021, *Acta Psychologica*, *221*, https://doi.org/10.1016/j.actpsy.2021.103428, p. 13. *Creative Commons Attribution 4.0 International License* (CC BY) (https://creativecommons.org/licenses/by/4.0/).

B.2 Stimulus Material of Experiment 1 and 2

List 1		List 2	
Speed	Human motion sentences	Speed	Human motion sentences
slow	Anna wandert die Küste entlang.	fast	Anna sprintet die Küste entlang.
slow	Benno latscht die Allee entlang.	fast	Benno prescht die Allee entlang.
slow	Clara humpelt das Ufer entlang.	fast	Clara hastet das Ufer entlang.
slow	Patrick bummelt das Gleis entlang.	fast	Patrick huscht das Gleis entlang.
slow	Emma torkelt den Bach entlang.	fast	Emma flitzt den Bach entlang.
slow	Nils wankt den Gehweg entlang.	fast	Nils saust den Gehweg entlang.
fast	Gudrun joggt die Klippen entlang.	slow	Gudrun hinkt die Klippen entlang.
fast	Klaus rast die Linie entlang.	slow	Klaus schreitet die Linie entlang.
fast	Iris stürmt die Promenade entlang.	slow	Iris spaziert die Promenade entlang.
fast	Jonathan jagt den Fluss entlang.	slow	Jonathan schlendert den Fluss entlang.
fast	Katharina eilt den Weg entlang.	slow	Katharina trottet den Weg entlang.
fast	Ingo rennt den Pfad entlang.	slow	Ingo schlurft den Pfad entlang.
fast	Rebecca sprintet über die Alm.	slow	Rebecca wandert über die Alm.
fast	Daniel prescht über den Schulhof.	slow	Daniel latscht über den Schulhof.
fast	Vera hastet über die Wiese.	slow	Vera humpelt über die Wiese.
fast	Tina huscht über den Dorfplatz.	slow	Tina bummelt über den Dorfplatz.
fast	Christian flitzt über die Straße.	slow	Christian torkelt über die Straße.
fast	Ulrike saust über den Hinterhof.	slow	Ulrike wankt über den Hinterhof.
slow	Florian hinkt über die Kreuzung.	fast	Florian joggt über die Kreuzung.
slow	Sarah schreitet über die Tanzfläche.	fast	Sarah rast über die Tanzfläche.
slow	Heinrich spaziert über den Strand.	fast	Heinrich stürmt über den Strand.
slow	Pia schlendert übers Feld.	fast	Pia jagt übers Feld.
slow	Kai trottet über den Parkplatz.	fast	Kai eilt über den Parkplatz.
slow	Nora schlurft über den Flur.	fast	Nora rennt über den Flur.
slow	Moritz wandert durchs Gebirge.	fast	Moritz sprintet durchs Gebirge.
slow	Hannah latscht durchs Schloss.	fast	Hannah prescht durchs Schloss.
slow	Robert humpelt durch den Wald.	fast	Robert hastet durch den Wald.
slow	Franziska bummelt durchs Dorf.	fast	Franziska huscht durchs Dorf.
slow	Sebastian torkelt durchs Museum.	fast	Sebastian flitzt durchs Museum.
slow	Daniela wankt durch die Menschenmenge.	fast	Daniela saust durch die Menschenmenge.
fast	Alexander joggt durch die Stadt.	slow	Alexander hinkt durch die Stadt.
fast	Barbara rast durch den Garten.	slow	Barbara schreitet durch den Garten.

fast	Annika stürmt durch den Park.	slow	Annika spaziert durch den Park.
fast	Bernd jagt durch die Empfangshalle.	slow	Bernd schlendert durch die Empfangshalle.
fast	Claudia eilt durchs Kino.	slow	Claudia trottet durchs Kino.
fast	Simon rennt durch die Gasse.	slow	Simon schlurft durch die Gasse.
fast	Ina sprintet zum See.	slow	Ina wandert zum See.
fast	Niklas prescht zur Tafel.	slow	Niklas latscht zur Tafel.
fast	Katja hastet zum Rastplatz.	slow	Katja humpelt zum Rastplatz.
fast	Lukas huscht zum Rathaus.	slow	Lukas bummelt zum Rathaus.
fast	Michelle flitzt zum Krankenhaus.	slow	Michelle torkelt zum Krankenhaus.
fast	Jan saust zur Schule.	slow	Jan wankt zur Schule.
slow	Olivia hinkt zum Markt.	fast	Olivia joggt zum Markt.
slow	Günther schreitet zur Veranstaltung.	fast	Günther rast zur Veranstaltung.
slow	Ronja spaziert zur Brücke.	fast	Ronja stürmt zur Brücke.
slow	Erik schlendert zum Supermarkt.	fast	Erik jagt zum Supermarkt.
slow	Tanja trottet zur Kirche.	fast	Tanja eilt zur Kirche.
slow	Yvonne schlurft zur Eisdiele.	fast	Yvonne rennt zur Eisdiele.

List 1		List 2	
Speed	Object motion sentences	Speed	Object motion sentences
slow	Der Düsenjet segelt die Insel entlang.	fast	Der Düsenjet zischt die Insel entlang.
slow	Der Segelflieger schwebt die Landebahn entlang.	fast	Der Segelflieger rauscht die Landebahn entlang.
slow	Die Dampflok kriecht die Gleise entlang.	fast	Die Dampflok rattert die Gleise entlang.
slow	Der Neuwagen rollt die Teststrecke entlang.	fast	Der Neuwagen schießt die Teststrecke entlang.
fast	Der Bus düst die Haltespur entlang.	slow	Der Bus tuckert die Haltespur entlang.
fast	Der Bollerwagen brettert den Feldweg entlang.	slow	Der Bollerwagen holpert den Feldweg entlang.
fast	Der Hubschrauber donnert den Waldrand entlang.	slow	Der Hubschrauber gleitet den Waldrand entlang.
fast	Das Motorboot braust das Ufer entlang.	slow	Das Motorboot treibt das Ufer entlang.
fast	Der Zeppelin zischt über die Stadt.	slow	Der Zeppelin segelt über die Stadt.
fast	Das Segelflugzeug rauscht über den Wald.	slow	Das Segelflugzeug schwebt über den Wald.
fast	Der ICE rattert über die Brücke.	slow	Der ICE kriecht über die Brücke.
fast	Der Sportwagen schießt über die Ziellinie.	slow	Der Sportwagen rollt über die Ziellinie.
slow	Der LKW tuckert über die Landstraße.	fast	Der LKW düst über die Landstraße.
slow	Der Traktor holpert über den Acker.	fast	Der Traktor brettert über den Acker.
slow	Das Motorrad gleitet über die Autobahn.	fast	Das Motorrad donnert über die Autobahn.
slow	Das Kreuzfahrtschiff treibt über das Meer.	fast	Das Kreuzfahrtschiff braust über das Meer.

slow	Das Schiff segelt durch den Fjord.	fast	Das Schiff zischt durch den Fjord.
slow	Das Flugzeug schwebt durch die Luft.	fast	Das Flugzeug rauscht durch die Luft.
slow	Die Metro kriecht durch den Schacht.	fast	Die Metro rattert durch den Schacht.
slow	Der Rennwagen rollt durch die Boxengasse.	fast	Der Rennwagen schießt durch die Boxengasse.
fast	Der Geländewagen düst durch das Unterholz.	slow	Der Geländewagen tuckert durch das Unterholz.
fast	Das Quad brettert durch den Parcours.	slow	Das Quad holpert durch den Parcours.
fast	Der Panzer donnert durch die Straßen.	slow	Der Panzer gleitet durch die Straßen.
fast	Die Yacht braust durch die Hafeneinfahrt.	slow	Die Yacht treibt durch die Hafeneinfahrt.
fast	Das Sportflugzeug zischt zum Hangar.	slow	Das Sportflugzeug segelt zum Hangar.
fast	Die Seilbahn rauscht zum Gipfel.	slow	Die Seilbahn schwebt zum Gipfel.
fast	Die Eisenbahn rattert zum Bahnhof.	slow	Die Eisenbahn kriecht zum Bahnhof.
fast	Das Taxi schießt zur Hofeinfahrt.	slow	Das Taxi rollt zur Hofeinfahrt.
slow	Der Oldtimer tuckert zum Messegelände.	fast	Der Oldtimer düst zum Messegelände.
slow	Der Jeep holpert zum Wasserloch.	fast	Der Jeep brettert zum Wasserloch.
slow	Der Helikopter gleitet zum Unfallort.	fast	Der Helikopter donnert zum Unfallort.
slow	Das Segelboot treibt zum Anlegeplatz.	fast	Das Segelboot braust zum Anlegeplatz.
slow	Die Drohne segelt ins Tal.	fast	Die Drohne zischt ins Tal.
slow	Die Rakete schwebt in die Atmosphäre.	fast	Die Rakete rauscht in die Atmosphäre.
slow	Die Straßenbahn kriecht in die Unterführung.	fast	Die Straßenbahn rattert in die Unterführung.
slow	Der Smart rollt in die Kurve.	fast	Der Smart schießt in die Kurve.
fast	Das Auto düst in die Sperrzone.	slow	Das Auto tuckert in die Sperrzone.
fast	Der Bagger brettert in den Tunnel.	slow	Der Bagger holpert in den Tunnel.
fast	Die Vespa donnert in die Fußgängerzone.	slow	Die Vespa gleitet in die Fußgängerzone.
fast	Die Fähre braust in den Hafen.	slow	Die Fähre treibt in den Hafen.
fast	Das Raumschiff zischt auf den Mond.	slow	Das Raumschiff segelt auf den Mond.
fast	Der Sessellift rauscht auf die Spitze.	slow	Der Sessellift schwebt auf die Spitze.
fast	Die Zahnradbahn rattert auf den Berg.	slow	Die Zahnradbahn kriecht auf den Berg.
fast	Das Skateboard schießt auf die Kreuzung.	slow	Das Skateboard rollt auf die Kreuzung.
slow	Die Pistenraupe tuckert auf den Schneehügel.	fast	Die Pistenraupe düst auf den Schneehügel.
slow	Der BMW holpert auf den Bürgersteig.	fast	Der BMW brettert auf den Bürgersteig.
slow	Der Airbus gleitet auf das Rollfeld.	fast	Der Airbus donnert auf das Rollfeld.
slow	Der Fischkutter treibt auf das offene Meer.	fast	Der Fischkutter braust auf das offene Meer.

List 1 & List 2		
Human static sentences	Human nonsense sentences	
Finn verharrt an der Küste.	nonsense: movement	Clara drängt die Windböe hinauf.
Petra bleibt in der Allee.	nonsense: movement	Nils kriecht das Gemälde vernünftig.
Hannes sitzt am Ufer.	nonsense: movement	Gudrun krabbelt die Tür gesund.
Frank steht an den Steilhängen.	nonsense: movement	Iris pirscht die Kirschbaumblätter heraus.
Leonie ist auf dem Gehweg.	nonsense: movement	Olga stampft durch den Artikel.
Karl wartet bei den Klippen.	nonsense: movement	Daniel stiefelt den Schnee holzig.
Jana hockt auf der Linie.	nonsense: movement	Christian tänzelt durch die Waschmaschine.
Martin wohnt an der Promenade.	nonsense: movement	Katharina läuft auf die Kirsche ein.
Helena liegt am Fluss.	nonsense: movement	Sarah tollt den Ball eckig.
Ole ruht neben dem Weg.	nonsense: movement	Kai trippelt den Tisch über.
Fiona kauert auf dem Pfad.	nonsense: movement	Max wandelt das Einhorn schläfrig.
Manuel kniet am Abgrund.	nonsense: movement	Oskar watschelt den Verkehr flüssig.
Doris monologisiert am Bach.	nonsense: movement	Julia wuselt über den CO2-Ausstoß.
Antonia verschnauft auf der Alm.	nonsense: movement	Franziska zuckelt die Straßenbahn quer.
Ben verweilt auf dem Schulhof.	nonsense: movement	Alexander tanzt die Musik lecker.
Charlotte schläft auf der Wiese.	nonsense: movement	Ralf durchquert die Geige.
Stefan döst im Flur.	nonsense: movement	Gabi radelt das Pflaster lätschig.
Raphael liest im Hinterhof.	nonsense: movement	Katja zieht den Kirchturm quer.
Gisela lebt an der Kreuzung.	nonsense: movement	Ronja verläuft die Farbe entlang.
Peter jubelt auf der Tanzfläche.	nonsense: movement	Erik galoppiert zum Zeilenumbruch.
Isabelle kichert auf dem Dorfplatz.	nonsense: movement	Tanja trabt durch den Textkörper.
Nico gähnt auf dem Feld.	nonsense: movement	Clemens stolpert das Salto entlang.
Karin gestikuliert auf dem Parkplatz.	nonsense: movement	Ute schleudert durch das Gespräch.
Lars meditiert auf der Lichtung.	nonsense: movement	David rutscht die Diskussion auf.
Mira beobachtet die Hochebene.	nonsense: static	Benno sitzt den Druck hindurch.
Julius duscht am Strand.	nonsense: static	Patrick steht die Vorhangschiene hinein.
Regina malt das Gebirge.	nonsense: static	Jonathan kniet das Leder grün.
Theresa rastet im Wald.	nonsense: static	Katharina schaukelt den Stuhl groß.
Christoph zeichnet das Dorf.	nonsense: static	Marie wartet die Seife entlang.
Dennis sieht den Park.	nonsense: static	Emil wohnt das Shampoo entlang.
Ursula isst in der Stadt.	nonsense: static	Rebecca hockt das Garn entzwei.
Felix kontrolliert die Menschenmenge.	nonsense: static	Heinrich ruht die Maus entlang.
Sabine strickt im Garten.	nonsense: static	Pia kauert das Bild bunt.
Hendrik musiziert im Museum.	nonsense: static	Nora verschnauft das Schlafzimmer ein.

Paula schaut durch die Gegend.	nonsense: static	Laura schläft durch den Baum.
Julian arbeitet im Kino.	nonsense: static	Moritz döst den Lippenstift zusammen.
Nicole posiert vor der Gasse.	nonsense: static	Hannah gestikuliert die Lampe entlang.
Karsten schnarcht im Gebüsch.	nonsense: static	Robert gähnt die Milch über.
Lisa fotografiert den Raum.	nonsense: static	Daniela kichert das Blatt wolkig.
Oliver sonnt sich am See.	nonsense: static	Annika strickt die Kugel waldig.
Heike schreibt an die Tafel.	nonsense: static	Claudia dreht den Hund rosa.
Tim parkt auf dem Rastplatz.	nonsense: static	Simon rotiert die Veranda haushoch.
Phil liegt im Krankenhaus.	nonsense: static	Niklas wirft das Motorrad ratlos.
Dagmar lernt im Klassenzimmer.	nonsense: static	Lukas bricht den Bürgermeister müde.
Anton feilscht auf dem Markt.	nonsense: static	Michelle haut den Computer lockig.
Birgit versackt in der Kneipe.	nonsense: static	Günther beschleunigt die Herberge.
Cornelia singt in der Kirche.	nonsense: static	Yvonne balanciert die Formatierung.
Esther kauft beim Bäcker ein.	nonsense: static	Sandra musiziert das Reiten locker.

List 1 & List 2

Object static sentences	Object nonsense sentences	
Der Düsenjet stoppt auf der Insel.	nonsense: movement	Das Motocrossrad stürzt den Baum quadratisch.
Der Segelflieger verharrt auf der Landebahn.	nonsense: movement	Das Wohnmobil knattert über das Gesprächsthema.
Die Dampflok pausiert auf dem Gleis.	nonsense: movement	Der Roller eiert die Wiese scharf.
Der Neuwagen brennt auf der Teststrecke.	nonsense: movement	Das Floß fährt den Teich leer.
Der Bus versperrt die Haltespur.	nonsense: movement	Der Aufzug erscheint oben am Pfannenstiel.
Der Bollerwagen zerbricht auf dem Feldweg.	nonsense: movement	Der Güterzug dampft den Bahnübergang heimwärts.
Der Hubschrauber brummt auf dem Landeplatz.	nonsense: movement	Das Polizeiauto biegt den Strand kaputt.
Das Motorboot legt am Ufer an.	nonsense: movement	Der Rettungswagen wendet zügig an der Sonne.
Der Zeppelin blinkt über der Stadt.	nonsense: movement	Der Katamaran schippert schwungvoll ins Koma.
Das Segelflugzeug hängt in den Bäumen.	nonsense: movement	Der Laster röhrt über die Wolken.
Der ICE zerschellt an der Brücke.	nonsense: movement	Der Schlitten überwindet zögernd Uranus.
Der Sportwagen qualmt an der Ziellinie.	nonsense: movement	Der Porsche schlittert den Boden aus dem Ring.
Der LKW hupt auf der Landstraße.	nonsense: movement	Der Kinderwagen taumelt den Weg kopfüber.

Der Traktor steckt im Acker fest.	nonsense: movement	Der Rollator kracht von hinten an das Lied.
Das Motorrad lehnt an der Zapfsäule.	nonsense: movement	Der Stocherkahn gondelt den Fluss gelb.
Das Kreuzfahrtschiff ist auf dem Meer.	nonsense: movement	Das Ruderboot trudelt den Wasserfall entzwei.
Das Schiff dümpelt im Fjord.	nonsense: movement	Das Boxauto rammt das Pferd scheckig.
Das Flugzeug glänzt in der Luft.	nonsense: movement	Der Achterbahnwagen schlenkert den Looping hungrig.
Die Metro wartet im Schacht.	nonsense: movement	Der Airbus fliegt dank Salz und Zucker.
Der Rennwagen tankt in der Boxengasse.	nonsense: movement	Der Cityroller umrundet die Schallmauer.
Der Geländewagen steckt im Unterholz.	nonsense: movement	Die Achterbahn klappert die Kurve schön.
Das Quad verkeilt sich im Parcours.	nonsense: movement	Der Linienbus pendelt zwischen Ästen empor.
Der Panzer verteidigt das Dorf.	nonsense: movement	Die Tram steuert geradeaus nach unten.
Die Yacht belegt die Hafeneinfahrt.	nonsense: movement	Die U-Bahn wackelt durch den Bienenstock.
Das Sportflugzeug entsteht im Hangar.	nonsense: static	Die Schwebebahn öffnet die Tür bunt.
Die Seilbahn ist am Gipfel.	nonsense: static	Das Müllauto stinkt die Kirche reich.
Die Eisenbahn hält am Bahnhof.	nonsense: static	Der Transporter wiegt außergewöhnlich schön.
Das Taxi hupt vor der Hofeinfahrt.	nonsense: static	Der Shuttlebus raucht gefährlich in die Tüte.
Der Oldtimer begeistert auf der Automesse.	nonsense: static	Das Dampfschiff ankert mit dem Knoten in der Küche.
Der Jeep erreicht das Wasserloch.	nonsense: static	Der Kahn verbrennt unbemerkt das Ofenholz.
Der Helikopter funkt vom Unfallort.	nonsense: static	Der Mercedes rostet an den Haarspitzen.
Das Segelboot lagert im Bootshaus.	nonsense: static	Der Gabelstapler piepst die Melodie auswendig.
Die Drohne fotografiert das Tal.	nonsense: static	Der Abschleppwagen zieht die Hauswand empor.
Die Rakete explodiert in der Atmosphäre.	nonsense: static	Das Fahrrad klingelt in die Wiese.
Die Straßenbahn leuchtet in der Unterführung.	nonsense: static	Der Jetski blitzt hinter dem Monolog.
Der Smart bleibt in der Kurve liegen.	nonsense: static	Die Kutsche zerdrückt den Wunsch genüsslich.
Das Auto parkt in der Sperrzone.	nonsense: static	Der Fallschirm zerreißt die Sonne eckig.
Der Bagger gräbt im Tunnel.	nonsense: static	Der Reisebus wirbt mit Schnee für Nudeln.

Die Vespa hält in der Fußgängerzone.	nonsense: static	Der Mähdrescher lärmt den Raps reif.
Die Fähre blockiert den Hafen.	nonsense: static	Der Wagen schließt das Blumenbeet.
Das Raumschiff berührt die Mondoberfläche.	nonsense: static	Der Krankenwagen ertönt von der Mondfinsternis her.
Der Sessellift stoppt an der Spitze.	nonsense: static	Das Taxi dreht in der neuen Woche.
Die Zahnradbahn knarzt am Bahnsteig.	nonsense: static	Der Rollstuhl stützt die Zeitung hinaus.
Das Skateboard zersplittert auf der Kreuzung.	nonsense: static	Das Postauto behindert die Litfaßsäule mechanisch.
Die Pistenraupe plättet den Schneehügel.	nonsense: static	Der Eiswagen liefert kalte Gefühle.
Der BMW parkt auf dem Bürgersteig.	nonsense: static	Das Bobbycar verstaubt nachts rückwärts.
Der Airbus steht vor dem Rollfeld.	nonsense: static	Der Rasenmäher stockt am Rand der Verzweiflung.
Der Fischkutter schaukelt auf dem Wasser.	nonsense: static	Das Baustellenfahrzeug verfärbt die Stimmung schwerwiegend.

Appendix C (refers to Chapter 4)

C.1 Distance Rating

The study was designed to obtain pairs of PPs that only differed with respect to the distance they denoted in the context of a sentence describing human motion by foot. For this purpose, 120 nouns in combination with a preposition were collected that were matched as potential pairs based on number of characters and frequency class. Moreover, 22 additional nouns were included in the stimulus material that served as potential substitutes in case the ratings of the 120 pre-classified and pre-matched nouns would be rated too inconsistently or should not correspond to the pre-classification. To prevent that distance ratings were biased by an assumed or associated means of transportation all prepositional phrases were combined with the same proper name and the same motion verb (i.e., *laufen* 'walk') to yield an entire sentence. *Laufen* was chosen because it is relatively neutral with respect to its manner and denotes a speed that was previously rated to be in the middle area of the 7 point Likert scale (rated speed of *laufen* in the pre-study described in Chapter 3: $M = 4.18$; rated speed of *laufen* in the rating study described in Appendix D.1: $M = 4.22$). Thus, participants saw sentences of the form *Theo läuft zum Hafen* 'Theo is walking to the harbour' (pre-classified as a long-PP) or *Theo läuft zum Tisch* 'Theo is walking to the table' (pre-classified as a matched short-PP) and were asked to indicate on a 7 point Likert scale (1 = *sehr kurz* 'very short'; 7 = *sehr weit* 'very long'), how far Theo was going to walk (*Wie weit läuft Theo?*). To provide the participants with a reference frame for the rating, they were told that *sehr kurz* was implying a distance of centimetres to a few meters, while *sehr weit* was implying a distance of a few kilometres. Moreover, they were given examples prior to the rating (see Figure C.1.1). We informed participants that the information at which place Theo starts to walk was not given and that we wanted them to answer as intuitively as possible despite this missing information.

To ensure that participants were paying attention to the task and understood the instruction 20 control items were included in the stimulus material. More specifically, twelve sentences that did not denote a distance (e.g., *Theo steht am Eingang* 'Theo is standing at the entrance') and eight sentences that denoted a distance, but which exceeded the scale according to the instruction (e.g., *Theo läuft zum Südpol* 'Theo is walking to the South Pole') were added to the stimulus material. An additional scale with the options *keine Distanz* 'no distance' and *nicht abbildbar* 'not representable' was incorporated on which participants were asked to respond in case of control items. Thus, the study consisted of 162 items in total.

The study was programmed in PsychoPy v2020.1.3. 40 participants were recruited via Prolific (www.prolific.co).[28] The data of three of the initial 40 participants was replaced by the data of three new participants, since their accuracy for the control items was below or equal to 75%. Pre-selection criterion for PPs to be included in the stimulus

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[28] The data was anonymized subsequent to data collection.

material was that the standard deviation of the ratings did not exceed 1.5 and that the mean ratings of the PPs had a difference of at least 0.7 to the mean of the scale.[29] The remaining PPs were arranged according to length and frequency, which resulted in a final selection of 48 short-PPs and 48 long-PPs that were matched as pairs.

Figure C.1.1. ***A screenshot taken from the rating study that was used as illustration for participants to serve as example for a long distance (i.e.,* Nachbardorf *'neighbouring village').***

Theo läuft ins Nachbardorf.

Wie weit läuft Theo?

○ ○ ○ ○ ○ ○ ●

sehr kurz … sehr weit

Alternativ:

○ keine Distanz

○ nicht abbildbar

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[29] Short-PPs were allowed to have a maximum mean rating of 2.8, long-PPs a minimum mean rating of 4.2. These values were considered boundaries, because the value of 3.5 was taken as the scale's mean by mistake.

C.2 Stimulus Material

List 1			List 2		
Speed	Distance	Motion sentences	Speed	Distance	Motion sentences
slow	long	Anna bummelt auf die Anhöhe.	slow	long	Anna bummelt zur Bibliothek.
slow	short	Anna bummelt auf den Laufsteg.	slow	short	Anna bummelt zum Gartentor.
fast	long	Anna rennt auf die Anhöhe.	fast	long	Anna rennt zur Bibliothek.
fast	short	Anna rennt auf den Laufsteg.	fast	short	Anna rennt zum Gartentor.
slow	long	Benno hinkt auf den Weinberg.	slow	long	Benno hinkt zum Hafen.
slow	short	Benno hinkt auf den Balkon.	slow	short	Benno hinkt zum Tisch.
fast	long	Benno stürmt auf den Weinberg.	fast	long	Benno stürmt zum Hafen.
fast	short	Benno stürmt auf den Balkon.	fast	short	Benno stürmt zum Tisch.
slow	long	Clara humpelt zum Krankenhaus.	slow	long	Clara humpelt zum Casino.
slow	short	Clara humpelt zum Wandschrank.	slow	short	Clara humpelt zum Fenster.
fast	long	Clara prescht zum Krankenhaus.	fast	long	Clara prescht zum Casino.
fast	short	Clara prescht zum Wandschrank.	fast	short	Clara prescht zum Fenster.
slow	long	Patrick latscht auf das Postamt.	slow	long	Patrick latscht zur Zahnarztpraxis.
slow	short	Patrick latscht auf die Terrasse.	slow	short	Patrick latscht zum Wasserspender.
fast	long	Patrick huscht auf das Postamt.	fast	long	Patrick huscht zur Zahnarztpraxis.
fast	short	Patrick huscht auf die Terrasse.	fast	short	Patrick huscht zum Wasserspender.
slow	long	Emma schlendert zum Marktplatz.	slow	long	Emma schlendert zum Wasserfall.
slow	short	Emma schlendert zur Steckdose.	slow	short	Emma schlendert zum Kühlregal.
fast	long	Emma jagt zum Marktplatz.	fast	long	Emma jagt zum Wasserfall.
fast	short	Emma jagt zur Steckdose.	fast	short	Emma jagt zum Kühlregal.
slow	long	Nils schlurft zum Kino.	slow	long	Nils schlurft zur Markthalle.
slow	short	Nils schlurft zum Zaun.	slow	short	Nils schlurft zum Stuhlkreis.
fast	long	Nils hastet zum Kino.	fast	long	Nils hastet zur Markthalle.
fast	short	Nils hastet zum Zaun.	fast	short	Nils hastet zum Stuhlkreis.
slow	long	Gudrun schreitet auf den Berggipfel.	slow	long	Gudrun schreitet zum See.
slow	short	Gudrun schreitet auf die Eisfläche.	slow	short	Gudrun schreitet zum Sofa.
fast	long	Gudrun flitzt auf den Berggipfel.	fast	long	Gudrun flitzt zum See.
fast	short	Gudrun flitzt auf die Eisfläche.	fast	short	Gudrun flitzt zum Sofa.
slow	long	Klaus taumelt in die Altstadt.	slow	long	Klaus taumelt ins Dorf.
slow	short	Klaus taumelt in den Garten.	slow	short	Klaus taumelt ins Zelt.
fast	long	Klaus eilt in die Altstadt.	fast	long	Klaus eilt ins Dorf.
fast	short	Klaus eilt in den Garten.	fast	short	Klaus eilt ins Zelt.

slow	long	Iris stiefelt zur Berghütte.	slow	long	Iris stiefelt zum Park.
slow	short	Iris stiefelt zum Wecker.	slow	short	Iris stiefelt zum Auto.
fast	long	Iris spurtet zur Berghütte.	fast	long	Iris spurtet zum Park.
fast	short	Iris spurtet zum Wecker.	fast	short	Iris spurtet zum Auto.
slow	long	Jonathan torkelt auf das Revier.	slow	long	Jonathan torkelt zur Arztpraxis.
slow	short	Jonathan torkelt auf die Bühne.	slow	short	Jonathan torkelt zum Briefkasten.
fast	long	Jonathan sprintet auf das Revier.	fast	long	Jonathan sprintet zur Arztpraxis.
fast	short	Jonathan sprintet auf die Bühne.	fast	short	Jonathan sprintet zum Briefkasten.
slow	long	Katharina trottet zum Fitnessstudio.	slow	long	Katharina trottet zur Herberge.
slow	short	Katharina trottet zum Lichtschalter.	slow	short	Katharina trottet zur Werkbank.
fast	long	Katharina saust zum Fitnessstudio.	fast	long	Katharina saust zur Herberge.
fast	short	Katharina saust zum Lichtschalter.	fast	short	Katharina saust zur Werkbank.
slow	long	Ingo wankt zur Kneipe.	slow	long	Ingo wankt zum Schloss.
slow	short	Ingo wankt zur Garage.	slow	short	Ingo wankt zum Spiegel.
fast	long	Ingo rast zur Kneipe.	fast	long	Ingo rast zum Schloss.
fast	short	Ingo rast zur Garage.	fast	short	Ingo rast zum Spiegel.

List 3			List 4		
Speed	Distance	Motion sentences	Speed	Distance	Motion sentences
slow	long	Anna bummelt zur Baustelle.	slow	long	Anna bummelt zur Landebahn.
slow	short	Anna bummelt zur Rezeption.	slow	short	Anna bummelt zum Vogelhaus.
fast	long	Anna rennt zur Baustelle.	fast	long	Anna rennt zur Landebahn.
fast	short	Anna rennt zur Rezeption.	fast	short	Anna rennt zum Vogelhaus.
slow	long	Benno hinkt zum Freibad.	slow	long	Benno hinkt zur Höhle.
slow	short	Benno hinkt zum Schuppen.	slow	short	Benno hinkt zum Grill.
fast	long	Benno stürmt zum Freibad.	fast	long	Benno stürmt zur Höhle.
fast	short	Benno stürmt zum Schuppen.	fast	short	Benno stürmt zum Grill.
slow	long	Clara humpelt zum Theater.	slow	long	Clara humpelt zum Tierheim.
slow	short	Clara humpelt zum Computer.	slow	short	Clara humpelt zum Backofen.
fast	long	Clara prescht zum Theater.	fast	long	Clara prescht zum Tierheim.
fast	short	Clara prescht zum Computer.	fast	short	Clara prescht zum Backofen.
slow	long	Patrick latscht zur Kegelhalle.	slow	long	Patrick latscht zur Zeitungsredaktion.
slow	short	Patrick latscht zur Käsetheke.	slow	short	Patrick latscht zur Umkleidekabine.
fast	long	Patrick huscht zur Kegelhalle.	fast	long	Patrick huscht zur Zeitungsredaktion.
fast	short	Patrick huscht zur Käsetheke.	fast	short	Patrick huscht zur Umkleidekabine.

slow	long	Emma schlendert zum Rummelplatz.	slow	long	Emma schlendert zum Biergarten.
slow	short	Emma schlendert zum Wühltisch.	slow	short	Sarah schlendert zum Kühlschrank.
fast	long	Emma jagt zum Rummelplatz.	fast	long	Emma jagt zum Biergarten.
fast	short	Emma jagt zum Wühltisch.	fast	short	Emma jagt zum Kühlschrank.
slow	long	Nils schlurft zur Pferdekoppel.	slow	long	Nils schlurft zum Friedhof.
slow	short	Nils schlurft zum Spielautomaten.	slow	short	Nils schlurft zum Buffet.
fast	long	Nils hastet zur Pferdekoppel.	fast	long	Nils hastet zum Friedhof.
fast	short	Nils hastet zum Spielautomaten.	fast	short	Nils hastet zum Buffet.
slow	long	Gudrun schreitet zum Museum.	slow	long	Gudrun schreitet zur Kirche.
slow	short	Gudrun schreitet zum Regal.	slow	short	Gudrun schreitet zum Gemälde.
fast	long	Gudrun flitzt zum Museum.	fast	long	Gudrun flitzt zur Kirche.
fast	short	Gudrun flitzt zum Regal.	fast	short	Gudrun flitzt zum Gemälde.
slow	long	Klaus taumelt zur Praxis.	slow	long	Klaus taumelt zum Sportplatz.
slow	short	Klaus taumelt zur Tafel.	slow	short	Klaus taumelt zum Sitzplatz.
fast	long	Klaus eilt zur Praxis.	fast	long	Klaus eilt zum Sportplatz.
fast	short	Klaus eilt zur Tafel.	fast	short	Klaus eilt zum Sitzplatz.
slow	long	Iris stiefelt zum Gipfelkreuz.	slow	long	Iris stiefelt zum Restaurant.
slow	short	Iris stiefelt zur Hantelbank.	slow	short	Iris stiefelt zum Blumenbeet.
fast	long	Iris spurtet zum Gipfelkreuz.	fast	long	Iris spurtet zum Restaurant.
fast	short	Iris spurtet zur Hantelbank.	fast	short	Iris spurtet zum Blumenbeet.
slow	long	Jonathan torkelt zum Rathaus.	slow	long	Jonathan torkelt zur Feuerwache.
slow	short	Jonathan torkelt zum Telefon.	slow	short	Jonathan torkelt zum Infostand.
fast	long	Jonathan sprintet zum Rathaus.	fast	long	Jonathan sprintet zur Feuerwache.
fast	short	Jonathan sprintet zum Telefon.	fast	short	Jonathan sprintet zum Infostand.
slow	long	Katharina trottet zur Fabrikhalle.	slow	long	Katharina trottet zum Büro.
slow	short	Katharina trottet zum Waschbecken.	slow	short	Katharina trottet zum Herd.
fast	long	Katharina saust zur Fabrikhalle.	fast	long	Katharina saust zum Büro.
fast	short	Katharina saust zum Waschbecken.	fast	short	Katharina saust zum Herd.
slow	long	Ingo wankt zum Bahnhof.	slow	long	Ingo wankt zum Messegelände.
slow	short	Ingo wankt zur Haustür.	slow	short	Ingo wankt zum Kleiderschrank.
fast	long	Ingo rast zum Bahnhof.	fast	long	Ingo rast zum Messegelände.
fast	short	Ingo rast zur Haustür.	fast	short	Ingo rast zum Kleiderschrank.

List 1 - 4

Static sentences	Nonsense sentences	
Finn verharrt an der Küste.	nonsense: movement	Clara drängt die Windböe hinauf.
Petra bleibt in der Allee.	nonsense: movement	Nils kriecht das Geschirr vernünftig.
Hannes sitzt am Ufer.	nonsense: movement	Gudrun krabbelt die Tür gesund.
Frank steht an den Steilhängen.	nonsense: movement	Iris pirscht die Kirschbaumblätter heraus.
Leonie ist auf dem Gehweg.	nonsense: movement	Olga stampft durch den Artikel.
Karl wartet bei den Klippen.	nonsense: movement	Daniel marschiert den Schnee holzig.
Jana hockt auf der Linie.	nonsense: movement	Christian tänzelt durch die Waschmaschine.
Martin wohnt an der Promenade.	nonsense: movement	Katharina läuft auf die Kirsche ein.
Helena entspannt am Fluss.	nonsense: movement	Sarah tollt den Ball eckig.
Ole ruht neben dem Weg.	nonsense: movement	Kai trippelt die Schrift über.
Fiona kauert auf dem Pfad.	nonsense: movement	Max wandelt das Einhorn schläfrig.
Manuel kniet am Abgrund.	nonsense: movement	Oskar watschelt den Verkehr flüssig.
Doris monologisiert am Bach.	nonsense: movement	Julia wuselt über den CO2-Ausstoß.
Antonia verschnauft auf der Alm.	nonsense: movement	Franziska zuckelt die Straßenbahn quer.
Ben verweilt auf dem Schulhof.	nonsense: movement	Alexander tanzt die Musik lecker.
Charlotte schläft auf der Wiese.	nonsense: movement	Ralf durchquert die Geige.
Stefan döst im Flur.	nonsense: movement	Gabi radelt das Pflaster lätschig.
Raphael liest im Hinterhof.	nonsense: movement	Katja zieht den Kirchturm quer.
Gisela lebt an der Kreuzung.	nonsense: movement	Ronja verläuft die Farbe entlang.
Peter jubelt auf der Tanzfläche.	nonsense: movement	Erik galoppiert zum Zeilenumbruch.
Isabelle kichert auf dem Dorfplatz.	nonsense: movement	Tanja trabt durch den Textkörper.
Nico gähnt auf dem Feld.	nonsense: movement	Clemens stolpert das Salto entlang.
Karin gestikuliert auf dem Parkplatz.	nonsense: movement	Ute schleudert durch das Gespräch.
Lars meditiert auf der Lichtung.	nonsense: movement	David rutscht die Diskussion auf.
Mira beobachtet die Hochebene.	nonsense: static	Benno riecht den Druck hindurch.
Julius duscht am Strand.	nonsense: static	Patrick pausiert die Vorhangschiene hinein.
Regina malt das Gebirge.	nonsense: static	Jonathan friert das Leder grün.
Theresa rastet im Wald.	nonsense: static	Katharina schaukelt den Stuhl groß.
Christoph zeichnet den Gletscher.	nonsense: static	Marie dämmert die Seife entlang.
Dennis sieht die Konditorei.	nonsense: static	Emil thront das Shampoo entlang.
Ursula isst in der Stadt.	nonsense: static	Rebecca haust das Garn entzwei.
Felix kontrolliert die Menschenmenge.	nonsense: static	Heinrich schweigt die Maus entlang.
Sabine strickt im Wohnzimmer.	nonsense: static	Pia zögert das Bild bunt.
Hendrik musiziert im Foyer.	nonsense: static	Nora atmet das Schlafzimmer aus.

Paula schaut durch die Gegend.	nonsense: static	Laura träumt durch den Baum.
Julian arbeitet im Stall.	nonsense: static	Moritz kampiert den Lippenstift zusammen.
Nicole posiert vor der Gasse.	nonsense: static	Hannah niest die Lampe entlang.
Karsten schnarcht im Gebüsch.	nonsense: static	Robert zweifelt die Milch über.
Lisa fotografiert den Raum.	nonsense: static	Daniela hält das Blatt wolkig.
Oliver sonnt sich am Meer.	nonsense: static	Annika befindet die Kugel waldig.
Heike schreibt auf der Schreibmaschine.	nonsense: static	Claudia dreht den Hund rosa.
Tim parkt auf dem Rastplatz.	nonsense: static	Simon rotiert die Veranda haushoch.
Phil liegt im Sand.	nonsense: static	Niklas wirft das Motorrad ratlos.
Dagmar lernt im Klassenzimmer.	nonsense: static	Lukas bricht den Bürgermeister müde.
Anton feilscht auf dem Basar.	nonsense: static	Michelle haut den Salat lockig.
Birgit versackt an der Bar.	nonsense: static	Günther beschleunigt die Synagoge.
Cornelia singt im Konzertsaal.	nonsense: static	Yvonne balanciert die Formatierung.
Esther kauft beim Bäcker ein.	nonsense: static	Sandra starrt das Reiten locker.

Appendix D (refers to Chapter 5)

D.1 Speed rating

Motion verbs of the pre-study were extracted from Schröder (1993) based on the pre-condition that they denoted human motion. This resulted in the incorporation of 71 motion verbs out of the 191 motion verbs that are listed in Schröder. Additionally, five control verbs denoting no motion (four verbs) or being a non-word (one verb) were included in the pre-study. Since some of Schröder's verbs are antiquated, participants were asked to either rate the speed of the motion verbs on a 7-point scale from 1 (very slow) to 7 (very fast), or choose one of the three alternative options *Word does not denote locomotion*, *I am not acquainted with the word*, or *Speed is not univocal*. The pre-study was programmed using PsychoPy v1.90.3. Pre-selection criteria for motion verbs as stimulus material was that at least 80% of the participants had selected to rate the speed (applied to 39 of the 71 motion verbs) and that standard error of the speed rating was below 0.2 (applied to 49 of the 71 motion verbs and led to the exclusion of 2 of the 39 previously mentioned motion verbs). Additionally, a balance of fast and slow motion, frequency class, number of characters, and number of syllables was considered in the selection of the final stimulus material, which consisted of 28 motion verbs. The mean percentage of participants who chose to rate the speed and did not make use of the alternative options for these 28 motion verbs was 93.13% (SD = 4.35%) and the mean standard error of the speed rating was 0.13 (SD = 0.02). Two further verbs, *schlurfen* and *joggen* that are not listed by Schröder, were included in the stimulus material. The two verbs' speed had been rated by 40 participants in a different pre-study of another, independent experiment. Participants in that pre-study were only asked to rate the speed on a 7-point scale. They did not have alternative options to choose from. To ensure that the rating was comparable in both pre-studies, a Pearson correlation was calculated on the means of the 26 motion verbs that were tested in both pre-studies (r = .99), showing a high correspondence. To classify the verbs as either slow or fast, a value of 3.5 in the speed rating was taken as boundary value.

The resulting group of slow-speed verbs had a mean rating of 2.36 (SD = 0.59), while the group of fast-speed verbs had a mean rating of 5.68 (SD = 0.68). This difference of means was significant $t(14) = 31.60$, $p < .001$. Importantly, neither the difference of number of characters, number of syllables, nor frequency was significantly different in the two speed of motion verb conditions (number of characters: $t(27.54) = 1.51$, $p = .143$; number of syllables: $t(28) = 0.00$, $p > .999$; frequency: $t(21.06) = 0.61$, $p = .551$).

D.2 Sensitivity analysis for Experiment 1 – including *Block* as predictor

To evaluate whether including Block as predictor had an impact on the results of the fixed factors included in the preregistered analysis of Experiment 1, we predicted RD as a function of Stimulus Duration (1050 ms and 1300 ms; mean centred), Speed (*fast* and *slow*; with *fast* as reference category), Number of Characters (encoded numerically and mean centred), and Block (three levels, effect coded). The random effects structure was determined analogous to the random effects structure of the main analysis. The one with the best AIC value included random intercepts for Subjects and Items, and random slopes for the factor Speed for Subjects. All reduced models converged such that model comparison using χ^2 difference tests could in principle be conducted. However, to provide better comparability, *p*-values were again obtained using the Satterthwaite's approximation for degrees of freedom of the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R (Version 4.0.3, 2020). The full model suggests an intercept of β = 1598.22 ms. As in the main analysis, the estimated coefficients of Stimulus Duration (β = 587.75, *SE* = 35.43, *t*(10028.77) = 16.59, *p* < .001), Number of Characters (β = 7.13, *SE* = 1.62, *t*(65.44) = 4.41, *p* < .001), and Speed (β = 95.57, *SE* = 37.36, *t*(53.62) = 2.56, *p* = .013) were significant. The estimated coefficients of Block 1 (β = -94.91, *SE* = 6.26, *t*(10030.80) = -15.17, *p* < .001) and Block 2 (β = 27.08, *SE* = 6.27, *t*(10032.30) = 4.32, *p* < .001) were also significant. This yields an estimated mean of 1503.31 ms for Block 1, of 1625.30 ms for Block 2, and of 1666.05 ms for Block 3.

D.3 LMEM for Experiment 1 including *Frequency* as predictor

As outlined in the preregistration, we conducted an exploratory analysis to assess the potential effect of the verbs' frequencies on RDs. Firstly, a repeated measures correlation was calculated using the *rmcorr* package in R (Bakdash & Marusich, 2017). The mean correlation coefficient across participants was not significant (*r*(10141) = .00, 95% CI [-0.02, 0.02], *p* = .899). Secondly and in addition to the preregistered exploratory analysis, we included Frequency as fixed factor in the LMEM. Thus, we predicted RD as a function of Stimulus Duration (1050 ms and 1300 ms; mean centred), Speed (*fast* and *slow*; with *fast* as reference category), Number of Characters (encoded numerically and mean centred), and Frequency (encoded numerically and mean centred). The random effects structure was determined analogous to the random effects structure of the main analysis. There was only one model that converged and that did not over-fit the data. It included random intercepts for Subjects and Items. To provide comparability, *p*-values were again obtained using the Satterthwaite's approximation for degrees of freedom of the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R (Version 4.0.3, 2020). The full model suggests an intercept of *β* = 1597.94 ms. As in the main analysis, the estimated coefficients of Stimulus Duration (*β* = 584.22, *SE* = 37.05, *t*(10095.93) = 15.77, *p* < .001), Number of Characters (*β* = 7.19, *SE* = 1.57, *t*(61.52) = 4.57, *p* < .001), and Speed (*β* = 76.47, *SE* = 9.44, *t*(9804.17) = 8.10, *p* < .001) were significant. However, the estimated coefficient of Frequency was not significant (*β* = -0.76, *SE* = 2.03, *t*(481.84) = -0.37, *p* = .709).

D.4 Sensitivity analysis for Experiment 2 – including *Block* as predictor

Analogous to Appendix D.2 and Experiment 1, Block was included as predictor in the preregistered analysis of Experiment 2. Consequently, RD was predicted as a function of Stimulus Duration (900 ms, 1100 ms, and 1300 ms; mean centred), Speed (*fast* and *slow*; with *fast* as reference category), Number of Characters (mean centred), Verb Form (*infinitive*, *inflected with personal pronoun*, and *inflected without personal pronoun*; *infinitive* was used as reference category), an interaction of Speed and Verb Form, and Block (three levels, effect encoded). The random effects structure was determined analogously to the previous analyses. The one with the best AIC value included random intercepts for Subjects and Items, and random slopes for the factor Speed for Subjects and for Items, as well as random slopes for the factor Stimulus Duration for Subjects. Since the full model did not converge when estimated with maximum likelihood instead of restricted maximum likelihood, model comparison using χ^2 difference tests could not be conducted. Thus, *p*-values were again obtained using the Satterthwaite's approximation for degrees of freedom of the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R (Version 4.0.3, 2020). The full model suggests an intercept of β = 1273.57 ms. As in the main analysis, the estimated coefficients of Stimulus Duration (β = 563.89, *SE* = 35.12, *t*(58.86) = 16.06, $p < .001$), Number of Characters (β = 7.62, *SE* = 2.74, *t*(30.09) = 2.78, *p* = .009), the Verb Form *inflected with personal pronoun* (β = 48.90, *SE* = 11.40, *t*(159.25) = 4.29, $p < .001$), and the interaction of Speed with the Verb Form *inflected without a personal pronoun* (β = 24.39, *SE* = 12.18, *t*(15444.47) = 2.00, *p* = .045) were significant. Neither Speed in the reference Verb Form *infinitive* (β = 13.23, *SE* = 13.05, *t*(33.25) = 1.01, *p* = .318), nor the Verb Form *inflected without personal pronoun* (β = 0.45, *SE* = 8.86, *t*(5511.31) = 0.05, *p* = .959), nor the interaction of Speed and the Verb Form *inflected with personal pronoun* (β = -12.33, *SE* = 12.18, *t*(15652.94) = -1.01, *p* = .311) were significant. However, the estimated coefficients of Block 1 (β = -65.62, *SE* = 3.50, *t*(15750.31) = -18.74, $p < .001$) and Block 2 (β = 16.21, *SE* = 3.51, *t*(15750.27) = 4.62, $p < .001$) were both significant. This yields an estimated mean of 1207.95 ms for Block 1, of 1289.78 ms for Block 2, and of 1322.98 ms for Block 3.

D.5 LMEM for Experiment 2 with Verb Form List 3 as reference category

The re-estimated LMEM with Verb Form List 3 (i.e., *inflected without personal pronoun*) as reference category contained the same fixed effects as the preregistered analysis of Experiment 2. RD was predicted as a function of Stimulus Duration (900 ms, 1100 ms, and 1300 ms; mean centred), Speed (*fast* and *slow*; with *fast* as reference category), Number of Characters (mean centred), Verb Form (*infinitive*, *inflected with personal pronoun*, and *inflected without personal pronoun*; *inflected without personal pronoun* was used as reference category), and an interaction of Speed and Verb Form. The random effects structure was determined analogously to the previous analyses. The one with the best AIC value included random intercepts for Subjects and Items, and random slopes for the factor Speed for Subjects and for Items, as well as random slopes for the factor Stimulus Duration for Subjects. Due to the same reason as outlined in the analysis of Experiment 1, *p*-values were again obtained using the Satterthwaite's approximation for degrees of freedom of the summary method of the *lmerTest* function (Kuznetsova et al., 2017) in R (Version 4.0.3, 2020). The full model suggests an intercept of β = 1274.04 ms. The estimated coefficients of Stimulus Duration (β = 564.00, *SE* = 35.06, $t(58.86) = 16.09$, $p < .001$), Number of Characters (β = 7.71, *SE* = 2.75, $t(29.99) = 2.80$, $p = .009$), and the Verb Form *inflected with personal pronoun* (β = 48.21, *SE* = 13.05, $t(97.07) = 3.70$, $p < .001$) were significant. Importantly, the effect of Speed in the reference Verb Form *inflected without a personal pronoun* was also significant (β = 37.56, *SE* = 13.30, $t(34.06) = 2.82$, $p = .008$). Moreover, the effect of Speed in the other two Verb Form Lists were significantly different from the effect of Speed in the reference Verb Form (Speed × Verb Form *infinitive*: β = -24.55, *SE* = 12.33, $t(15460.27) = -1.99$, $p = .046$; Speed × Verb Form *inflected with personal pronoun*: β = -36.86, *SE* = 12.29, $t(15760.04) = -3.00$, $p = .003$). The Verb Form *infinitive* was not significant (β = -0.39, *SE* = 8.96, $t(5632.55) = -0.04$, $p = .965$).

D.6 Sensitivity analysis for Experiment 3 – more lenient outlier handling

To ensure that the results of the main analyses were not sensitive with respect to the exclusion of the seven participants, whose estimated TBPs were outside the range of the comparsion durations (i.e., below 400 ms or above 1200 ms), we conducted the analyses as outlined in the Results Section of Experiment 3 of Chapter 5, but included the respective seven participants. Like in the main analysis there was only a main effect of Comparison Duration on the proportions of ‘long’ responses [$F(4, 2024) = 5299.57$, $p < .001$, $\eta_p^2 = .91$, *p*-value was Greenhouse-Geisser adjusted], but no main effect of Speed [$F(1, 506) = 0.36$, $p = .549$, $\eta_p^2 < .01$], and no interaction of Speed and Comparison Duration: [$F(4, 2024) = 2.19$, $p = .084$, $\eta_p^2 < .01$, *p*-value was Greenhouse-Geisser adjusted].

Analogously, there was no significant main effect of Speed in the three one-way ANOVAs on TBP [$F(1, 506) = 0.63$, $p = .429$, $\eta_p^2 < .01$.], JND [$F(1, 506) = 1.62$, $p = .204$, $\eta_p^2 < .01$], and Weber fraction [$F(1, 506) = 0.10$, $p = .746$, $\eta_p^2 < .01$].

D.7 Stimulus Material of Experiment 1

Item	Expressions with slow-speed verbs	Expressions with fast-speed verbs
1	über die Wiese hinken	über die Wiese marschieren
2	zur Tafel humpeln	zur Tafel huschen
3	durch den Park schlurfen	durch den Park joggen
4	zum Gipfelkreuz taumeln	zum Gipfelkreuz eilen
5	zum Fluss torkeln	zum Fluss hasten
6	durchs Museum bummeln	durchs Museum spurten
7	über die Straße wanken	über die Straße preschen
8	zum Rathaus schlendern	zum Rathaus hetzen
9	über den Schulhof latschen	über den Schulhof rennen
10	zur Bahnhofshalle trotten	zur Bahnhofshalle sausen
11	zur Almhütte spazieren	zur Almhütte jagen
12	zum Rastplatz stiefeln	zum Rastplatz stürmen
13	zum Turm schreiten	zum Turm flitzen
14	übers Feld gehen	übers Feld sprinten
15	die Küste entlang wandern	die Küste entlang rasen
16	die Treppe hoch hinken	die Treppe hoch marschieren
17	zum Gartentor humpeln	zum Gartentor huschen
18	die Allee entlang schlurfen	die Allee entlang joggen
19	auf die Brücke taumeln	auf die Brücke eilen
20	zur Kapelle torkeln	zur Kapelle hasten
21	zur Schule bummeln	zur Schule spurten
22	zur Tür wanken	zur Tür preschen
23	zum Eingang schlendern	zum Eingang hetzen
24	zum Supermarkt latschen	zum Supermarkt rennen
25	ans Ufer trotten	ans Ufer sausen
26	durch die Gasse spazieren	durch die Gasse jagen
27	den Pfad hoch stiefeln	den Pfad hoch stürmen
28	den Flur entlang schreiten	den Flur entlang flitzen
29	zum Tisch gehen	zum Tisch sprinten
30	den Weinberg runter wandern	den Weinberg runter rasen
31	den Bürgersteig entlang hinken	den Bürgersteig entlang marschieren
32	zum Apfelbaum humpeln	zum Apfelbaum huschen
33	zur Raststätte schlurfen	zur Raststätte joggen

34	zur Apotheke taumeln	zur Apotheke eilen
35	zur Couch torkeln	zur Couch hasten
36	zur Haltestelle bummeln	zur Haltestelle spurten
37	auf den Balkon wanken	auf den Balkon preschen
38	zum Zigarettenautomat schlendern	zum Zigarettenautomat hetzen
39	zum See latschen	zum See rennen
40	zum Gartenhaus trotten	zum Gartenhaus sausen
41	durch den Kreuzgang spazieren	durch den Kreuzgang jagen
42	in die Scheune stiefeln	in die Scheune stürmen
43	durch die Bibliothek schreiten	durch die Bibliothek flitzen
44	ins Bootshaus gehen	ins Bootshaus sprinten
45	durch die Savanne wandern	durch die Savanne rasen

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